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TRACING MATHEMATICAL ECONOMICS:
ESSAYS IN THE HISTORY OF (DEPARTMENTS OF) ECONOMICS

RASTREANDO A ECONOMIA MATEMÁTICA: ENSAIOS SOBRE A HISTÓRIA DE (DEPARTAMENTOS DE) ECONOMIA

São Paulo
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Tracing Mathematical Economics: Essays in the History of (Departments of) Economics

Tese apresentada ao Programa de Pós-Graduação em Teoria Econômica do Departamento de Economia da Faculdade de Economia, Administração e Contabilidade da Universidade de São Paulo, como requisito parcial para a obtenção do título de Doutor em Ciências.

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To my cats: Roberto Carlos, Rita Lee, Elza Soares and Zezinho

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Writing a doctoral dissertation is never an easy task, but doing it amidst a global pandemic caused by a novel virus proved to be even harder. It would not be possible to conclude this work without the altruistic help of multiple people and the support of my friends and family. On this personal note, I would like to thank my mother for her unconditional support for my decision to follow an academic career and my spouse Michelle Allendes for her loving care throughout the ups and downs of writing this dissertation and for her support, patience, and understanding in the past few years.

The more science is absolutely pure, the more it is intimately bound up with the fabric of society (Latour 1993, 43)

RESUMO EM LÍNGUA PORTUGUESA

Esta tese estuda a história do desenvolvimento da economia matemática no século XX através de histórias de departamentos de economia. No primeiro capítulo, estudamos como o pequeno departamento de economia da Universidade de Stanford tornou-se um importante lugar para o desenvolvimento da economia matemática desde o período entreguerras, beneficiando-se de relações interdisciplinares particulares ao contexto acadêmico de Stanford. No período pós-guerra, o crescimento das ciências comportamentais em Stanford permitiu que a economia matemática superasse as limitações de tamanho do departamento de economia, associando-se em grupos interdisciplinares com a matemática, estatística e psicologia. No segundo capítulo, estudamos como a economia matemática encontrou se espaço dentro do departamento de economia da Universidade da California, Berkeley, anteriormente dominado por uma tradição metodológica com pouco interesse pela economia matemática. Apesar da presença na universidade de alguns importantes economistas matemáticos como Griffith Evans no período entreguerras e Robert Dorfman no imediato pós-guerra, foi apenas no fim da década de 1950 que a economia matemática conseguiu garantir seu espaço no departamento de economia, com a mobilização da administração universitária cumprindo papel central nesse processo. Finalmente, o último capítulo contribui para a história da economia matemática no Brasil seguindo a história da disciplina no Instituto de Matemática Pura e Aplicada (IMPA). O IMPA, embora dedicado à matemática, manteve laços com a disciplina da economia desde os primeiros anos através do então jovem engenheiro Mário Henrique Simonsen que frequentou o instituto nos anos 1950. A partir do início da década de 1980, o IMPA criou um programa de pós-graduação em economia matemática organizado por Aloisio Araujo, ex-aluno do instituto que havia retornado ao Brasil após concluir o PhD no exterior. As três experiências distintas contribuem para uma história mais completa do complexo processo do crescimento e disseminação da economia matemática no século XX.

Palavras-chave: História da economia matemática, Stanford, IMPA, Berkeley, História do pensamento econômico recente

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ABSTRACT

This dissertation studies the history of the development of mathematical economics in the XX century through histories of economics departments. In the first chapter, we study how the small economics department at Stanford University became an important place for the development and expansion of mathematical economics in the United States since the interwar period, benefitting from cross-disciplinary connections which were particular to Stanford's institutional environment. In the postwar period, the rise of the behavioral sciences at Stanford allowed mathematical economics to bypass the limitations of the smaller department of economics, through the association with mathematics, statistics, and psychology. In the second chapter, we analyze how mathematical economics was able to find room at the department of economics at the University of California, Berkeley, despite the presence of an established economics tradition which mostly scorned the use of mathematics in the discipline. Despite the presence of some important mathematical economists in the university, such as Griffith Evans in the interwar period, and Robert Dorfman in the immediate postwar period, it was only by the end of the 1950s that mathematical economics could hold on its space in the department of economics, with the mobilization of university administration playing a central role in this process. Finally, the last chapter contributes to the history of the internationalization of mathematical economics to Brazil, by following the history of the Institute of Pure and Applied Mathematics (IMPA – *Instituto de Matemática Pura e Aplicada*). IMPA was created through the actions of the Brazilian mathematical community, but since its beginnings held some connections to economics. Although it began as an idiosyncratic connection through the young engineer turned economist Mário Henrique Simonsen, mathematical economics at IMPA rose to be considered an important part of applied mathematics research at the institute. Through this process of breaking fuzzy discipline borders, IMPA performed as a disseminator of the subject to Brazil and became an international actor within such intellectual community. By telling such different histories, the dissertation sheds light on the complex and multifaceted process of expansion and dissemination of mathematical economics in the XX century.

Keywords: History of mathematical economics, Stanford, Berkeley, IMPA, History of recent economics

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INTRODUCTION: Tracing mathematical economics

> My dictionary defines "literary economics" as "euphemism for non-mathematical economist." (Samuelson 1954, 359)
>
> Non-mathematical contributions to economic analysis often tend to be fat, sloppy, and vague (…) Clarity of thought characterizes mathematical economics. (Klein 1954, 360)
>
> Why do so many graduate students want to include a little mathematics in their education? (…) Why is the line between mathematical economists and general economic theorists growing fuzzier and vaguer? As a good Darwinian I believe that is no accident (…) Survival in the literature is a test of fitness, if an imperfect one. If mathematical techniques continue to produce good economics then, still as Darwinian, I predict that long before the appendix has disappeared from the human digestive tract most people interested in economic theory will as a matter of course learn some mathematics. (Solow 1954, 373-374)

Historians of economics in the past decades have debated the transformation of economics in the twentieth century from a verbal to a mathematical science (Morgan and Rutherford 1998, Mirowski 2002, Weintraub 2002, Leonard 2010, Erikson et al 2013, Düppe and Weintraub 2014b, Leeds 2016, and many others). Although explicit defenses of using mathematics to approach economic problems can be traced back at least to French engineers in the middle of the XIX century (Ingrao and Israel 1991, chapter 3; Ekelund and Hébert 1999), the process that turned mathematical economics from an exotic to a hegemonic way of doing economics in the United States during the twentieth century is of particular relevance due to the predominant role played by American economics in the dissemination of the subject in the post-World War II period (Coats 1996). The transformation of American economics "from interwar pluralism to postwar neoclassicism" (Morgan and Rutherford 1998) was not an American-only story. The influence of scholar's immigration to the United States, initially due to the pressures of war in Europe, and then by the prominent role of American academia in the international community, was fundamental to the development of

mathematical economics. What remains distinctly American in those episodes of the history of mathematical economics is the fact that those scholars, coming from various backgrounds and places, were able to construct mathematical economics by their association through American institutions. The institutional architecture of American universities has thus played a defining role in the organization, expansion and dissemination of mathematical economics in the second half of the twentieth century.

Multiple institutions have acted as supporters or patrons of academic economics, such as the higher education system, the US federal government and the military, private foundations, think-tanks, research institutions, and business corporations (Goodwin 1998). A fundamental difference between the universities and other institutions was their exclusive ability of granting a PhD degree. Fourcade (2009, chapter 2) has observed that a major element of the professionalization of economics in the United States was "the redefinition of the PhD, an academic credential providing evidence of specialized scholarly competence, as the primary mechanism for certifying expertise in both scientific and practical matters" (ibid, 72). In particular the departmentalization of universities gave to departments of economics the unique power of granting a specialized PhD in economics. As a result, the identity of American economics has been firmly located within the department of economics.[1] Therefore, a history of the transformation of American economics must also be a history of the transformation of the departments of economics.

This dissertation studies the history of the dissemination of mathematical economics after World War II by taking a closer look at important developments within the departments of economics and their relationships with other departments such as statistics and mathematics. It shall tell the history of three different places. In Part I, we will be concerned with the development of mathematical economics in the departments of economics of two important institutions established around the San Francisco area: Stanford University and the University of California, Berkeley (which will be often referred as only 'Stanford 'or 'Berkeley ' throughout the dissertation).In those departments, mathematical economics not only carved

[1] The centrality of the department of economics, however, does not mean that scholars from other fields have been prevented from contributing to mathematical economics by some institutional gatekeeping mechanism. On the contrary, the cross-disciplinary character of mathematical economics has engaged and mobilized mathematicians, statisticians, engineers, among others. However, it is the case that the identity of economics has been intimately related to what happens within departments of economics. Even in the case of concepts developed in other disciplines – for instance, 'Nash equilibrium', 'Shapley's value', and 'fixed point theorems– ', they have become relevant to economics as they were mobilized and used by economists within departments of economics.

out a central place in research and teaching, but also established connections with departments abroad.

The dissemination of mathematical economics across national borders is part of the complex and multifaceted process of internationalization of economics taking place during the postwar years. The historical process of the internationalization of the discipline has been a topic of interest for historians of economics for some time. An important volume was edited by Coats (1996), with research on this process in multiple countries. Aslanbeigui and Montecinos (1998) wrote about the importance of foreign students in PhD programs in the United States for this internationalization (see also Montecinos and Markoff 2010). Coats (2000) have also written on the internationalization of economics in Europe. More recent work include Fourcade's (2009) book on the professionalization and the emergence of particular academic cultures in the United States, France and Britain, Maes and Buyst's (2005) research on Belgium economics, and Rossier and Bülhman (2018) on Switzerland. The particular histories of economics in different regions of the world are shaped by the much distinct institutional contexts of each place. Part II of the dissertation will tell the history of the dissemination of mathematical economics from American universities to Brazil. The Brazilian academic context is too distinct from its American counterpart. In the first decades of the XXth century in Brazil, universities were new institutions in the making. The first one was created in the interwar period gathering together traditional professional schools of medicine, engineering, and law.[2] The development of a local scientific community was much inspired initially by the institutional framework of countries like France and Italy in particular, and then the United States in the Cold War period. Problems of economic development mobilized most of the scientific interest and funding in economics in Brazil. In this context, more abstract threads of American mathematical economics reached Brazil through the international scientific community of mathematics. In the last chapter, we explore such movements by centering our narrative in a research institute of mathematics, the *Instituto de Matemática Pura e Aplicada* (IMPA, Institute of Pure and Applied Mathematics). IMPA encouraged research in mathematical economics as a way of developing research in applied mathematics. The institute held an important proximity with the University of California, Berkeley and its Departments of Mathematics and Statistics, and the development of mathematical economics in those departments supported the incursions of IMPA into

[2] The first universities in Brazil were the Universidade do Paraná (1912), Universidade de Manaus (1913), and Universidade do Distrito Federal (currently named Universidade Federal do Rio de Janeiro) (1920).

mathematical economics. Through former students with interests in economics and mathematics who later began teaching at the institute, an important intellectual community in mathematical economics was created at IMPA. Through the networks weaved with departments of economics in Brazil and the American academia, IMPA was able to play an important role in the dissemination of mathematical economics to Brazil.

1. The changing meaning of mathematical economics: Defining a scientific community

The conceptual and methodological framework underlying the construction of the histories presented in this dissertation must be made explicit before we move on to the main chapters. First of all, it is necessary to be explicit about what we understand by "mathematical economics". There is more than a single possible definition for such scientific group, and as a result there is more than one possible history of mathematical economics to be told. However, a discussion about which is the best definition of mathematical economics will be avoided. That would require a thorough philosophical discussion which is not the goal of this project. Nevertheless, a workable definition of the scientific group associated with mathematical economics in the US is necessary for the delineation of our history.

The designation "mathematical economics" refers to the scientific practices put forward by a social group in a particular time and place. We remain agnostic on which are those practices, letting the relevant actors define them by themselves. Using Bruno Latour's (2005) actor-network theory framework, it must be acknowledged that "social aggregates are not the object of an ostensive definition (…) but only of a performative definition. They are made by the various ways and manners they are said to exist" (ibid, 34). Instead of pre-establishing a fixed list of requirements for a scholar to be classified as a 'mathematical economist', the puzzle which the historian must deal with is how a diverse array of scholars, institutions, theories and scientific practices in their particular contexts became identified with this specific label, mathematical economics. Latour presents a list of traces left by the formation of such groups, which can be helpful for their identification:

> First, to delineate a group, no matter if it has to be created from scratch or simply refreshed, you have to have spokesperson which 'speak for 'the group existence (…) all [groups] need some people defining who they are,

> what they should be, what they have been. These are constantly at work, justifying the group's existence, invoking rules and precedents, and (…) measuring up one definition against all others. (…)
>
> Second, whenever some work has to be done to trace or retrace the boundary of a group, other groupings are designated as empty, archaic, dangerous, obsolete, and so on. It is always by comparison with other competing ties that any tie is emphasized. So for every group to be defined, a list of anti-groups is set up as well. (…)
>
> Third, when groups are formed or redistributed, their spokesperson looks rather frantically for ways to *define* them. Their boundaries are marked, delineated, and rendered fixed and durable. Every group, no matter how small or big, requires a *limes* like the mythical one traced by Romulus around nascent Rome. (…)
>
> Fourth, among the many spokesperson that make possible the durable definition of groups, one must include social scientists, social sciences, social statistics, and social journalism (…) any study of any group by any social scientist is part and parcel of what makes the group exist, last, decay, or disappear (Latour 2005, 31-33)

According to Latour then, by tracing the spokespeople for mathematical economics, the anti-groups as defined by the mathematical economists, and how the boundaries of mathematical economics are constantly under redefinition by the work of mathematical economists, we can delineate our scientific group. There are some advantages in using Latour's framework. First, by relying on the studied actors to define what mathematical economics is, instead of using a predefined conceptual definition, it makes historical context a central element in the definition of the scientific group. Using such criteria to identify mathematical economists across different contexts makes it possible to trace the continuous reworking of its boundaries and changing identities. Second, such an analytical framework is useful to trace mathematical economics as a group at distinct levels of generality. It can be used to delineate a larger "mathematical economists in the United States" group as well as to identify a particular subgroup, for instance "mathematical economists at Stanford." Subgroups can hold different and even contradictory views about scientific matters while still keeping together as a group or coalition – indeed it is through continuous negotiations among compounding subgroups that a group is defined. The associations and mutual impacts between what is local and what is global are crucial in the social reorganization that makes possible the rendering of the defined scientific group. The main chapters of the dissertation

are focused on local groups, nevertheless they tell a history about the global mathematical economics community as well as about the particular local histories.

Based on this framework and drawing on the established literature in the history of mathematical economics, it is possible to sketch a brief outline of the changing meaning of mathematical economics in the United States during the twentieth century. Until the interwar period, mathematical economics was a marginal topic among economists in the United States, with some individual scholars interested in the topic based on scattered institutions pursuing research on the field from various perspectives. Irving Fisher's 1892 dissertation *Mathematical Investigations in the Theory of Value and Price* was a notably early exceptional example of mathematical economics in the United States. In Columbia, there was Henry Ludwell Moore's statistical economics and his attempts to construct an empirical version of Walras 'general equilibrium framework. In the 1920s, some mathematicians/statisticians were also drawn into this research area, such as Harold Hotelling at Stanford and then Columbia, Griffith Conrad Evans at Rice University and then Berkeley, and Edwin B. Wilson at Harvard, each with their unique mathematical background and own particular influence to later economics.[3] It was by the beginning of the 1930s that some institutional consolidation of the mathematical economics community emerged, with the participation of those individual actors, as well as European scholars such as Ragnar Frisch (Bjerkholt 1995). In December 1930 the Econometric Society held its inaugural meeting in Cleveland, while the Cowles Commission for Research in Economics was established in 1932 in Colorado.[4]

The onset of World War II promoted drastic transformations to this scientific community. The new demands of the war effort, the displacement of European scholars fleeing from war to America, the emergence of the military as a fundamental sponsor of research on the rationalization of strategic behavior were some of the many forces that acted upon the mathematical economics community as a direct consequence of WWII. A set of institutions created as part of the war effort brought together mathematicians, statisticians, and economists to find solutions for war problems, such as the Statistical Research Group at Columbia with Harold Hotelling as principal investigator (including the presence of Abraham

[3] On Hotelling, see Gaspard and Mueller (2021); on Evans, see Weintraub (2002, ch. 2); and for Wilson, see Carvajalino (2018).

[4] Louçã (2007), and Bjerkholt (2017) are important references on the history of the foundation of the Econometric Society and the early years of mathematical economics in the United States. Dimand (2019) is a recent contribution to the history of the Cowles Commission for Research in Economics and its successor since 1955, Cowles Foundation, particularly for the history of its formative years. See also Dimand and Velloce (2007).

Wald, Allen Wallis, Milton Friedman, among others). The Cowles Commission welcomed scholars fleeing Europe and was much reshaped by them. Jacob Marschak, director of research at Cowles from 1943 to 1948, and Tjalling Koopmans, his successor in office, played central roles in the reorganization of Cowles (Herfeld 2018). The scientific structure born under World War II circumstances did not disappear with the end of the War, particularly with the Cold War tensions emerging immediately after the global conflict. Breakthroughs such as linear programming were developed in this new context within the structure built during WWII (Erickson, Klein, et al 2013, ch. 2). New institutions also played crucial roles, such as the Rand Corporation (Leonard 2010, ch. 13). Entangled with this dynamical network, there were a number of notable young American economists graduating during this period, such as Paul Samuelson (PhD Harvard, 1941), Milton Friedman (PhD Columbia 1946), Don Patinkin (PhD Chicago 1947), James Tobin (PhD Harvard, 1947), Robert Dorfman (PhD Berkeley, 1950), Robert Solow (PhD Harvard, 1950), and Kenneth Arrow (PhD Columbia, 1951).

From the late 1940s up to the early 1960s, this diverse group of scholars associated with various institutions strove to define an alternative approach to economics by turning it into a mathematical science. Scientific conferences were important manifestations of how the group defined the emergent new economic science, collectively discussing and comparing their particular views of mathematical economics, from the 1949 Cowles Comission's Activity Analysis Conference (Düppe and Weintraub 2014b) to the 1963 Vatican Conferences (Dupont-Kieffer 2019), and multiple other events between those two. A series of founding works were published in this period advancing different perspectives toward mathematical economics, including (but not limited to) Von Neumann and Morgenstern (1944), Samuelson (1947), Koopmans (1957), Dorfman, Samuelson and Solow (1958), and Debreu (1959). Some attempts to consolidate the general research in mathematical economics to the mathematically trained reader such as Herstein (1953) and Koopmans and Bausch (1959) are illustrative of the variety of topics identified with the subject in the 1950s – including general equilibrium, game theory, econometrics, management science, welfare economics, trade theory, and growth theory. Sometimes the mathematical economists engaged in debates with their anti-groups to advance their methodological stance, although such events were less frequent. The quotes which open this introduction, for example, come from a discussion about the advantages of mathematical models to economics research, published in the *Review of Economic and Statistics* in 1954. Another notable exchange was the "measurement without

theory debate" (Mirowski 1989, Biddle and Hamermesh 2017) in 1947. Mathematical literacy was a constraint to the expansion of mathematical economics within economic departments and numerous efforts to educate economists and other social scientists in mathematics and statistics took place beginning in the second half of the 1950s (Orozco-Espinel 2020).

A demographic boom in the 1960s provided a great opportunity for a rapid dissemination of mathematical economics. In that decade, the annual number of PhD in economics granted in the United States grew about four times (Fourcade 2009, 75; Barber 1996). The organization of a common training core for graduate programs in the postwar period included training in mathematics and statistics as a basic requirement for a PhD economist since the Bowen Report in Graduate Education in Economics (Bowen 1953). Becoming an economist thus increasingly required the mastering of the methodological standards of mathematical economics, and the boundary between the subgroup and the discipline itself became increasingly fuzzier (like Solow's prediction stated on his opening quote).

The great expansion of the mathematical economics network made it increasingly identified with the economics discipline itself. Economic theory increasingly became synonymous with mathematical modeling of action based on utility-maximizing behavior by economic agents. By the end of the 1970s, even fields initially resistant to mathematical modeling, such as labor economics and economic history, had now well-developed scientific groups using econometrics and general equilibrium as basic analytical framework (see Netto, 2021; Lamoreaux 1999). Much of the research done in the 1950s as mathematical economics became reinterpreted later under the realms of microeconomics, macroeconomics, and econometrics. The previous meaning of "mathematical economics", as opposed to "literary economics" becomes increasingly irrelevant. Nevertheless, the label "mathematical economics" did not disappear in the 1970s. Instead it was reinterpreted as characterizing a smaller scientific subgroup. Since that decade, the prestige of empirical research done with mathematical tools such as econometrics was on the rise, to a point that some empirical work had become a mandatory part even of theoretical research (Hamermesh 2013, Rodrik 2015, Backhouse and Cherrier 2017). Under those conditions, "mathematical economics" as a label was snatched by mathematicians/economists doing more abstract theory or more interested in the mathematical content of economic theory models rather than its economic content (Debreu 1986). The creation of *The Journal of Economic Theory* in 1969 and *The Journal of Mathematical Economics* in 1974 illustrates the emergence of such scientific group, as well as

its progressive, although slowly and not outright, marginalization from the more prestigious academic journals. In the history told in Part II of this dissertation, such continuous proximity between economics and mathematics has made possible the connection of IMPA with the larger international community of mathematical economics. The histories of Part I stop short of the 1970s, but it is important to acknowledge this transformation of the meaning of mathematical economics as a major part of the IMPA chapter develops in the last decades of the XX century.

2. Tracing mathematical economics through the history of departments

The adoption of a performative criteria to define mathematical economics shapes the historical narrative to be constructed. If our definition of mathematical economics was based on some core theories of the research of a group of scientists, then the history of mathematical economics should have their published research as a fundamental object of its narrative. On the other hand, the performative definition puts on limelight the associations made among scientists, institutions, theories, and other objects to establish a common scientific identity. Scientific research still plays a central role in this narrative, since it is a fundamental part of the work of a mathematical economist. But research is just one of the various actions that are part of the academic job. In the context of our history, mathematical economists also taught classes at the undergraduate and graduate levels, tutored PhD students, chaired departments, applied for government and private foundations grants, organized conferences and workshops, dealt with conflicts with their peers, worked for projects with the industry/military/government, among other important actions. Thus there were multiple layers of transformation in post-World-War-II economic thought. Transformations that are visible on the published record of top journals are not necessarily simultaneously paralleled in all levels of academic activities. A complete account of the history of mathematical economics must encompass such various actions which are embedded in the formation of such a scientific community.

One advantage of constructing a historical narrative which is centered on an institution rather than some theoretical idea is that it can account for relevant actors which are often omitted in the published record. By focusing on the historical development of the department

of economics, we can assess the contributions of actors such as the chairpeople, the classroom, the graduate research seminar group, and the university administration, to the organization, growth and dissemination of the mathematical economics scientific network. All of those actors and their related actions are important for the weaving of the scientific community, but a focus on the history of published research can undervalue their real contribution to the historical process.

The project of writing a history of economics focused on particular institutions does not have the goal of writing some sort of purely institutional history. We are interested in studying the history of such institutions as spaces of scientific activity, thus creating a history of departments of economics not as the history of an institution per se, but as a place where a collective history is unraveled. Svorenčik (2018) has recently called the attention of historians of economics for the usefulness of prosopographic research for the construction of collective histories of scientists in a particular context. The author defines prosopography as "a historiographical method that collects and analyzes shared biographical features of a group of people from a specific, well-defined historical or social context. Prosopography is both the collection as well as the analysis of such data (…) It is the study of biographical details of individuals in aggregate, not a biography of groups." The method could be suitable for identifying relationships and hierarchies that could go unnoticed in individual histories, and can be used in eclectic contexts since it can be combined with different historiographical methods. Svorenčik writes that a typical prosopography begins with the delineation of a target population, for instance a specific department of economics, or PhDs who had a common dissertation advisor. Then the researcher should proceed by assembling a collection of biographical data and relational structures of the individuals, yielding a core biographical directory empirically grounded about the population. This method is used in different parts of this dissertation to enrich our history of mathematical economics in the departments studied.

Finally, it is necessary to face the question about how the history of the particular departments can contribute to the understanding of the global process of the transformation of economics. The cases studied here should not be interpreted as some particular examples of an ethereal transformation, but as sites that are shaped by and shape the transformation itself. It is in the local sites that we can account for the multiple connections, negotiations, translations, resistances, and displacements that characterize a global process. As put by Latour (2005, 187): "the Big Picture is just that: a picture. And then the question can be

raised: in which movie theatre, in which exhibit gallery is it *shown*? Through which optics is it *projected*? To which audience is it *addressed*?" This dissertation aims to address such questions, through the local accounts of the transformation of economics.

3. Stanford, Berkeley and IMPA

Before moving to the histories of each of the three institutions, the last step is to discuss the relevance and the reasons why those three particular departments were chosen to be studied. We break this discussion in two parts, like the dissertation itself, making first a discussion about the relevance of the American departments to the development of mathematical economics and the objectives of our research on them. Second, we make a similar discussion about IMPA and its place within the Brazilian context.

Why do I study Stanford and Berkeley, and not the departments of economics at the MIT, Chicago, or Harvard, which arguably are even more important to understand the development of American economics? One reason to choose the former departments over the latter group is that there is much less research on their history until now.[5] A recent exception is Cherrier and Saïdi (2020), who have explored the mutual influences between engineers and economists at Stanford. Of course, an understudied topic does not necessarily imply that there is a gap in the literature to be completed. Has economics at Stanford and Berkeley had any relevance to the history of the development and dissemination of mathematical economics?

The departments of economics at Stanford and Berkeley have risen in prominence in the second half of the twentieth century as producers of PhDs relative to other institutions. Using data from Barber (1996), we have that from 1945-51 Berkeley was in 12th place in a ranking of universities by the number of doctorates in economics awarded. Meanwhile, the small department of Stanford was not yet ranked among the top twenty graduate programs by such

[5] Economics at the University of Chicago has attracted much attention from historians, possibly due to the strong political influence of the 'Chicago School of Economics'. Emmett (2010) and Van Horn, Mirowski, and Stapleford (2011) are important references that consolidate much of such research. The MIT has received a supplement in the *History of Political Economy* (Weintraub 2014) dedicated to the department and its participation in the transformation of economics. History of Harvard economics has drawn interest since Mason and Lamont (1982). More recently Carvajalino (2018) has unveiled the Harvard connection between E. B. Wilson's image of mathematics and Paul Samuelson's mathematical economics. The history of economists from those institutions also contributed to the history of such spaces – notably Backhouse (2017), since Samuelson has been part of those three departments in different moments.

criteria. However, in the 1970-71 academic year, Berkeley had risen to third place while Stanford was now ranked in 10th place, having surpassed the traditional departments from the University of Chicago, Columbia University, and the University of Wisconsin. By 1990-91, Stanford was ranked in fourth place, while Berkeley was ranked in sixth place, each awarding respectively 31 and 29 PhDs in economics in that academic year. Backhouse (1998, 102) also shows that in the 1950s, Berkeley and Stanford were for the first time listed at the top of a ranking of contributors to the *American Economic Review* by affiliation.[6] Thus, the rise of prominence of Stanford and Berkeley as major formation centers of economists coincided with the postwar transformation of American economics.[7] A first objective would be to understand the relationship between these two processes by focusing mainly on the period 1945-1970. If by the beginning of this period, minimal to no mathematical economics was being done at Stanford's and Berkeley's departments of economics, by its end there were organized and relevant groups of mathematical economists within each place.

Stanford and Berkeley had a relatively minor influence in American economics during the interwar period. However, both universities hosted important beacons of interwar mathematical economics in the period, Harold Hotelling and Griffith Conrad Evans. In common they have the fact that neither held a position in a department of economics. Hotelling got his first academic position at Stanford through the newly created Food Research Institute, while Evans was hired to lead the department of mathematics at Berkeley when he was already established as an important mathematician in the American scientific community. In this thesis I want to understand if they were able to create any kind of lingering culture in these institutions that could have helped to shape the development of mathematical economics. Moreover, there were also significant mathematical economists in the departments of economics at Stanford and Berkeley in the postwar period, notably general equilibrium theorists Kenneth Arrow and Gerard Debreu (Düppe and Weintraub 2014a). Constructing a collective history of mathematical economics at Stanford and Berkeley helps to better understand the individual biographies of such economists, and also of other actors that typically receive less attention in the existing historiography.

An advantage of studying these two particular departments simultaneously is the fact that the institutions were geographically very close, which raises the question of whether such

[6] Berkeley and Stanford had 14 contributors to the AER in the 1950s, followed by the MIT (13), Chicago (10), and Yale and Michigan (9).

[7] A similar process took place at the MIT. Duarte (2014) compares the early years of the Industrial Economics graduate program with the late state of the department in the 1950s and 1960s.

proximity translated into impact in the dynamics of the departments. Stanford and Berkeley are both located in the San Francisco Area, a short trip by car separating both campuses. Possible associations between the departments and related scholars could go from mutual cooperation among scholars to some underlying competition for regional status.

The particular histories told in the following chapters should shed some light in the global history of the expansion of the mathematical economics network from different perspectives. As argued above, in American academia the departments of economics held some privileged powers over the reproduction of networks of economists. From the "supply-side" perspective, departments have the power of granting PhDs in economics, the definitive signal of professionalism and expertise in economics. Within departments scientific associations between PhD advisors and advisees are in making, in this process shaping the ideas of the economists of the following generation. As we shall see in chapter 1, in the 1950s and 1960s, the economics department at Stanford was a supplier of mathematical economists to the economics community. Although it was a small department with few faculty vacancies, the development of the behavioral sciences at Stanford was decisive to generate interest among a large group of graduate students in mathematical economics research. Such group of graduates (including economists such as David Cass, Karl Shell, and Steve Goldman) helped expand the network of mathematical economics by enlarging its uses in economic theory.

On the other hand, from the "demand-side" perspective, the department of economics is the major employer of academic researchers in economics, although definitely not the only employer. Moreover, the researcher employed in a department of economics has a unique edge in the reproduction of the discipline, due to their proximity to graduate students and their work as dissertation advisors. The complex decision process that defines which kind of economists will be demanded by the universities is influenced by multiple actors, but it takes place ultimately within each department through negotiations between administration, sponsors, chairpeople, and faculty. Furthermore, the career of the academic economist in the university is regulated by a tenure track system defined in the department. Associations made during the graduate years of the economist play a key role in the process, through adviser's letters of recommendations and the status given by the institution's degree. In chapter 2, we study how the department of economics at Berkeley, whose members mostly scorned the usefulness of mathematical economics to economic analysis during the first fifty years of the department, became a major employer of young mathematical economists in the 1960s after a

series of negotiations and changes in the administration of the department and of the university.

The last part of the dissertation, dedicated to the history of mathematical economics at IMPA in Brazil, fits into the history of the expansion of such networks initially bred within American universities. As it will be explored in the last chapter, the community of mathematical economists at IMPA was very influenced by the relation of the institute with the University of California, Berkeley and its department of mathematics. The internationalization of mathematical economics followed various paths depending on the multiple contexts to which it has traveled. In the Brazilian part of the internationalization story, IMPA stands as a stronghold for more abstract mathematical economics, despite being an institute of mathematics without much interest in social science. Indeed, one of the founders of the first graduate program in economics in Brazil during the 1960s, engineer turned economist Mário Henrique Simonsen, began his teaching career in mathematical economics at IMPA. Mainly through connections with EPGE, a graduate school in economics founded by Simonsen in the second half of the 1960s, the institute of mathematics was able to exert an important influence in Brazilian economics. But IMPA's mathematical economists made an impact not only on local Brazilian academia. Some scholars were recognized globally in the mathematical economics community, among whom we have Aloisio Araujo, Marilda Sotomayor, and José Alexandre Scheinkman, the latter having chaired the department of economics at the University of Chicago in the 1990s. With the history presented in the last chapter, we unwind the associations through history that made possible such a particular institute like IMPA to become entangled into the global network of mathematical economics.

PART I. Mathematical Economics in the United States

CHAPTER 1: A Cross-Disciplinary Nest for a New Generation of Economists: Mathematical Economics at Stanford University, from Hotelling to Arrow

Stanford University is considered one of the foremost centers of academic economics nowadays. Renowned economists such as Robert Wilson, Paul Milgrom, Alvin Roth, David Kreps, and Guido Imbens have spent important parts of their academic careers at Stanford. It was also there that Kenneth Arrow, one of the fathers of modern economic theory, found his first academic job. However, when Arrow arrived at Stanford in 1949, there was not much indication that the future would grant the department a central place within American economics. As he wrote to Stanford President Wallace Sterling in December of that year, conditions of the department of economics were deplorable: "Twelve members of the staff are sharing offices… Less than half… have such petty amenities as filling cabinets and adequate bookcases. Lighting, heating, and ventilation are primitive. Janitorial services beggars description." (Arrow cited in Lowen 1992, 409-410). In the immediate postwar period, Stanford was still a regional university of the second rank aiming to become an academic center of national reputation (ibid, 392). The university, however, had to deal with tight financial constraints. Administration's strategy to develop the university was to rely on funding coming from governmental contracts to invest in engineering and hard sciences research projects. The Department of Economics was not initially part of that strategy, just like a major part of the humanities at Stanford.

Although the department of economics could not secure enough resources through such strategy, mathematical economics at Stanford would profit from increased interest and funding to the so-called behavioral sciences during the 1950s (Lowen 1997). Mathematical economists became important actors in the intellectual community of the behavioral sciences since the beginning of the decade, after the Ford Foundation began to offer generous grants to the development of the new cross-disciplinary subject. The mathematical theories of rational behavior developed by that group were integrated into the large range of behavioral sciences supported by the Ford Foundation (Herfeld 2017).

Stanford became one of the major destinations of funding to the behavioral sciences in the 1950s. In 1954, the Center of Advanced Studies in the Behavioral Sciences (CASBS), an independent institute developing cross-disciplinary research among several disciplines in the humanities and natural sciences, was founded and its headquarters were located at the Stanford campus. [8] Arrow teamed up with professors from statistics and psychology to create a Stanford-affiliated institute - the Institute for Mathematical Studies in the Social Sciences, IMSSS - to serve the CASBS with their mathematical expertise. Through the cross-disciplinary space of the IMSSS the intellectual community of mathematical economics was expanded by attracting researchers from mathematics and statistics to the subject, as well as by generating a dynamical research space that was appealing to graduate students of economics.

In this chapter, we explore how the cross-disciplinary character of mathematical economics and its relation with the behavioral sciences was crucial for its expansion in the department of economics at Stanford. Cross-disciplinarity was a key element in the emergence of mathematical economics at Stanford since the 1920s. In section 1, we trace the origins of the Food Research Institute at Stanford during the interwar period, and its influence to quantitative research in economics and the teaching of statistics on campus, particularly through researchers such as Holbrook Working and Harold Hotelling. We also trace the onset of the first classes in mathematical economics offered at Stanford, through a cross-disciplinary statistics graduate program with crucial participation of the department of economics. In section 2, we trace the impacts of World War II on the research in mathematical economics and its effects at Stanford. The Statistical Research Group at Columbia University, an important wartime laboratory where mathematics, statistics, and economics intertwined, was led by researchers with Stanford connections, such as Hotelling and Wilson Allen Wallis. The organization and institutional structure of the research group would strongly influence the creation of a department of statistics with cross-disciplinary inspirations at Stanford which would become an important partner for the field of

[8] We use “cross-disciplinary” as a general term encompassing different degrees of interaction and integration among participant disciplines (see Klein 1990, ch. 3; also Fontaine 2014). Klein (1990, ch, 3) proposes different definitions for different levels of cross-disciplinarity. A simple juxtaposition of scholars coming from multiple disciplines to work on some common problem can be characterized as a multidisciplinary effort; an interdisciplinary exchange occurs when disciplines are transformed by the interaction with other disciplines; while transdisciplinary research should refer to a movement to create a common framework, such as an amalgamation of several disciplines. So a multidisciplinary endeavor might not involve any interdisciplinary exchange, just as interdisciplinary exchanges may not create any meaningful transdisciplinary contribution. Nevertheless, for the purposes of this dissertation we use only the term “cross-disciplinary” for the sake of simplicity.

mathematical economics. In section 3, we trace the changes in the department of economics at Stanford in the immediate postwar period. With a large group of professors retiring in the late 1940s, open vacancies allowed the department to reorganize almost the entire faculty. One specialist in mathematical economics was appointed professor (with a joint appointment in statistics), Kenneth Arrow. If there was no space for a large group of mathematical economists within the department of economics, the cross-disciplinary structure of the behavioral sciences developed in Stanford proved to be a favorable environment for this area of research. In section 4, we briefly sketch the history of the behavioral sciences in Stanford, and how Arrow and colleagues from other departments were able to create a dynamic research group combining mathematics, statistics, and economics. In the last section, we trace the effects of that dynamic research community in the development of a new generation of mathematical economists within Stanford. The group of economics graduate students doing research within the cross-disciplinary spaces fostered the development of several contributions to mathematical economics, from growth theory to econometrics. Despite the success of the mathematical economics group in attracting graduate students of economics to their field of research, this did not increase the number of mathematical economists appointed as professors in the department of economics. This dismal prospect at Stanford ultimately led to a disbandment of the original group of Stanford mathematical economists to various other departments, with relevant consequences for economics in places like the MIT, Harvard, Chicago, and Yale.

1. Mathematical Economics and Statistics at Stanford before World War II

1.1. Economics in the early years of Stanford

The Leland Stanford Junior University was opened to the students on October 1, 1891 by Jane and Leland Stanford, barons of the railroad industry, in homage to their only child, dead at seventeen due to disease seven years before. Built on an 8,000 acres ranch in the Santa Clara Valley owned by the Stanfords, the university offered training in science, technology, and humanities. An internal pecking order existed between those three areas. Leland Stanford considered that education was “training for usefulness in life” and remembered students, in his university opening speech, “that life is, above all, practical; [and] that you are here to fit yourselves for a useful career” (cited in Nash 1988, 5). So Economics, which at the time “was

widely feared as the seat of radicalism and the means of corruption of the young" (Goodwin 1998, 54), was not really a priority for the university in its early years. The first economics professor at Stanford was appointed in 1895 within the School of Humanities and Social Sciences. Edward Ross, a Johns Hopkins PhD, managed to enrage Jane Stanford by his outspoken support of the Democratic Party presidential campaign in 1896, by making speeches in the Socialist Club of Oakland and finally for his harsh public criticism of Asian immigration to California, which was a major source of cheap labor to the railroad industry. Ross was fired in 1900 after pressures made on administration by Mrs. Stanford. Such an affair was considered across the nation as a major attack to academic freedom, drawing setbacks to the institution (Cherrier and Saïdi 2020, 89). For instance, the economist Frank A. Fetter, who joined the new department of Economics and Political Science in 1898 coming from Cornell, resigned following Ross's dismissal.

After Jane Stanford's passing in February 1905, university president David Starr Jordan became the major force within the administration. He sought to make up for the losses for the economics department with the Ross' affair by offering a position to Walter F. Willcox, professor at Cornell University, but without much success. Willcox had a position at Cornell (Jordan's alma mater, and a major influence to Stanford itself) since 1891, where he was responsible for creating one of the first courses in statistics in the American universities (Leonard 1961). Between 1899 and 1901, Willcox worked at Washington D.C. in the organization of the 1900 US Census. There he met Allyn Young, a graduate student from the University of Wisconsin who served as his assistant and impressed him for his statistical knowledge.[9] Young spent summers at Cornell working with Willcox, who served as his de facto dissertation adviser, the official one being Richard T. Ely (Blitch 1995, 13). After spending some time in temporary positions at the Western Reserve University, Dartmouth College, and the University of Wisconsin, Young was recommended to Jordan by Willcox for the open position in the department of economics at Stanford. After some difficult negotiations with Jordan about his duties, Young accepted the offer and moved to Stanford in 1906 as head of the economics department. While negotiating his contract with Stanford, Young also approved the appointment of Thorstein Veblen in that same year (ibid, 22). They joined Harry Millis, a University of Chicago PhD who studied under John Commons, and Albert Whitaker, a former Stanford undergraduate and 1904 PhD at Columbia University, as

[9] Writing a letter of recommendation to Berkeley's professor Carl Plehn in 1902, Willcox described Young as having "perhaps more genius for statistical work and more training and insight in mathematical statistics than any graduate student I got from other universities" (Blitch 1995, 15).

the economists in the department that still hosted political scientists. Wesley Clair Mitchell, a friend of Young and professor at the neighboring University of California at Berkeley, was a frequent visitor to the Stanford campus.

Although promising, Young's stint as head of Stanford's department was short and with many pitfalls. His relationship with Jordan was marked by "distrust, miscommunication, and misunderstanding" (Blitch 1995, 18). There were many disagreements. Young wanted to develop graduate studies in economics and to separate them from the political scientists, but Jordan refused, giving preference to the expansion of undergraduate teaching. The department resources for library acquisitions and for hosting visiting faculty were much smaller than promised during the negotiations, as a massive earthquake took place in April 1906 severely damaging the university facilities, to the frustration of Young's plans for departmental development. He also had less power than expected, as Jordan repeatedly blocked the promotion of Veblen, considered a prolific researcher by Young, instead supporting the promotion of Whitaker, the better undergraduate teacher (ibid, 23-26). Veblen's publicly known extramarital life was a particular nuisance to Jordan, who forced him to resign in 1909. Following this event, Young resigned from his position in 1910, followed by Millis in 1911. Economics remained a secondary department within the university.

1.2. The Food Research Institute and the emergence of mathematical economics at Stanford

Herbert Hoover was one of the first Stanford's graduates, receiving his bachelor degree in geology in 1895. He grew rich very fast by working for a British mining corporation in West Australia and China, becoming a full partner in 1901. Hoover held gratitude to Stanford, while the administrators saw him as an ideal alumnus (Nash 1988, ch. 3). Hoover acted as a benefactor to the university with funding donations since the turn of the century, and by 1906 (then only 32 years old) he was planning to retire from his position in the mining corporation, to move back to Stanford and to try to help in the organization and development of the financial resources of the institution (ibid, 26). Hoover eventually did it in the end of 1912, returning to Stanford elected as a member of the board of trustees. He thus became a strong influential force within university administration, and when Jordan retired from office in

1916, Hoover's long-time friend Ray Lyman Wilbur was appointed the new president of Stanford.[10]

With the outbreak of World War I, Hoover's attention was drawn to the European conflict. He worked in food relief to occupied Belgium in the first years of the war. When the United States entered the war in 1917, Hoover served first as head of the US Food Administration, and then head of the American Relief Administration with the end of the conflict. With this new and challenging experience, on his return to Stanford he became a major sponsor of the establishment of a research institute in food policy. The Carnegie Corporation provided funding for the creation of the new institution. In 1921, the Food Research Institute (FRI) was established at Stanford University. As stated in the announcement of the institute creation in *Science*, the FRI was directed "for the purpose of intensive scientific study of the problems of the production [and] distribution of food… as an outgrowth of war experience."[11] The chemist Carl Alsberg was appointed as the first head director at the FRI. Alsberg worked in the US Department of Agriculture from 1908, becoming the head of the Bureau of Chemistry (later renamed Food and Drug Administration) in 1912. The FRI received department status for the purpose of directing research and recommending degrees within Stanford.

The FRI was not designed as an economics research center, but problems in the agenda of the institute, such as the estimation of demand for food, were related to the discipline. The institute was a cross-disciplinary space that relied on data accumulated during Hoover's war administration. The availability of data demanded research on statistical methods to use them. The agricultural economists (and brothers) Elmer and Holbrook Working did some influential work in the statistical determination of demand during the 1920s at the Food Research Institute (E. Working 1925, H. Working 1927). Through such work, the Working brothers were among the first members of the Econometric Society created in 1932, presenting papers in the first meetings of 1933 and 1934, later published in *Econometrica* (Working 1934, Working 1935). By bringing together agricultural economists, mathematicians, statisticians, and chemical engineers, the FRI worked as a cross-disciplinary space where different methods mixed to solve practical problems.

[10] Wilbur remained Stanford's president until 1943, with a hiatus in 1929-1933 while serving as Secretary of Interior during Hoover's US presidential mandate.
[11] See The Food Research Institute of Stanford University (1922, 100).

Harold Hotelling was a mathematician who got his first academic job at FRI. His sojourn at the institute was important for the later development of his career as a mathematical economist and for the development of Stanford's department of economics. Born in a small rural town in Minnesota in 1895, Hotelling enrolled in the University of Washington to study journalism, receiving his BA in 1919 after his studies were briefly interrupted by the participation of the United States in World War I. [12] The journalist career frustrated Hotelling's ambitions that it could "offer both a means of livelihood and an opportunity to stimulate proper action on public matters… I concluded that it had been overrated in both respects" (Hotelling in Darnell 1990, 3). He returned to his alma mater in January 1920, upon the stimulus of mathematician Eric T. Bell, to pursue a master's degree in mathematics. In the summer of that year, after attending a mathematics summer school at the University of Chicago, he developed a rather unusual idea of using his mathematical skills to study economics problems. Hotelling applied to a fellowship for graduate studies in economics at Columbia, without success. On the other hand, his application for a fellowship that payed for graduate studies in mathematics at Princeton was accepted, so Hotelling decided to continue his studies in the discipline. Not finding any support for his ideas of doing mathematical economics at Princeton either, he began to research topology, differential geometry, and analysis under the supervision of Oswald Veblen. Hotelling was finally able to find peers interested in mathematical economics when he began to attend the American Mathematical Society's meetings. There he joined a small group interested in the topic, most notably Griffith Conrad Evans, then professor at the Rice Institute, elected as Vice-President of the Society in 1924. With this new stimulus, Hotelling was confident in pursuing his interests in economics.

Upon receiving his PhD in June 1924, Hotelling returned to the West Coast as a junior associate in the FRI. There he could find a middle-of-the-road position between mathematics and economics departments that would be hard to find in any other American institution during the 1920s. He was appointed assistant professor in the department of mathematics in 1927, where he taught topology, statistics and differential geometry. Teaching was not his only academic duty. Hotelling also served as a mathematical consultant to the faculty of the department of economics in need of help (Darnell 1990, 6). During his years at Stanford, Hotelling prepared works that would appear in important economics outlets, such as the *Economic Journal* (Hotelling 1929) and the *Journal of Political Economy* (Hotelling 1931,

[12] Hotelling's biographical information comes from Darnell (1990) and Gaspard and Missemer (2019).

1932). He left Stanford in 1931 after receiving an offer to become professor of mathematical economics at Columbia, replacing the recently retired Henry Ludwell Moore.

1.3. The Committee on Instruction in Statistics

The presence of the Working brothers and Hotelling at Stanford was particularly influential in the department of economics to John B. Canning, a professor who headed the accountancy division.[13] Canning was a self-declared follower of Irving Fisher, with whom he maintained contact in the late 1920s while writing his most important work, the book *Economics of Accountancy* (1929). In the 1920s, an interest in statistics grew across different departments at Stanford, from Civil Engineering by the influence of Eugene L. Grant to the recently created Graduate School of Business by the influence of Theodore Kreps. The growth of such cross-disciplinary subject, but without a formal department to host it, led to the creation in 1934 of the Committee on Instruction in Statistics, acting independently of any other school or department in the university. The Committee was formed by Kreps, James Uspensky from the department of mathematics, and chaired by Canning. A graduate student at Stanford then had the opportunity of graduating with a minor in statistics, after completing a number of required credits in statistical courses (Stanford University Bulletin 1934, 291-292).[14]

The creation of the minor in statistics greatly expanded the supply of courses in the subject at Stanford. A course in "Mathematical Economics" was one of the new offerings created as part of the advanced credits of the statistics minor. The departure of Hotelling in the beginning of the decade had left a void in mathematical economics at Stanford, and due to

[13] John B. Canning was born in Huron County, Michigan, in 1884, the eldest of the six children of a family of farmers. He worked in his parents' farm during his youth and entered high school only at the age of twenty-one. Nonetheless, Canning was able to enter the University of Chicago in 1909, completing his major in political economy in 1913. He received a scholarship and began his graduate studies in the same discipline, but his studies were interrupted in 1917 after the United States entered World War I. After serving, Canning accepted a position at Stanford University. He was recommended by Whitaker, who during a one-year visit to Chicago had Canning as his student. Although not a prolific writer during his career, his PhD dissertation, completed in 1929, became an influential book to accountancy theory. Canning was one of the pioneers in the theory of asset valuation based on future expectations (Zeff 2000).

[14] The establishment of the Food Research Institute did not mark the beginning of the study of statistics in the university, as the discipline had been taught in other departments such as psychology and in the school of education, particularly under the influence of Lewis Terman (see Stigler 1999). Terman conducted influential (and also controversial) research in the measurement of intelligence in the early XX century. He was responsible for the publication of the Stanford revision of the Binet-Simon Scale in 1916. After WWII, his son Frederick Terman would play a crucial role at Stanford as provost.

the relative novelty of the subject there were not many options for replacement. The young Francis McIntyre was appointed instructor in the department of economics in 1934 and became responsible for such courses. He had received his bachelor degree in economics from Stanford in 1931, and had spent a couple of years as research assistant at the Social Science Research Committee (SSRC) at the University of Chicago (Cowles Commission 1937, 4). The courses were split in two quarters. In the autumn quarter, *Mathematical Economics I* (Econ 162) approached "the mathematical contributions to and simplifications of economic theory, with reference to… monopoly, imperfect competition, competition. The equations of general equilibrium. Attention will be devoted primarily to the writings of Cournot, Jevons, Edgeworth, Walras and Pareto." In the winter quarter, *Mathematical Economics II* (Econ 163) treated "modern developments in mathematical economics. The treatment of productivity, utility, cost, the general pricing process, and economic equilibrium, as developed in the writings of Evans, Frisch, Hotelling, Roos, Schultz, and others." Both courses required "easy familiarity with the calculus." (Stanford University Bulletin 1934, 255-256). McIntyre also taught two advanced courses in statistics primarily for economics graduate students.[15] His stint at Stanford was short lived, however. In September 1937, he was appointed associate professor at Colorado College and a research associate at the recently created Cowles Commission for Research in Economics. When the commission moved to the University of Chicago in 1939, McIntyre went along, beginning his graduate studies in economics in that university during the World War II years.

With McIntyre's departure, the department of economics, now under the leadership of Chairman Bernard Haley,[16] went on to search for another economist who could also excel in mathematics and statistics. Haley was able to hire the young professor Wilson Allen Wallis out of Yale University. Wallis had received his bachelor's degree from the University of Minnesota in 1932, with a major in psychology and minors in mathematics and sociology. Working with experimental psychology as an undergraduate was Wallis's first experience

[15] The topics covered in *Advanced Statistics I* (Econ165) were described as such: "Correlation and curve fitting; Simple, multiple, partial, and curvilinear correlation. Correlation analysis and inference. Basic least-squares theory. The fitting of linear and non-linear regressions curves, including special types of time series. Tests of significance of parameters, of goodness of fit, of reliability for interpolation and extrapolation." *Advanced Statistics II* (Econ 166) required Econ165 and covered "Probability, sampling, and frequency distributions. The philosophical and mathematical bases of probability. Probability distributions. Frequency distributions. The fitting of Pearsonian and other frequency curves. Reliability of estimation from large and small samples. Distribution of important statistical parameters. Tests of homogeneity and goodness of fit. A priori probability. Maximum likelihood. Theory of induction and statistical inference." (Stanford University Bulletin 1934, 256).

[16] See https://news.stanford.edu/pr/93/930608Arc3228.html

with statistical analysis.[17] He stayed in Minnesota for one more year doing graduate work in economics, but moved to the University of Chicago looking for a more suitable place to study mathematical economics and statistics. He expected that Henry Schultz would help him, but soon found out that "he was hopeless as far as my learning anything about statistics was concerned" (Wallis in Olkin 1991, 123). After a summer job in 1935 at the National Resources Committee in D.C., Wallis moved to Columbia to study statistics under Hotelling. With the market for economists with statistical skills in high demand, Wallis did not complete his graduate studies. In 1936, Wallis went back to D.C. for his job in the National Resources Committee, then in the following year was appointed professor in Yale after rejecting a proposal from Indiana University. In 1938, moving from Yale to Stanford for his second academic position, Wallis was still twenty-five years old (Olkin 1991, 121-123). He became responsible for McIntyre's courses in mathematical economics and statistics, and created a new seminar in statistics intended for economic graduate students interested in pursuing research in the field (Stanford University Bulletin 1938, 284-289). After teaching for three years at Stanford, he went on leave for World War II service. With the end of the war, Wallis returned to Stanford for a semester in 1946, but soon left for the University of Chicago, where he was offered the position of chairman of a newly created department of statistics. Despite his very short time on his return to Stanford, Wallis's war experience proved important for the development of mathematical economics and statistics at the university.

2. From War Labs to University Life: Creating a department of statistics

2.1. The Statistical Research Group at Columbia

With the entrance of the United States in World War II by the end of 1941, the American society prepared itself for the war effort. Wallis was well aware of the usefulness of statistics for war planning. In April 1942, he wrote to his friend W. Edwards Deming of the Census Bureau, asking for advice on how to restructure Stanford's statistical education in order to reflect the novel needs imposed by the war:

> Those of us teaching statistics in various departments here [at Stanford] are trying to work out a curriculum adapted to the immediate statistical requirements of the war. It seems probable that a good many students with

[17] His undergraduate work was published in a psychology journal in Wallis (1935).

> research training might by training in statistics become more useful for war than in their present work (…) It is difficult for us to design such a program because we do not have a picture of the statistical work of the war. You are probably in as good a position as anyone to observe what kinds of statistical training are needed for a wide variety of purposes. (Wallis to Deming apud Wallis 1980, 320).

Deming answered that since time was a very scarce resource in the urgency of war, short courses on the application of statistical quality control methods would be very important for training engineers and other students with statistical knowledge to be helpful in the war planning effort.[18] Such educational project had to be carried out by other faculty, since soon after this exchange with Deming, Wallis received a telegram from Warren Weaver, head of the Applied Mathematics Panel (AMP) within the National Defense Research Committee,[19] inviting him to "take charge of a statistics group that he was assembling with Harold Hotelling as principal investigator." Wallis promptly accepted the invitation, taking a leave of absence from Stanford for war work at Columbia.

The Statistical Research Group (SRG) was set up in Columbia University in 1942. The institution hired multiple young mathematicians, statisticians and economists as researchers, such as Abraham Wald, Jacob Wolfowitz, Jimmie Savage, Herbert Solomon, Milton Friedman and George Stigler. Until the end of war, the SRG provided advice to their military clients informed by statistical data, making research on new methods of inspection based on sampling, optimal ways of loading ammunition, and the best setting for the deployment of proximity fuzes for air bursts of artillery shells against ground troops (Wallis 1980, 322). Sequential analysis was arguably the most important scientific breakthrough developed within SRG. Research on the topic was stimulated in early 1943 by a Navy Captain request of a better way to evaluate the probability of a hit by anti-aircraft fire on an approaching dive bomber, using smaller samples than the ordinary requirements for experimental testing in order to save shells. Wallis and Friedman approached Abraham Wald for help with this problem. In order to solve it, Wald developed the sequential probability ratio test, using the

[18] Statistical quality control methods were developed in the 1920s in the Bell Laboratories by the physicist Walter Shewhart to deal with telephone industry problems. Klein (2000) presents a historical account of the development of statistical quality control in the 1920s and 1930s by Shewhart and its subsequent application for war planning.

[19] The NDRC, headed by Vannevar Bush (later founder of the National Science Foundation), was responsible for funding a great deal of mathematical research during the war effort. On the impacts such patronage caused on the mathematical sciences after the war, see Rees (1980).

sequential information of the first tests to shorten the sample size for inspection clearance (Leonard 2010, 279).

Besides those primary contributions SRG gave to the development of statistics during the war, the strengthened networks built up within the organization were very relevant for the development of economics and statistics at Stanford. The close experience between economists and statisticians at SRG would be reorganized at the Stanford campus. Two members of SRG played a significant role after the war in the university: Abraham Girshick and Albert Bowker.

2.2. Abraham Girshick, Albert Bowker, and the department of statistics at Stanford

Albert Bowker received his bachelor's degree in mathematics from MIT in 1941, on the eve of the entrance of the United States in WWII. In the beginning of the war effort, Bowker worked for two years in weather forecasting for a military project at his alma mater before moving to the SRG at Columbia. Remembering his years in the organization in a later interview, Bowker said that the SRG "probably had a major influence on my thinking and career, because I believe it was the most distinguished and creative collection of statisticians ever assembled" (Olkin 1987, 473). His major responsibility in the new institution "was to organize and manage the computing, which was done by about [thirty] young women, mostly mathematics graduates of Hunter [College] and Vassar [College, women's colleges based in New York]" (Wallis 1980, 322). When he took such an administrative position, Bowker was twenty-four years old.

Abraham Girshick was a more experienced statistician. He had emigrated from Russia to New York City with his family in 1923, at the age of fifteen, although he never lost his accent (see Arrow 2011). Due to good results in high school, Girshick was able to enter Columbia University in 1929, continuing there for graduate school in 1934 studying under Hotelling. After receiving his degree in 1937, Girshick worked in public service, teaching statistics in the Graduate School of the US Department of Agriculture (Rutherford 2011a). Working for the government, he took up statistical research on body measurements of American children to help the manufacturing industry to improve the sizing garments system. In 1939, he became the principal statistician in the Bureau of Agricultural Economics (Blackwell and Bowker 1955). During the war effort, Girshick worked at the SRG, hired by

Wallis. They had met before during Wallis's graduate year in Columbia University. In the SRG, Girshick worked closely with Wald in the theory of sequential analysis, his contributions having been published after the end of the war in two parts (Girshick 1946a, b). With the end of the war and the dissolution of SRG, Girshick got a researcher position in the newly established Project RAND, an endeavour funded by the US Air Force and the Douglas Aircraft Company, in a continuation of the military interest in optimizing methods as the emergence of the Cold War and the expectation of possible nuclear warfare maintained demand for such techniques. Project RAND became the Rand Corporation in 1948, separating from Douglas Aircraft and establishing its own headquarters in Santa Monica, California. As Rand Corporation's clients were not that different from SRG's clients, the research topics pursued in Santa Monica proceeded in similar lines to those done at Columbia.

Wallis returned to Stanford with the dissolution of the SRG. Mina Rees, formerly assistant to Weaver in the AMP, was now developing the Applied Mathematics Program for the Office of Naval Research (ONR), a major patron of American universities in the immediate postwar period. She offered financial support to Wallis for the development of a department of statistics at Stanford University. Wallis, however, had already committed to a move to the University of Chicago in the fall of that year after receiving an offer from the institution to head their new department of statistics. But funds were still available to Stanford for the creation of a statistics department through the AMP, and administration might have been interested in keeping up with local competition. Since the appointment of Jerzy Neyman in the department of mathematics at Berkeley, Stanford had lost its local leadership in statistics established since Hotelling's days. Looking for a substitute that could put forward the organization of a new department, Wallis wrote to Bowker, who was still in Columbia completing his graduate studies and inviting him to join Stanford. Wallis also recommended the appointment of Girshick (already based in California) to university administration, but recognizing that he did not have the required administrative skills to put up a new department. Wallis believed that Bowker, despite his young age, had such skills and both would make a good team for developing the department of statistics (Stigler 1999, 265).

Bowker was appointed assistant professor at the department of mathematics in 1947, after spending the preceding fall in the University of North Carolina at Chapel Hill, working on his dissertation under his advisor, the Chinese statistician Pao-Lu Hsu, who moved from Columbia to that university (Olkin 1987, 474). The department of statistics at Stanford was formally created in the following academic year, being composed only by Girshick, who was

appointed professor in statistics, and Bowker. The third professor listed in the department was Quinn McNemar, a senior Stanford psychologist who worked in Lewis Terman's tradition of the Stanford-Binet IQ tests. But McNemar, despite being listed as professor in the department, was on leave of absence, returning to the university only in 1950 (Stanford University Bulletin 1949, 274). Other than Girshick and Bowker in the first year, there were two research associates, Zivia Wurtele and Gladys Rappaport (later Garabedian).[20] Bowker, then twenty nine years old and yet without a PhD degree, was responsible for the organization of the new department. With his experience at the SRG being his major professional experience, Bowker expressed in later reminiscences that he tried to recreate the Columbia environment at Stanford:

> [At the SRG] I was able to meet and talk with many people who were or were to be major leaders in the field. I could drop in on most of them. Doors were frequently left open and I must say I found it very exciting intellectually to be treated as a colleague and friend rather than as a student. I had many conversations with people who like to talk and explain. I tried to pattern the department at Stanford on SRG. (Bowker cited in Wallis 1980, 329).

Bowker also played a key role in raising money to the department through contracts with the federal government and the military. Despite its small size, the Department of Statistics stood out among its peers at Stanford for its ability to raise funds for scientific research. As observed by Lowen (1997, 152) by 1949 the department was already raising more money to Stanford in the form of contract overhead with the federal government and the military than it was costing the university funds. By 1951, the number of contracts administered by Bowker had risen so much that it was necessary to create a separate institution, the Applied Mathematics and Statistics Laboratory, to keep track of the projects.

Girshick, on the other hand, was responsible for attracting promising young researchers to the department (see Figure 1.1 for a chronological view of the faculty in the department of statistics). Bowker recalled him as "a warm and attractive person who drew other scholars" (Bowker in Olkin 1987, 475). Girshick's time at the Rand Corporation proved important for

[20] Their names, however, are not listed in the Stanford University Bulletin. We know of their presence in the department's first years due to an interview with Bowker (Olkin 1987, 475). Rappaport had worked for Bowker in SRG writing programs for the group of (women) calculators, also supervising their work and double checking results (Royden 1989). She later assisted her husband, Stanford professor of mathematics Paul Garabedian, by writing numerical procedures for his work on computational fluid dynamics (McFadden 2010). Wurtele moved to Columbia for graduate studies, receiving her PhD in mathematical statistics in 1954. She published some papers in important outlets of mathematical economics such as *Econometrica* and the *Review of Economic Studies*, working with properties of input-output models (Wurtele 1959, 1960).

the recruiting of researchers to Stanford. In the summer of 1948, he brought the young chairman of Howard University's department of mathematics, David Blackwell to work with him at Rand. They had first met in Washington in 1945, when Girshick, still working in the Department of Agriculture, presented a lecture in the American Statistical Association on sequential analysis that was attended by Blackwell. Blackwell became lively interested on Girshick's exposition of the Wald's equation,[21] and further correspondence with Girshick led them to work jointly on extending that result (Blackwell and Girshick 1946, also Blackwell 1946). Their collaboration remained strong for the next few years, as in a later interview Blackwell acknowledged that "Girshick was certainly the most important influence on me" (Blackwell in DeGroot 1986, 43).[22] Blackwell was a visiting professor at Stanford's statistics department in 1950-1951, and ultimately moved to the neighbourhood in 1954, joining the department of statistics at the University of California, Berkeley by invitation of Neyman. In the same year he published a book with Girshick applying game theory to statistics which consolidated much of their joint research on the topic (Blackwell and Girshick 1954).

Another visitant to Rand in that summer of 1948 working with Girshick and Blackwell was Kenneth Arrow, who would play a pivotal role in the advancement of mathematical economics at Stanford's department of economics. Indeed, Arrow had been linked to that department even before his sojourn at Rand. When Wallis left Stanford to Chicago, the appointment of Bowker was not considered a replacement for the department of economics. Bernard Haley, who had hired Wallis out of Yale before the war and was still the department's chairman, wanted an economist with some mathematical training in the department, a tradition since Hotelling's years at the FRI. Bowker knew Arrow from Columbia, where he was a graduate student studying under Hotelling, and recommended him

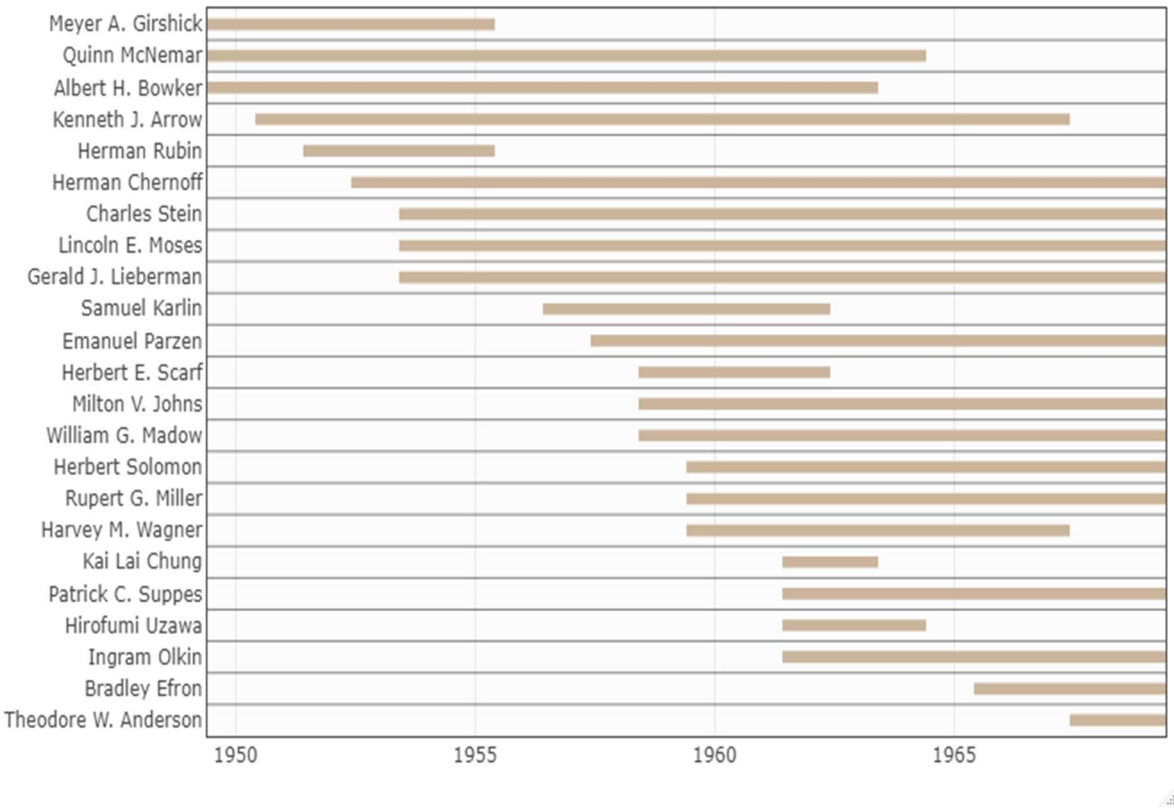
[21] The equation gives a simplified way of calculating the expected value of a cumulative sum of a number of random variables. The first version of such an equation was given in Wald (1944, 1945).

[22] Born in 1919 into a small African American community in southern Illinois at "the border of the segregated South" (DeGroot 1986, 42), Blackwell developed a keen interest in mathematics while attending high school. In 1935 he entered the University of Illinois at Urbana-Champaign, where he pursued his major in that discipline. Three years later, Blackwell continued in the institution for graduate studies. Thus, in 1941 and only twenty-two years old, he became the first African American to receive a PhD in mathematics at the University of Illinois, studying under Joseph Doob, a Harvard mathematician who turned his interests to statistics after postdoctoral studies under Hotelling at Columbia. Blackwell then was awarded a Rosenwald Postdoctoral Fellowship to pursue studies at the Institute for Advanced Studies (IAS) at Princeton, where he met and discussed ideas with John Von Neumann, Jimmie Savage and Samuel Wilks. However, due to racist reasons, Blackwell had no chance to receive an appointment at Princeton. Indeed, the university had even resisted his appointment as visiting professor during his stay at the IAS (Agwu, Smith and Barry 2003). He took first a job at Southern University in Baton Rouge, Louisiana and then at Clark College in Atlanta, experiencing life in the American South for the first time. Finally, in 1944, he settled at Howard University. All institutions were dedicated to the education of African American students.

to the economics department. Haley then wrote a first invitation for Arrow to join the department, in February 5, 1947:

> Dear Mr: Arrow: Mr. Bowker has suggested to me the possibility that you might be interested in the vacancy in this department in the fields of advanced statistics and econometrics… As a result from Mr. Bowker's suggestion, I have corresponded with Professor Wald and Professor Hotelling, and both recommend you very warmly. I understand however that you're accepting a position with the Cowles Commission very shortly, and I realize that consequently you may not be free to consider the opportunity we have in mind in any event. It occurs to me however that you might be in a position to accept employment beginning, for example, January 1, 1948… (Haley to Arrow, Kenneth J. Arrow Papers, Accession 2008-0031, Stanford, cited in Düppe and Weintraub 2014a, 14)[23]

Figure 1.1 Stanford's Statistics Faculty (1948-1970)



[23] Other than his connection to Bowker during his graduate studies at Columbia, Arrow acknowledged that other networks weaved through Columbia's SRG played a role on his recommendation to Stanford: "It must've been Wald, I think, who told Allen Wallis about me, because that played a role later. Since Wald worked during the war for the statistical research group of which Wallis was the effective head, it was a networking situation. The world of econometrics, I mean statistics, was very small. It was a very small—well, statistics still isn't very big, especially theoretical statistics, you know, not that many departments of statistics in the United States." (46)

Arrow had entered graduate school in the department of mathematics at Columbia in 1940 with the purpose of studying statistics. Academic life and economics was not really among his major interests by that time, as his plans after graduation were to work as an actuary (Düppe and Weintraub 2014a, 7). Arrow aspired to study under Hotelling, who was then a reference in statistics at Columbia. But Hotelling recommended Arrow to apply to economics, where it would be easier for him to receive scholarship funds for his studies. After taking Hotelling's course in mathematical economics during his first year in Columbia, Arrow moved to the department of economics in 1941. Following the U.S entrance in World War II in the end of the year, he had to pause his studies for serving in the military. He only returned to Columbia after being discharged in 1946, therefore missing much of the development of the SRG in the university. During the war, Arrow worked mostly as weather officer for the Army Air Force.

Upon his return to Columbia, Arrow's work was highly demanded. While worrying about how to finish his PhD dissertation and yet uncertain about his future in academic life, he received an invitation from Tjalling Koopmans in July 1946 to join the Cowles Commission as researcher, and the mentioned offer from Haley. He decided to go to Cowles in 1947, hoping to finish his dissertation there. Specially, he longed for the "atmosphere" at Cowles, with many scholars with his same research interests, while at Stanford he "wasn't going to learn anything" (Arrow 2011, 49). Haley continued to look for replacements for the mathematics/statistics chair in his department. Knowing that Stanford had hired Bowker to create a new department dedicated to the subject, and maybe worrying if it could take funding or influence away from his own department, he protested to the university administration by the end of the year: "It is our feeling that it is very important that instruction in the applications of statistical technique should continue to be under the departments in whose subjects the applications of statistical technique are made. (…) Both statistics and economics gain if statistical work is done by economists" (Haley cited in Stigler 1999, 265). Unwilling to let instruction in statistics cease in the economics department, Haley hired Paul Byron Simpson to the acting assistant professor job offered to Arrow (Stanford University Bulletin 1947, 282). Simpson remained in the department until 1949.[24]

[24] Simpson was a Cornell PhD in economics that had taught economics and statistics in his alma mater and Princeton before the war. After his discharge from service in the Navy, he was hired at Stanford, where he remained until 1949. Simpson then moved to his home state to teach in the University of Oregon, where he remained for most of his career. A summary of his career is available in a memorial page hosted in the University of Oregon website in https://economics.uoregon.edu/whats-happening/department-newsletter/fall-2013-newsletter-online-supplement/paul-simpson-memorial-page/

In Chicago, in his year working at Cowles, Arrow met and married Susan Schweitzer, a graduate student in the university under the Sarah Frances Hutchinson Cowles Fellowship for Women pursuing quantitative work in the social sciences (Düppe and Weintraub 2014a, 17-18). By some coincidence, Schweitzer had worked as a typist and calculator for Girshick in the Department of Agriculture at Washington before moving to Chicago (Arrow 2011, 51). It was through this particular personal connection that Girshick invited Arrow to spend the summer of 1948 in the Rand Corporation, which proved an important movement in the process of ultimately bringing him to Stanford (ibid, 55-56).

In the Rand Corporation, Girshick, Blackwell and Arrow worked jointly on a paper under the title 'Statistics and the Theory of Games' to be presented at a meeting of the Econometric Society, jointly organized with the Institute of Mathematical Statistics, in September of that year at Madison, Wisconsin. They dealt with the same problems Girshick had studied at the SRG, that is, the problems that spurred the development of sequential analysis. The trio's paper was published in 1949 in Econometrica, being the first of Arrow's publications in the journal where he would publish most of his contributions in the 1950s.[25] Their work became an important transition from Wald's sequential analysis developed at the SRG to the dynamical programming methods that would be developed at Rand by Richard Bellman. Indeed, in that summer of 1948 Bellman was also visiting Rand for the first time, after completing his PhD in mathematics at Princeton. In the Fall Semester, Bellman began a teaching job at the department of mathematics at Stanford, still visiting Rand in every opportunity in order to continue with his research (Klein 2007).

After his summer visit at Rand, Arrow returned to the Cowles Commission, but found out that it would be difficult to get a position at the University of Chicago, due to the conflicts between the commission and the department. On the other hand, Stanford contacted him again, this time with an offer for a joint position at the university, as a professor of economics and statistics, negotiated by the chairmen of those departments, Bowker and Edward Shaw (who substituted Haley). This double position would prove important later for Arrow to create a group of mathematical economics based on interdepartmental collaboration.

[25] It was also the first time Arrow had to deal with the problem of scientific credit, since their results overlapped with a work of Wald and Wolfowitz that was first presented in a conference two months before theirs. According to Blackwell, Wolfowitz was angered with the authors for not acknowledging the previous work (DeGroot 1986, 43).

In the early 1950s, the small scale of the department of statistics allowed for a close relationship between graduate students and faculty, as had been envisioned by Bowker. The early expansion of the department included the appointment of some home-grown statisticians such as Herbert Solomon (PhD 1950, appointed professor in 1959), Lincoln Moses (PhD 1950, appointed assistant in 1953 and professor in 1960), and Gerald Liebermann (PhD 1953, instructor since that year, becoming professor in 1959). There were few other additions to the department until Girshick untimely death in March 1955. Herman Rubin, a PhD in mathematics from the University of Chicago was appointed assistant professor in 1951, and Herman Chernoff was recruited by Arrow, (both having met at the Cowles Commission) becoming associate professor in 1952 (Bather 1996, 340). Charles Stein, after leaving Jerzy Neyman's group at the University of California, Berkeley due to the oath controversy in 1950 (see next chapter), joined the department in 1953 after some time at Chicago (Lehmann 1993, 337).

The paths of the department of statistics and economics at Stanford, disciplines that were imbricated in the university since before World War II through Hotelling and Wallis, would again intertwine to create a lively place for the development of mathematical economics with the establishment of the Institute for Mathematical Studies in the Social Sciences (IMSSS) in 1955. Before approaching such history, however, it is necessary to trace the changes in the department of economics since the end of WWII.

3. The department of economics after World War II

3.1. The Haley-Shaw rebuilding of the economics department

Besides the leaving of Wallis soon after the end of WWII, the department of economics at Stanford suffered other important losses in the immediate postwar period with the retirement of three important professors from the interwar years, Whitaker, Canning, and Theodore Boggs. The heads of the department during this period, Bernard Haley and his former student Edward Shaw had to lead an almost complete department rebuild. The university was under a tight budget constraint, with not much interest in the expansion of the department of economics, but those vacancies opened the possibility of making changes to the

department.[26] To better understand the new context of the department, we go through some biographical and professional background information about the new appointed faculty and the heads of the department (Figure 1.2 shows the changes in faculty in the department of economics during this period).

Haley was born in New Brunswick, Canada in 1898.[27] After serving in World War I with the Canadian Signal Corps, he moved to California and signed up at Stanford for college education. He received his master's degree in economics in 1923, and in the following year he began working in the department as instructor. While keeping his teaching job at Stanford, Haley attended graduate school at Harvard, receiving his M.A degree in 1926 and his PhD in 1933 in economics. He became Chairman of the department in 1931, remaining in office until 1948, with the exception of his leave of absence for war service in the Office of Price Administration and the Secretary of State. Haley then served as editor of the *American Economic Review* from 1952 to 1962.[28] Between the end of WWII and 1948, Haley was responsible for the appointment of two new faculty to the department, both educated in England in the 1930s, where the Keynesian ideas had been broiling: Lorie Tarshis and Tibor Scitovksy.

Tarshis was hired from Tufts College in 1946. Canadian-born like Haley, Tarshis went from Toronto to Cambridge for graduate studies in 1932, where he took several lectures from John Maynard Keynes, and even got to read early versions of *The General Theory* before its publication.[29] He took his first teaching job at Tufts in 1936, before completing his PhD dissertation. There he was just a few miles away from Harvard, becoming a frequent visitor to the department of economics, and one of the first conveyors of Keynesian ideas to American soil. After serving in the war, Tarshis moved to Stanford. He brought with him an advanced draft of a pioneering introductory textbook of economics with Keynesian insights, which would be published in the following year under the title *The Elements of Economics: An Introduction to the Theory of Price and Employment*. However, the book suffered from a

[26] Arrow commented about this period, when he arrived in the department, in a later interview: "One of the things that happened was when the department became low in numbers, Haley and Shaw thought this was an opportunity to rebuild the department. They had no resources other than the fact they had vacancies… the university was broke." (Arrow 2011, 66). On the financial constraints of Stanford in the early 1950s see Lowen (1992).

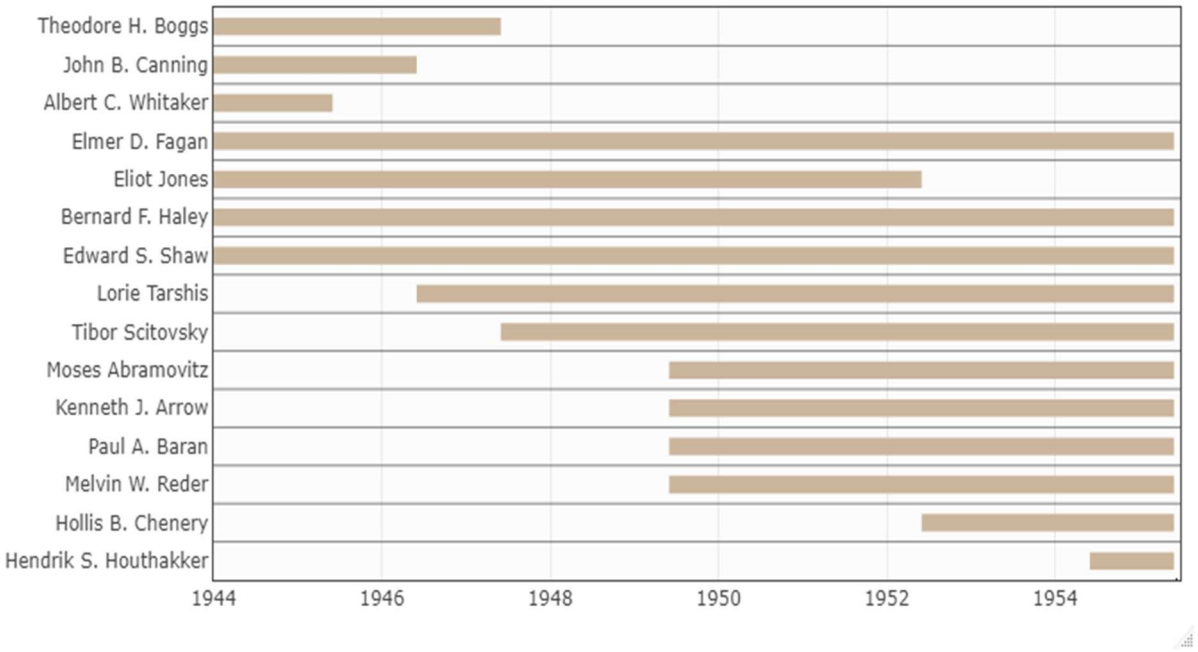
[27] Biographical information on Haley comes from Bernard Haley Memorial Resolution, Stanford University, Faculty Senate Records (SC0193).

[28] The next editor of the *American Economic Review*, who served from 1963 to 1968, was John G. Gurley, PhD student of Shaw and Stanford's professor of economics, meaning that for the major part of the 1950s and 1960s the AER editor was linked to the economics department.

[29] Indeed, Tarshis' notes have become important historical sources for those interested in Keynes's teaching. See Harcourt (1993, ch. 5).

proto-McCarthyist political persecution carried out by a right-wing organization called National Economic Council, particularly through a bashing review classifying the book as "Marxist-Keynesian" that was sent to several banks with a fundraising plead for help on "purifying college textbooks" (see Backhouse 2017, 567-569, also Weintraub 2017). Despite such pressures, Tarshis remained with support from the profession and within the department, where he served as Chairman in the early 1960s.

Figure 1.2: Stanford's Economics Faculty (1944-1955)



The Hungarian émigré Scitovsky was appointed professor at Stanford in 1947.[30] He had studied in the 1930s under Lionel Robbins at the London School of Economics (LSE), but crossed the Atlantic in the end of the decade as the War broke out, following advice from Robbins himself. In his first year in the United States, he visited Columbia, Harvard, and Chicago for three months each, meeting and learning from names such as Abraham Wald, Wassily Leontief, Joseph Schumpeter, and Oskar Lange. In 1942, he married Anne Aiekelin, a former colleague at LSE who was then working in Washington D.C. Without a job due to his pending immigration documents, Scitovsky tried to put work on paper. His efforts were stopped by his arrest for deportation, leaving on bail until his hearing. Under such pressure,

[30] Scitovsky's biographical information comes from his unpublished memoirs, Tibor Scitovsky Papers, Rubenstein Library, Duke University. See also Bianchi (2012) and di Giovinazzo (2019).

Scitovsky was drafted by the US Army for service at war, a golden opportunity to formalize his migratory status. After serving in different roles in the United States and Europe, Scitovsky was discharged by the end of 1945, with a job in the Department of Commerce in Washington. What he wanted, however, was to return to academic life. The opportunity came through Stanford, to where he moved in the following year.

Ed Shaw succeeded Haley as Chairman in 1948. Like Haley, his academic career was strongly linked to Stanford's department of economics. Shaw completed his whole education at Stanford, from undergrad to PhD. He first entered campus in 1925 as a student, began to teach economics in 1929 and was appointed full professor in 1941, while serving as a Chairman in relief of Haley, who was serving in the war effort. His experiences in the department shaped his own economic thinking. For instance, Shaw's later contributions to monetary theory, mostly authored with his Stanford student and later colleague John G. Gurley, carries much influence from accounting methods he learned from Canning in the interwar period (Mehrling 1998, 301). In 1949, while Chairman, Shaw was responsible for the appointment of four new professors, consolidating a core faculty to the department for the next decade. Hindsight might deem Arrow the most important addition to the department among that group, but at the time it was Moses Abramovitz the only to receive a tenured job offer (Stanford University Bulletin 1949). He was the most senior among the new faculty, having received his bachelor from Harvard in 1932, and then his PhD in economics in Columbia in 1939, where he had worked closely with Wesley Clair Mitchell. After his graduation, Abramovitz worked at the NBER on the business cycle project organized by Mitchell and Arthur Burns before moving to Stanford. (Rutherford 2011b, 274).[31]

Melvin Reder and Paul Baran completed the group of four new faculty hired in 1949. Reder had begun college at the neighboring University of California, Berkeley, receiving his bachelor degree in economics in 1939. Labor economics and the budding fields of industrial relations were then strong fields of specialization at Berkeley (see next chapter) and Reder would later pursue studies in that area. He left for the University of Chicago, where he spent two years studying in the economics master's program. In Chicago, Reder met Oskar Lange and Abba Lerner, two important influences on his early work (Lazear 1984, 145). For his doctoral studies, Reder joined Columbia, where he received his PhD in 1946 studying under the American institutionalists Frederik C. Mills and J. Maurice Clark. Those influences led

[31] Rutherford (2011b, 275-277) argues that an important reason for Abramovitz to leave NBER for Stanford after the war was the limited scope of research supported by Burns as the director of research. Similar dissatisfaction might also have pushed Simon Kuznets out of the institution.

him to publish works on industrial relations, labour and welfare economics. Before accepting his job at Stanford, Reder held teaching positions at the Carnegie Institute of Technology and the University of Pennsylvania.

Baran was born in the Russian Empire in 1909, facing displacement and several difficulties in his childhood due to World War I and revolutions that ravaged the region in the first decades of the century.[32] His family moved to Moscow in 1925, where he was able to enter university and receive a diploma in economics in 1928. He then moved for graduate studies in Berlin, but just as he received his doctorate degree in 1933, the menace of Nazism rose in Germany. He had to live as an immigrant again, initially in Paris, then Moscow, London and finally to the United States in 1939 where he decided to retake his academic career. He got a teaching job at the New School in New York, and entered graduate school once again in Harvard, receiving a master's degree in 1941. During the war, Baran worked for the Office of Strategic Service and in the US Strategic Bombing Survey. After discharge, he took up a job in New York at the Federal Reserve Bank. However, he had come to America for an academic career, and as an opportunity at Stanford appeared, he promptly accepted to move to California.

This new faculty group arrived at a department under financial problems. Economics, as most other departments in the School of Humanities and Sciences, was not a priority to receive funding from the university administration. The engineering school was the true crown jewel to Stanford, due to its ability to attract industrial clients to fund new research projects (Cherrier and Saïdi 2020, Lowen 1992).

Arrow arrived at the campus in the end of 1949, finding a department of economics in bad shape. As we saw in the introduction, he wrote a letter to Wallace Sterling, then Stanford's president, to complain about the department's conditions. In 1950, Chairman Shaw also complained to administration about the lack of funds, citing how it had stalled the expansion of the number of professors and forced him to cut down the number of courses offered to the "bare bones" (Lowen 1992, 410). Sterling, however, kept pressing the departments to cut spending, declaring a "financial emergency" in 1951, asking every department to cut their budgets by five percent. The openness to alternative sources of funding shaped the financial constraints of each department. In response to Sterling's demand for cuts, Shaw had to reduce the number of teaching assistants in the department, but refused

[32] Biographical information on Baran comes from Memorial Resolution, Paul A. Baran, 1909-1964, Paul Alexander Baran Papers.

to support graduate students to look for research funds through governmental contracts in order to ease financial restrictions. He wrote that such strategy to raise funds "would lead to distortion of Stanford's graduate program in economics and to an inappropriate channeling of a student's interest" (Lowen 1997, 156). In the Department of Statistics, where Arrow had become professor, matters were different. Chairman Bowker responded to Sterling's demand, refusing to take any budget cuts. He remembered Sterling of the profitability of his department to Stanford, and instead demanded a salary raise to its professors (ibid).

A feature of the Haley-Shaw rebuilding of Stanford's economics was that almost all new additions to the department were of Jewish scholars. That might not tell us necessarily about a special preference for Jewish people at Stanford, but about the prevalent Anti-Semitism within some of the top departments of economics in the United States in that period that was mostly missing at Stanford's economics. The degree of openness to Jewish economists has shaped the development of various departments in the United States, as well as the careers of those scholars (Weintraub 2014). Arrow remembered in an interview the different status of prejudice against Jewish people in the American departments during the 1940s: "Chicago was one of the few places where anti-Semitism was not an issue. I should also say at Columbia it was not an issue, Columbia and Chicago. But Harvard and Yale certainly were anti-Semitic." (Arrow 2011, 50; see also Synnott 2010). Indeed, his own academic path and that of some of his Jewish colleagues at Stanford had passed through the departments of economics in Chicago and Columbia. With the new additions under Haley and Shaw, the number of Jewish professors at Stanford went from none to half of faculty (five out of ten).

Another important feature of the department's rebuilding was its methodological pluralism. The new appointed faculty came from various training backgrounds. Tarshis and Scitovsky came from the English Keynesian milieu, Baran was trained on Marxism in Moscow and Berlin, Arrow learned mathematical economics from Hotelling, and Reder was trained on the American Institutionalist tradition. Such plurality had spillovers to the graduate students, who explored various topics with different methodological approaches during the 1950s. Cherrier and Saïdi (2020, 96) gathered data for faculty participation as supervisor or sitting on dissertation committees, showing no dominance of participation of any particular faculty during that decade.[33] Beginning in the 1960s, they found a growing participation of

[33] During the 1950s, Scitovsky participated the most on committees (even though he left the department before the end of the decade), for a total of seven dissertations. He is followed by Tarshis (five), Shaw, Arrow, and

Arrow, particularly fuelled by the creation of the Operations Research department in 1962. Indeed, as we shall see, mathematical economics needed support from other departments (statistics, OR) as well as cross-disciplinary spaces in order to keep expanding at Stanford. Within the department of economics there would be resistances to a possible hegemony of Arrow's group. On the other hand, outside pressures connected to the Cold War political environment impacted the later dwindling plurality at Stanford. Other than the already mentioned pressures on Tarshis's Keynesianism, the different fates of Arrow and Baran by the end of the decade were much decided by the underlying political constraints. While Baran survived quite undamaged the McCarthy years, his later outspoken support of Fidel Castro and the Cuban revolution proved too much for the Stanford administration, which began to pressure the economist for his resignation, by blocking promotions and keeping him on a low salary (Lowen 1997, 208).[34] On the other hand, Arrow's research benefited from the enhanced interest of the military to fund mathematical research into the behavioural sciences, stemming from the worsening conditions of the Cold War in the late 1950s.

3.2 Arrow's years as Chairman

The summer of 1953 was a hallmark date for Arrow's career, with the submission of his paper on the existence proof of equilibrium, co-authored with Gerard Debreu, to *Econometrica* (see Düppe and Weintraub 2014a). A less remembered achievement of that same period was his appointment as Chairman of Stanford's department of economics, replacing Shaw. Arrow remained in office until 1956. During his term there were few changes on faculty, as professors appointed during the Haley-Shaw administration received tenure and the department opened few new positions. Indeed, only one assistant professor appointed during Arrow's term would later receive tenure status at Stanford: someone particularly important in the development of mathematical economics in the department, Hendrik Houthakker.

Abramovitz (four each). For the 1960s, Arrow led participation in thirty-two committees, followed by Tarshis and Ronald McKinnon (fifteen each), and Reder and John Gurley (fourteen each). Still, those numbers are not enough to claim any dominance of mathematical economics at Stanford.

[34] At the height of McCarthyism persecution, Baran travelled to England considering the option of returning to Europe due to the anti-communist environment at California. Arrow, then the Chairman of the department, wrote to Baran to reassure him that his job was not at risk: "Selma and I wish you a very pleasant trip and time in England. However, we're selfish enough not to wish you luck in finding a position while you're over there. We'd much rather have you in Stanford. Both personally and intellectually, your going would be a loss to us and the community. I don't think I'm rationalizing when I say that staying has many advantages for you too. The hysterical wave seems to be receding, at least for the present." (Arrow to Baran, July 16, 1953, PABP)

Born in Amsterdam in 1924, Houthakker, as a Jewish young man, had to survive the Nazi invasion of the Netherlands by hiding on the countryside, around the towns of Bunnik and Odijk. With the war behind, he entered the University of Amsterdam in 1945 to study economics. Four years later, he passed his *doctoraal examen* and took a job at the Department of Applied Economics at the University of Cambridge, then led by Richard Stone (Cramer and Fase 2013). In the following year, he published in *Economica* his first research paper (Houthakker 1950), having his results acclaimed by Samuelson in a later number of the same journal volume, acknowledging that "a chapter in the history of utility theory has now been brought to a close by Mr. Houthakker's important discussion of integrability" (Samuelson 1950, 355).[35] With that accolade from such an influential researcher, Houthakker's result got him the attention of the American community of mathematical economists. He moved to the United States to join the Cowles Commission at Chicago as a researcher in 1952.[36] After two years doing research in the institution, he joined Stanford economics as an associate professor in 1954. Houthakker received tenure at Stanford in 1957. He continued to teach in the department until 1960, when he moved to Harvard.

Despite the few changes on faculty, there was an important expansion on the number and variety of graduate classes in mathematical economics during Arrow's tenure as chairman. He was able to accomplish it by bringing his colleagues from the department of statistics to teach economic statistics at the graduate level – in contrast to Haley's past resistance to statisticians teaching statistics to economists. From Arrow's perspective, there was no need for an economist to translate the subject. From 1951 to 1953, there were only two courses on mathematical economics taught at the graduate level, both by Arrow. Their scope was not much different from what had already been taught since Hotelling's time. Novel quantitative classes were *Statistical Analysis,* a semester long course on "elementary probability theory, statistical inference, sample survey techniques, [and] regression analysis", and *Quantitative Analysis in Economics*, which covered "identification and estimation of econometric models [and] input-output analysis" during a quarter (Stanford University

[35] Houthakker's paper introduced the concept now known in economics as the "strong axiom of revealed preference" that gave sufficient conditions to state the equivalence of the Hicksian utility approach to demand theory to Samuelson's revealed preference approach.

[36] Before moving to the United States, Houthakker reviewed Arrow's PhD dissertation, reprinted in 1951 as Cowles Monograph no. 12, for the , considering it "(…) a work of unmistakable importance. It poses the problem more clearly than has ever been done before… Its place in the history of welfare economics is assured; no longer can anybody speak with authority on that subject if he has not digested its conclusions." (Houthakker 1952, 358)

Bulletin 1953, 160).[37] In the 1954-55 academic year, Arrow brought Herman Chernoff from the department of statistics to teach the first quarter of *Statistical Analysis* in the department. Moreover, Arrow created the new course *Special Topics in Mathematical Economics*. As it was dedicated to teaching cutting-edge theory, the content of such courses would "vary from year to year" but always stressing "the application of mathematical methods to economic problems" (Stanford University Bulletin 1954-55, 158). In the following couple of academic years, Houthakker taught the *Special Topics* course, covering linear programming in the first year and economic dynamics in the second (Stanford University Bulletin 1955-56, 162; Stanford University Bulletin 1956-57, 163).

With such expansion of courses in mathematical economics, in Arrow's last year as Chairman the department implemented a reform on the format of the specialization fields which the graduate study in economics should choose in the program. To receive a PhD in economics, a Stanford student needed to qualify in six fields of study (i.e. to complete graduate courses offered in each field), including four required core fields (Price and Allocation Theory; Money, Income, and Employment; Economic Statistics; and Economic History and Development) and two optional fields chosen by the students. In 1955, a graduate student could choose among Public Finance, Labor Problems, Economics of Industry, International Trade and Finance, or Food Research (Stanford University Bulletin 1955, 158). Beginning in 1956, a new optional field was created, Econometrics. The more basic courses of *Statistical Analysis*, mostly taught by faculty from the statistics department, remained mandatory for every graduate student as part of the Economic Statistics field, while the more advanced *Special Topics in Mathematical Economics* and *Quantitative Analysis in Economics* would be dedicated to an audience with research interests in the field. In the following year, however, another reform took place making Economic Statistics also an optional field for graduate students (Stanford University Bulletin 1957, 155-156).

There was another important faculty addition to the field of mathematical economics in the last year of Arrow's chairmanship. In the first half of the 1950s, the departments of economics of the University of Tokyo and Stanford had an exchange deal that took Stanford faculty to Japan for seminars, on the other hand bringing Japanese scholars on visiting

[37] *Quantitative Analysis in Economics* could be substituted, as the 1953-54 Announcement of Courses informed, by "any upper division course in the curriculum of the Department of Statistics", or the course on Mathematical Statistics taught at the department of mathematics (Stanford University Bulletin 1953, 160). So before faculty from the department of statistics taught in the economics department, there was already an incentive to learn statistics from the statisticians.

positions. In the summer of 1954-55, Houthakker made the trip to Tokyo to give a month-long seminar course on demand analysis. To the students, he brought some unpublished manuscripts written by Arrow and Leonid Hurwicz on stability problems of general equilibrium theory and decentralized system for economic planning. One of the students attending Houthakker's course was a mathematics graduate named Hirofumi Uzawa. The dire economic and social conditions of postwar Japan had drawn Uzawa from the abstract theorems of mathematics to a keen interest in economics and social science. His path in the discipline began through the study of Marxian economics, a very influential tradition among Japanese scholars in the postwar period (Akama 2000). He was then surprised to see the mathematical techniques he was familiar with being used to understand economics. The seminar course gave Uzawa ideas, and he wrote a short manuscript to Arrow on the stability issue, in exchange to which he received a letter inviting him to Stanford (Okuno-Fujiwara and Shell 2009, 395-396).[38]

However, the department of economics had no openings after a series of recent appointments at the end of the preceding decade. Furthermore, even if Arrow could see in Uzawa a very promising researcher, it was harder to convince his peers that this mathematician with no previous training in economics should receive an appointment in the department. Thus, the invitation was not to join the department of economics, but offered an assistant research position in a new institute that had been created by Arrow in association with professors from other departments dedicated to the study of the "behavioral sciences". The Institute of Mathematical Studies in the Social Sciences (IMSSS), established in 1956, would play a crucial role for the expansion of mathematical economics at Stanford - in the context of an economics department that could not grow any more - as a cross-disciplinary space where it thrived as a common ground among the different scholars. As it will be seen in the next sections, one of the most important impacts of the IMSSS on mathematical economics was to allow Uzawa to have a home at Stanford.

Nevertheless, with the development in mathematical economics during Arrow's chairmanship, there was still much plurality within the department. Arrow himself supported such plurality, even though it was mathematical economics which could command more funds to scientific research. Despite the fact that the Office of Naval Research was supporting mathematical economics and not other fields of interest in the department, Arrow wrote to

[38] The manuscript Uzawa sent to Arrow was published as "Gradient Method for Concave Programming: Global Stability" in Arrow, Hurwicz and Uzawa (1958).

Sterling in December 1955 that it would be "a mistake to channel all research into the particular lines for which outside support is available and into relatively large projects (…) The Individual Scholar, working in a field which may or may not be currently fashionable plays a very vital role in economics" (cited in Lowen 1997, 161).

4. Mathematical Economics and the Behavioral Sciences at Stanford in the 1950s and 1960s

4.1. The Ford Foundation and the Behavioral Sciences

American private foundations were crucial patrons to the social sciences in the post-World War II period (Geiger 1988).[39] The Ford Foundation was a particularly important actor in the funding of the social sciences. After the death of Henry Ford in 1947, the foundation became the trustee of a large part of the Ford Corporation, making it soon the largest philanthropic institution in the world (Solovey 2013). The choice to finance in large part the social sciences was mostly motivated by the relative lack of funding from the federal government to the field and by the potential seen in those scientific efforts to achieve the higher philanthropic goal of advancing human welfare. A study team was assembled within the foundation during its early years by study director H. Rowan Gaither to decide the destination of funding (ibid, 113).[40] The result was the 1949 *Report of the Study for the Ford Foundation on Policy and Program.* The report cataloged five program areas linked to a particular goal: I) to promote world peace, financing international relations studies, II) to strengthen democracy, funding political science, III) to strengthen the national economy, funding economics, IV) fostering education in a democratic society, and V) promoting scientific knowledge of individual behavior and human relations (ibid, 114). It was Program V that received most of Ford Foundation's funding to the social sciences, particularly after Gaither got promoted to the post of President of the foundation in 1953. Before becoming

[39] The patronage of the American foundation of the social sciences in the post World War II period has been the subject of several researches in the history of the social sciences. Crowther-Heyck (2006, 422-429) offers an important discussion of such historiography.

[40] Gaither, a San Francisco lawyer, had worked as an administrator in MIT's Radiation Laboratory during World War II. In addition to his work at the Ford Foundation, Gaither also served as board chairman at the Rand Corporation (Solovey 2013, 113).

president, he had helped create the Behavioral Sciences Program (BSP) within the Ford Foundation in order to organize the funding of the behavioral sciences.

But what exactly was behavioral science and what were its practical uses for the Ford Foundation to have that much interest in funding it? The 1949 report did not define the behavioral sciences by the development of a universal theory, but as an opportunity to develop a universally valid scientific methodology for human sciences. By creating a scientific method for rational decision, the behavioral sciences could serve as a benchmark for planning and decision making to leaders in charge of execution of policy. The widespread postwar enthusiasm with cross-disciplinary studies also permeated the definition of behavioral science, since it could not fit under one single discipline label (Solovey 2013, 114-116). The choice of the name "behavioral" (avoiding the "social") was of specific political interest in the Cold War context of the early 1950s. The behavioral sciences were mainly formed by the disciplines of psychology, social psychology, anthropology, and sociology, as other social sciences were contemplated in the other programs (Geiger 1993, 101).[41] However, it did impact several other disciplines, including economics and mathematical sciences. This was particularly true in the case of Stanford.

The Center for Advanced Studies in the Behavioral Sciences (CASBS) was founded in 1954 with the patronage of the Ford Foundation. Its building rested on top of a hill facing Stanford's campus, but despite the geographical proximity the center was officially independent from the university administration. The CASBS received more funds from the Ford Foundation than any other endeavor in the behavioral sciences. A total of US$3 million was granted by the BSP in 1954 only for the creation of the center, and support for the institution in the following years summed up US$10 million in grants (Solovey 2013, 129). The CASBS was modeled after the Institute for Advanced Studies at Princeton, gathering a permanent group of appointees, distinguished annual visitors, and carefully selected postdoctoral students in various fields (Geiger 1993, 104). Its first director was the American educator Ralph Tyler, who planned the operation of the center. Tyler's strategy for making the CASBS impact higher education was built around the motto of "making the peaks higher", that is, focusing on the training of elite professionals, who would then later make

[41] One must not mistake "behavioral science psychology" for the behaviorist psychology tradition, best represented by the names of B. F. Skinner and John B. Watson, or with the work conducted by psychologist James Miller at the Behavioral Sciences Group at the University of Chicago during the late 1940s and early 1950s. For a distinction of such traditions see Solovey (2013, 114).

transformations in top level institutions.[42] Graduate students at those top level institutions could then impact research in many other higher education institutions.

4.2. The IMSSS at Serra House

Mathematical work played a central role to what was envisioned at CASBS and Ford Foundation as a truly scientific social science. However, in the 1950s people with mathematical training in other social sciences was even rarer than in economics. This situation led the Ford Foundation to finance efforts in the mathematical training of social scientists. At Stanford, the Institute for Mathematical Studies in the Social Sciences (IMSSS), created in 1956, would actively participate in the training of social scientists in mathematical tools. The IMSSS then occupied a house not far away from the CASBS headquarters, at Junipero Serra Boulevard. Due to its location, the house was named "Serra House".[43] It had been originally constructed to host former Stanford's president David Starr Jordan in his retirement, and had been unoccupied since his widow passed away in 1952. The IMSSS, differently from the CASBS which was an independent institute, was part of Stanford University. Among other organizations, it composed the Applied Mathematics and Statistics Laboratory at Stanford, "a kind of holding company for government projects and a unifying force in providing administrative services of a fairly high caliber compared to what most people had available in those days" according to its founder and director, Bowker from the department of statistics (Olkin 1987, 477). From the middle of the 1950 decade on, Bowker was becoming increasingly involved in the university administration. He acted first as an

[42] Did the "making the peaks higher" strategy work? Between 1954 and 1960 a total of nine future winners of the Nobel Memorial Prize in Economics were at CASBS on a one-year fellowship: Leonid Hurwicz (1955-56), Kenneth Arrow, W. Arthur Lewis and Theodore Schultz (1956-1957), Milton Friedman, Robert Solow, and George Stigler (1957-58), Ronald Coase (1958-1959) and Gerard Debreu (1960-1961). See https://casbs.stanford.edu/people/past-fellows-research-affiliates-and-visiting-scholars

[43] Although Serra House still exists at Stanford nowadays, it has changed both its location (twice) and name. The house was moved from its original location in the 1980s during an expansion planning of Stanford's campus. The change of name is a more recent development. Father Junipero Serra, the Franciscan friar who set up several Spanish missions in California during the eighteenth century, has become a controversial figure with growing criticism on the consequences of his actions to the Native American population. Such controversies led the Stanford administration to change the Serra House name to Carolyn Lewis Attneave House in 2019. Attneave received her PhD in psychology from Stanford in 1952. She has Native American ancestry. See https://www.stanforddaily.com/2019/02/28/serra-dorm-renamed-to-honor-sally-ride-serra-house-renamed-for-carolyn-lewis-attneave/

assistant to Fred Terman, provost at the University, afterwards becoming Dean of Graduate Studies.[44]

The development of IMSSS was carried out by two Stanford's professors who shared their interests between social sciences and mathematics: Arrow and Patrick Suppes. Suppes had followed a professional path that resembled to some degree that of Arrow. He received his bachelor in meteorology from the University of Chicago in 1943, then serving during the War as weather officer for the Army Air Force. After being discharged in 1947, Suppes entered Columbia University as a graduate student in Philosophy. His dissertation advisor was the logical positivist Ernest Nagel, and his graduate work was in logic and philosophy of science (Suppes 1978, 261-2). Suppes arrived at Stanford in 1950 as instructor in the small department of philosophy, receiving tenure as associate professor in 1955 (Suppes 2007, 10). The establishment of the CASBS at Stanford introduced Suppes to the behavioral sciences. He was affiliated to the Center in 1955 as research fellow, and it was there that he became particularly interested in learning theory and mathematical psychology (Suppes 1978, 267).

The IMSSS was divided into three disciplines, mathematics, psychology, and economics. Suppes led the psychology part, while Arrow was responsible for economics. The mathematics part was led by a newcomer to Stanford, Samuel Karlin. He came from the department of mathematics at the California Institute of Technology, where he held a position after completing his PhD in mathematics at Princeton in 1947. Since his move to California, Karlin had also worked as consultant to the Rand Corporation (Leonard 2010, 301). He was hired to Stanford in 1956 in a joint appointment at the departments of mathematics and statistics, one of his main tasks being to help build up the IMSSS.[45] Each professor hired an assistant to create a small research outfit, completing the staff at Serra House. Uzawa was the research assistant in economics, Herbert Scarf in mathematics, and Richard Atkinson in psychology. The three were appointed to Stanford initially to work at the IMSSS, but by the beginning of the 1960s all had been appointed as professors in the university (see Figures 1.1 and 1.3).

[44] It was Bowker's work as an administrator that took him away from Stanford in the 1960s. Between 1963 and 1971 Bowker was the Chancellor at City University of New York, and then until 1980 the Chancellor at the University of California, Berkeley.

[45] There is some ambiguous information about Karlin's date of arrival at Stanford. Karlin only appears listed as professor of mathematics and statistics in the university's bulletin beginning in 1956 (Stanford University Bulletin 1956, 294), but we know that he was dissertation advisor to John W. Pratt in the department of statistics (Anderson, Athreya, and Iglehart 1989, xxxix). Pratt completed his PhD dissertation in 1955.

The IMSSS offered a busy academic and research life to its members. The institute sponsored various workshops, symposia, and collective efforts for bringing together scholars from the different disciplines and to develop behavioral science theory. Technical reports circulated among IMSSS members and ideas were discussed in various seminars. A large portion of technical reports and papers developed and presented at the institute in symposia was published by the university press in a series of books under the label *Stanford Mathematical Studies in the Social Sciences*, with a total nine volumes published between 1958 and 1964. Major areas of research were mathematical programming (Arrow, Hurwicz, and Uzawa 1958), operations research and inventory theory (Arrow, Karlin, and Scarf 1958, 1962), statistical methods for psychological experiments (Solomon 1961), and mathematical learning theory (Bush and Estes 1959, Suppes and Atkinson 1960, Chriswell, Solomon, and Suppes 1963). Volume 4 of the series, which was edited by the three directors of the institute (Arrow, Karlin, and Suppes 1960), brings together multiple contributions from each topic studied at the institute. Arrow edited the first section on mathematical economics, Karlin edited the section on management science, and Suppes the section on psychology. The papers published came from a symposium held at the institute in June 1959 on "mathematical methods in the social sciences". The contributions to the mathematical economics section illustrate the multifarious character of the subject in the 1950s. As a contemporary reviewer noticed (Fisher 1961), mathematical economics in the book ranged from Debreu's highly abstract topological analysis of cardinal utility theory to a practical discussion of operational implications of imperfect econometric models by Henry Theil and Teun Kloek. Other contributions were on the stability of general equilibrium theory, growth theory and technological progress, turnpike theory, and resource allocation mechanisms. Among the key contributors were Paul Samuelson, Robert Solow, and Lionel McKenzie, as well as the hosts Arrow and Uzawa.[46]

Despite the tripartite division, disciplinary boundaries were very thin. In the late 1950s and early 1960s, the non-economists at IMSSS contributed regularly to the mathematical economics literature. The modern concept of the core in general equilibrium theory might be the best known of such contributions developed at the time, being introduced initially by Scarf in 1962 (Cogliano 2020). Karlin published a whole book dedicated to mathematical economics in 1959, which influenced the developing literature in the early 1960s on turnpike theory, particularly the contributions of his Berkeley neighbor, Roy Radner (Radner 1961,

[46] There was also an economist contributing to another section of the book. Jacob Marschak had a paper published in the psychology section on the existence of random utility functions based on decision theory.

Assaf and Duarte 2020). Suppes published twice in *Econometrica* on the axiomatization of utility theory (Davidson and Suppes 1956, Suppes 1961).[47]

The mathematical education of social scientists was a fundamental duty at the IMSSS, as expected by their patrons. A decisive partner to the institute in its early years for promoting mathematical education was the Social Science Research Council (SSRC). Funded by the Ford and other private foundations, the behavioral science ideal for the social sciences was influential within the SSRC in the post-WWII period (Solovey 2004). In the spirit of promoting the behavioral sciences, in December 1952 the SSRC appointed a Committee on the Mathematical Training of Social Scientists to overcome a perceived lack of basic training in mathematics in social science (Orozco Espinel 2020). The committee lasted until 1964. It was formed by a small group of scholars from various disciplines: mathematics, statistics, economics, sociology, and psychology. During the 1950s, it was chaired by the University of Illinois' mathematician William G. Madow.[48]

Using grants from Ford Foundation's BSP, the committee organized a series of summer institutes in mathematical sciences for social scientists, starting in 1953 with a first event held at Dartmouth College. In 1955 the committee organized two summer institutes, one of which was hosted at Stanford. In 1957, the newly created IMSSS became a partner of the committee in the promotion of the summer institutes. Two events were organized, with different audiences and purposes. The *Summer Institute on Mathematics in Social Science* targeted college teachers of mathematics.[49] Among its stated purposes were to "increase the number of mathematicians who are familiar with existing applications of mathematics in social sciences" and to "facilitate cooperation between mathematicians and their social scientists colleagues."[50] On the other hand, the *Summer Institute on Applications of Mathematics in Social Science Research* was targeted to the "mathematically trained social scientist." The

[47] Researchers at IMSSS were not shy to apply their mathematical models to the most diverse disciplines, in various cases with success. For instance, in the 1960s Karlin applied programming models to genetics, which proved seminal for the emergence of the mathematical population genetics field (Bodmer 2009).

[48] Economists with participation in the committee include Jacob Marschak (1952-1954), Robert Solow (1955-1958), Carl Christ (1958-1960), and Robert Dorfman (1960-1964). See Orozco Espinel (2020, 35).

[49] Information on both Institutes sponsored by the SSRC and held at Stanford in 1957 comes from their published announcements and from the Minutes of the meeting of the staffs and sub-committees from the 1957 Summer Institutes held at the SSRC offices. Both documents are archived in Box 1, Lionel W. McKenzie Papers, David M. Rubenstein Rare Book and Manuscript Library, Duke University.

[50] There were workshops on economics (organized by Koopmans), sociology and social psychology (Duncan Luce), and psychology (William K. Estes). There were also seminars in the mathematics of the social sciences, with lectures on linear and quadratic programming by Albert Tucker from Princeton's department of mathematics, as well as a course that covered the axiomatics of decision theory, game theory, and operations research taught by the mathematician John G. Kemeny from Dartmouth College.

participants were split in five thematic groups according to their specialization and research interests where they collectively worked on a particular cutting edge topic. Two groups did work on mathematical economics, one on Linear Economic Models, coordinated by Robert Dorfman and another on International Trade and Taxation, coordinated by Lionel McKenzie. The work done within Dorfman's group illustrates the cutting edge nature of the research pursued during the summer program. Participants of the panel discussed the von Neumann model, studying an alternative proof to the existence of equilibrium in the context of the model (eventually published in Howe 1960).[51] The model, initially developed in the 1930s by the Hungarian polymath, made a comeback among mathematicians after the publication of Gale (1956) and Kemeny, Morgenstern and Thompson (1956). It would grow in popularity among economists particularly for its application to turnpike theory, following Dorfman, Samuelson and Solow's 1958 book on linear programming, published a year after the Stanford summer institute, and the aforementioned Karlin's book.

The approximation between the SSRC Committee on Mathematics in Social Sciences and the IMSSS continued into the 1960s, as Suppes became the Chairman of the committee while keeping his post as director of the Stanford's institute. In the first half of the 1960s, the university was hosting more than half of the activities promoted by the SSRC Committee on Mathematics in Social Sciences, including senior conferences and summer research institutes (Orozco Espinel 2020, 36-37). William Madow, formerly Chairman of the SSRC Committee moved from Illinois to Stanford's department of statistics in 1958.

The expansion of institutional spaces devoted to cross-disciplinary social sciences unified under a common mathematical framework within Stanford (with fundamental support by the Ford Foundation) allowed mathematical economics to develop within the university. The number of graduate students in the department of economics dealing with problems in mathematical economics grew considerably after the creation of the IMSSS (Cherrier and Saïdi 2020, 96). The work they produced as graduate students was of particular relevance, many of such contributions still being a central part of contemporary economic theory. However, if mathematical economics thrived within cross-disciplinary spaces, it encountered

[51] According to Howe (1960), the principal discussants on the Von Neumann model were Charles Howe and John Haldi, both graduate students of economics at Stanford. Other attendants in the Dorfman group who contributed to the proof were John Fei (PhD Economics at MIT, 1952), Michael C. Lovell and Richard Quandt (both received their PhD in economics from Harvard in 1957). There were two other participants in Dorfman's group that were not mentioned in the paper: William Garrison from the department of Geography of the University of Washington, and one of his graduate students, Duane Marble. Garrison and his students were central actors of the so-called 'quantitative revolution' in Human Geography (Barnes 2004).

resistance to growing within the department of economics. Such resistances, as well as an increase of status of the subject within other departments, led to the dissolution of the group of mathematical economists in the department of economics at Stanford by the end of the 1960s.

5. Mathematical Economics expanding in a *"too small"* Department of Economics

5.1. The economics graduate students at Serra House

In the community of behavioral science scholars at Stanford, mathematical economics was the representative of economic science. Nevertheless, within the department of economics it was still only a specializing area among a plurality of different approaches. By the beginning of the 1960s, the department had yet not changed much since the multiple appointments ten years before. The dynamism of the behavioral science community would impact the department of economics more through its graduate students. Not only the number of students doing work in mathematical economics grew in the early 1960s, but also their profile had changed from previous graduate students who arrived in Stanford in the first half of the 1950s. Stanford's graduate students such as Robert Summers (PhD 1956), Thomas Marschak (PhD 1957), and John Harsanyi (PhD 1958) arrived in the university before the hype about behavioral sciences. Their interests in mathematical economics preceded their arrival on campus.[52]

For those graduate students who enrolled in the department of economics from the second half of the 1950s onwards, the influence of the cross-disciplinary spaces of the behavioral sciences was stronger. Bagicha Singh Minhas arrived at Stanford with an MA in Economics from the Punjab University (1953) and a MS in Agricultural Economics from the University of Illinois at Urbana (1955). Interested in economic planning for development, Minhas worked under Hollis Chenery gathering data from the industrial census. Using a Cobb-Douglas production function to make sense of his data proved unsatisfactory, and Minhas sought Arrow's help. Arrow noticed that the data could fit better under an alternative specification of the production function, similar to the one Solow had been working on at the

[52] Arrow (2011, 88) has described his first encounter with Harsanyi during his application process to the PhD at Stanford: "He had actually published a couple of papers before he even showed up as a graduate student. I Interviewed him when he applied and I asked, 'Why are you coming for the doctorate? You already know all about it.' (...) He had pretty much worked on his dissertation before he came here. While I gave him some advice, I find it a little hard to take much credit for anything he did. He was really very self-propelled."

CASBS during his 1957-58 fellowship in the center. Through this network it was created the constant elasticity of substitution (CES) production function. The four economists published an analysis of such functions in *The Review of Economics and Statistics* in 1961, and since then has become a widely used specification for production functions in empirical work (Biddle 2020, Arrow et al 1961). After concluding his PhD in 1960, Minhas returned to India as part of the Planning Unit of the Indian Statistical Institute in Delhi (Parikh, Srinivasan, and Tendulkar 2005).

Another student arriving in the late 1950s was Menahem Yaari. Born in Jerusalem, Yaari had a bachelor degree in Economics and Philosophy from the Hebrew University when he arrived at Stanford.[53] Yaari was one of the first graduate students to receive an office at Serra House (Spear 2001, Okuno-Fujiwara and Shell 2009). During his PhD, he worked on how to model an optimal consumption path over time for an individual, which would serve as a basis for the optimal growth literature that got traction in the department in the following years (Yaari 1964). Yaari was the oldest member among the larger group of graduate students incoming in the 1960s with interests on mathematical economics, later collaborating with some of them particularly on overlapping generations modeling (see Cass and Yaari 1966). After concluding his PhD, Yaari took a position at the Cowles Commission at Yale University – the first of a series of Stanford's graduates going to Yale in the following years. Yaari remained teaching at Yale until 1967, when he returned to Jerusalem as professor in the Hebrew University.

For graduate students who arrived in the early 1960s and decided to pursue research in mathematical economics, the dynamism they saw in the behavioral sciences sites of research was crucial for their decision on which professional path to follow. This impact was voiced by some of those students in interviews reviewing their careers. Karl Shell arrived at Stanford in 1961 with some previous knowledge on linear programming and other mathematical tools from his experience as an undergraduate student in mathematics in Princeton. He decided to apply to economics after taking a class in economic theory from William Baumol, choosing Stanford due to the presence of Arrow in the department. There, he would soon find that it was in the cross-disciplinary spaces, particularly at Serra House, that he would fit best:

[53] The American connection at the Hebrew University was Don Patinkin, formerly a professor in the department of economics at the University of Chicago, who migrated to the newly created state of Israel in 1949. Another incoming graduate student from the Hebrew University in the early 1960s was Michael Bruno who completed his PhD under Arrow in 1963. Arrow served as his dissertation advisor at Stanford. Bruno made a career in macroeconomics as well as policymaker, serving as Governor of the Bank of Israel between 1986-1991 and Chief Economist of the World Bank between 1993-1996.

> I was assigned space at Serra House, the quarters of the IMSSS […] I participated from the start in the Serra House workshop on mathematical economics and econometrics run by Ken [Arrow] and Marc Nerlove. Manny [Yaari] and Ken-Ichi Inada were among the regular participants during my first year. Herb Scarf was an occasional participant. Hiro Uzawa was on leave at the CASBS. (...) Manny presented some of his work on the consumption-loan model, my first exposure to OG [overlapping generations]. Marc presented a wide review of work on expectations, including a careful rendition of Muth's paper on rational expectations [RE]. This was before RE had become widely known. Ken gave a first draft of learning by doing. His seminar was my first exposure to modern growth theory, certainly my first exposure to endogenous technical change, and my first serious exposure to the calculus of variations (Shell in Spear and Wright 2001, 703-704)

David Cass, on the other hand, arrived at Stanford with "virtually no math training" (Spear and Wright 1998, 537). Cass was an Economics/Russian Studies major from the University of Oregon. He then spent one year of graduate study at Harvard Law School, then dropping it to serve one year in the army. It was only after struggling in the first quarters of his graduate studies at Stanford learning calculus and statistics that he got to the Serra House, through his classmate Shell. In a later interview, Cass was not shy to recognize the cross-disciplinary space of the Serra House as "where the good people were" in the department of economics:

> I viewed Stanford's graduate program as being completely chaotic... at Stanford, you were kind of left on your own as a graduate student. There was just no coherence in the program... [T]here was a mathematical economics group who had offices separate from the department in a little house on campus called Serra House, and that is where what I consider the really good people at Stanford were: Arrow, Uzawa , Scarf. (...) Karl [Shell] introduced me to Serra House. (...) the last two years at Stanford (I stayed four years), I basically spent at Serra House working with Uzawa. He always had seminars going. (Cass in Spear and Wright 1998, 535)

Cass and Shell arrived at Serra House in a context where growth theory was becoming a central topic of interest in the economics profession (see Boianovsky and Hoover 2009), and particularly to mathematical economists. Besides the co-authorship in the CES function paper, Arrow contributed to such literature in the beginning of the decade with his "learning by

doing model" (Arrow 1962).[54] Following the empirical results in Solow (1957) and the work that the MIT economist had brought to the IMSSS aforementioned conference (Solow 1959) on the role of technological progress to the growth of per capita income, Arrow's model assessed the impact of increases in labor productivity to the rate of economic growth. Shell's dissertation followed this thread initiated by Arrow, by trying to merge labor and capital productivity by modeling "inventive activity" (Shell 1966). As in the 1963-64 academic year Arrow moved to England as a visiting professor in the Churchill College, University of Cambridge, Shell became closer to Uzawa and the group of graduate students under his research supervision. Uzawa was also a participant in the growth theory literature of the beginning of the decade with his two-sector model (Uzawa 1961, 1963), an extension to Solow's (1956) aggregative model.[55] However, the appearance of a novel mathematical technique would steer Uzawa's interests towards optimal growth models. In 1962, the work of the Soviet mathematician Lev Pontryagin and co-authors (originally published in 1956) on the mathematics of optimal processes was translated to English. Uzawa later remembered that Karlin showed him Pontryagin's work as a way to bypass issues with corner solutions (Okuno-Fujiwara and Shell 2009, 406-7). Cass remembered that it was Uzawa's "fascination with the maximum principle" that motivated him to work on optimal growth theory during his PhD: "Our bible at the time was Pontryagin's original book on the maximum principle," he added (Spear and Wright 1998, 539). Equipped with the maximum principle, Cass prepared in 1963 a first draft on an aggregative capital model of optimal growth, which would later become the now well-known 1965 paper with the "Cass contribution" to the so-called Ramsey-Cass-Koopmans model.[56] Other Uzawa's graduate students at Serra House used the basic framework developed by Cass to work on their own extensions of his model, including Steven Goldman (PhD 1965), Aaron Douglas (PhD 1966), and Harl Ryder (PhD 1967). By the second half of the decade, the mathematical modeling developed in the cross-disciplinary spaces had become relevant even for those who were not part of the group. For instance, the

[54] Arrow's model was one of the first to incorporate the concept of rational expectations introduced by Muth (1961) to growth theory. For a history of the learning by doing model, see Ballandonne (2015) and Thomas (2020).

[55] Uzawa was not the only economist pursuing this thread of research. In a letter of January 9, 1961, Solow warned him about a similar paper that was published by Yochi Shinkai in the *International Economic Review* (Shinkai 1960) that dealt with the fixed-coefficients production function case (Robert Solow Papers, Box 61). Uzawa's main contribution was not the two-sector framework itself, but his work on the global stability of such a model.

[56] By about the same time, Koopmans, then director of Cowles Commission, and Edmond Malinvaud were discussing similar problems to those of Cass at Stanford, although with different methods and motivations (Assaf and Duarte 2020). See also Spear and Young (2014) for possible further connections between Cass, Koopmans and Malinvaud.

work of Yoram Weiss (PhD 1968), who had his dissertation supervised by Melvin Reder, modeled the optimal individual decision on occupational choice using the typical routine of maximizing an integral of a discounted utility function using Pontryagin's control theory methods (Weiss 1971).[57] A similar formulation had been used by his classmate Hajime Oniki (PhD 1968) for modeling demand for education (Oniki 1968). Oniki was the last of Uzawa's students at Stanford, completing his studies under Arrow's supervision.

Uzawa left Stanford in 1964 to the University of Chicago, where he stayed for a couple of years before deciding to leave the United States for good by the end of the decade, deeply affected by the social effects of the Vietnam War (Okuno-Fujiwara and Shell 2009, 401).[58] Nevertheless, he was still able to remain influential among Stanford's students. During the summer of 1964, Uzawa organized a summer school in Chicago which was attended by all of his Stanford's students (Spear and Young 2018, 1701). He repeated the summer seminar in the following year, this time mostly attended by MIT graduate students, including George Akerlof, Mrinal Datta-Chaudhuri, William Nordhaus, Eytan Sheshinski, and Joseph Stiglitz (Spear and Young 2018, 1719). Solow, who had become the major advisor of graduate students in the MIT by the 1960s (Duarte 2014, 89), was the dissertation advisor to all of them, with the exception of Datta-Chaudhuri, who had Samuelson as his advisor. By the influence of Solow, the group became interested in growth theory. The students had also been trained in Pontryagin's maximum theory, as Solow taught the technique in his economic theory classes (Burmeister 2009, 38). Also in 1964, Shell was appointed assistant professor in the MIT, where he played an important role among the group of graduate students (Boianovsky and Hoover 2014, 213). In the academic year of 1965-66, Shell organized a series of seminars on optimal growth models and extensions at the MIT. The published volume with the contributions presented in Shell's seminars (Shell 1967) features many of his

[57] Even in the 1960s, Reder was hardly an enthusiast of mathematical economics. Cass (Spear and Wright 1998, 537) in a later interview remembered his experience in class with Reder: "The micro class was more problematic. It was taught by a guy named Melvin Reder, a labor economist, and he came in the first day of class and put his feet on the chalkboard and said something depreciating about economic theory, so I never went back."

[58] Uzawa listed a number of reasons for his departure: "I thought that Stanford was too rich and languorous a place to bring up our children (…) Somehow I thought if they were brought up in such an apolitical and quiet and unexciting place… Then they would end up very dumb. That was the first reason why I decided to accept the offer from Chicago. There was another reason: the influence on me of Ken Arrow was too great and serious. I felt so small in front of him… I couldn't do anything on my own. I had to borrow his shadow. (…) There was another reason… at Chicago I had a close friend, almost my teacher: the late Professor Lloyd Metzler." (Uzawa in Okuno-Fujiwara and Shell 2009, 400). His move to the department of economics at the University of Chicago, which had recently purged the Cowles Commission to Yale, disgusted some of his mathematical economist fellows: "When I accepted the appointment at Chicago, quite a few people were very upset. Koopmans told me 'You are a traitor'" (401).

former colleagues at Stanford (Cass, Ryder, Goldman, Bruno) along with Solow's MIT students. In the introduction to the volume, Shell locates this new group of young PhD economists in a "Ramseyan" tradition, although Frank Ramsey's 1928 growth model was considerably different from this post-WWII development that relied on new mathematical developments in control theory (Duarte 2009). Indeed, the optimal growth model developed by Cass at Serra House during his graduate studies stands out as a much more clear influence to the class of models presented in Shell (1967), being cited in almost every chapter of the book.

The cross-disciplinary academic environment created at the Serra House was important to attract a remarkable group of economics graduate students at Stanford to mathematical economics. The collaboration of faculty and graduate students at the IMSSS produced a series of important contributions to the growing literature in mathematical economics in the passage of the 1950s to the 1960s. The influence of research done at Stanford was felt at other major economics departments such as the MIT and the University of Chicago. The dynamic atmosphere of the Serra House, nevertheless, contrasted with the situation of Stanford's Department of Economics. With few new vacancies and still operating on a small scale, the department could not keep up with the growth of mathematical economics at the IMSSS. As we see in the next section, this would restrain the continuation of the Serra House group at Stanford in the late 1960s.

5.2. A "too small" department of economics in the 1960s

The Serra House group was also important for the creation of the Operations Research Program, established in 1962 by Arrow's initiative within the School of Engineering. The program became a separate department in 1967. Arrow, Karlin, and Scarf were among the small group of five professors at the beginning of the program. Gerald Liebermann, from the department of statistics, took up the administrative tasks to organize the new Operations Research Department. Among some important faculty appointments in the department during the 1960s there were Harvey Wagner, a former undergraduate student at Stanford who had just got his PhD in economics from the MIT, and George Dantzig, one of the pioneers in the

new science of linear programming.[59] The intertwining of engineering and economics was not limited to the Operations Research program. By the end of the 1950, the Californian engineers brothers Russell and Sigurd Varian offered funds to establish a graduate studies fellowship in economics at Stanford to students "with engineering or scientific training" (Cherrier and Saïdi 2020, 92).[60] Roy E. Murphy, after receiving a Master's degree in Electrical Engineering from the University of Connecticut in 1956, began his graduate studies in Economics under such fellowship, completing his PhD in 1962.[61] With his graduate work supervised by Arrow, Murphy tried to bring the entropy concept from physics to economics. His dissertation was published in 1965, under the title *Adaptive Processes in Economic Systems* (Murphy 1965). Although it attracted the interest of people such as Richard Bellman (ibid, xi), the work faced some severe criticism in reviews published in important economics outlets. Karl Borch concluded his review of the book in *Econometrica* in a harsh tone: "The ideas behind the book are good, and they are 'in the air.' They are bound to be taken up by other economists, who probably will start from scratch, bypassing this book" (Borch 1968, 198). A critical review was also published in the *American Economic Review* (Gailitis 1968). Another engineer at Stanford's department of economics in the early 1960s was Lionel Stoleru (PhD 1963), although not with the Varian Fellowship. Arrow was the dissertation advisor to Stoleru, who was working along with Uzawa, Cass, and others on applying Pontryagin's Maximum Principle to solve optimal growth models (see Stoleru 1964). His particular model was designed to assess growth of the Algerian economy.[62]

Despite the limited impact of his research, Murphy worked from 1963 to 1966 as assistant professor at Stanford's department of economics. He was one of the few additions of mathematical economists to the economics faculty in the 1960s, despite the notable rise of interest in the subject among graduate students. The share of professors in the department teaching and doing research in mathematical economics did not change much in the 1960s. The plurality of the faculty appointed in the early 1950s meant that internal conflicts would arise in the decision of the hiring process. The new appointments in the first half of the 1960s

[59] On the importance of the Operations Research department for the development of economic theory in the following decades, see Cherrier and Saïdi (2020).

[60] The Varian brothers, particularly Russell Varian, had been very impacted by their Stanford education. Frederick Terman, their main mentor, became Provost at Stanford in 1955, serving in the office for ten years.

[61] Murphy's 1956 MS thesis is available at UConn Library: https://search.lib.uconn.edu/primo-explore/fulldisplay?docid=01UCT_ALMA21353044930002432&context=L&vid=01UCT&lang=en_US&search_scope=01UCT_ALMA&adaptor=Local%20Search%20Engine&tab=default_tab&query=lsr05,contains,58391561,AND&mode=advanced

[62] Stoleru had studied at the École Polytechnique and the École de Mines in Paris before joining Stanford. After the PhD, he returned to France where he had an important career in public administration – particularly during the François Mitterrand government in the 1980s.

meant to appease the different research groups. John G. Gurley and Ronald McKinnon, both appointed in 1961, had been students of Ed Shaw. Emile Despres, who also joined Stanford in that year, was linked to Lorie Tarshis (Harcourt 1982, 616). Paul A. David joined the department to work with Moses Abramovitz in the Research Center in Economic Growth.

Besides internal disputes between different research programmes for any new vacancies in the department, the market for graduated mathematical economists by the beginning of the 1960s decade was in short supply and in high demand. Marc Nerlove, the most relevant addition to the mathematical economics group at Stanford in the 1960s, experienced the consequences of the tight labor market in the beginning of his career. After completing his PhD in the Johns Hopkins University (his dissertation advisor being Carl Christ) in 1956, and serving for a year as a lecturer in his alma mater, the MIT and the University of Minnesota entered a fierce competition to hire Nerlove to their departments of economics. Minnesota won, to the dismay of Solow who considered him "certainly one of the best prospects of the recent years."[63] But Nerlove's stint at Minnesota was short lived, as Stanford stepped up with an even better offer and hired him to replace Houthakker, who left the department to Harvard in 1960. Nerlove's move was motivated by a promotion. If he was still an Associate Professor in Minnesota, Stanford offered a full professorship and tenured position.

Nerlove replaced Houthakker as the professor in the mathematical economics graduate classes. Courses offered in the fields of *Economic Statistics* and *Econometrics* in the first half of the 1960s remained quite similar with what was being offered in the second half of the 1950s. Additionally, Nerlove shared with Haley (until his retirement in 1963, then substituted by Reder) the required core courses in the *Price and Allocation Theory* field – which had become in the 1960s a mix of history of economic thought (taught by Haley) and modern microeconomics.[64] As an advisor to graduate students, Nerlove complemented the department's faculty as an econometrics specialist, which was lacking in the department.[65] Nerlove's most prominent student during his time at Stanford was the Swiss economist Pietro Balestra (PhD 1964). Their work on the estimation of a dynamic demand function for natural

[63] Robert Solow letter to Andreas Papandreou, October 28 1958. Robert M. Solow papers, Box 57, "Correspondence K" Folder, Rubenstein Library, Duke University. Papandreou was then Chairman of the department of economics at the University of California, Berkeley. Solow warns Papandreou: "good men in quantitative empirical economics are hard to find and much in demand... if you want to hire a first-class man as a normal assistant professor almost the best you can hope for is someone who has just finished his PhD." See the next chapter on Berkeley.

[64] See, for instance, Stanford University Bulletin 1962, 188-189.

[65] See for instance Jan Kmenta's interview (Lodewijks 2005) about his graduate years in Stanford in the late 1950s.

gas in the United States was an important breakthrough in the literature of panel data econometrics (Balestra and Nerlove 1966; Dupont-Kieffer and Pirotte 2011, 271-277).[66]

Besides the tenured professors Arrow and Nerlove, the mathematical economics courses were also taught by assistant professors, such as Murphy. Uzawa was appointed assistant professor in the department of economics in 1962, after a year as a fellow at CASBS, but soon left to the University of Chicago. Despite his influence among the mathematical economics graduate group, he had limited participation as professor in the department of economics. His advising of graduate students took place mostly within the IMSSS. In 1963, Uzawa was one of the four professors sharing the *Price and Allocation Theory* classes, and the *Special Topics in Mathematical Economics* class covering topics in economic growth and planning, which was the topic of his research with graduate students at Serra House. G. S. Maddala was another assistant professor who taught mathematical economics at Stanford, having been appointed in 1964. He had just received his PhD from the University of Chicago, where he studied econometrics under Zvi Griliches.[67] After his arrival, the *Econometrics* field for graduate students increased the number of courses offered, including *Linear Programming*, *Non-Linear Programming and Welfare Economics*, and *Dynamic Programming*.

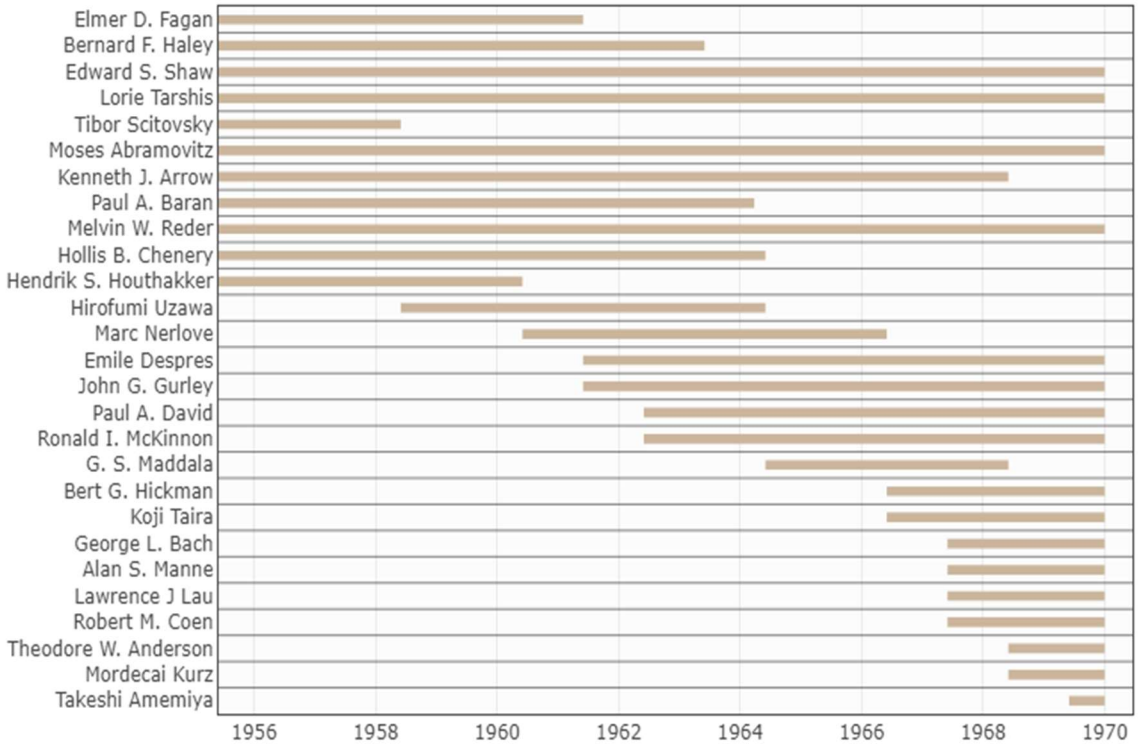
In 1965, another major reform took place in the organization of Stanford's economics graduate program. A "Core Theory Curriculum" was created, including History of Economic Thought, Price and Allocation Theory and Theory of Income and Economic Fluctuations. Two seminars, one in "Macroeconomics" and another in "Microeconomics", appeared for the first time as a core part of the curriculum (*Stanford University Bulletin* 1965, 263). Indeed it was the first time the now widespread used macroeconomics and microeconomics labels appeared in the curriculum for graduate students at Stanford. The *Economic Statistics* and *Econometrics* fields of study were reorganized in *Mathematical Economics* and *Econometrics*. The former encompassed courses in programming taught in the previous year, plus the new

[66] The estimator due to this work has become known in the literature as the "Balestra-Nerlove estimator". See Hausman and Taylor (1981, 1381), and Arellano (2004, 35).

[67] Maddala was born in 1933 in India, in a family of modest means. He moved to Bombay in 1947 to enter college, but illness (he contracted tuberculosis) and political unrest during the Partition of India led him to drop out of school after six months. His older brother died in Pakistan, and afterwards his mother became ill and also passed away. Maddala was only able to return to college in 1953, through a 2-year college degree in a minor school at Ajmere, Rajastan. In 1955 he returned to Bombay, where he entered a master degree course in statistics, completing his studies in 1957. Maddala was accepted in the PhD program of statistics at Berkeley, where he hoped to study under Jerzy Neyman. However, without proper funding, he could not move to the United States. In Bombay, Maddala became a research assistant at the Bombay Institute of Economics. In 1960, he received a Fullbright Fellowship, but instead of applying to statistics, he went for a PhD in economics at the University of Chicago. According to him, the reason was the promise of a guaranteed job at the Bombay Institute of Economics after completing his studies.

course *Dynamical Programming*, and the *Special Topics in Mathematical Economics* course. The courses were taught by Arrow and Murphy in 1965. The new *Econometrics* field included the basic statistics courses from *Economic Statistics* (still taught by professors from the statistics department), and two quarter courses named *Econometrics I* and *II*, taught by Maddala and Nerlove. Although the Econometrics field existed in the department since 1956, the Econometrics courses created in 1965 were the first comprehensive courses in modern econometrics offered in the department – which might seem confusing, but illustrates how the meanings of "mathematical economics", "econometrics", or macro and microeconomics, were not yet stabilized during the 1950s.

Figure 1.3: Stanford's Economics Faculty (1955-1970)



The reform was not enough to appease dissatisfactions among the mathematical economics researchers who were longing for a larger number of appointments. With the particular exception of Murphy, none of the graduate students who excelled at Serra House was hired by the department. Also, there was no indication of an expansion in the numbers of mathematical economists in the department in the following years. The mathematical economists at Stanford shared the view that they were a group of success and were disappointed by not seeing any increase in their numbers within the department. This led to a general disbandment in the second half of the 1960s (see Table 1.1). Arrow, the leader of the

group, has voiced some setbacks while remembering that period in an oral history interview (Arrow 2011, 91-92):

> I left (…) because I felt that Stanford could be in a position—maybe I was being grandiose—of being an absolutely first class department, but that one of the problems was that we were too small. The department was highly expanded by 1968—the situation was much better than in 1949--but nevertheless, I felt it that it was hardly optimum. One of the department's basic problems was that there were a lot of bright young people coming up but we couldn't make offers because there weren't any vacancies.
>
> I visited MIT, where I had some very good friends, in the fall of '65, and they had a student [Robert Hall] who was very bright. So I wrote back to the department to hire him. There had been a few problems in the department because I was pushing young people, and there was a suspicion I was pushing just mathematical types. I later got to say that I recommended three people who got the Nobel Prize and the department didn't appoint them. [Laughter] The department was not unresponsive. I felt I did not push only mathematical types, [but] that I was pushing policy, whatever it was.
>
> But, you know, this fellow looked like a first class student, so I wrote back and said we've got to hire him, and the answer I got back was, we have no vacancies in the department. (Arrow 2011, 91-92)

The same disappointment was expressed by other mathematical economists such as Maddala.[68] The Stanford group of mathematical economists engendered within the cross-disciplinary spaces of the behavioral sciences made their impacts to the mathematical economics done in the various departments to where they left. As previously discussed, Uzawa and Shell organized relevant seminars and workshops in Chicago and the MIT disseminating the modeling strategies to growth theory developed at Serra House. Yale University, where the Cowles Commission had been established since 1955, became the new home not only to Yaari, Nerlove, and Cass, but also to Scarf, who was appointed professor in the department of economics and statistics in 1963. Indeed it was Scarf who impacted Yale's department of economics the most, since Yaari left in 1967 to Hebrew University, Nerlove in 1969 to Chicago, and Cass in 1970 for Carnegie-Mellon University in Pittsburgh. Scarf tutored a group of mathematical economists at Yale in the 1960s and 1970s, including Rolf Mantel (PhD 1966), Duncan Foley (PhD 1966), M. Ali Khan (PhD 1973), and Timothy Kehoe (PhD 1979). The department of economics at Berkeley, which since the end of the

[68] See also Maddala (Lahiri 1999, 757): "In 1967 I was offered a tenured job at Rochester. Arrow, Nerlove, and Uzawa had left Stanford, and I was advised to leave too. That is how I moved to Rochester."

1950s was expanding its group of mathematical economists, became the new home for Steven Goldman and Aaron Douglas, the former having retired from the institution in 2006. Arrow spent a decade at Harvard before returning once again to Stanford's economics. During this period he advised three graduate students who would later be awarded a Nobel Prize in Economics: Michael Spence (PhD 1972), Roger Myerson (PhD 1976), and Eric Maskin (PhD 1976).

Table 1.1: Stanford Diaspora, from Houthakker to Arrow

Position at Stanford	Name	Departure Year	Institution
Professor	Hendrik Houthakker	1960	Harvard
PhD	Menahem Yaari	1962	Yale
Professor	Hirofumi Uzawa	1964	University of Chicago
PhD	Karl Shell	1964	MIT
Professor	Marc Nerlove	1965	Yale
PhD	David Cass	1965	Yale
PhD	Steven Goldman	1965	University of California, Berkeley
PhD	Aaron Douglas	1965	University of California, Berkeley
PhD	Harl Ryder	1966	Brown
Professor	G. S. Maddala	1967	University of Rochester
Professor	Kenneth Arrow	1968	Harvard

6. Concluding Remarks

With Arrow's departure to Harvard in 1968, the entire original group of Serra House economists had left the department. However, this disbandment did not make mathematical economics disappear from Stanford, since the departures meant that there were new open vacancies. With few senior economists available in the market as substitutes to Arrow, the statistician Theodore W. Anderson was appointed as professor of Economics and Statistics in 1968. A 1945 Princeton graduate in mathematics who did research in statistics, Anderson had worked previously in the Cowles Commission and in the department of statistics at Columbia (Phillips 1986, 254). His appointment at Stanford was conducted through the department of statistics, and the offering of a joint position at economics followed Arrow's departure to Harvard (Anderson 2012, 15). In the department of economics, Anderson shared the *Econometrics* graduate classes with Takeshi Amemiya (PhD 1964 Johns Hopkins

University).[69] Amemiya had visited the IMSSS in 1966 as postdoctoral researcher, then spent two years as a researcher in Japan (his home country) before returning to Stanford as a professor in the department of economics by the end of the decade (Powell 2007, 166). To lead the field of *Mathematical Economics*, the department hired Mordecai Kurz out of the Hebrew University. Kurz, a 1962 Yale PhD in Economics, had his Serra House connections. He was at the IMSSS in the early 1960s as postdoctoral researcher before moving to Israel in 1963. Kurz's early work in two sector models of growth was directly related to Uzawa's models. He replaced Arrow in his position as the director of the economics section of the IMSSS. The department of economics also went after recent PhDs, such as Lawrence J. Lau (MA 1966, PhD 1969), who did his undergrad at Stanford in the early 1960s before moving to Berkeley for graduate studies in economics under Dale Jorgenson.

The substitutes to the departing mathematical economists still did not make it hegemonic within the department of economics. Cross-disciplinary spaces remained very important for research in mathematical economics at Stanford. The IMSSS continued to receive generous funding under the leadership of Kurz, while the Operations Research department would put forward some influential research in market design, particularly under the leadership of Robert Wilson (Cherrier and Saïdi 2020).

[69] Amemiya had applied for a PhD at Stanford, not intending to study econometrics, but socialist economics under Paul Baran (Powell 2007, 163). However, he was not accepted by Stanford as a graduate student and moved to econometrics at Johns Hopkins by the influence of Carl Christ and Geoff Watson.

CHAPTER 2: From Griffith Evans to the Evans Hall: Mathematical Economics's Forays into the Department of Economics of the University of California, Berkeley

The Department of Economics at the University of California, Berkeley currently occupies the fifth and sixth floors in a tall concrete building of brutalist architecture situated at the northeast corner of campus. It has as neighbors the Department of Mathematics and the Department of Statistics that occupy the floors above and below. This grouping of academic departments seems to justify the building's name, Evans Hall (see Figure 2.1). It is a tribute to Griffith Conrad Evans, the head of the Department of Mathematics at Berkeley from 1934 to 1949. Evans was responsible for the appointment of Jerzy Neyman in 1938 to the department, and Neyman was later responsible for the creation of the Department of Statistics in 1955. Also, Evans had a prolific research in mathematical economics in the later years of his career. In Berkeley, the major part of his graduate students in the Department of Mathematics worked on mathematical economics.

Moreover, nowadays a group of professors share appointments between economics and mathematics or statistics. However, the current locational and research proximity among the Department of Economics with its neighbors at the Evans Hall is a more recent development. The department only completed its transition to Evans Hall slowly from 1989 to 2005, as more space was becoming available in the building (Moore 2007, ch. 15). Back when Evans was professor in the Department of Mathematics at Berkeley, between 1934 and 1950, despite cultivating a relatively dynamic environment of research in mathematical economics in his department, there was almost no interest coming from the Department of Economics. Some important members of the department's formative years have expressed their contempt towards the transformation of economics from a verbal to a mathematical science in the postwar period. Ira Cross, Chairman of the department in the 1920s, acknowledged in a 1967 interview that he "still receive[d] the American Economic Review, but so many of the articles are based on calculus reasoning that [it] is no longer interesting to me. Personally, I cannot see what is accomplished by the use of calculus in analyzing human behavior" (Cross 1967, 10). The social economist Emily Huntington retired in 1961, "two years earlier than the date of compulsory retirement", one of the reasons being, on her account, the rapid development of mathematical economics resulting "in the neglect of other types of methodology and analysis" that social economics could offer (Huntington 1971, 89).

Figure 2.1: The Evans Hall

Photo Courtesy by Roberto Hsu, 2019

The Evans Hall was planned in the 1960s and inaugurated in 1971 to serve as a new home to the Departments of Mathematics, Statistics and Computer Science. The Department of Economics then occupied the Barrows Hall, sharing the space with other social science departments. However, by the time of the inauguration of the Evans Hall, there was a group of economists whose work had more resemblance to what was going to be done in the new building than to what was being done at Barrows. Indeed, some faculty shared appointments in the departments of economics, mathematics, and statistics, such as Roy Radner and David Gale. This group of mathematical economists moved their offices to Evans Hall by the time of its inauguration, a pair of decades before the reallocation of the Department of Economics.

For mathematical economics to find space at Berkeley's Department of Economics it was no smooth transition. A small group of mathematical economists (including Robert Dorfman and Hans Brems) were appointed as professors in the department in the early 1950s. However, by the middle of the decade they all left Berkeley, seeing the department as hostile to their area of research. It was necessary for the mobilization of various actors across the university administration for a group of mathematical economists to endure in the department. That was mostly accomplished by the appointment of Andreas Papandreou as professor and head of the department of economics in the middle of the 1950s. Arbitrating and settling down conflicts in the department, Papandreou was able to appoint as professors a group of recent PhDs that did research in mathematical economics that became known in the department as the "Andy´s Boys". Their endurance and further expansion in the 1960s to what would become the Evans Hall group in the early 1970s was supported by their continued participation in the department administrative affairs, best represented by Roy Radner's term as Chairman of the department in the second half of the decade.

In this chapter I trace the forays of mathematical economics into Berkeley's Department of Economics, the resistances it encountered and how it mobilized various actors who managed to grant it an important institutional space in the department. In section 1, we study the plurality of approaches to economics developed in the department, from its foundation in 1903 to the late 1940s, and their common skepticism towards the uses of mathematics in economics. The first strides of mathematical economics in the university took place in the Department of Mathematics after the arrival of Evans as its new Chairman, as we shall see in section 2. Evans was the advisor of a considerable group of graduate students who did research in mathematical economics, but his influence in the Department of Economics was very limited. It was only with the arrival of Dorfman in the postwar period (first as graduate student and then as professor) that the subject gained some traction within the Department of Economics. However, the group could not get established in the department in the conflictive period of the early 1950s. In section 3, we explore how the turbulent political atmosphere of that period impacted the resistance from established faculty towards the mathematical group. To cool down such tensions in the department, Berkeley's Chancellor Clark Kerr needed to appoint a chairperson with the necessary interpersonal skills to settle conflicts and reorganize the department. As we see in section 4, his choice was Papandreou, who was hired from Minnesota to accomplish such a task as the head of Berkeley's economics. In section 5 we follow the strategies adopted by Papandreou as Chairman to

gather support from established faculty, while restructuring teaching and research in mathematical economics through the appointment of recent PhDs who did research in the subject, including Roy Radner and Dale Jorgenson. They played a central role in the expansion of mathematical economics at Berkeley in the 1960s. In the last section of this chapter we analyze how the "Andy's Boys" continued mobilizing the networks Papandreou established with the university administration to expand their group and create an important milieu for mathematical economics in the United States. That group would eventually move to Evans Hall in the beginning of the 1970s. Thus, by telling the history of the growth of mathematical economics at Berkeley, through the necessary negotiations and mobilization of actors in the university administration, we seek to deepen our understanding of the development and fast expansion of this intellectual community in the postwar period.

1. Prelude: Berkeley's department of economics before World War II

1.1. The first years of the Department of Economics (1903-1932)

The Department of Economics at Berkeley was created in 1903, coming from the larger and older Department of History and Political Science. At its foundation, there were only three professors listed as full faculty: Carl Plehn, public finance scholar, professor in the university since 1898 (Johnson 2014); Adolph C. Miller, the first chairman, hired from Chicago University to take up on this role, and also a public finance scholar; and Miller's brother-in-law, Wesley C. Mitchell, who had in Berkeley his first academic appointment after graduating from Chicago University under Thorstein Veblen.[70] Associates complementing the teaching duties included professors from other departments and young doctorates with appointments either as instructors or lecturers. Some of them would become full professors later, such as the social economist Jessica Peixotto, Plehn's former student (Cookingham 1987). Other important professors joined the department in its first decade of existence, such as the railroad economist Stuart Daggett and the accountant Henry Hatfield, both of whom would spend their entire career in the department. In that first decade, the bay area became an interesting milieu of American Institutionalism, also because Veblen and Harry Millis were

[70] Information on members of the department of economics in this section comes from the 1903 to 1941 volumes of the Register of the University of California.

teaching at Stanford, in the department headed by Allyn Young, Mitchell's friend (ibid). It was in Berkeley that Mitchell drafted most of his 1913 book on business cycles.[71]

The department was still small in its numbers when it went through the first important changes in faculty. Mitchell left Berkeley to Columbia University in 1913 and Miller left in the following year to become a member of the newly created Federal Reserve Board. With their replacements, Carleton Parker and Ira Cross, the field of labor studies thrived in Berkeley in that decade. Parker left in 1917 to head Washington University's department of economics, but suffered a premature death in the following year (Perlman 1958, 40-1). During his brief period at Berkeley, he supervised important studies on migrant labor in the California Commission on Immigration and Housing, along with his Berkeley students Frederick C. Mills and Paul Brissenden (Rutherford 2012, 192-197). Cross was an undergraduate and master student at the University of Wisconsin, and through a recommendation from John Commons to Young, completed his doctoral studies at Stanford in 1909. He was working as teaching assistant to Veblen in Stanford before joining Berkeley as professor in 1914 (May and Dimand 2009, 191). Cross became well known in Berkeley for his lectures in elementary economics (Ariff 1967). Finally, Solomon Blum was another labor economist to join the department in 1918. At Berkeley he wrote the textbook *Labor Economics*, published in 1925 as one of the first textbooks in the field (Blum 1925). Social Economics also grew in the department with the appointment of Lucy Stebbins, Jessica Peixotto's student, in 1917.

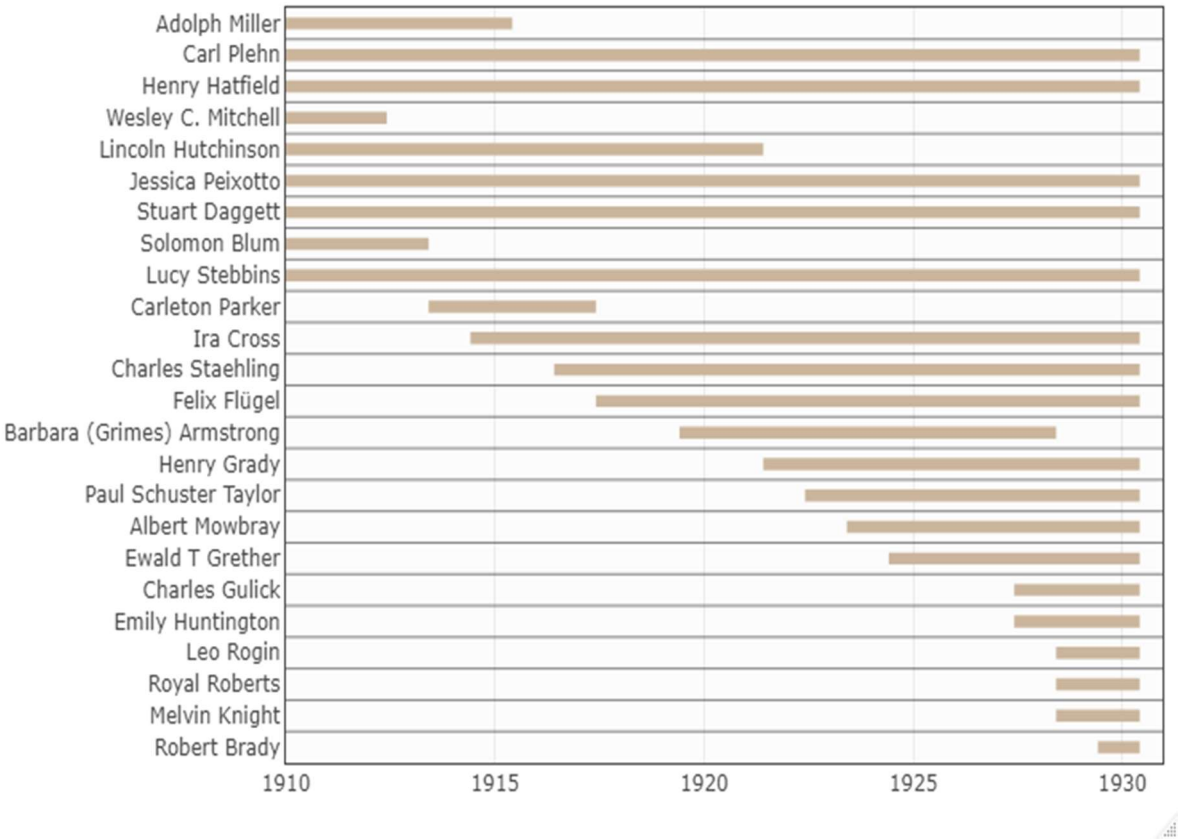
The 1920s was a decade of rapid expansion for the department, who saw its numbers raise from ten professors in 1920 to twenty in 1929, on the eve of the Great Depression. The department's national status received a boost with the election of Plehn, then the Chairman of Berkeley's department of economics, as president of the American Economic Association in 1923. His successor as president of the AEA would be Mitchell, then at Columbia University.

The department expanded mostly in the same intellectual lines. Studies in social economics continued to flourish at Berkeley in the 1920s. Peixotto headed the Heller Committee on Research in Social Economy in that decade, which was a crucial source of support for research in the field. Along with Professor Lucy Stebbins, they created a dynamic group of research in the department joined by many doctoral students and researchers who

[71] On the economic ideas that emerged through Mitchell's work in business fluctuations, see Morgan (1990, ch. 2) and Friedman (2014, ch. 5).

were mainly women (Dzuback 2006). Emily Huntington, a graduate from Radcliffe College, was hired in 1928 to the social economics group in Berkeley. Many of the new appointments in the department during that decade were Berkeley graduates, such as the social economist Barbara (Grimes) Armstrong, labor economists Paul S. Taylor and Ewald T. Grether, economic historian Felix Flügel, and insurance and actuarial science specialist Albert Mowbray. The latter was appointed in 1923, in a return to academia after spending a thriving career in insurance business after his graduation from Berkeley in 1904. Mowbray replaced Plehn as the Chairman in 1928. Graduates from Columbia's institutional economics milieu were welcomed as Berkeley's faculty in the second half of the decade. Charles Gulick was appointed professor in 1926 and Robert Brady in 1929. Some important late institutionalists were graduate students at Berkeley at that time, such as Allan Gruchy and Douglas Dowd (Rutherford 2011, 339).

Figure 2.2: Berkeley's Economics Faculty (1910-1930)



Berkeley in the 1920s created yet another space for institutionalist influence: the department of agricultural economics was founded in 1926 and had a large influence of

Wisconsin's institutionalism. Henry Erdman, who graduated and taught economics at Wisconsin, was hired in 1922 to the small Rural Institutions division in the College of Agriculture. In 1926, it merged with other small divisions, such as Farm Management, Agricultural Education and Agricultural Extension to become the Agricultural Economics Department. The new department got closer to the Department of Economics, as members of the latter taught a quarter of the classes for the Agricultural Economics program. Erdman helped to organize the new program following the agricultural economics he learned in Wisconsin from Henry Taylor and Richard T. Ely.[72] One of the most famous graduates coming out from Berkeley's Agricultural Economics program before World War II was John Kenneth Galbraith, who received his degree in 1934. His doctoral dissertation was a public finance study of county expenditures in California, which illustrates the intersections of thematic interests between economics and agricultural economics at Berkeley (Parker 2005, ch. 1).

The Great Depression stalled the expansion of the department of economics due to necessary budget cuts. Moreover, changes in the university administration would foster changes in the department. Under the leadership of a professor coming from Stanford's Food Research Institute, the appointments of a new generation of economists coming from Harvard and Cornell reduced the influence of institutionalism and social economics that was salient up to the 1920s in Berkeley.

1.2. Robert Sproul, Robert Calkins, and reform in the Department of Economics (1932-1949)

The election of Robert Gordon Sproul as the new president of the University of California in 1930 meant a reassessment of the organization of various departments, including the department of economics and others such as the department of mathematics (see next section). Sproul appointed a committee composed of members of other departments to evaluate how economics was being done at Berkeley. The committee recommended changes in the department, as narrated by Grether in an interview:

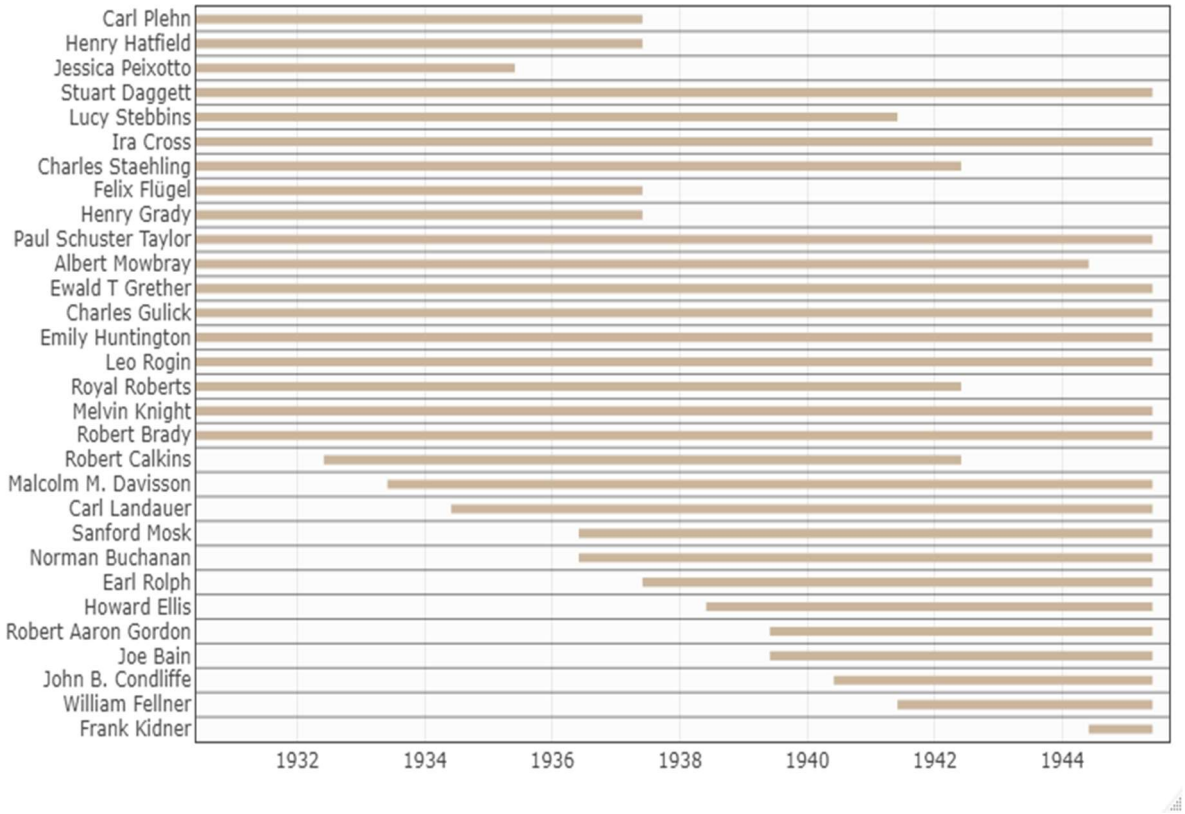
> In the year 1935, President Sproul (…) appointed a committee to consider the present status and future development of the Department of Economics. Why he did this, I'm not certain; he must have had some questions as to

[72] On Erdman's role in the institutionalization of Berkeley's agricultural economics, see his interview for the Berkeley Oral History Project, Erdman (1971).

> what was going on here. (…) [The committee suggested] that every effort be made to secure as chairman of the academic department a scholar of outstanding qualifications. Then finally this little thing that must have really been enjoyed by the department: that pending the appointment of a new chairman, the status quo in respect to academic rank of all members of the department be maintained. (…) No promotions. So there must have been in the minds of the president or somebody some questions about what was going on in the department. (Grether 1993, 490-492)

If motivations for doing so are unclear, it is certain that Sproul wanted a profound change in leadership. The newly appointed Chairman was not related to any of the established groups of researchers from the department of economics. Robert D. Calkins had arrived in Berkeley as a lecturer in 1932, before completing his PhD studies in Stanford's Food Research Institute (FRI) under Holbrook Working. Calkins had a fast rise at Berkeley, going from an untenured assistant professor in 1933 to being appointed the Chairman of the Department of Economics in 1935, at the age of 32, by newly elected president Sproul. He continued in the Chairman position until 1940, and left Berkeley in 1941 to become the head of Columbia University's Business School. During his term as Chairman, the department recovered from the budget cuts taken due to the Great Depression, with the number of faculty reaching a total of twenty six appointed professors at the end of the decade. Also in the second half of the decade, some of the most influential older members of the department retired, such as Peixotto, Plehn, Hatfield and Stebbins, opening vacancies for more appointments. However, if before Calkins the major source of new appointments were Berkeley or Columbia graduates, the new group of hired professors had graduated from Harvard and Cornell. From the former university came Howard Ellis and Aaron Gordon in 1938, and Joe Bain in 1940, and from the latter Norman Buchanan in 1937 and Earl Rolph in 1938. It was during Calkins's term as Chairman that the first courses in mathematical economics appeared in the Department of Economics, although they were offered by a professor from another department – indeed, the chairman of the department of mathematics, Griffith Conrad Evans.

Figure 2.3: Berkeley's Economics Faculty (1930-1945)



The Department of Agricultural Economics also went through a transformation in the 1930s, with the establishment of the Giannini Foundation as a major sponsor of the department. The foundation was initially under the direction of Howard Tolley and then of Carl L. Alsberg, the latter being one of the founders of the FRI at Stanford in 1921.[73] As chairman Erdman noted, "with the arrival of Dr. Carl Alsberg as director [in 1937], particularly when he added staff members, such as George Kuznets and Sidney Hoos… heavy stress [was placed] on the mathematical aspects of the training, and requirements for taking courses in the mathematics departments, and maybe some supplement over here, heavily on that statistical side" (Erdman 1967, 111). Kuznets, Hoos and their students played an important role in the department of economics later in the 1950s.

Calkins's departure from the department of economics in 1941 was not an exception with the outbreak of World War II. Other professors such as Ellis, Gordon, Rolph and the social economist Huntington left the department for some period to serve in the war. The

[73] It was at the FRI that Alsberg, formerly a chemical biologist in the United States Department of Agriculture, "took an active interest in economics… He was especially fascinated by the problem of adapting the methods of research employed in the natural sciences to research in the social sciences" (Voorhies 1941, 33).

1940s proved to be a difficult time for the university and particularly for the economics department. The creation of the School of Business in 1942 and the tight budget during the war effort meant that funding from the economics department was reallocated to business. By the end of the decade, even after the return of faculty from the war, the department was reduced in its numbers to twenty listed professors. During the war years, Malcom Davisson served as the Chairman, remaining in the post until 1951. Davisson studied in Berkeley during his undergraduate years in the 1920s, and moved to Harvard for his graduate studies, receiving his PhD in economics in 1932. He began working at Berkeley as soon as 1931 as a lecturer, becoming professor in 1935. Davisson substituted Plehn after his retirement as a professor of public finance and was not part of Calkins's attempted reform. The tensions between Calkins's appointed faculty and the first generations of Berkeley's economists would soon reappear after the end of the war.

2. The first wave: Mathematical economics in the department of mathematics

2.1. Griffith Conrad Evans: from mathematics to mathematical economics and to Berkeley

Mathematical economics arrived in Berkeley not through the department of economics, but as an unintended consequence of a reform that the department of mathematics went through in the 1930s.[74] The then recently appointed president Sproul received from his provost Monroe Deutsch a report on that department in 1932: "The Department seems to most men rather dead… In view of the importance of mathematics, it seems to me that we should… bring in a strong man as professor" (Deutsch apud Moore 2007, 55). Sproul followed a recommendation from a committee headed by the local star chemist Gilbert N. Lewis, and negotiated the hiring of the mathematician Griffith Conrad Evans to become the new head of Berkeley's mathematics (ibid, 58). Evans was then at Rice Institute and enjoyed the respect of the American mathematics community. He had been elected vice-president of the American Mathematical Society in 1924, and beginning in 1927 he served as the editor of the *American Journal of Mathematics*. In 1933 he was elected to the National Academy of Sciences. Evans's appointment at Berkeley in 1934 therefore commanded a high salary, even amidst the

[74] The department of mathematics was under the direction of Mellen W. Haskell from 1909 to 1933. During this period, the department focused on its teaching mission rather than developing research. The lack of elite research done in the department was the central reason why it received a poor grade from administration. See Moore (2007, ch. 4).

Great Depression that was causing cuts in faculty salaries all across the university (Rider 1988, 285-88).

Evans was a Harvard trained mathematician, having completed his PhD in 1910. Through a scholarship earned during his graduate studies, he spent a couple of years at the University of Rome as a postdoctoral student, where he studied under the Italian mathematician Vito Volterra. His stint at the University of Rome exerted crucial influence on his interests and methodological views of mathematics (Weintraub 2002, 42). In 1912, he returned to the United States as an assistant professor of mathematics at the Rice Institute (now Rice University). Evans became a full professor in 1916, and during that decade he published several works on integral equations and potential theory, areas to which Volterra also had major contributions. Volterra's view of scientific rigor (and therefore mathematical rigor) as an inquiry based directly on the underlying physical reality, apprehended through experimentation and observation, and not on axiomatic abstract structures (Weintraub 2002, 48), permeated Evans's own views of mathematics. Such views informed Evans's work in mathematical economics, a research interest he started pursuing in 1920 (Pomini 2018).[75] He published a series of papers in journals targeted to a mathematically trained audience, including a mathematical treatment of a static oligopoly problem (Evans 1922), and an application of the classical calculus of variations in a dynamic monopoly problem, where demand was linearly dependent of price variation (Evans 1924).[76]

In 1930 Evans publishes the book *Mathematical Introduction to Economics*, which covers several applications of mathematics to economic problems: monopoly, units of measurement, competition, price, cost and demand, taxation, tariffs, rent, rates of exchange, the theory of interest, the equation of exchange and price level indices (Weintraub 2002, 62-63). In the last chapters, Evans puts forward a critique of utility theory as used by past mathematical economists such as Jevons and Walras. He criticizes it through the integrability problem and the non-quantifiable character of the theory.[77] Such rejection of utility theory became an unorthodox stance in mathematical economics in the postwar period that rendered

[75] Pomini (2018, 62) presents two letters from Evans to Volterra in which the former discusses his research developments in mathematical economics. The first was written in 1920, where Evans tells his former adviser that he began studies in that field, whereas the second, from 1925, shares his project of writing a textbook in mathematical economics. That would become his 1930 volume *Mathematical Introduction to Economics* (Evans 1930).

[76] On Evans's contribution to the application of calculus of variation in economics and his dynamical economic models, see Weintraub (2002, ch. 2), Duarte (2016) and Reginatto (2019).

[77] Volterra had also discussed the integrability problem in his 1906 review of Pareto's *Manual of Political Economy*, what Paul Samuelson called "one of the few services professional mathematicians have ever rendered to economic theory" (Samuelson 1950, 355).

Evans a marginalized position in the field (Weintraub 2002, ch. 2). He did however impact the beginnings of the Econometric Society and the Cowles Commission, mainly through his former student at Rice, Charles Roos, one of the founders of the former and the first research director of the latter (Louçã 2007).

2.2. Mathematical Economics and Statistics in the Department of Mathematics

Evans continued working on mathematical economics in Berkeley, from his arrival to his retirement in 1954. Since his arrival in the university, Evans dedicated a part of his teaching load to mathematical economics. Beginning in 1935, he offered an undergraduate course in the Department of Mathematics titled "Mathematical Introduction to Economics." The course covered topics in "monopoly, competition, theory of dimension, taxation, utility, (and) economic dynamics" (Register University of California 1935-36, v.1, 298) and it can be inferred that the courses' reading assignments might have included Evans's homonymous book.

In addition to teaching mathematical economics, between 1935 and 1940 Evans supervised the dissertations of a group of mathematics graduate students on this topic. Among them, the names of Francis W. Dresch, Ronald W. Shephard, and Kenneth O. May stand out. Evans worked closely with his students. Dresch and Shephard developed in their graduate work some results in index numbers and aggregation that were a central part of Evans's 1937 presentation at the Third Annual Research Conference organized by the Cowles Commission. Dresch completed his PhD in 1937, while Shephard received his degree in 1940. May's path at Berkeley was particularly troubled. He was a stellar undergraduate student who caught the attention of Evans during his first years at the university. After completing his master's degree in 1937, and while pursuing his PhD studies, he was hired as a teaching assistant at the department of mathematics. Evans and May co-authored a paper in mathematical economics, published in 1939,[78] but May's path in the university was interrupted in 1940. Due to his publicly-known connections with the Communist party, May was expelled from Berkeley in

[78] The paper was published in *Reports of a Mathematical Colloquium*, series 2, no. 1, edited by Karl Menger. In their paper, Evans and May discussed conditions for the stability of equilibrium in a model with two producers. They applied their model to labor markets to argue that a union able to control the labor supply could make the adoption of machinery unprofitable for the capitalist.

that year. It was only after serving the war that he was pardoned and able to return to the university and finish his PhD in 1946 (Jones, Enros and Tropp 1984).[79]

Another field Evans planned to build in the department of mathematics at Berkeley was statistics. In 1936, he was able to attract the famous statistician Ronald A. Fisher to be a Hitchcock Lecturer in the department, spending a total of three weeks in Berkeley. His lectures, however, were not very well received among Evans's colleagues, in particular the chairman of the physics department, Raymond T. Birge (Reid 1998, 143-144; Moore 2007, 73).[80] A researcher with interests in statistics himself, Birge suggested to Evans the appointment of Polish statistician Jerzy Neyman, who was then teaching at the University College London (Reid 1998, 146). After completing his PhD dissertation, Evans's student Dresch moved in the fall of 1937 to Cambridge University in the hope of studying under John Maynard Keynes (146). Evans seized this opportunity to ask Dresch to attend lectures by statisticians in the UK under consideration by the department, including Neyman. In November, Dresch wrote back to Evans: "I have just returned from London with a most favorable impression of Neyman… [Michal] Kalecki claimed that some rated Neyman as the greatest theoretical statistician on the continent…" (147). Soon after, Evans sent a letter inviting Neyman to "coordinate the work in statistics in the University" (148).[81] This prospect of building a statistics group from zero would prove important for Neyman's decision to join Berkeley in 1938, where he started to develop the Statistical Laboratory (156). However, Evans's insistence that statistics remained as part of the mathematical department made the creation of a separate department of statistics under Neyman possible only after Evans's retirement.

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[79] May became a critic of the non-quantifiable axiomatic mathematical economics of the postwar period and of the marginalist tradition as a whole, following Evans's concerns with non-quantitative reasoning. May's review of Samuelson's *Foundations* concludes that "the book is concerned almost exclusively with formal mathematics structure… primarily with the formal foundations of only a limited sector of economics – marginal statics and comparative statics… Leo Rogin [professor of economics at Berkeley] once remarked that this type of theory reminded one of a painstaking scientific description of a unicorn, except that a unicorn is more like a horse than marginal theory is like the real world" (May 1948, 93). His last published paper in mathematical economics (May 1954) criticized utility theory, both cardinal and ordinal, for being unable to deal with common violations of the transitivity axiom. However, utilitarian concerns remained a central part of mathematical modeling in economics, and May eventually abandoned his research interests in mathematical economics, becoming an important researcher in the history of mathematics (Jones, Enros and Tropp 1984).

[80] When Birge discovered that Fisher was coming to Berkeley in 1936, he wrote to the Dean "interested to know who was directly responsible for getting Dr. Fisher to come here, since… I have not yet been able to find anyone here at Berkeley… interested [in his] work" (Reid 1998, 143).

[81] As Reid (1998) showed, it is hard to argue that Dresch's letter impacted Evans's initial decision. Dresch's letter to Evans was dated from November 8, 1937, while Evans's letter to Neyman offering him the position was dated from November 10 in the same year. However, it may have ratified the decision.

Dresch also returned to Berkeley in that year as an instructor in the department of mathematics. After helping in the recruitment process of Neyman, he worked as an assistant in the Statistical Laboratory, sharing the teaching load in Neyman's statistics classes. As he remembered, teaching with Neyman also meant learning such a new approach to the subject, since "[w]e were all kind of learning statistics together – Neyman's kind of statistics at any rate… we all attended Neyman's lectures" (Dresch apud Reid 1998, 168). During his first year in Berkeley, Dresch also taught Evans's undergraduate course in mathematical economics. In 1939, Dresch and Evans joined forces and offered a course in "Mathematical Economics" to graduate students for the first time in the department of economics. The course demanded as a prerequisite the "Mathematical Introduction to Economics" course offered in the department of mathematics, and the topics discussed in class were the "economics of a simplified system; moving equilibrium, and dynamical economics."[82]

Such approximation of mathematics and economics proved important to Lawrence Klein, an undergraduate student coming from Los Angeles College, joining Berkeley economics in 1940. Although his "undergraduate advisers gave [him] no encouragement to study mathematics along with economics" he still did it, finding in the library "journals for a whole budding subject, carried out at an advanced level" probably stemming from Evans's interest in mathematical economics (Breit and Spencer 1982, 22).[83] He remembered in an interview that "[t]here was rather a good environment in Berkeley around 1940 for someone interested in mathematical economics and econometrics, though it was not a leading center for mathematical economics. At that time mathematical economics was really fighting for recognition, and in many respects it was blocked or suppressed" (Klein 1987, 410). Klein learned index theory from Dresch's classes, crediting him for the inspiration in his "first attempts to deal with the problem of aggregation" (Klein 1997, xx-xxi). He participated in Neyman's seminars, as remembered by Dresch: "At one point, I remember, Neyman was also conducting a seminar in economics, (…) which was held at Evans's house. Evans was there… Also Larry Klein… I think our first student from 'outside' was (George) Dantzig" (Dresch apud Reid 1998, 168). Dantzig was then a graduate student under the supervision of Neyman

[82] The course's description in Berkeley's Register carries a clear resemblance to Dresch's work presented in that same year in the Cowles Commission's Fifth Annual Research Conference, *A Simplified Economic System with Dynamic Elements.*

[83] Another instance of the distaste in this period of the economics faculty for the mangling of mathematics and economics comes from Jewell, Lewy and Oliver's (1988) essay in memory of Ronald Shephard. They state that Shephard earned a bachelor's degree in mathematics and economics in 1935, but "was discouraged from pursuing a joint graduate degree, because, as a spokesman of the Economics Department put it, 'Mathematics has nothing to do with economics'" (135).

and an assistant in the Statistical Lab. Although he never took courses directly from Evans, Klein has carried Evans's notion of mathematical rigor on his future practices as a macroeconometrician (Pinzón-Fuchs 2019). After graduating and before moving to the MIT for his graduate studies, Klein spent a summer working with George Kuznets in the Giannini Foundation, part of the department of agriculture, assisting him in the estimation of demand functions for lemons in California (Klein 1987, 411). Kuznets had a graduate degree in psychology, but moved from the psychometrics problems he was working on in the 1930s into econometrics and statistical analysis of economic phenomena. He was Simon Kuznets's younger brother.

World War II and its subsequent demand for statisticians who could work with economics problems displaced the budding community of mathematical economics in Berkeley. Neyman lost his two main assistants in the Statistical Laboratory: "Unfortunately, the war came and, prior to its actual outbreak [in 1941], both Dresch and Dantzig… were claimed by the armed forces and… I had to begin the training of junior personnel anew" (Neyman apud Reid 1998, 211). Shephard, who concluded his PhD in 1940, substituted Dresch for a year as a lecturer in the department of mathematics, teaching probability and statistics. However, he enlisted in the Navy in the following year. After the war, Shephard became professor of mathematics at New York University. Dresch remained working for the Navy until the end of the 1940s, then moving to the Stanford Research Institute, where he remained doing consultant work for the Department of Defense.

Despite the emergence of a dynamical community doing research in mathematical economics at Berkeley after the arrival of Evans in the Department of Mathematics, the effects to the Department of Economics were minimal. Faculty and students of that department continued uninterested in mathematical modeling, with the relevant exception of Lawrence Klein. As we see in the next section, it was only in the postwar period that a graduate student in economics did research in mathematical economics at Berkeley, and although Evans assisted in that research, it was not the influence of his group that sparked interest in the subject at the Department of Economics.

3. Postwar: Robert Dorfman in a disputed department of economics

3.1. The year of the Oath (1949-50)

Before continuing with the history of mathematical economics at Berkeley, it is necessary to take account of the tensions that emerged in the University of California in the immediate postwar period. With the end of the war and the aggravation of Cold War tensions, anti-communism sentiment disseminated in American society. In California, the campaign known as McCarthyism in the early 1950s generated pressures to be felt in the university, represented by the event of the oath controversy.[84] The Constitutional Oath had been a regular requirement in the university, a dispositive that required faculty to swear to the constitution of the United States and of the state of California in order to join the institution. The controversy arose in June 1949 when the Board of Regents of the University of California, with the support of President Sproul, added to the Constitutional Oath a clause requiring faculty to swear not to be part of the Communist Party.[85] The failure to sign the oath would provoke dismissal. The requirement of sign the anti-communist document under the threat of losing a tenured position found a strong resistant among faculty.

In "the year of the oath", between 1949 and 1950, the University of California faculty "went to oath meetings, and talked oath, and thought oath" (Stewart 1950, 9). In July, a group of non-signers met in Los Angeles to resist the requirement put forward by the Regents. In September, Sproul was considering ways to withdraw the initial requirement, but polarization around the subject made it impossible to stop the controversy around the oath (Stadtman 1970, 329). It continued for the whole year, amidst resistance of faculty, including signers and non-signers, and disputes within the Board of Regents. The results of the oath controversy were the dismissal of thirty one faculty members who resisted to sign the anti-communist document in August 1950, and to generate an environment of disunion among administration, faculty and other members of the University of California (ibid, 335).

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[84] Schrecker (1986, 116-125) discusses the Californian oath controversy in the context of McCarthyism and the Cold War in postwar America. It must be noted, however, that a narrative that McCarthyism raised the "economists' desires to be safe, to seek the protective coloration of mathematics and statistics" (Weintraub 2017, 572) prompting a 'mathematization' of economics in the post war period, although generally accepted, is unsupported by good evidence. See Weintraub (2017).

[85] The Board of Regents approved in June 24, 1949, to add to the original Constitutional Oath the following statement: "that I am not a member of the Communist Party, or under any oath, or a party to any agreement, or under any commitment that is in conflict with my obligations under this oath." (Stewart 1950, 145).

The department of economics was caught in the middle of such controversy. Members of the older generation of the department, trained in the American Progressive Era and with sympathetic views toward socialism, were strongly opposed to the oath. However, they had to deal with the backlash of confronting the administration and of associating themselves with socialist views. Malcolm Davisson, Chairman of the department during the year of the oath, was one of the leaders of the opposition to the requirement. However, as remembered by some of Davisson's students at Berkeley, his anti-oath stance "also made him a controversial figure and seriously undermined his research efforts" (Break and Letiche 1994). Robert Brady was another leader of the fight against the oath and the frustrations associated with this process seriously damaged his health and work in the following years (Dowd 1994, 1057). Emily Huntington was one of the last non-signers to capitulate to the oath under the credible threat of losing her job (Innis 1992).

Such combativeness towards the oath, however, was not shared among all the members of the department. Another faculty group with no sympathies towards socialist ideas had not any problem with signing the document, or even implicitly supported the requirement. Most of such group had joined Berkeley during (or after) Calkins's term as Chairman. For instance, Howard Ellis had been invited in 1947 by F. A. Hayek to join what would become the Mont Pèlerin Society (Caldwell 2020, fn9). Gordon and Bain, who held less conservative political views, also did not engage in the oath controversy. The acceptance or indifference to the loyalty by the new generation caused resentment among the older generation who saw the event as an inexcusable threat to academic freedom. Thus the oath controversy also caused frictions among the faculty members of the Department of Economics.

The controversy did also deeply affect the career path of another member of the Department of Economics: Clark Kerr. He began his graduate studies in 1932 at Stanford, but his interests to study labor cooperatives formed during the depression years led him to transfer to Berkeley.[86] Such studies led him into the blooming field of Industrial Relations.[87] After some years teaching in the University of Washington, Kerr returned to Berkeley in 1945 as

[86] As a graduate student, Kerr remembers particularly the influence of Ira Cross and Paul Taylor on his work. Taylor's eagerness to take students to the field to see the real conditions of the working class impacted him. In 1933 Kerr registered for Taylor's seminars on labor, and the professor sent him to the cotton pickers' strike in San Joaquin Valley to talk to farmers, sheriffs, and strikers, where he faced "life in raw as I had never seen it" (Kerr 2001, 4).

[87] Industrial Relations emerged in the 1920s as an independent field of study, being an area of specialization of the Wisconsin department of economics. The field grew particularly in the universities of Princeton, Harvard and Chicago (Kaufman 1993, 10-11).

the director of the newly created Institute of Industrial Relations (IIR).[88] In the year of the oath, Kerr signed the document, but joined opposition with non-signers in resistance to the requirement. He participated in several meetings in the Board of Regents and the Academic Senate speaking up against the oath.[89] His defense of the non-signers right earned him a good reputation among faculty. As we see below, it would also help him to achieve an influential position within the university administration.

It was during this conflictive period of the oath controversy that mathematical economics emerged as a research topic within the department of economics. Robert Dorfman, the first mathematical economist in the department of economics, arrived at Berkeley as a PhD student coming from a mathematical statistics background. As we see in the next section, although Dorfman began teaching the subject at Berkeley after concluding his PhD, the tumultuous period in the department hindered the development of mathematical economics in the first half of the 1950s.

3.2. Robert Dorfman: from mathematical statistics in Columbia to mathematical economics in Berkeley

Robert Dorfman arrived as a graduate student in the Department of Economics at Berkeley in 1947, on the eve of the beginning of the oath controversy. Before World War II, he had received his master's degree in mathematical statistics from Columbia in 1937, having Harold Hotelling as his advisor (Dorfman 1997, xiii).[90] Dorfman struggled to find a job in the depression context, teaching statistics in a preparatory course for Federal civil service examinations, ultimately taking the examination himself and landing in the Bureau of Labour Statistics (BLS), where he worked for two years. With the higher demand for economic statisticians due to the war effort, Dorfman moved to the Office of Price Administration (OPA) in 1941, where he stayed for another two years as section chief in the Research

[88] The Institute for Research on Labor and Employment (IRLE), still active in Berkeley, has succeeded the Institute of Industrial Relations. The *Industrial Relations* journal, first published by the IIR in 1961, continues to be edited by the IRLE.

[89] In his memoir, Kerr attributes his fight against the oath to his Quaker upbringing, and the freedom of conscience belief held up in high regard by that religious community (Kerr 2001, 9).

[90] As a student under Hotelling, Dorfman published a paper in the 1938 volume of *The Biometric Bulletin* that Ver Hoef (2012) has identified as the original contribution of the delta method in statistics.

Division (ibid).[91] Willing to be more involved with the war itself, he applied in 1943 to work as an operation analyst for the Air Force, where he analysed and compared the results of various bombing strategies used in the South Pacific (ibid, xiv).[92] At the Air Force Headquarters in the Pentagon, Dorfman became close to Dantzig, Neyman's former student. By the end of the war, he applied for the economics PhD program at Berkeley.

Dorfman wrote that he chose economics "because [his] work in the BLS and OPA gave [him] a head start in that field, and … [it] seemed to embrace the critical social problems then confronting the country and the world" (ibid, xiv-xv). Berkeley was chosen because he "had visited San Francisco several times during the war and was charmed, and… had been greatly impressed by several people [he] had met, mostly statisticians, connected with the campus" (ibid, xv).[93] Indeed, there was not much mathematical economics happening in the economics department (see Figure 2.3 at the end of this section for a look on Berkeley's economics faculty), and given Dorfman's background it was the statisticians that should have impressed him the most. Evans did not offer the mathematical economics course between 1947 and 1950. The senior mathematician W. Leonard Crum, who had helped foster education in mathematical economics at Harvard in the 1930s along E. B. Wilson and Joseph Schumpeter (Carvajalino 2018), was hired from Harvard in 1948 for a joint appointment in economics and business administration but in his first years he was only teaching statistics courses to undergraduates.

After completing the course requirements for the doctorate, Dorfman took up his job in the Air Force, "lured back… by the prospect of earning a living wage" (Dorfman 2000, xv). In the Pentagon, he met Dantzig again "in a state of great excitement and frantic activity" (ibid). While Dorfman was taking economics classes at Berkeley, Dantzig's work on a linear programming model and a solution strategy to it brought him closer to a community of economists around the Cowles Commission, then headed by Tjalling Koopmans. In the Air Force, Dantzig was heading the newly created Project for the Scientific Computation of Optimum Programs (SCOOP), applying the programming tools he developed to the planning

[91] During his time at the Office of Price Administration, Dorfman wrote a short note on the inspection of defective products in large manufacturing problems that was published in the *Annals of Mathematical Statistics* (Dorfman 1943).

[92] Dorfman also helped in the planning and conduction of the experimental deployment of two atomic bombs in the Bikini Atoll in 1946, as one of his last works for the Air Force (Dorfman 2000, xiv).

[93] Dorfman received the prestigious Newton Booth Fellowship in Economics, awarded to only one graduate student in the department each year, which may have helped in his decision to go to the economics program (University of California Register 1946-47 v.2, 119). For instance, the availability of funding was the reason Arrow decided to pursue a PhD in economics in Columbia, and not in mathematics, on his own account, see Arrow (2011, 24).

of military operations, although the computational limits of that time restrained the applicability of his model for larger problems (Erikson, Klein, et al 2013, ch. 2). Dantzig's linear programming tools caught Dorfman's attention, and he applied it to theory of the firm for his PhD dissertation.

The dissertation was published including a section in quadratic programming in Dorfman's 1951 book *Application of Linear Programming to the Theory of the Firm* (Backhouse 2012).[94] In the introduction to the book, he acknowledges that "if this volume were dedicated to anyone, it would be to the officers and officials who staff the headquarters of the United States Air Force" and that "the research (…) was originally undertaken as part of [a program of research in the theory of programming] and was inspired to a great extent to the needs of the Air Force". (Dorfman 1951, vii-viii). Dantzig is also praised for giving "much of his time to instructing the author in the concepts and methods of linear programming" (ibid, vii).[95] Dorfman's dissertation advisor in Berkeley was Aaron Gordon, one of the professors appointed during Calkins's term as Chairman. Evans took part of his dissertation's committee, and received Dorfman's praise by serving "as the guardian of the author's mathematical conscience" (vii). By choosing to take up on research in linear programming, Dorfman became part of a small but relevant community of economists that were applying such tools to economic problems. Dorfman presented a paper in the Activity Analysis Conference organized by the Cowles Commission in 1949 (Koopmans 1951) and then proceeded to collaborate with MIT economists Paul Samuelson and Robert Solow in a book on the subject that would be published later in the decade (Dorfman, Samuelson, Solow 1958). Dorfman's mathematical skills earned him an appointment as a professor of economics in Berkeley in 1951, after teaching for a year as a lecturer (see Figure 2.3). As he remembered, "my principal responsibility when I joined Berkeley faculty was to introduce instruction in mathematical methods in economics" (Dorfman 1997, xv).

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[94] The section on quadratic programming was part of a research project that he would pursue along with Berkeley mathematician Edward Barankin during the early 1950s. Dorfman (1997, xv) recalled it as follows: "My doctoral dissertation… was not entirely successful because the profit of [monopolistic and semi-monopolistic firms] are presumed to maximize is not a linear function of their production and marketing choices. At best it can be represented as a quadratic function. How to apply the programming approach to problems involving nonlinear functions was then still unsolved…. So I teamed up with Edward Barankin, a professor in the mathematics department, to solve this problem. We struggled with it for several years and produced a monograph [Barankin and Dorfman 1958], with several interesting theorems but no practicable solution." Those problems were advanced by Philip Wolfe (1959), Barankin's student in Berkeley's department of mathematics.

[95] Dorfman was remembered by Dantzig as one of the important actors on the creation and stabilization of programming as a mathematics-economics amalgam. For instance, Dantzig credited Dorfman for the "mathematical programming" label given to the new set of mathematical tools, since he had felt "as early as 1949 that the term *Linear Programming* was too restrictive" (Dantzig 2002, 46).

A small group of recent PhDs assisted Dorfman with the most part of the teaching load of mathematical methods classes. Up to the end of the 1940s there were no courses in mathematical economics offered by the department of economics, thus making Dorfman's group classes a new option that became available for Berkeley's graduate students (see Table 2.1). Peter Otto Steiner was appointed assistant professor in Berkeley in 1950, just after completing his graduate studies at Harvard. Like Joe Bain, Steiner was very influenced by the industrial organization research put forward by Edward Mason and Edward Chamberlin. In addition to the position in the Department of Economics, Steiner also worked as a research associate at Kerr's directed IIR. He shared with Dorfman the teaching of the core course on economic theory for graduate students, and also collaborated with him in a couple of works.[96].

Another addition to the mathematical economics teaching group was the Danish economist Hans Brems, who was appointed lecturer in the department in 1951. Brems had graduated from the University of Copenhagen in 1949, studying under Frederik Zeuthen, a mathematical economist of Walrasian influence who had contributed to the theory of monopolistic competition and general equilibrium (Brems 1976). After graduating, Brems moved to the United States, spending a year as a visitor at Harvard under a Rockefeller Foundation scholarship. While at Harvard, Brems published an extended version of his doctoral dissertation in English, a theoretical piece on monopolistic competition (Brandis, 2001, Brems 1951). Also in 1951, George Break was appointed assistant professor of economics. Break was a Berkeley recent graduate, who became a specialist in public finance and the theory of taxation studying under Earl Rolph.[97] He collaborated as a professor of economic statistics in the department.

[96] Dorfman and Steiner (1954) joint paper on differentiated competition analyzed the problem using an optimizing firm framework, in contrast to the typical literature on the topic based in verbal reasoning and diagrammatic representation (for instance, the authors cite Abbott 1953 as a then recent illustration of the literature). They also collaborated in a statistical report regarding the economic consequences of America's rapidly aging population, commissioned by the IIR (Dorfman and Steiner 1957)

[97] A short outline of Break's dissertation, on theoretical aspects of capital taxation, was published in the Journal of Finance (Break 1953).

Table 2.1: Selected economics graduate courses at Berkeley (1951-1954)

1951	1952
Advanced Economic Theory *Brems, Norman Buchanan, Dorfman*	Advanced Economic Theory *Brems, Buchanan, Dorfman, Steiner*
Mathematical Methods of Economics *Dorfman*	Mathematical Methods of Economics *Dorfman, Gordon*
Mathematical Economics *Evans*	Advanced Economic Statistics *Dorfman, Break*
Advanced Economic Statistics *Crum, Dorfman*	

1953	1954
Advanced Economic Theory *Brems, Dorfman, William Baumol (visiting)*	Fundamentals of Economic Theory *Dorfman, Rolph, Steiner*
Mathematical Methods of Economics *Dorfman*	Mathematical Methods of Economics *Dorfman*
Advanced Economic Statistics *Break, Dorfman*	Advanced Economic Statistics *Break*

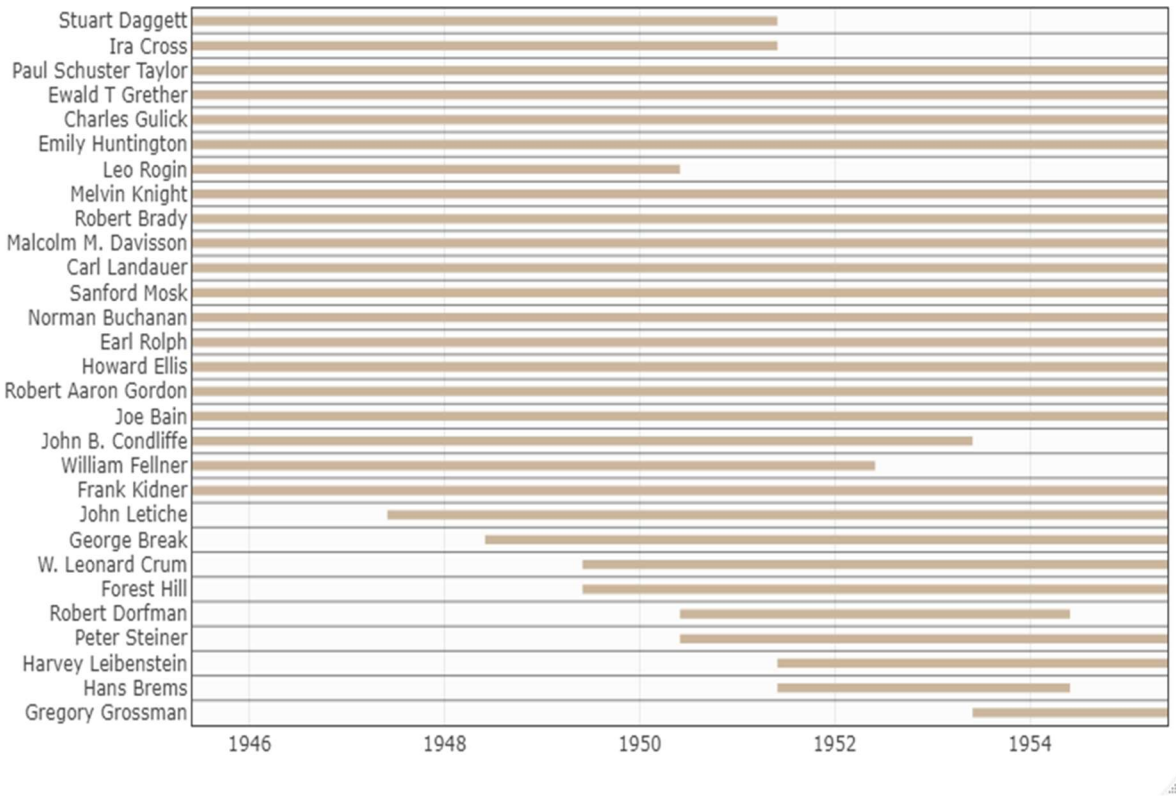
Sources: UC Register 1950-51, 51-52, 52-53, 53-54

However, the conflictive and divisive environment in the Department of Economics in the aftermath of the oath controversy became an obstacle for Dorfman's career to develop at Berkeley. As he described (Dorfman 1997, xvi), by the time of his appointment as professor, the department was divided in in two different factions, which "were… not only unfriendly towards members of opposing factions, but worse, contemptuous of them on the grounds that they were unfeeling, or ignorant, or both." He remembered the division as such:

> The economics department at Berkeley was split into two factions along doctrinal and generational lines, which coincided. It is pretty accurate to call them the institutional and theoretical factions. The institutional faction had joined the department, typically during the deepest years of the Great Depression. They were strongly motivated by sympathy with the sufferings of workers, the unemployed, the tenant farmers and the propertyless in general, under a callous capitalist regime… The theoretical faction was younger. Its members were recruited mostly during the heyday of the New Deal, after the publication of Keynes's General Theory and during or immediately after World War II. They believed that the free markets that flourished under capitalism were the essential force of a productive economic system, that they understood that force, and that they knew how

> to correct the socially disruptive side-effects that it engendered. The reader should have no trouble conjecturing to which faction I belonged.
>
> The split in the department not only breached collegiality, but generated both open and covert conflicts over every proposal to change an academic requirement or, most important, every proposed tenure appointment or promotion. Some time after I joined the department, I found out that the proposal to invite me had created a lively brouhaha, and that the then chairman [Davisson], an institutionalist, had tried to avert the appointment by 'losing' some essential documents. (Dorfman 1997, xvi-xvii)

Figure 2.4: Berkeley's Economics Faculty (1945-1955)



Under such circumstances, Dorfman and the group of young researchers disbanded as soon as better opportunities were available (see figure 2.4 above). Brems, just after being promoted to assistant professor in 1953, left Berkeley in 1954 to the University of Illinois. In that same year, Dorfman left to the Department of Economics at Harvard where, according to him, "applied economics enjoyed more attention and prestige" (Dorfman 1997, xvii). Steiner left in 1957 to the University of Wisconsin in Madison. Break was the only to remain in the department, indeed for his entire remaining career until retiring in 1990.

Such disbandment however did not represent an abandonment of mathematical economics at Berkeley. On the contrary, by the beginning of the 1960s the department would

become a relevant milieu in the United States for research in the subject, with the presence of Gerard Debreu in the department, along with other important mathematical economists. On one hand, the retirement of some of the older generation of Berkeley's economists helped to cool down such conflicts within the department. However, as we see in the next section, some political action was necessary to stabilize the conflicts in the department of economics and to develop mathematical economics at Berkeley. This role was played by Andreas Papandreou, who was appointed professor in the department with the mission to work as the Chairman and to mitigate the internal conflicts that had been hindering the development of the department.

4. Mediating conflicts in the department of economics: Clark Kerr and Andreas Papandreou

4.1. Chancellor Clark Kerr and the aftermath of the oath controversy

The conflicts had unveiled the need for reform within the administrative organization of the university. Also, the number of enrolled students had grown more than three times with the end of World War II and the GI Bill (see Kerr 2001, 73), creating new challenges to the centralized administration in an institution needing to expand to accommodate the rising demand for higher education. A major reform of university administration took place in the aftermath of the oath controversy. The creation of the Chancellor's office at Berkeley in 1952 was part of a new organization plan designed to "streamline the administrative machinery of an enlarged University, to clearly define the duties of the University officers and to give to the different campuses the maximum degree of autonomy consistent with unity."[98] By creating the Chancellor position, Sproul was compromising in sharing executive responsibilities within the university administrative structure. In this reform, the university was divided into Northern Section, based in Berkeley, and Southern Section, based in Los Angeles, with an appointed Chancellor for each section.

Clark Kerr was the first appointed Chancellor of the Berkeley campus. Kerr represented a compromise from administration towards faculty, for being one who stood up against the oath, but was above any suspicions of communism (Stadtman 1970, 358).[99]

[98] Minute of the Board of Regents, Berkeley March 30, 1951, as cited in the appendix of Kerr 2001, 460.

[99] In 1955, Columbia sociologist Paul F. Lazarsfeld conducted research on the outcomes of McCarthyism pressures on higher education based on several interviews with university teachers (Weintraub 2017, 583-586). As additional anecdotal evidence to Kerr's appeasing nomination, see this 'short synopsis' provided by

Although the Chancellor office promised decentralized power, Kerr observed that the duties of the position were really ill defined in the early years (Kerr 2001, 24). On his account, Sproul wanted the Chancellor to perform more as an assistant to the president, as a continuation of the work the former provost Monroe Deutsch had done along with him for sixteen years, until 1947. The pledged decentralization of power had to be negotiated between Kerr and Sproul. An important dispute in this process was the decision of who would hold the power of appointing the Chairman of each department. Kerr saw the Chairman position as crucial to heal the fractures opened during the oath controversy in Berkeley's departments. Although initially Sproul resisted to relinquish such powers to the Chancellor, he acquiesced to Kerr's requirement to appoint the heads of each department in March 1953 (ibid, 51). Kerr remembered this episode as "the only major delegation of authority made to me as Chancellor by Sproul" (ibid, 64).

As Chancellor, Kerr held the important power of appointing chairpeople in the reorganization of the various departments in the 1950s. There were multiple problems to be solved. Internally, there were multiple departments in the university with ongoing conflicts amidst faculty after the oath controversy. Externally, there were multiple pressures coming from the academic community as a repercussion of the required oath, culminating with the blacklisting of the university by the American Association of University Professors in 1956. Kerr remembered a particular episode that took place in a board meeting of the CASBS at Stanford as an example of the academic environment towards Berkeley in that period:

> To illustrate the climate of opinion, one day at a board meeting of the Center for Advanced Study in the Behavioral Sciences at Stanford, Paul Buck announced an engrossing topic for our discussion. (…) That day he said that the most interesting question currently in the academic world was "Who will take Berkeley's place" in the Big Six? Several institutions were nominated as the argument advanced. As a founding member of the board I sat there, facing the assumption of the inevitable decline of Berkeley burning into my soul as though it were a red-hot poker. My answer to Paul's question was "no one". (Kerr 2001, 28).

Weintraub (2017, 597) of a Berkeley's professor answers: "McCarthyism has caused harmful effects. Feels his own academic freedom has been threatened. No change in research. No change in selection of reference materials. Loyalty oath questions—UC Berkeley firings over refusals to sign oaths. Interference by the Board of Regents—believes that Clark Kerr as the new chancellor will help improve matters. Five to 10 years at university. PhD. Teaches Economic Theory and Statistics. Assistant professor."

The first chairman of the Department of Economics during Kerr's term as Chancellor was his former dissertation advisor Paul Schuster Taylor (1952-56; see Kerr 2001, 444). A well respected member of the older generation of Berkeley's faculty, the appointment of Taylor might have helped appease the displeased faculty in the aftermath of the oath controversy. On the other hand, it was not an encouraging nomination for the younger generation, expressed by the almost complete disbandment of the mathematical economics group during Taylor's term.

Furthermore, by the beginning of the 1950 decade, the organization of graduate studies in economics was under fundamental reappraisal. In the academic year of 1950-51, the executive committee of the American Economic Association commissioned a study to reflect on future directions to graduate education (Barber 1996, 16). The study was to be prepared by Howard Bowen, a social economist then teaching at Williams College (Coats 1992). The minimal standards that should be attained in order to receive a PhD in economics were one of the many topics discussed in the report. Bowen sketched a common core of knowledge to be shared among economists, "although there is, and should be, great diversity among economists in their intellectual equipment and interests" (Bowen 1953, 42). According to the report, the core of a graduate program in economics should consist "primarily of economic theory including value, distribution, money, employment, and at least a nodding acquaintance with some of the more esoteric subjects such as dynamics, theory of games, and mathematical economics" (ibid, 43).

The report was discussed in a round table at the Sixty-Sixth Annual Meeting of the American Economic Association held in December 1953.[100] Kerr presented a paper in the meeting, and as Berkeley's Chancellor and a member of its Department of Economics, was certainly aware of the report and the discussion, as he planned the future of Berkeley's Department of Economics. Also present in that meeting was Andreas Papandreou, a professor at the University of Minnesota. I could find no evidence of a relevant encounter between Kerr and Papandreou in the December 1953 meeting, nevertheless, it was Papandreou who would be chosen by Kerr to lead the reorganization of Berkeley's economics. Papandreou was hired in 1955, and in 1956 he was appointed by Kerr the new Chairman of the Department of Economics.

[100] The discussants at the round table were Howard Bowen, Robert Calkins (then president of the Brookings Institution), Thomas Carroll (Ford Foundation), James Early (Wisconsin), Albert Hart (Columbia), Walter Heller (Minnesota), Fritz Machlup (Johns Hopkins), Jacob Marschak (Chicago), Arthur Smithies (Harvard), Joseph J. Spengler (Duke), George Stocking (Vanderbilt), and Willard Thorp (Amherst). See Program, 1954.

4.2. Andreas Papandreou arrives at Berkeley

Andreas Papandreou was born in 1919 in Chios, one of the Greek islands in the Aegean Sea. His father Georgios Papandreou was an important politician during the interwar period, as a supporter of Liberal leader Eleftherios Venizelos (Kaloudis 2019). Andreas migrated to the United States in 1940 after suffering political persecution as a socialist-leaning law student under the authoritarian military regime instituted in 1936 headed by Ioannis Metaxas. Receiving good letters of recommendation from his professors, Papandreou was admitted as a graduate student of economics at Harvard, completing his studies in 1943 (Draenos 2012). Like Bain and Steiner, Papandreou was influenced by the so-called "Harvard School" of industrial organization. His dissertation approached the theory of the entrepreneur, following the work of another Harvard professor, Joseph Schumpeter (Papandreou 1943; Hébert and Link 2009, 75). Papandreou continued to publish in industrial organization after completing his graduate studies, with research on market structures and monopolistic power (Papandreou 1949, Papandreou and Wheeler 1954), but his interests spanned other fields as well.

Papandreou became interested in methodological issues in economics, influenced by the writings of Harvard's star graduate student Paul Samuelson. Papandreou was keen on the questions regarding the empirical relevance of axiomatic theoretical economics. In his methodological work, Papandreou praised Samuelson's work, especially in the *Foundations*, for its search for "operationally meaningful theorems" as a solution to such problems. On the other hand, he also feared that "the very attempt of economic analysis to build a theory of universal validity (…) takes it into the path of operational meaningless" (Papandreou 1950, 721). Papandreou argued that the interaction with other social sciences, such as anthropology and sociology, could equip axiomatic economics with "the tools necessary to impose restrictions upon the relations among economic variables in order that operationally meaningful hypotheses may be formulated" (ibid, 722). In this search for empirical validity of economic axioms, Papandreou presented in conferences of the Econometric Society some projects of experiments designed to test the transitivity axiom of choice theory (Papandreou 1952, Papandreou et al 1954). This new work regarding the axiomatic basis of mathematical economics was incited by the arrival of Leonid Hurwicz to the University of Minnesota in 1951. Papandreou was a professor at the university since 1948, after spending some years as

lecturer at Harvard following his graduation. Papandreou and Hurwicz, both Europeans émigrés, developed in Minnesota an enduring friendship. [101]

In continuation to his investigations on methodology, Papandreou published the book *Economics as a Science* in 1958, where he discusses some of the methodological issues of theoretical economics.[102] In the book, Papandreou acknowledges the need for mathematical training among economists, stating that "during the last quarter of a century powerful mathematical techniques, notably set theory, linear algebra, and topology, have been employed in economics at an increasing pace. This has been particularly evident in the United States where mathematical economics is well on its way to assuming a significant, if not dominant, role in the training of economists" (Papandreou 1958, v). He takes up on the methodological problem of empirical relevance opposing Friedman's (1953) stance to Samuelson's (1947), praising again the latter for the testable character of his operationally meaningful theorems compared to the "sterile form of interpretation" of Friedman's models.

Table 2.2: Selected economics graduate courses at Berkeley (1955-1956)

1955	1956
Fundamentals of Economic Theory *Rolph, Steiner, Papandreou*	Fundamentals of Economic Theory *Papandreou, Leibenstein*
Advanced Topics in Economic Theory *Bain, Papandreou*	Advanced Topics in Economic Theory *Papandreou, Lee*
Mathematical Methods of Economics *Papandreou*	Mathematical Methods of Economics *Minsky*
Advanced Economic Statistics *Boles, Lee*	Advanced Economic Statistics *Break, Kuznets*

Sources: UC Register 1955-56, 56-57

Papandreou was appointed professor in the Department of Economics in 1955 with the initial responsibility of substituting Dorfman in the teaching of the mathematical and

[101] Hurwicz and Papandreou met in the University of Minnesota in 1952, after the latter returned from a visiting year at Northwestern University (Hurwicz's interview, Draenos 2002). They remained close friends, and Hurwicz's visits to Stanford to work with Arrow in the late 1950s allowed them to meet again in California. When Papandreou was imprisoned in 1967 in Greece, Hurwicz called Samuelson for help in reaching Walt Rostow (who was the National Security Advisor) to build on the international pressure for his release (Samuelson in Draenos 2002).

[102] The book was reportedly written to help in classes of economic theory, economic method and mathematical economics, so it probably contains material of his teachings in Berkeley (Papandreou 1958, v).

statistical training graduate courses. During his first year in the department, he taught "Mathematical methods in economics" and participated in the teaching of the core courses in economic theory (see Table 2.2). In the "Advanced Topics in Economic Theory" course, Papandreou taught the methodological issues in axiomatic economics that interested him. Interdepartmental relations also proved important to make up for the need for professors to teach some courses in mathematical economics. The support of the agricultural economics department was crucial. Quantitative methods applied to agricultural economics were in vogue in the department since the appointment of Kuznets and Hoos. In the 1940s and 1950s, they were applying econometric analysis to study agricultural price and stock variation and advising graduates on those tools (such as James Boles, Arnold Zellner, and Ivan M. Lee).[103] As Table 2.2 shows, they were of important support to the Department of Economics as teachers of graduate level statistical courses by the time of Papandreou's arrival. Kuznets, Hoos, Lee, and Boles taught various courses on mathematical methods in the department of economics during the second half of the 1950s. As Chairman in the next following years, Papandreou cultivated those links with the agricultural economics department. Those professors received a joint position in the Department of Economics. It was Lee who offered the first graduate course in econometrics in Berkeley.

5. The Andy's Boys: towards economics as a mathematical science

5.1. Andreas Papandreou as Chairman of the Department of Economics

Papandreou's youthful interest in politics had resurfaced while he was at the University of Minnesota. In 1952, he joined a committee of professors in support of the Democratic Party candidate in that year's presidential election, Adlai Stevenson. Although Stevenson lost on a landslide, the experience awakened on him "feelings that [he] had carefully kept on ice for many years" (Draenos 2012). During his leadership period in Berkeley, he would need to apply his political and interpersonal skills to run the department. It proved to be a first political experience for him before becoming a prominent politician in

[103] Linear regression methods had been used for forecasting in agricultural economics, but the need to set prices during the war forced the field to deal with problems in the theory of demand during the 1940s (Banzhaf 2006). The group of agricultural econometricians in Berkeley emerged in this context.

his home country. In 1956, Papandreou was appointed by Kerr to a three year term as Chairman of the Department of Economics. The university was still healing the wounds opened during the oath controversy, and Papandreou needed to calm tensions in the Department of Economics in order to make it develop. As a new professor in the department, he was not a partisan of any of the battling groups that existed within it. In order to gain respect with the older generation of the department, Papandreou reached Huntington, a much respected member of that group, to support him by taking the job of Vice-Chairman, as she remembered in an interview:[104]

> I remember vividly the day Andy came to my study and said, "I have been appointed Chairman of the department, and wish to recommend your appointment as vice chairman. Will you do this? I need your help as a long-time member of the department. I haven't been long in Berkeley." I think Andy was fearful that I would not want to work under a much younger person. However that was no problem to me, and I had no desire to assume the administrative problems of a Chairman. We made satisfactory arrangements as to the division of responsibility, and we always worked together well. (Huntington 1971, 49)

The challenge faced by Papandreou was described by Dale Jorgenson (one of the economists recruited by him to the department during his chairmanship) as such: "the setting of the Berkeley department was one in which there were two factions that were essentially at each other's throats; neither one was able to make progress against the other" (Draenos 2002, 2). Jorgenson credited Papandreou's "incredible personal political skills" for overcoming such a situation. Papandreou "was able to deal with a very large variety of different personalities, (…) after all, the university department was made up of people that were as much as 40 years apart in their careers" (ibid, 3). Moreover, he recognized that Papandreou might had the necessary mischievousness to accomplish the difficult task of being an academic politician:

> I suppose that if you wanted to be critical of Andy's behavior, you could say (…) there was a little bit of an opportunistic approach in dealing with these dissident factions. Somebody who is a little more cynical about this than I, might say, well, he may have talked out of the two sides of his mouth. Maybe he said one thing to these people and another thing to the others, knowing that they were not talking to each other. There may have been a little bit of that; I can't say that that was totally absent from Andy's personality. There was an opportunistic flavor to some of the things that he

[104] It might have helped the fact that Papandreou was good friends with Berkeley's former star graduate John Kenneth Galbraith, who held a positive opinion of Papandreou's work (Draenos 2012).

> did. But if he did that, it was certainly very effective; he never raised any hackles and left everybody with a very positive feeling. (Jorgenson in Draenos 2002, 6)

There were not only conflicts among faculty, but also among neighboring departments. In his first year as chairman, Papandreou sought to settle the institutional relationship with the School of Business. Not only the creation of that department in the previous decade had taken a large part of the Department of Economics funds, since it was created from it, the School of Business was beginning to compete for the appointment of new economists as professors. Ewald Grether, the dean of the College of Commerce (the college encompassing both departments) and a major sponsor of the creation of the School of Business reported one of Papandreou's actions to constrain the competition among both departments:

> Papandreou, as chairman of Economics, had gone to Kerr to raise the question of the relationships between the Department of Economics and the School of Business... He requested two things: (1) the adoption of the principle that economics courses should be taught by the Economics Department, and that Business Administration cease from adding economists to its staff; and (2) the creation of formal machinery at the dean's or chancellor's level to arbitrate disputes between Economics and Business Administration (…) Kerr was the ideal person to be in the chancellorship at the time, because he was a member of our faculty and also an economist, and so this thing was handled very nicely and there was no rupture. (Grether 1993, 540-542)[105]

Nevertheless, conflicts over new appointments within the College of Commerce were softened by the booming environment of American higher education in the 1950s. This allowed the School of Business and the Department of Economics to expand fast in the second half of the decade. In the time-span of Papandreou's chairmanship, the number of active professors in the Department of Economics (excluding associates from other departments and lecturers) grew from 25 to 32, while the larger School of Business grew from 47 active professors in 1956 to 59 in 1959.

[105] Such memories, Grether reports, were based on one of his archival files: "Here's a file that I had forgotten about; I discovered it recently in our files. It's entitled 'Notes on Conference in Chancellor Kerr's Office, November 20, 1957.' I find that this represented a conference between Chancellor Kerr, Vice Chancellor James D. Hart, Dean Lincoln Constance of Letters and Science, Dean Grether [himself], Andreas Papandreou (chairman of Economics), and Maurice Moonitz of Business Administration." (Grether 1993, 540-542).

Papandreou admired Samuelson's works and tried to lure the MIT economist in order to put Berkeley's economics among the elite departments in the United States Papandreou wrote a letter to Samuelson on November 28, 1956, in his first months as Chairman, with an open invitation for him to join the department as a Research Professor for "an academic year of your choice within the next five years, beginning with the academic year 1957-58." The offer was supported by a Ford Foundation grant. The position of research professor had no teaching obligations and commanded a high salary "in the neighborhood of $20,000."[106] Samuelson never accepted the offer, but wrote back recommending alternative names. In March 28, 1957, he replied Papandreou with a letter recommending Tibor Scitovsky to Berkeley, mentioning that "he would be widely regarded as one of the outstanding economic theorists anywhere in the world" and could be "a pleasant addition to any academic community, a stimulus to his colleagues, and an excellent teacher and mentor of graduate study."[107] As seen in the last chapter, Scitovsky was a professor at Stanford University, and part of the postwar rebuilding of that department. In his unpublished memoirs, Scitovksy wrote about Papandreou's haste to hire him out from the neighboring department: "Stanford and Berkeley had a 'gentleman's agreement' not to lure each other's faculty members… [but] Papandreou (…) who made me that generous offer [a raise of salary from $6000 to $13000], was not a man to abide by gentlemen's agreements that went against the interests of gentlemen."[108] Scitovsky moved to Berkeley in the summer of 1957 on a permanent appointment.

Visiting positions, like the one offered to Samuelson, became an important strategy in Berkeley to attract senior professors to the department. Some important additions to the department were made through visiting positions, and in some cases visitors became permanent faculty. That was the case of the macroeconomist Hyman Minsky, who arrived in 1956 in Berkeley as visiting professor. Minsky was a Harvard graduate who had been a

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[106] Andreas Papandreou to Paul Samuelson, November 28 1956. Paul A. Samuelson Papers, Box 58, "Papandreou, Andreas" Folder, Rubenstein Library, Duke University.

[107] Samuelson to Papandreou, March 28 1957. Paul A. Samuelson Papers, Box 58, "Papandreou, Andreas" Folder. Almost a decade earlier, while at the University of Minnesota, Papandreou had written to Samuelson on recommendations to a "Money and Banking" and "Econometrics" positions. Papandreou felt this position would be "a real opportunity for a growing man" and that "someone among the men trained under you at MIT may qualify for this job" listing the names of Franco Modigliani, Lawrence Klein and others. Samuelson, however, wrote that he did not "have any constructive suggestion to make concerning available top-notch people for the Minnesota Department". He thought the cited names would not be interested in moving to Minnesota due to their current appointments. Papandreou to Samuelson, October 28 1949, and Samuelson to Papandreou, October 31 1949. Paul A. Samuelson Papers, Box 58, "Papandreou, Andreas" Folder.

[108] "A Joyful Economist: Memoirs of Tibor Scitovsky" 1995, 92. Tibor Scitovsky Papers Box 2, "Memoirs" Folders, Rubenstein Library, Duke University.

teacher at Brown University since 1949, although he only completed his PhD dissertation in 1954. He taught the graduate course in mathematical methods during his first year at Berkeley as a visitor (see Table 2.2), also offering an advanced course in the theory of interest, capital, and employment (Register 1955-56, 105-106). He was hired by Papandreou in 1958 and remained in the department until 1965. In that period, Minsky's research programme was mostly related to studies on employment and poverty, in reaction to the War on Poverty program put forward by the Kennedy-Johnson administration (Wray 2016). Although he taught some mathematical economics courses at Berkeley, Minsky could be hardly classified as a mathematical economist, in the sense discussed in the introduction to this dissertation. The same applies to Abba Lerner, visitor for the first time at Berkeley in 1958, coming from Roosevelt University. Lerner returned to Berkeley as visiting professor in the summers of 1960, 1962 and 1963, finally being hired in 1966 (Landes 1994, 226). Another important visitor was Nicholas Kaldor, who received a research appointment in the department in 1958.

During Papandreou's term as chairman the department hosted mathematical economists every year as visiting professors. Eberhard Fels, a German postdoctoral fellow in the Center for Advanced Studies in the Behavioral Sciences at Stanford, taught at Berkeley in the academic year of 1956-57 as a visiting professor. In the following year, the department hosted British economist Roy G. D. Allen, who had recently published one of the early and important textbooks on mathematical economics. It was at Berkeley that he prepared the second edition of his textbook (Allen 1959, vii-viii). In 1959, Frank Hahn also crossed the Atlantic, coming from the University of Birmingham, sharing with Minsky and Scitovsky the core graduate course of *Fundamentals of Economic Theory*. Hahn was then interested in stability problems of general equilibrium, his visit to Berkeley enabling him to be closer to Arrow, with whom he already had exchanged correspondence on the topic.[109] Finally, in the 1960-1961 academic year, the department welcomed Arrow and Uzawa from the booming center of mathematical economics at Serra House as visiting professors. Uzawa shared with Scitovsky classes in welfare economics, while Arrow taught the "Mathematical Economics" course with a young scholar who had been appointed in Berkeley by Papandreou, Roy Radner (General Catalog 1960-1961, 144).

[109] See Arrow, Block and Hurwicz (1959, 88, fn.6).

5.2. The Andy's Boys: A new generation of (mathematical) economists

Although the department received several mathematical economists as visiting scholars, those temporary appointments were insufficient to fulfill the offerings in mathematical economics courses with the departure of Dorfman and others in the middle of the decade. However, finding senior economists with mathematical training was not an easy task during the 1950s. The academic market for mathematical economists was the topic of an exchange of letters between Robert Solow and Papandreou. In October 28, 1958, Solow wrote to Papandreou that "good men in quantitative empirical economics are hard to find and much in demand. The result, naturally enough, is that the price is high and rising" (Solow to Papandreou 1958). Solow's solution to the problem was that "if you want to hire a first-class man as a normal assistant professor, almost the best you can hope for is someone who has just finished his PhD. Such people are hard to identify and the risk is inevitably high."[110] He recommended Edwin Kuh, who was a professor at MIT, and Edwin Mills, professor at Yale, as possible senior recommendations. But competition was fierce, and neither was hired to Berkeley.[111]

Like Solow suggested in his letter, Papandreou's strategy to create a mathematical economics group at Berkeley was to appoint young PhDs. Roy Radner received his PhD in mathematical statistics from the University in Chicago in 1956, under Jimmie Savage. In Chicago, he had been working as an assistant researcher in the Cowles Commission since 1951. He worked for Cowles during his first year at Yale University, but after receiving an offer from Papandreou, he moved to Berkeley as an assistant professor in 1957.[112] Upon his arrival at Berkeley, Radner taught the courses "Applied Economic Statistics" and "Mathematical methods of economics," replacing the agricultural econometricians who remained teaching statistical and econometrics courses. In 1957, Papandreou also made an attempt to hire another senior economist from the Cowles Commission. The French

[110] Robert Solow to Andreas Papandreou, October 28 1958. Robert M. Solow papers, Box 57, "Correspondence K" Folder, Rubenstein Library, Duke University. In the same letter Solow expresses sorrow for MIT losing Marc Nerlove "who is certainly one of the very best prospects of recent years" to the University of Minnesota. Nerlove accepted an Associate Professor position and the MIT "could not hope to match an offer like that."

[111] Following his letter to Papandreou, Solow wrote to MIT Dean E. P. Brooks telling about his conversation: "I have been the recipient of a long phone call from Andy Papandreou (…) I find this uncomfortable because I am in a position of having to give a strong recommendation for someone whom I should hate to see leave MIT. Kuh is clearly one of the outstanding younger members of the school of econometrically-minded economists." Solow to Brooks, December 10 1958, Robert M. Solow papers, Box 57, "Correspondence K".

[112] The main research carried on by Radner on the Cowles Commission was on the theory of teams and organizations, a long lasting effort with the economist Jacob Marschak that was later published as a book (Marschak and Radner 1972; Cherrier 2010).

economist Gerard Debreu was then an untenured associate professor at Yale University, while holding a research position at Cowles. Debreu, however, did not accept such an offer, being at the time considering returning to his home country. In 1961, Radner pushed the department (and the then Chairman Robert Gordon) to renew the offer to Debreu, who at this time accepted the proposal (Düppe and Weintraub 2014a, 178-179). Radner remained an important member of Berkeley's economics faculty until 1979, and during this time he contributed to several topics in mathematical economics, such as turnpike theory and economic growth (Radner 1961, 1967), and general equilibrium under uncertainty (Radner 1968, 1970, 1972).

Another young PhD appointed by Papandreou who played a crucial role in the development of mathematical economics in Berkeley was Dale Jorgenson. Papandreou and Jorgenson had first met in 1954, when the latter, as an undergraduate student from Reed College, visited the University of Minnesota for a summer research project. They remained in contact while Jorgenson was a graduate student in economics at Harvard, where he entered in 1956 (Jorgenson 2002, 1). Jorgenson was hired to Berkeley just after receiving his PhD degree in 1959. He developed a close friendship with Radner, and remembered the two of them together with Papandreou as forming a team in the department. They were the core of a group who became known among their department peers as the "Andy's Boys," the new generation of economists graduated in the 1950s who joined Berkeley by the recruiting of Papandreou (ibid, 2). Radner and Jorgenson shared work in teaching, research and consultancy. As teachers, they expanded the graduate course in "Mathematical methods of economics" into two modules (see Table 2.3). The first, taught by Radner, focused on "theories concerning individual economic agents" while Jorgenson's module approached economic problems of "groups of agents."[113] They shared work as consultants to the Rand Corporation, creating a model of optimal replacement of equipment that required inspection for the Logistic departments (Jorgenson and Radner 1960) and a mathematical proof of the optimality of a class of maintenance policies for manned aircraft and ballistic missile systems to the US Air Force (Radner and Jorgenson 1963).

[113]Register of the University of California 1958-59, v.1, 114. In Radner's module, classes would approach "decision under uncertainty, planning through time, investment, production, inventory control, (and) other applications." In Jorgenson's module, the study of groups of agents included "organization and game theories, equilibrium, stability, welfare aspects of various price systems and of other processes of economic adjustment."

Table 2.3: The Andy's Boys, selected economics graduate courses (1958-1959)

1958	1959
Fundamentals of Economic Theory *Leibenstein, Scitovsky, Caves, Minsky*	Fundamentals of Economic Theory *Hahn, Leibenstein, Minsky, Scitovsky*
Advanced Topics in Economic Theory *Bain, Hoos*	Advanced Topics in Economic Theory *Bain, Hoos*
Mathematical Methods of Economics *Radner*	Mathematical Economics *Radner, Jorgenson*
Applied Economic Statistics *Radner*	Applied Economic Statistics *Radner, Jorgenson*
Seminar in Economic Theory *Radner*	Introduction to Econometrics *Lee*
Introduction to Econometrics *Lee*	

Sources: UC Register 1958-59, 59-60

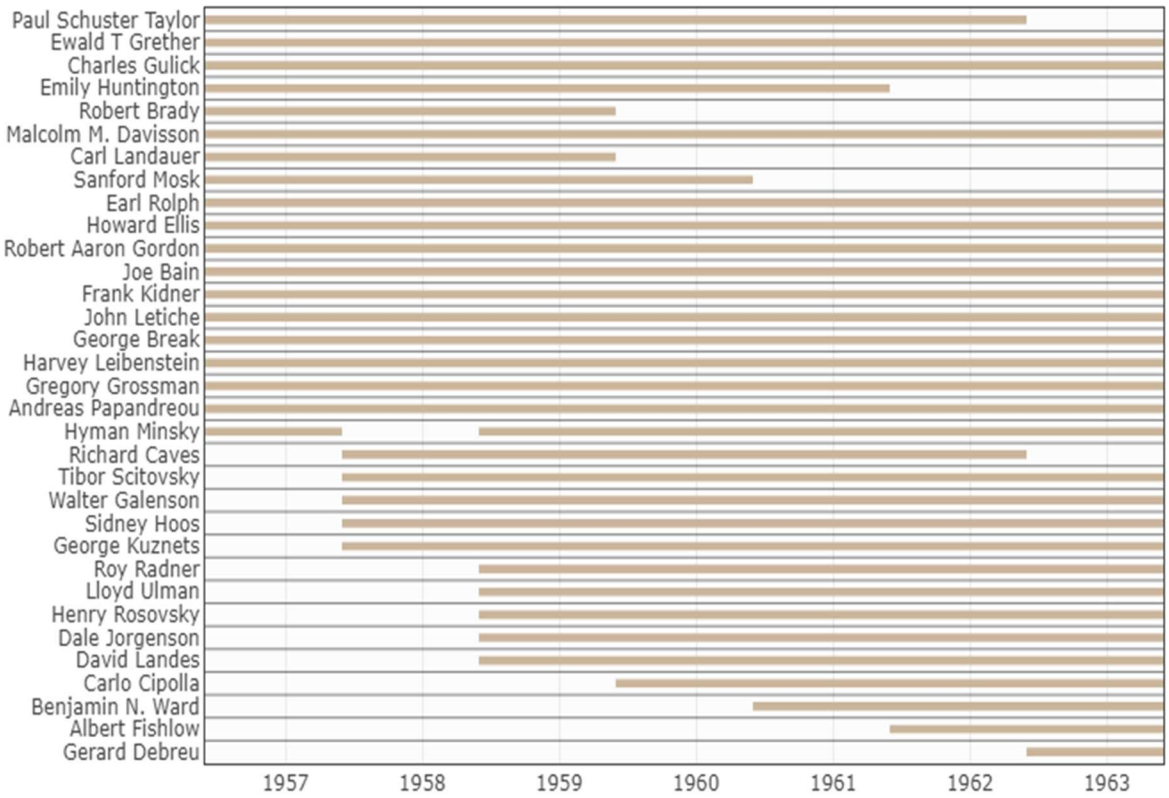
Harvard graduates such as Jorgenson, Minsky, and Papandreou himself, were the main target of Berkeley's recruiting in the second half of the 1950 decade.[114] Four other Harvard graduates were appointed professors in Berkeley during Papandreou's chairmanship: the industrial organization economists Lloyd Ulman and Richard Caves and the economic historians David Landes and Henry Rosovsky.

At the end of his term as Chairman, Papandreou took a sabbatical year returning to Greece, where he founded the Center for Economic Research (KEPE) in 1959. KEPE (renamed Center of Planning and Economic Research in 1964) was designed to be a think tank dedicated to study the state of the Greek economy. As Papandreou stated to Samuelson in a letter, KEPE was financed by the Greek government, the American embassy and "hopefully, by one or more foundations."[115] The creation of KEPE was an expression of Papandreou's underlying interest in entering Greek politics. After a year on leave, he returned to Berkeley, still keeping his ties with politics in the home country. His successor in the

[114] Berkeley PhDs were not hired unless they got some experience in other universities before coming back. For instance, Irma Adelman said in an interview: "Andreas Papandreou was Chair at Berkeley, and the tradition in Berkeley was that 'we don't hire them unless they prove themselves elsewhere.' So, you know, he sent me to Stanford so that I could prove myself… At Stanford, I was the first woman to be hired at academic rank." (Adelman, Zilberman and Kim 2014, 6)

[115] Papandreou to Samuelson, November 20, 1960. Paul Samuelson Papers, Box 58, "Papandreou, Andreas" Folder. In the letter, Papandreou is communicating his decision to leave the United States permanently to lead the center. He asks for Samuelson's help "to assist (the center) in discovering persons who may be willing to join us in Greece for a limited period of time." The Foundations that financed the center were the Rockefeller and Ford Foundation.

Figure 2.5: Berkeley's Economics Faculty (1956-1963)



Chairman office was Robert Gordon. Gordon's Vice-Chairman was Caves, one of "Andy's Boys". Like Papandreou, Caves developed an interest in academic administration. In 1962, he returned to Harvard, where he would serve as Chairman of the Department of Economics between 1966 and 1969. During Gordon's term there were some important additions to the fields of economics history and methodology with the appointment of Carlo Cipolla, Albert Fishlow, and Benjamin Ward.[116]

In 1962, Scitovsky was appointed the new Chairman, with Radner as the Vice-Chairman, two professors appointed by Papandreou who had become the head of the department's recruiting committee (Williamson 2005, 495). Although he was in his last year at Berkeley and already eyeing the Greek elections of 1964, Papandreou "engaged in a last recruiting trip, which essentially finished the job of recruiting 'Andy's Boys'" (Jorgenson in Draenos 2002, 2). In hindsight, this last recruiting effort proved to be extraordinary, as three future Nobel Prize winners who had just completed their PhDs were recruited to Berkeley:

[116] Those appointments were made in the wake of the retirement of Carl Landauer and Robert Brady in 1959, members of the older generation group, and the passing away of economic historian Sanford Mosk in 1960 (King 1961).

Peter Diamond, a stellar student from the MIT Department of Economics;[117] Oliver Williamson, a recent PhD in economics from Carnegie-Mellon University; and Daniel McFadden, formerly a physics student who entered economics through the behavioral sciences in the University of Minnesota under the influence of Papandreou's former colleague and close friend, Leonid Hurwicz. Papandreou also set the appointment of two other graduate students, David Laidler from the University of Chicago and Sidney Winter from Yale University. Such a large number of appointments were made possible by the retirement of various members of the older generation of the department of economics in the first half of the 1960s, including Taylor, Gulick, Huntington, and Grether.

Papandreou's reorganization of the Department of Economics received good approval by the university administration. Kerr, who succeeded Sproul as the president of the University of California in 1958, was then leading a fast expansion of the university facing the growing demand for higher education. In 1960, Kerr offered to Papandreou the position of Chancellor of the newly established San Diego Campus (Kerr 2002). But politics at home had already hooked Papandreou. He officially entered into politics running for the Greek Parliament in February 1964. He was affiliated to the EK party, whose leader was his father Georgios. The party had success on the ballot, winning more than half of the popular vote in the legislative elections. However, a military coup overthrew the government, and Papandreou was held in prison in 1967 (Papandreou 2014). The academic ties and networks he developed in America proved helpful at this difficult moment. Economists from France, England and the United States exerted pressure through diplomatic organizations and the general press demanding Papandreou's release from prison and concession of political asylum. In the US, a letter was published in the *New York Times* defending Papandreou as "a talented economist (…) that during the many years of our association with him he manifested a consistent devotion to democracy and rejected totalitarian forms of government."[118] The

[117] For the high esteem held by his MIT peers, see for instance Robert Solow's letter to MIT Dean Cary Brown in 1969, recommending Diamond for a quick promotion (he had returned to MIT as professor in 1966): "It may be the best thing I ever did for MIT was to be Graduate Registration Officer the day Peter Diamond turned up (…) to say that he had a fellowship to begin graduate work in mathematics at MIT but thought he would like to transfer to economics. I took one look at his record, called George Thomas to arrange the transfer, and here we are." He called Diamond "an extraordinarily talented and clear-minded economic theorist" and one of "three economic theorists in the world aged thirty who are worth talking about." McFadden is cited as being "few years older" (Solow to Brown, October 7, 1969. Robert M. Solow papers, Box 2, Folder "September-December 1969." See also Cherrier (2014, 27).

[118] See Arrow, Galenson, Leibenstein, Radner and Rosovsky (1967, 597).

signers were Kenneth Arrow, two of his former colleagues in Berkeley (Walter Galenson and Harvey Leibenstein) and two "Andy's Boys" (Radner and Rosovsky).[119]

6. Paving the way to the Evans Hall: Mathematical Economics in Berkeley after Papandreou

6.1. An "up-and-coming" department amidst political turbulence

The arrival of the large group of young PhDs in the Department of Economics through Papandreou's recruiting greatly expanded the graduate teaching of mathematical economics in Berkeley. With such expansion, mathematical economics in Berkeley was transformed from being an "esoteric subject" taught by a couple of professors of the department to two research fields that a graduate student could specialize into. Beginning in 1964, a Berkeley graduate student in economics could decide to follow a more theoretical "mathematical economics" research seminar, led by McFadden and Karl Vind[120] (who was visiting professor in that year under a Rockefeller Fellowship), or followed the econometrics research seminar supervised by Jorgenson and Winter (also see Table 2.4). Diamond, later reflecting on his stint at Berkeley, remembered:

> I started [in Berkeley] in 1963, I taught two years full time, and then I took leave in my third year. That leave was at the Churchill College in Cambridge, and then I returned to the MIT, but I spent part of that year back at Berkeley. The first year I was there was just an unbelievably exciting time to do research on campus. The department was an exciting and up-and-coming place. The interaction was with young people who have remained close friends ever since, the kind of bond you form as Assistant Professors. (Diamond in Moscarini and Wright 2007, 548).

The dynamic state of the department was however affected by novel political conflicts taking place on the Berkeley campus. The year of 1964 saw the Free Speech Movement spreading protests across campus, which was considered by the time "the most serious student demonstrations ever to have occurred in the United States" (Gales 1966, 1). Some of Andy's

[119] Under international pressure, Papandreou was released under the condition of exile. He moved to Sweden, teaching in Stockholm University. As the regime fell in Greece in 1974, he again returned to his home country and into politics, creating the new party Panhellenic Socialist Movement (PASOK). A central figure in the restored Greek republic, Papandreou was the Prime Minister between 1981 and 1989, and then again from 1993 to early 1996. His eldest son, born in 1952 in Minnesota, also served in this position between 2009 and 2011.

[120] Vind was a graduate student from the University of Copenhagen, and just as Hans Brems, was a student of Frederik Zeuthen and was influenced by his Walrasian economics. Vind returned to Copenhagen in 1966, where he stayed until his retirement in 2003.

Boys were directly involved in the conflits, in particular Rosovsky. He played an important role as faculty representative in the conflict mediation between students and administration. During the protests of the Free Speech Movement, Rosovsky led an unofficial group of fifteen faculty (including Radner) trying to find a solution to the October protest that featured a massive students' sit-in to prevent the arrest of protester Jack Weinbert (Zelnik 2002, 272-274). Some faculty saw the turbulence on campus as a distraction and an obstacle for research (see Diamond in Moscarini and Wright 2007, 548) and a large part of the Andy's Boys left Berkeley to other appointments in the aftermath of the Free Speech Movement (Jorgenson in Draenos 2002, 5). Following Caves and Landes who had already returned to their alma mater, Rosovsky moved to Harvard in 1965.[121]

Table 2.4: Selected economics graduate courses at Berkeley (1963-1964)

1963	1964
Fundamentals of Economic Theory *Scitovsky, Diamond*	Fundamentals of Economic Theory *Laidler, Winter, Diamond*
Advanced Economic Theory *Papandreou, Scitovksy*	Advanced Economic Theory *Winter, McFadden, Scitovsky, A. Sen*
Advanced Topics in Economic Theory *Winter, Debreu, Radner*	Advanced Topics in Economic Theory *Bain, Radner, Rolph*
Mathematical Economics *Debreu, Radner*	Mathematical Economics *Vind, Debreu*
Applications of National Economic Planning *Papandreou*	Linear Models of Economic Theory *Vind*
Applied Economic Statistics *Jorgenson*	Mathematical Economics Seminar *McFadden, Vind*
Economic Statistics *Jorgenson*	Introduction to Econometrics *Jorgenson*
Introduction to Econometrics *Jorgenson*	Econometrics *Jorgenson*
	Econometrics Seminar *Jorgenson, Winter*

Sources: UC Register 1963-64, 1964-65

[121] Dale Jorgenson was another Berkeley professor to join Harvard, moving to that university in 1969. This influx of faculty from Berkeley to Harvard, including Dorfman in the 1950s, might have played an important role in the development of the latter department. According to Samuelson (interview to Draenos 2002, 3-4), the "Andy's Boys" became the "Berkeley Mafia" in Harvard, and that "many people think that the revival of Harvard after a somewhat lean period, was importantly related to the infusion of Berkeley blood". At Harvard, Caves became the Chairman of the Department of Economics in the second half of the 1960s, while Rosovksy became the Dean of the Faculty of Arts and Sciences in the 1970s, and even served as acting president of the university in 1984 and 1987.

Also in 1965, Minsky left for Washington University at St. Louis, Williamson accepted an untenured associate professor position at the Department of Economics at the University of Pennsylvania (Williamson 2005, 495), and Winter left for a full time job at the Rand Corporation. In 1966, Diamond returned to the MIT as professor and Laidler left to the United Kingdom, as a lecturer at the University of Essex (according to him to avoid being drafted to the Vietnam War; Leeson 2010, 8). Nevertheless, the core of the mathematical economics group of Berkeley remained in the department through the 1960s, and with the new vacancies there were opportunities for new appointments.

6.2 Chairman Roy Radner and approximation with the exact sciences

The first appointments following the various faculty departures from the Department of Economics were newly minted PhDs from Stanford's Economics Department: Steven Goldman and Aaron Douglas in 1965. Both were part of the IMSSS mathematical economics group as discussed in the previous chapter. In 1966, Radner was appointed the new Chairman of the Department of Economics. During his term, Radner sought to expand the number of mathematical economists in the department by the appointment of recent PhDs and the approximation with the other departments on campus.

In the preceding decade, the recent PhDs appointed by Papandreou to Berkeley came mainly from Harvard. During Radner's term as Chairman, however, another department had cracked into the top of the pecking order of American economics graduate programs. The MIT PhD program in Industrial Relations in the postwar period had been deeply reformulated. Like in Berkeley, several recent Harvard graduates in economics had an important impact in the transformation of the MIT program into a central PhD program in economics in the United States, notably Paul Samuelson and Robert Solow (Duarte 2014, 90). The latter played a very important role in the MIT PhD program as dissertation adviser for graduate students. During Radner's term as Chairman, five recent PhDs from the MIT were appointed professors of economics at Berkeley, three of them having been Solow's advisees. George Akerlof was appointed professor at Berkeley in 1966, Robert Hall, Thomas Rothemberg (who had Franklin Fisher as his advisor), and Richard C. Sutch (a student of Franco Modigliani) in 1967, and Avinash Dixit in 1968. Hall had some previous links with Berkeley. Before going to the MIT for his graduate studies, he had been an undergraduate student at Berkeley, where he was very influenced by Jorgenson as his professor. They remained in contact while Hall was at the

MIT, which paved the way for his later return as professor to Berkeley. Their shared work during Hall's undergraduate years yielded a paper in taxation policy and investment theory that became Hall's first major publication (Hall and Jorgenson 1967).

Another strategy adopted by Radner to expand mathematical economics in the department was to offer joint appointments to Berkeley faculty from other departments, like Papandreou had done before appointing professors from the agricultural economics department. From the School of Business, Radner invited the game theorist John Harsanyi to a joint appointment in the Department of Economics. After concluding his graduate studies at Stanford under Arrow, Harsanyi had taught at the Australian National University between 1958 and 1961, then returned to the United States for an appointment at Wayne State University, moving to Berkeley's School of Business in 1964. He received his joint appointment in the Department of Economics during the 1966-67 academic year.

There was also an important partnership with a novel cross-disciplinary department. George Dantzig returned to Berkeley as a professor at the college of engineering in 1960, where he joined Ronald Shephard (who had returned to Berkeley in 1957 as professor of Industrial Engineering) and C. West Churchman (professor at the School of Business) to create a graduate course in Operations Research, thus creating the new department of Industrial Engineering and Operations Research (IEOR). For the 1966-67 academic year, the IEOR and Mathematics departments offered a joint appointment to David Gale, who then was professor at Brown University. With the coming of Gale, a relevant actor in the mathematical economics scientific network in the 1960s, Radner promptly offered him a 0% appointment in the Department of Economics, a recently created dispositive of the University of California to spur cross-disciplinary collaborations, allowing a professor to become part of a department without required teaching (Moore 2007, 241). This appointment held no teaching obligations, but Gale actively participated in the department as a thesis advisor. He was the graduate advisor for some important Berkeley economists in the following years, including William Brock (PhD 1969), Hal Varian (PhD 1973) and Lawrence Benveniste (PhD 1975). Also, through such connections of the Department of Economics with the IEOR, Walter Diewert, an economics graduate student at Berkeley in the late 1960s advised by McFadden, was able to learn from the relative obscure work that Shephard (1981) had conducted in Princeton by the beginning of the 1950s on duality and production theory. Diewert's work in the early 1970s

proved important to stabilize one of the basic duality results in production theory as "Shephard's Lemma" (Diewert 1971).[122]

In this setting of expansion of mathematical economics in the department, more graduate students decided to follow research in the field and some relevant contributions to economics were weaved at Berkeley through the relationship of professors and their students. For instance, the building of McFadden's conditional logit model (or multinomial logit, McFadden 1974) began through interactions with his graduate student Phoebe Cottingham in 1965 at Berkeley (McFadden 2001).[123] Jorgenson shared with his graduate students Laurits Christensen (PhD 1968) and Lawrence Lau (PhD 1969) the important work on translog functions (Christensen, Jorgenson, Lau 1973, 1975).[124] Graduate teaching was also under transformation. In the academic year of 1969-70 there were some modifications in the organization of graduate classes in economics. The "Fundamentals of Economic Theory" course became targeted to master's students, while the "Advanced Economic Theory" was transformed into a yearly long course that consisted of the core training of Berkeley's PhD economists. Candidates were required to have intermediary training in economic theory, algebra and analysis, and applicants without proper training had to take preparatory courses on those topics at the Department of Economics (201A-B: Economic Theory) and at the Department of Mathematics (190A-B-C: Survey of Algebra and Analysis).[125] We quote from Berkeley's general catalog a detailed list of the topics studied in the Advanced Economic

[122] See other Berkeley's former students pioneering the use of the now widespread term "Shephard's Lemma" in Lau and Yotopoulos (1972) and Hall (1973). Duality was a central topic among mathematical economists at Berkeley. See Diamond (Moscarini and Wright 2007, 548) for instance: "Important in the profession generally, and particularly at Berkeley at the time, was the use of duality. The McFadden/Winter development of teaching micro out of duality, which influenced how Varian wrote his textbook, was something I was immersed in at the time. (…) The paper I have with McFadden on the use of the expenditure function in public finance was really stapling together different things that he and I had worked up that used expenditure functions, that is, the duality tools that I picked up at Berkeley."

[123] The nested-logit model, an application of the multinomial logit, was developed by McFadden in 1970 during a visit to the MIT, working along with his former colleague Diamond. McFadden remembered being "invited to operationalize" a model on consumer transportation decisions worked by Diamond and Hall. This led to the fabrication of the nested-logit model (McFadden 2001, 354). See also Dupont-Kieffer, Rivot, and Madre (2021).

[124] Jorgenson was a major thesis advisor at Berkeley during the 1960s. A considerable part of his students did not follow the academic career. Among some relevant academic economists who were advised by Jorgenson at Berkeley in that period are M. Ishaq Nadiri (PhD 1964, Professor at New York University), Richard W. Parks (PhD 1966, Professor at the University of Washington); N. E. Savin (PhD 1969, Professor at the University of Iowa), and Ajit Singh (PhD 1970, Professor at Cambridge University). See Lau (2000, lvi-lxi).

[125] Below we quote a description of the contents of such courses, from the Courses and Curricula section of the General Catalog, Berkeley for the academic year 1969-1970:

Math 190A: Analytic geometry, differential and integral calculus. Math 190B: Difference of equations, linear algebra. Math 190C: Calculus of several variables (partial differentiation, extremum problems), complex numbers and trigonometry, vectors and vectors spaces. (p.409)

Econ 201A-B: Basic preparation for the PhD program including: demand and supply, organization of economic data, the firm, consumer demand, capital, investment, general equilibrium, international trade, welfare, aggregative income and employment, the price level, stability, growth, policy analysis. (p.238)

Theory (202A-H) course that was taught in the fall, winter and spring terms to PhDs candidates in economics:

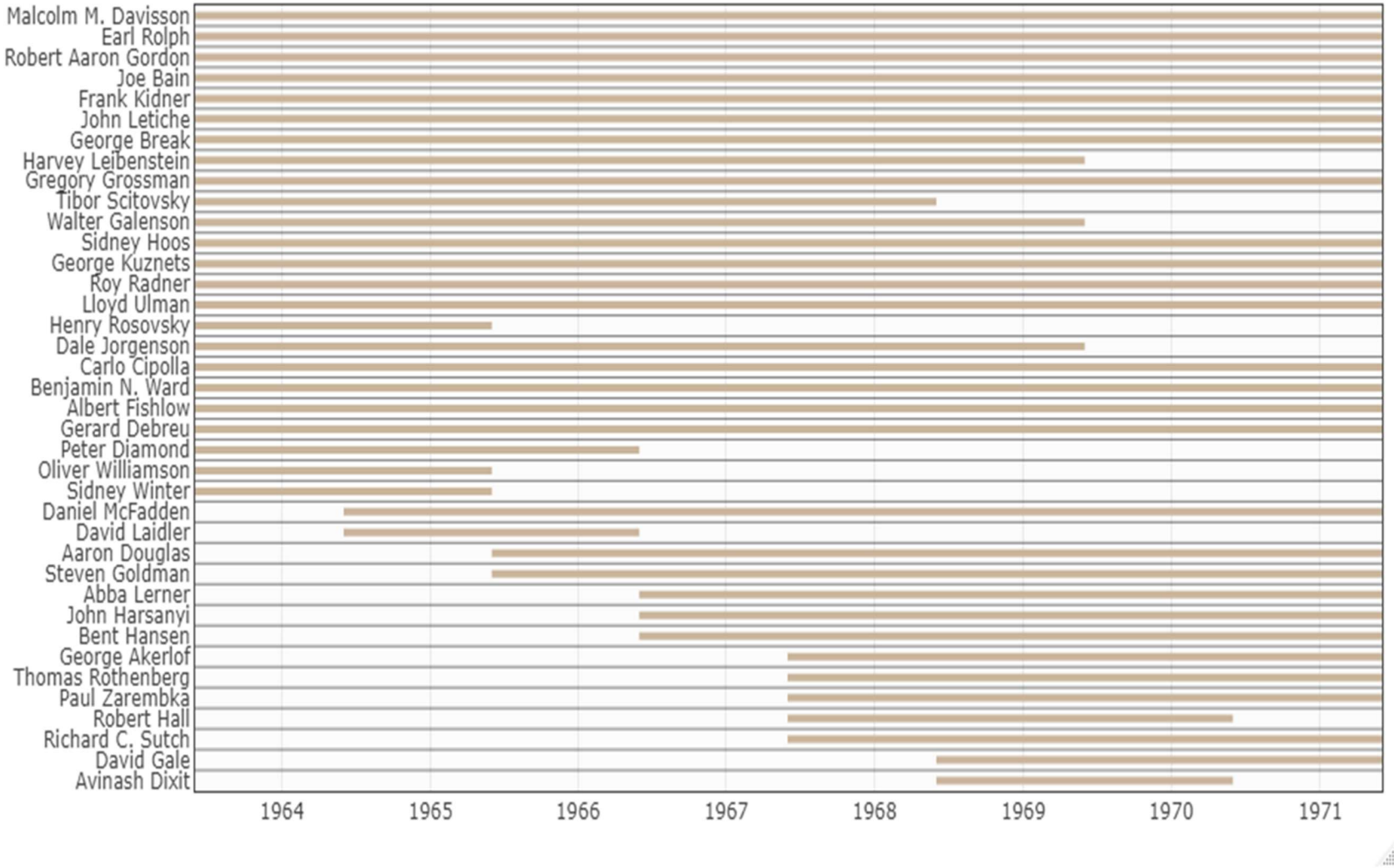
> 202A. Linear and Nonlinear Programming in Economic Models: Separation theorems, constrained optimization, linear and nonlinear programming, the Kuhn-Tucker theorem, local conditions.
>
> 202B. Theory of Value, General Competitive Equilibrium, Partial Equilibrium, Introduction to Decision Theory: Consumer Demand under certainty and uncertainty; various models of production, including activity analysis, input-output, production and cost functions, technical change; existence and optimality of competitive equilibrium, social welfare, externalities.
>
> 202C. Advanced Macro-Economic Theory: A cross-theoretical survey of major models, whether aggregate or disaggregate, which deal with the totality of the economy of a country or of the world. The accent is upon the comprehension of the structures of the various models.
>
> 202D. Capital, Investment, and Optimal Growth: The nature of capital; formal models of production possibilities in time; techniques of intertemporal optimization; efficient and optimal investment and growth for an economy; optimal investment for an enterprise or sector of an economy; examples of special investment problems.
>
> 202E. Decision, Information, Organization, and the Theory of Games: Representation of individual preferences; information and decision; group decision, teams, the theory of games.
>
> 202F. Welfare Economics: The role of ethics and values in social decision, social welfare functions, market optimality and second best theory, externalities and public goods, welfare implications for public policy, distribution theory.
>
> 202G. Econometric Models of Consumer and Firm Behavior: Problems of testing the axioms of economic theory and of building econometric models from theoretical propositions. Detailed consideration of a variety of special models suitable for econometric applications.
>
> 202H. Special Topics: Sections to be announced annually (General Catalog, Berkeley, 1969-70, 238)

Thus, by the end of the 1960s, the core knowledge required from an economics PhD candidate at Berkeley had come to include most of the novel mathematical techniques developed in the postwar period, such as linear programming, general equilibrium theory, game theory, and optimal growth. While there was no escape for the graduate student from

mathematical economics, there were still several possible research tracks for someone less enthusiastic about the subject. At the dissertation stage, PhD students had to present their graduate research in a specialized seminar which was under the responsibility of a professor. Besides the options of mathematical economics (led in that academic year by Debreu) and econometrics (led by Hall, McFadden, and Rothenberg), there were the Monetary Theory Seminar (led by Sutch), the Economic History Seminar (Cipolla), the Industrial Organization Seminar (Bain), the Public Economics Seminar (Rolph), and the Labor Economics Seminar (Ulman). Thus, although mathematical economics had become a necessary technique for a graduate student to learn, by the beginning of the 1970s there were still plurality in the research options.

The inauguration in 1971 of the Evans Hall, the new building dedicated to the mathematical sciences that was planned by administration in the 1960s, created a locational cleavage within the Department of Economics. The Departments of Mathematics, Statistics, and Computer Science held most of the space in the new building. However, office spaces were offered to some scholars from other departments (including economics, but also demography, electrical engineering, and journalism) that shared research interests with those disciplines (Moore 2007, 228-29). While the economists linked to mathematical economics and econometrics moved their offices to the Evans Hall, the less technical economists remained with their spaces in the Barrows Hall, the social sciences building. Even though the mathematical practices of those economists (i.e. Debreu, McFadden, Akerlof) were strikingly different, they clearly distinguished themselves from the non-mathematical economics practiced by the economists who stayed behind at Barrows Hall. The department would only reunite at a single location by the end of the century, when it fully moved to Evans Hall.

Figure 2.6: Berkeley's Economics Faculty (1963-1971)



7. Concluding Remarks

For the first half century of existence of the Department of Economics at the University of California, Berkeley, there was no mathematical economics being done in the institution, despite the presence of Evans in the Department of Mathematics. Evans offered classes in mathematical economics at Berkeley and advised research in the subject to a considerable group of mathematics graduate students since the 1930s. However, his influence on Berkeley's economists was very limited up to his retirement in the early 1950s. It was in the beginning of that decade that the first mathematical economists arrived at Berkeley, but found a discouraging environment during a tumultuous period for the department. Such an atmosphere led this first group of mathematical economists to leave the department still in the first half of the decade.

The reorganization of mathematical economics in the department after this initial disbandment needed the mobilization of administration to settle down conflicts within the department. In this context, the role played by Kerr as Berkeley's Chancellor and by Papandreou as Department's Chairman was crucial. In order to keep Berkeley's economics a competitive department, they appointed recent PhDs who did research in mathematical economics to renew teaching in the department. Through the influence of this incoming new generation of mathematical economists, along with the appointment of some senior specialists in the subject such as Gerard Debreu in the 1960s, mathematical economics at Berkeley changed, in the span of fifteen years, from non-existent to a necessary knowledge all graduate students should master.

This fast methodological transition of economics in the United States was not immediately reflected on the homogenization of methodological stances in faculty. The slower dynamics of the academic market, plus the larger size of Economics at Berkeley, allowed some plurality in research into the 1970s. However, the fact that tenured faculty could keep their jobs does not imply that they were not marginalized due to their out of fashion research. An illustrative case is that of former Department Chairman Malcolm Davisson, who spent the last decade of his professional career limited to teaching in the law school.

With the inauguration of the Evans Hall and the establishment of the mathematical economists in the building, more opportunities of cross-departmental and cross-disciplinary exchanges came up. One relevant academic encounter in Evans Hall was that of Gerard Debreu and the 1966 Fields Medalist Stephen Smale. Debreu's axiomatic method and rigorous formalism made it possible for him "to communicate easily about mathematics and economics" (Smale 1984, 61) to Smale, whose major work was in differential topology. Smale became enthusiastic about mathematical economics, making a number of contributions to the field in the 1970s, and even received a joint appointment in the Department of Economics in 1976 (Smale 181, 331). As we shall see in the next part of this dissertation, the Debreu-Smale connection played an unintended role in the dissemination of the formalist thread of mathematical economics to Brazil.

PART II. Mathematical economics in Brazil

CHAPTER 3: Applying pure mathematics: IMPA and the dissemination of mathematical economics in Brazil

In the mountains that rise above the central area of Rio de Janeiro, Brazil, lays the Tijuca Forest, one of the largest urban forests in the world. Going up the road that leads to the southern entrance of the forest's national park, there is a large three-story building of modern architecture amidst trees of the tropical forest. It is the *Instituto de Matemática Pura e Aplicada* (IMPA, Institute of Pure and Applied Mathematics), that hosts some of the most famous mathematicians of the country. The institute caught the attention of the lay public after Artur Ávila (PhD and researcher at IMPA) being awarded the 2014 Fields Medal, becoming the first Brazilian to receive such a prestigious scientific award.[126] IMPA was founded in 1952, and since then has been an example of elite mathematics practiced in Brazil. However, the institute has not limited itself only to "pure" mathematics. As its name suggests, IMPA has encompassed other disciplines that "apply" mathematics. The hard sciences would be the more obvious candidates, but it was economics who flourished among the mathematicians at the institute. In such a process, IMPA has turned Brazil into a participant of the internationalization of mathematical economics since the 1960s.

In the Cold War context, the internationalization of economics to the Third World had different features and served different purposes than the relationships between First World countries. Development economics had a major role in the internationalization of the discipline in a postcolonial world, in particular under the influence of modernization theory (Gilman 2003). The internationalization of economics to Latin America during the Cold War years was shaped by interest in development issues, from the ECLAC to the Chicago Boys (Montecinos 1996; Parmar 2015, ch. 7). Early internationalization of economics to Brazil relied on funding from governmental institutions such as the United States Agency for International Development (USAID) or private foundations such as the Ford Foundation. Groups of students around the so-called brazilianists such as Albert Fishlow at Berkeley and Werner Baer at Vanderbilt were important for the transmission of particular branches of American postwar economics into Brazil. However, the internationalization of the more abstract theory such as mathematical economics didn't occur through development economics. IMPA became crucial for creating a space where abstract economics could expand in Brazil.

[126]See for example the August 2014 special edition of Brazilian culture magazine *Piauí*, cited in Baranyi (2016).

Mathematical economics found a fertile environment at IMPA. By traveling across disciplinary borders, mathematical economics developed in the United States found its way to travel to Brazil. Several historical reasons account for how economics got into the institute. From international scholars who impacted the institute in its early years, and had some interest or influence in economics (i.e. Stephen Smale and the Bourbaki group as discussed in section 3.1) to a number of young students doing important work in both economics and mathematics (such as Mario Henrique Simonsen, Aloisio Araujo and José Alexandre Scheinkman), economics at IMPA was deemed a possible application of mathematics since its beginnings. The dissemination of mathematical economics to Brazil was weaved through IMPA's academic connections with the mathematics and the economics communities abroad. Those connections helped the creation of an economics graduate program at IMPA that was carried forward by many researchers with interests in both disciplines. This thesis' chapter contributes to a history of the internationalization of mathematical economics, showing that the dissemination of economic theory during the Cold War period has not been limited to departments of economics. It shows the relevance of the approximation of scientific communities of economists and mathematicians in Brazil and abroad. By telling the history of the dissemination of mathematical economics to Brazil through IMPA, this chapter shows how the two disciplines cooperated for their mutual expansion.

1. *Mathématiques Tropiques*: The foundation of IMPA and its early influences

1.1. Bourbaki's influence in the professionalization of Brazilian mathematics

After World War II, science went through dramatic transformations in the developed world, and the same was true for nascent scientific communities in the periphery. As in the United States, but on a smaller scale, the few newborn universities in Brazil were benefited by the temporary migration of scholars from war-ridden Europe. Founded in 1934, the *Universidade de São Paulo* (USP) held several European visiting professors in its ranks in its first years of existence (Skidmore 2003). One famous visitor was the French sociologist Claude Lévi-Strauss, who did anthropological work in Brazil between 1935 and 1939 as a visiting professor of sociology at USP.[127] Aside from his influence on the social sciences, Lévi-Strauss also played a role in the shaping of USP's mathematics. In 1944, he introduced

[127] The work done in Brazil led him to publish his famous memoir *Tristes Tropiques* (Lévi-Strauss 1955).

the French mathematician André Weil to the Brazilian geneticist André Dreyfus, who was Dean at USP.[128] The university had leaned on Italian professors to build up the teaching of mathematics, but after Brazil declared war against the Axis in 1942 it became necessary to look for visiting scholars elsewhere.[129] In the harsh context of World War II, Weil was an instructor of mathematics to undergraduates at Lehigh University, living off an expiring Rockefeller Foundation subsidy, and the prospect of visiting Brazil interested him. He accepted Dreyfus' invitation for a visiting professorship and moved to São Paulo in January 1945 (Weil 1992, 190).

Table 3.1: Bourbaki at USP

Bourbaki member	Visiting period
André Weil	1945-47
Jean Dieudonné	1946-47; 1952
Jean Delsarte	1948-51
Charles Ehresmann	1952
Samuel Eilenberg	1952
Laurent Schwartz	1952

Sources: *Anuário da FFCL* 1939-49, 615-616; *Anuário da FFCL* 1951, 364; *Anuário da FFCL* 1952, 352; USP archives

Weil was a well-known member of the Nicolas Bourbaki group of mathematicians who strived for an axiomatization of mathematics based on a particular view of the discipline.[130] He would become the first of a series of Bourbaki visitors at USP, as the university became a safe harbor for the group in the postwar period. After his first semester in Brazil, in June 1945 Weil had the opportunity to attend a Bourbaki group meeting in France for the first time since he left the country due to the war (Weil 1992, 189-191). Weil was then able to invite other members for visiting positions at USP. In the end of that year, the

[128] The encounter happened in New York, and is narrated by Weil (1992, 185).
[129] There were two main visiting mathematicians at USP during the 1930s: Luigi Fantappiè and Giacomo de Albanese. The former left in 1939 and the latter in 1942 (Abdounur and Matos 2012). Weil was cited as a possible replacement in the department meeting in June 1942. Another scholar mentioned as an option was the 1936 Fields medallist Jesse Douglas (see Minute of *Faculdade de Filosofia, Ciências e Letras* Congregation Meeting, 06/05/1942, USP archives).
[130] Corry (2004) has connected the Bourbaki group with the contemporary concept of mathematical structure – defined by that group as abstract forms, in which certain aspects of empirical reality could be fit into (Bourbaki 1950, 231). For their influence on Gerard Debreu's work, see Weintraub and Mirowski (1994) and Ingrao and Israel (1991, ch. 9).

Bourbakist Jean Dieudonné joined Weil as a visitor at USP (*Ata do Conselho Universitário* 11/14/1945, USP archives), staying for two academic years (*Anuário da FFCL 1939-49*, 615-616, USP archives). Until 1952, at least one member of the Bourbaki group visited USP each year, as Table 3.1 shows. They not only taught mathematical courses in the Bourbaki tradition at the university, but also helped transform the mostly isolated and small Brazilian mathematical milieu into a developing international community.[131]

The Bourbaki visitors at USP also paid visits to a springing community of young engineers interested in mathematics in Rio de Janeiro. They helped some of those students to get scholarships for research in mathematics in American universities. After returning to Rio, they would play central roles in the creation of IMPA in 1952.

1.2. The creation of IMPA in Rio de Janeiro

With the creation of *Fundação Getúlio Vargas* (FGV) in December 1944 in Rio de Janeiro, the veteran engineer and astronomer Lélio Gama was put in charge of a research group which was planned to become a department of mathematics.[132] Although such plans failed and the research group endured only until 1947, the networks weaved at FGV were crucial in the later creation and development of IMPA. The FGV group was able to host important short-time visitors such as Weil, then at USP, and University of Chicago mathematicians Marshall Stone and Adrian Albert. Despite the short duration of the FGV group, the networks weaved at FGV were crucial in the later creation and development of IMPA. In the research team of the FGV group were the young scholars Leopoldo Nachbin and Maurício Peixoto, two recently graduated engineers from the *Escola Politécnica* (EP, Polytechnic School), the engineering school at *Universidade do Brasil*, in Rio de Janeiro. Both had an interest in pure mathematics, studying the topic under Gama. With letters of recommendations received from the foreign visitors, Nachbin and Peixoto were awarded scholarships sponsored by the US State Department and American Private Foundations for

[131] Trivizoli (2015) shows how the recommendation from Bourbakists helped a number of young Brazilian mathematicians to receive funding from private foundations for post-doc positions at American universities.
[132] Before the FGV group, Gama led the department of mathematics at *Universidade do Distrito Federal* (UDF) from 1935 to 1938 in Rio de Janeiro (Dias 2003, 186). After a dictatorial regime was instituted in 1937, the university fell under direct political interference, and was renamed *Universidade do Brasil*. After a year in the new institution, Gama left it in 1939, becoming the director of the National Astronomical Observatory (*Observatório Nacional*).

working as assistant researchers at the University of Chicago, where Nachbin stayed from 1948 to 1950, and Peixoto from 1949 to 1951 (Baranyi 2016, 698; Peixoto 2003, 243).[133]

In 1951, the Brazilian federal government created the *Conselho Nacional de Pesquisa* (National Research Council, CNPq) for funding scientific research in the country. The main interest at the time was the financing of research in nuclear power technology. Brazil had some very promising physicists working on the topic at *Centro Brasileiro de Pesquisas Físicas* (CBPF) in Rio de Janeiro, a physics research institute founded in 1949 (Patti 2015) and nuclear technology received much attention from the government and the military in the aftermath of the nuclear bomb detonations in 1945.[134] Gama seized the opportunity created by such political and military interest in physics to create a mathematics institute (IMPA). After all, it was an easy argument that advanced mathematics was an obligatory passage point to nuclear physics. In 1952, CNPq sponsored the creation of IMPA by Gama and his team of four young mathematicians, Nachbin, Peixoto, Paulo Ribenboim, who had studied at University of Nancy under Dieudonné, and Luiz Henrique Jacy Monteiro, professor at USP who had worked closely with Bourbakist visitors. Their work at IMPA was a side job to their regular academic appointments, because the institute had not enough funds for hiring full-time faculty. The initial facilities for IMPA were restricted to two rooms inside CPBF's building. Early visitors, as the young bourbakist Alexander Grothendieck (who visited for two academic years in 1952-53), lived far from the institute for most part of the year, working at USP, that in turn helped paying their stipend (Gama, L. letter to CNPq, 11/12/1952, and Gama, L. letter to CNPq, 03/12/1953, IMPA archives). Nevertheless, by the second half of the decade the institute had become a recognized Bourbakist milieu, like Nancy and Chicago. As mathematician Paul Halmos noticed, "there [were] young men in Rio de Janeiro almost all of whose mathematical education was obtained from [Bourbaki] works" (Halmos 1957, 88).

During the 1950s, American private foundation's fellowships assisted the young mathematicians at IMPA to pursue research and forge academic connections in American universities and research institutes (Trivizoli 2015). As we saw, Nachbin visited the University of Chicago in 1956 and 1957 under a Rockefeller fellowship, and then spent the first semester of 1958 at the Institute of Advanced Studies (IAS) at Princeton University. His

[133] See also Mauricio Matos Peixoto fellowship files, 1950, Folder 1642, Box 89, Series 305, SG 10.1, FA244, Rockefeller Foundation records, Rockefeller Archive Center.
[134] One of the founding members was César Lattes, who contributed to important work on the pi meson in the late 1940s (Lattes, Muirhead, Occhialini and Powell, 1947).

research at the time was mostly focused on topology (Nachbin [1950]1965) and topological vector spaces (Nachbin 1954).[135] A former student of Nachbin at EP and one of the first students at IMPA, Elon Lages Lima went to the graduate program in mathematics at the University of Chicago, funded by a Rockefeller Fellowship. Lima's major research interest then was on algebraic topology (Lima 2003, 96). He completed his graduate studies at Chicago in 1958. In his last academic year at Chicago, Lima became friends with postdoctoral fellow Stephen Smale, who was visiting the department after receiving his PhD in mathematics from the University of Michigan in 1957. In the second semester of 1958, Smale was going to spend a year at Princeton's IAS. Through his IMPA connections, Lima knew that Peixoto was also going to the institute in that semester after the return of Nachbin. Lima then introduced Smale to Peixoto, which proved to be a changing event to the former's career per his own account (Smale 1980, 23). Peixoto's results on structural stability in a disk (Peixoto 1959, 1962) turned Smale's research interests towards dynamical problems. The interest in Peixoto's work led Smale to decline offers to teach at Harvard, Chicago and Berkeley, and instead go to Rio de Janeiro in 1960, spending the last semester of his National Science Foundation postdoctoral fellowship at IMPA (Batterson 2000, 50-54).

At IMPA, Smale developed a proof for the higher-dimensional Poincaré conjecture in topology (Smale 1960), and also did work on the horseshoe in dynamical systems, a hallmark on early chaos theory (Smale 1967). He enjoyed his mornings doing mathematics at Copacabana beach, and the afternoons at the institute discussing differential equations with Peixoto and topology with Lima (Smale 1990, 46).[136] With the end of his NSF fellowship in July of 1960, Smale accepted a position in the department of mathematics at the University of California, Berkeley. Other mathematicians who joined Berkeley in this period and were also close to the IMPA faculty include Morris Hirsch, Lima's PhD colleague at the University of Chicago, and their dissertation supervisor Edwin Spanier (Smale 1990). The breakthroughs in dynamical systems theory ignited by Peixoto's work in the late 1950s bestowed early international recognition to the small Brazilian institute. By the end of the decade, IMPA had

[135] After being translated to English in 1965 and having a second edition in 1976, Nachbin's PhD dissertation was rediscovered by mathematical economists such as Mehta (1977, 1981) and Chichilnisky (1977) - possibly due to the influence of his student José Alexandre Scheinkman, an important contributor that will appear will appear later in the chapter.

[136] "My best-known work was done on the beaches of Rio de Janeiro, 1960!" became a famous Smale quotation among mathematicians. This was an ironic statement in a letter to the NSF in 1966, after the foundation suspended his grant on the accusation that he was enjoying vacation in Europe and not performing any research duties, amidst Smale's political involvement with anti-Vietnam War protests at Berkeley (see Smale letter to Connick, 09/16/1966, cited in Batterson 2000).

become entrenched in the global networks of mathematics majorly through two fields. Topology, represented by Nachbin, had historical connections to the Bourbaki visitors in Brazil, while Dynamical Systems was the product of Peixoto's work and connections done while visiting IAS at Princeton.

2. Expansion across disciplines: Economists at IMPA in the 1960s

2.1. IMPA's early connection to Brazilian economics through Mário Henrique Simonsen

Despite its small budget and size, IMPA was able to establish some international recognition in pure mathematics, through the work of Nachbin and Peixoto and their connections with Bourbaki and American mathematics. On the other hand, IMPA's administration still desired to develop research in applied mathematics. Gama envisioned the institute doing applied mathematics as a strategic action. The institute needed to justify the relevance of its work in order to claim funds from an underdeveloped country's government, and pure mathematics was not as compelling as, for instance, research in nuclear power. Gama's annual reports to the CNPq show how he continuously sought to develop the applied mathematics field at IMPA and to approximate the pure mathematicians to other applied sciences. In 1957, Gama communicated that "something auspicious among us is the beginning of the activities in applied mathematics. Along with the formation courses in mathematics (…) to physicists and chemists, we will hold a course in game theory and linear programming, part of mathematical economics" (*Ofício 44/57*, Gama to CNPq 1957, IMPA archives).[137]

For an institute of mathematics that was born within a physics research facility, the emergence of mathematical economics as the first topic studied in applied mathematics is peculiar. This is explained by the interest on the topic by a particular individual, the engineer turned economist Mário Henrique Simonsen.[138] While an undergraduate student of engineering at EP, Simonsen entered a student internship at IMPA in March 1955 with his colleague Lindolpho C. Dias, who later became the director of the institute (*Portaria*, 03/08/1955, IMPA archives). In 1957, as a recently graduated twenty-three years old

[137] Citations from the IMPA archives or interviews held in Portuguese were translated to English by the author.
[138] For biographical information on Simonsen and on his influence as an economist, see Alberti, Rocha and Sarmento (2002), and Cabello (2012).

engineer, and already a professor of engineering economics in EP (Alberti, Sarmento and Rocha 2002), Simonsen was responsible for the first course on applied mathematics at IMPA. The mathematical economics course led to the publication of a monograph in 1958 that gives a glimpse on the topics approached. Simonsen (1958) explores novel results in duality applied to economics theory, the simplex method, and Koopmans's transportation model. The cited bibliography is evidence of Simonsen's awareness of the postwar developments in American mathematical economics, including important work such as the activity analysis conference volume (Koopmans 1951) and Dorfman, Samuelson and Solow's 1958 book on linear programming.

In the 1950s, academic training in economics in Brazil was still in its infancy, with the first proper Departments of Economics being established after World War II (at *Universidade Federal do Rio de Janeiro* and *Universidade de São Paulo* between 1945 and 1946; see Fernandez and Suprinyak 2018, 2). Nevertheless, economic problems were studied by a number of relevant Brazilian scholars, particularly topics in economic policy and Brazilian economic history. Such scholars who came to be the early generation of Brazilian economists were initially trained either in law (for instance, Octavio de Gouvêa Bulhões, Ignacio Rangel, and Celso Furtado) or engineering schools (Roberto Simonsen, Eugênio Gudin, Mário Henrique Simonsen), with an important exception being Antonio Delfim Netto, who received his bachelor degree in economics from USP in 1951 (Fernández and Suprinyak 2019, 757). Advanced training in economics was mostly pursued abroad in developed countries (for instance, Furtado in France and Bulhões in the United States). Graduate level training in economics in Brazil was only organized in the 1960s, with a decisive contribution from Mário Henrique Simonsen. In 1960, Simonsen left his job at EP and his position at IMPA for an appointment as an economist at FGV. The institution had already some tradition in economics, being responsible for the publication of the first Brazilian scholarly journal in the discipline (*Revista Brasileira de Economia* – Brazilian Journal of Economics), and the creation within its structure of *Instituto Brasileiro de Economia* (IBRE) in 1951, which played a central role in the designing of the Brazilian national accounts (Fernández and Suprinyak 2018, 3).[139] At FGV, Simonsen organized one of the first programs of graduate training in economics, the master's in economics at *Escola de Pós Graduação em Economia* (EPGE; 'Graduate School of Economics' in English), the other being USP's *Instituto de Pesquisas*

[139] FGV also hosted a number of important economists as visitors during the 1950s, including Gottfried Haberler, Nicholas Kaldor, Gunnar Myrdal, Ragnar Nurkse, Raúl Prebish, and Jacob Viner (Fernández and Suprinyak 2018, 3).

Econômicas (IPE; 'Institute for Economic Research') master's program led by Delfim Netto, both first offered in 1966 (Fernández and Suprinyak 2019, 759).

On the eve of the inauguration of those programs, Simonsen, Delfim Netto and other relevant representatives of academic economics in Brazil gathered at Itaipava, a hilly winter retreat near Rio de Janeiro, for a seminar sponsored by the Ford Foundation dedicated to discuss the current state and the future of advanced training in economics in Brazil. Simonsen and Delfim Netto, as the directors of the newly created graduate training programs in economics, offered their suggestions for improving Brazilian training in the discipline. Among the major problems on the formation of economists in Brazil diagnosed by Simonsen was the "lack of basic knowledge in mathematics and statistics" and "troubles with logical reasoning and incomprehension of the scientific method: most economists recently graduated in colleges do not understand what a model is or intends to be" (Simonsen 1966, 20). Simonsen proposed a core of mandatory courses that should be common to every department. The economics graduate student should be trained in four fields: (i) Mathematics, which included calculus, differential and difference equations, linear algebra, and linear programming; (ii) Statistics, covering probability theory and econometrics (iii) Microeconomics, covering consumer and firm theories, partial and general equilibrium theories, and imperfect competition and (iv) Macroeconomics, covering national accounts, Keynesian macroeconomics, and inflation and growth theory.[140] He suggested a small list of textbooks for each topic, including the 1958 book by Dorfman, Samuelson and Solow that he used previously at IMPA, along with other recently published textbooks with new developments in mathematical economics.[141] Learning English (and possibly French) should also be required of master's students (Simonsen 1966, 27). The master's program envisioned by Simonsen, by relying on technical training, had a two-fold objective: to send abroad (especially to the United States) students who excelled in the advanced training in Brazil to a PhD program in high ranked universities, or to prepare the student for a professional career in

[140] Delfim Netto's proposal, which also emulated recent developments in American PhD training, did not differ significantly from Simonsen's proposal. See Delfim Netto (1966) and Fernández and Suprinyak (2019, 761).

[141] Simonsen suggested Granville (1904), de la Vallée Poussin (1903, 1906), Dorfman, Samuelson, and Solow (1958) and R.G.D. Allen (1959) to the Mathematics course; Hoel (1947) and Johnston (1963) to the Statistics course, Henderson and Quandt (1958), Friedman (1962), Baumol (1961), Hicks (1939), and again Dorfman, Samuelson, and Solow (1958) to the Microeconomics course; Ackley (1961), Dernburg and McDougall (1960), and Baumol (1951) to the Macroeconomics course. Cf. Simonsen (1966) and Fernández and Suprinyak (2019, 760).

government or business.[142] The targeted audience included not only undergraduate students in economics but coming from other disciplines, in particular engineers interested in economics (like Simonsen once was). In the following years, new economics programs inspired in the common core presented at Itaipava were created in Brazil, including the programs at the *Universidade Federal de Minas Gerais* and *Universidade Federal de Pernambuco* in 1968 and at *Universidade Federal do Ceará* in 1972 (Fernández and Suprinyak 2019, 761).

The technical training program proposed by Simonsen and Delfim Netto at Itaipava was in line with the then prevailing ideas of modernization at the Ford Foundation. However, the beginning of the 1970s brought the demise of modernization theory and fundamental ideological change at Ford Foundation (Gilman 2003, ch. 6). As argued by Fernandez and Suprinyak (2019, 762-768), such new context of the early 1970s led to a promotion of a methodological and ideological pluralism in the development of Brazilian academic economics. Despite resistances and reactions from Simonsen and EPGE (FGV), the increasing importance of ANPEC (Portuguese acronym for 'National Association of Centers for Graduate Economics') in the organization of Brazilian graduate training in economics supported the creation of heterodox centers, notably the graduate program at the University of Campinas, which supported research in the ECLAC's structuralist tradition and Marxist economics (ibid). Thus, American postwar neoclassicism remained not hegemonic in Brazil up to the end of the century.

2.2. Aloisio Araujo and José Alexandre Scheinkman at IMPA in the late 1960s

Simonsen's linear programming course at IMPA was his first step in a path that put him at the center of the organization of advanced training in economics in Brazil in less than a decade. However, his course did not start the growth of IMPA's applied work in mathematical economics, or any applied mathematics. The institute's budget started to decline in 1957, as government interest on funding research on physics was replaced by a focus on economic development through major (and expensive) public works (Newton Ferreira Campos to Lélio Gama, 10/29/1956, IMPA archives). Reviewing the situation of the institute, Gama wrote to CNPq that "research at IMPA in 1959 won't reach 1958 levels" due to financial restrictions (*Ofício 6*, Gama to CNPq, 01/59, IMPA archives). The plan of hiring a foreign professor to

[142] In the aftermath of the creation of the advanced training programs in economics at FGV and USP, the heads of those departments became influential figures in economic planning during the military dictatorship regime that took power in 1964. Delfim Netto served as Minister and the head of economic policymaking from 1967 to 1974 and again from 1979 to 1985. Simonsen was his substitute from 1974 to 1979.

develop applied mathematics had to be canceled (1959 Annual report to CNPq, IMPA archives). Up to Gama's retirement in 1965, IMPA still could not organize a research group in applied mathematics.

An opportunity to raise IMPA's funding emerged in 1964, when the national development bank (BNDE) created a national fund for financing research in technology in Brazil, known as *Fundo de Desenvolvimento Técnico-Científico* (FUNTEC). The fund was designed to foster research in physics, chemistry, and engineering, and their resources were limited to those disciplines. Lindolpho Dias, who had just undertaken the direction of IMPA after Gama's retirement, worked to include mathematics within the scope of FUNTEC, and the discipline was included in the program after 1966 (Dias 2003, 192). With the expansion provided by FUNTEC resources, EP moved to a larger campus. IMPA then moved to the vacant building left by the engineering school, where many of the institute's researchers had studied, after a reform paid by FUNTEC funds. Compared to its former location, IMPA's new headquarters were five times larger, which enabled the institute to expand its activities. FUNTEC resources also allowed IMPA to raise salaries, expand the library, hire more employees, and to develop a master's and PhD program in mathematics (Cardoso and Oliveira 1968, 91; Dias 2003, 193). Contrasting to its financial situation in the late 1950s, by the end of the 1960s IMPA was considered "one of the Brazilian research institutes with best financial resources" (Carlos Chagas's letter to Lima, 03/21/68, IMPA archives). Private foundation money continued to fund scholarships to students and faculty to spend research periods in American academic institutions (William Carmichael's letter to Dias, 09/04/68, IMPA archives).

With the new graduate programs in place, IMPA was in search of new students. New offering of courses and workshops to interested students was a way to lure elite undergraduate prospects to the institute, coming from various areas. Aloisio Araujo was one of the students who arrived by this time at IMPA. Araujo was an undergraduate student in two universities. He entered college at UFRJ (*Universidade Federal do Rio de Janeiro*, formerly *Universidade do Brasil*) to study economics in 1964, and enrolled in the statistics program at the National School of Statistical Science (ENCE in the Portuguese acronym) the following year.[143] At ENCE, he took classes with Oscar Porto Carreiro, a pioneer in Actuarial Sciences and

[143] Founded in 1953, the school was much influenced by the Italian economist and demographer Giorgio Mortara, who fled Italy to Brazil in 1939 due to persecution by the fascist regime. See biographical information, in Portuguese, in https://memoria.ibge.gov.br/historia-do-ibge/pioneiros-do-ibge/20977-giorgio-mortara.html

Statistics in Brazil, enhancing his interest in mathematics and statistics (Araujo 2019).[144] Araujo and some classmates at the Department of Economics with interests in mathematics then visited IMPA and decided to enroll in some courses offered by the institute.[145] They contributed to the advertisement of the institute by affixing an IMPA poster offering mathematical courses in the hallways of the Department of Economics at UFRJ. If the impact of such promotion was limited, it became important to the career of José Alexandre Scheinkman, a younger undergraduate student of economics at UFRJ who learned about IMPA through such posters (Scheinkman 2003, 166). He first arrived at the institute in 1966 for a course in calculus, a whole new topic for him at the time.[146]

Both Araujo and Scheinkman began the master's program in mathematics at IMPA even though they were still undergraduate students, which was made possible due to the flexible rules of the institute. Scheinkman worked close to Nachbin, who since 1965 was sharing his time between IMPA and the Department of Mathematics at the University of Rochester, where he was appointed full professor. [147] It was Nachbin who first presented mathematical economics to Scheinkman, through a book from Hukukane Nikaido (Scheinkman 2003, 168).[148] After receiving his master's degree in mathematics from IMPA in 1969, Scheinkman decided to begin the application process for a PhD in the United States. He could feel some peer pressure to continue his studies in mathematics, but was still considering the option of pursuing a PhD in economics. Nachbin acted towards a middle of the road option. Knowing the mathematical quality of the work done by Lionel McKenzie in the Department of Economics at the University of Rochester, he suggested Scheinkman to apply to that department. The letter of recommendation from a professor of mathematics at the university, Nachbin, helped Scheinkman's acceptance to the graduate program in economics in 1970.[149]

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[144] On Porto Carreiro's participation in the professionalization of actuaries in Brazil, see Martins (2020, 373). For an example of his early work, see Porto Carreiro (1948).

[145] Araujo (2002) cites his friend Frederico Carvalho, along with Dionísio Dias Carneiro, Francisco Lopes, and Luís Otávio Façanha as colleagues at UFRJ in the 1960s interested in applications of mathematics and statistics to economics.

[146] Because IMPA's move to the former EP building only began in 1967, Araujo and Scheinkman were able to know the first headquarters of IMPA. The latter remembered the offices as a "very intimate site, a small house with few spaces where people got sort of crowded" (Scheinkman 2003, 166).

[147] LN.T.1.002, Leopoldo Nachbin archive, Museu de Astronomia e Ciências Afins, Rio de Janeiro. See also The University of Rochester Undergraduate Studies Official Bulletin 1964, 134.

[148] It is possible to infer that Nikaido's book that Scheinkman refers to in the 2003 interview is *Convex Structures and Economic Theory* published in 1968 (Nikaido 1968).

[149] Nachbin ended his association with IMPA in 1971, remaining at the mathematics department at Rochester, from where he retired in 1992.

Like Scheinkman, upon the conclusion of his master's degree at IMPA, Araujo applied for PhD programs in the United States. With a double major in economics and statistics, and a master's in mathematics, Araujo decided to apply to a statistics program. Such a decision was based on his views of statistics as a bridge between pure mathematics and applied social problems (Araujo 2003, 5). He was accepted as a PhD student in the Department of Statistics at the University of California, Berkeley in 1969. [150] The connections between IMPA and Berkeley mathematics played an important role for his move to California. As Araujo remembered, "there was a culture at IMPA in favor of Berkeley" (Araujo 2003, 5), with a number of recent Berkeley's PhDs returning to the institute as faculty at that time. Studying in the cross-disciplinary space of the Evans Hall (see ch. 2), Araujo remained close to economics through mathematical economics. He would eventually take economics as his main research interest instead of statistics in which he got his PhD. Araujo and Scheinkman played important roles in the development of economics at IMPA and its connections to the international scientific community. Nevertheless, while both were pursuing their PhD abroad in the early 1970s, IMPA continued its efforts to increase its applied mathematics sector.

3. Applying pure mathematics: Economics at IMPA in the 1970s

The expansion of IMPA's budget following the inclusion of mathematics into the scope of FUNTEC allowed the institute to grow the numbers of faculty members. IMPA began the 1970s with a larger faculty compared to its early years. After Nachbin left IMPA in 1971, Peixoto's and Smale's dynamical systems research became the major influence at the institute. By the beginning of the decade, most of the IMPA faculty had graduated in Berkeley and/or was conducting research in the field of dynamical systems (see Table 3.2). Applied mathematics was still finding its way in the institute. In the second half of the 1960s, the mathematician Djairo de Figueiredo (Courant Institute PhD, 1961) had been the responsible for applied mathematics at IMPA, but left the institute to *Universidade de Brasília* in 1971.

A new effort to develop applied mathematics at the institute was made in the beginning of the 1970s, with the appointment of Pedro Fernandez as professor. He received the responsibility of putting together a new graduate program in statistics and its applications.

[150] Araujo's dissertation supervisor was Lucien Le Cam, who had been influenced by the Bourbaki tradition as a student at the University of Paris in the 1940s (Moore 2007, 156).

Fernandez had completed his PhD in statistics at Berkeley in 1970 and joined IMPA in the following year. By his recommendation and intermediated by director Dias, American statistician Barry James was appointed professor at the institute in 1972 (Letter from Dias to James, 10/01/1971, IMPA archives). James was also a recent PhD from Berkeley, having received his degree in 1971.[151] The expansion of the Brazilian Institute of Geography and Statistics (IBGE, *Instituto Brasileiro de Geografia e Estatística*) in the 1970s helped the statistics graduate program at IMPA to attract students interested in advanced training.[152]

Table 3.2: IMPA Faculty, 1971

Professor	Research Field	PhD
Maurício M. Peixoto	Dynamical Systems	Universidade do Brasil, 1952
Elon L. Lima	Topology	University of Chicago, 1958
Manfredo P. do Carmo	Geometry	UC Berkeley, 1963
J. M. Sotomayor Tello	Dynamical Systems	IMPA, 1964
Pedro Nowosad	Analysis	NYU, 1965
J. L. Camacho	Analysis	Frankfurt, 1967
Jacob Palis Jr.	Dynamical Systems	UC Berkeley, 1968
Yves Lequain	Algebra	LSU, 1968
Sheldon Newhouse	Dynamical Systems	UC Berkeley, 1969
Floris Takens	Topology	University of Amsterdam, 1969
Pedro J. Fernandez	Statistics	UC Berkeley, 1970
César Camacho	Dynamical Systems	UC Berkeley, 1971
C. A. S. Isnard	Analysis	University of Chicago, 1971

Source: Annual Report to CNPq, 1971, IMPA archives

The IMPA administration envisioned the statistics program as being able of hosting other specialty areas such as mathematical economics. In November 1971, Dias wrote a letter to Araujo, then at Berkeley, telling about the beginning of the statistics graduate program at the institute (11/03/1971, IMPA archives). He told Araujo about the plan to include economics as a specialty area within the statistics program, and that he would be welcome

[151] In 1981, James published at IMPA an intermediate level textbook on probability in Portuguese that is still often used for statistics training in economics graduate programs in Brazil (see James 2015 for a recent edition).
[152] IBGE is the state agency responsible for the Brazilian census, national accounts and several data, statistics and geography research. The expansion of the institute in the 1970s was put forward by the economist Isaac Kerstenetzky, president of IBGE from 1970-1979. On this topic, see Senra (2006).

back at IMPA to lead such a project, due to his interest in both disciplines. Araujo would only return from the United States by the end of the decade, but meanwhile the statistics program kept the plan of including other fields in the graduate program. The hiring strategy adopted continued to focus on recent PhDs from elite American institutions, who often had studied at IMPA before entering graduate school. Operations Research became a field within the statistics program through the appointment of Ruben Klein, PhD in mathematics at MIT in 1974, and Jack Schechtman, who received his PhD in 1973 from the Department of Industrial Engineering and Operations Research (IEOR) at the University of California, Berkeley.

There is a shared perception among IMPA members that the institute's interest in mathematical economics was sparked by Smale's advances in the field in the 1970s.[153] Mathematical economics, however, had a presence at IMPA before Smale took any interest in it. Besides Simonsen's classes at IMPA in the late 1950s, as part of the institutional plan of attracting applied mathematicians, Elon Lima (who was acting as IMPA director) negotiated in 1970 the visit of Argentine mathematician Hector Sussman, a recent PhD from the Courant Institute, to teach a course on mathematical economics to IMPA students.[154] The course had a sixteen lecture plan, covering from general equilibrium theory to Keynesian models of aggregate demand and international trade using David Gale's infinite horizon models. Despite the effort done by both parts, bureaucratic issues hindered the materialization of the visiting period of Sussman at IMPA (Letter from Jean-Paul Jacob to Elon Lages Lima 05/28/1970, IMPA archives). In the following year, there was the already mentioned letter from Dias to Araujo regarding the plans of creating a specialization in economics within the scope of the graduate program in statistics.

Despite the absence of proper mathematical economists at IMPA, there was some demand from the students to learn the subject. The engineer Schechtman, who had David Gale as PhD dissertation supervisor, then began to offer courses in mathematical economics within the statistics program. His experience with mathematical economics during his graduate studies in the Evans Hall had even rendered a publication in the *Journal of Economic*

[153] See, for instance, Araujo (2003, 9), Scheinkman (2003, 171, 176). Smale had visited Brazil in 1971 to attend a conference on dynamical systems in Salvador, organized by Jacob Palis, his former student at Berkeley. I have found no evidence that Smale was already involved in mathematical economics in 1971 or that he influenced any IMPA faculty to pursue a program of economics as an applied field of mathematics at the institute during that visit.

[154] Another echo of economics at IMPA was the decision of mathematician José Hamilton G. Silva (a close friend of Lima) to turn to economics as a PhD candidate at Rice. See letters of Silva to Lima, 05/27/1969 and 08/16/1970, IMPA archives.

Theory on an optimal income fluctuation problem (Schechtman 1976). In the first semester of 1978, Schechtman offered an introductory class on mathematical economics, the first on the topic at IMPA since Simonsen's course twenty years before. The statistics master's student Pedro Valls Pereira was the only enrolled student of the course.[155] Valls Pereira was in the application process for graduate studies in economics abroad, moving in the next year to the PhD program in economics at the London School of Economics. In the first semester of 1979, Schechtman had more attendance in his classes. He taught an econometrics course at IMPA, having eleven enrolled students.[156] Among the group were two engineers who were under Schechtman's supervision in the master's program of statistics and operations research, Armando Castelar Pinheiro and Ricardo Paes de Barros. Both would then pursue a PhD in economics in the United States, the former at Berkeley and the latter at the University of Chicago.

Moreover, Schechtman was important in luring a mathematics graduate student, Marilda Sotomayor, to work with mathematical economics. Sotomayor had begun her career in mathematics as a primary school teacher, after concluding her studies in 1961 at a "Escola Normal" in Rio de Janeiro, an institution between high school and professional training that prepared (mostly women) for work as teachers in primary schools. After a couple years teaching mathematics in schools, she enrolled at *Universidade Federal do Rio de Janeiro* (UFRJ) as an undergraduate student in mathematics in 1964. She learned about IMPA during her last year in college, through Lindolpho Dias who besides leading IMPA was director of the Department of Mathematics at UFRJ. Her arrival at IMPA was decisive for her academic career. After some years as a research assistant, she enrolled into the master's program in mathematics at IMPA, receiving her degree in 1972.[157] While pursuing her master's degree, Sotomayor began teaching at the Department of Mathematics at PUC-Rio, a prestigious private university in Rio de Janeiro. She enrolled in the doctoral program in mathematics at PUC but it was at IMPA that Sotomayor pursued her research. Her dissertation supervisor at

[155]This information (and any other information in this paper on classes taught at IMPA) relies on the lists of enrolled students from the records of past classes at the institute's archives.

[156] Other courses taught by Schechtman in his first years at the operations research program at IMPA included queueing theory and network theory. After 1980, with the start of the mathematical economics PhD course and the hiring of Araujo, Schechtman focused on the operations research program, continuing to teach in his area of expertise, programming and stochastic processes.

[157]At IMPA, Marilda Sotomayor met her future husband, the Peruvian born mathematician Jorge Sotomayor Tello, one of the first graduate students in the institute, having completed his studies under Peixoto in 1964. He spent some years as an associate researcher in the Department of Mathematics at Berkeley, where he worked with Smale, returning to IMPA as professor by the end of the decade. According to Marilda Sotomayor, her husband was as an enthusiast of academia and science and was an important supporter to her decision to follow graduate studies and pursue a scientific career. They married in 1970 (Vasconcelos 2021).

IMPA was Schechtman. He presented to Sotomayor a possible extension for the model of economic growth that he developed during his PhD studies with Gale, on which she worked during her doctoral dissertation.

After receiving her doctoral degree from PUC in 1981, Sotomayor went to Berkeley in January 1983 on an associate researcher position, where she had the opportunity to work directly with Gale. When arriving at Berkeley, she found out that Gale was no longer interested in the growth problems she had been working on during her doctorate. He was then interested in matching problems that were being addressed in a burgeoning literature. Sotomayor then changed her topic of research from economic growth to game theory (Sotomayor 2009). She stayed at Berkeley as an associate researcher from 1983 to 1985, and then visiting researcher in the Department of Economics of the University of Pittsburgh on multiple occasions from 1985 to 1992.[158]

As we shall see in the next section, the return of Araujo and Scheinkman from the United States to IMPA in the late 1970s and the creation of the mathematical economics master's program in 1980 would help Schechtman's students to apply with success for PhD programs in economics in the United States. It also fostered interest in economics among other students at IMPA. However, while the mathematical economics graduate program was developing, the statistics and operations research group declined in the 1980s. Many professors linked to the statistics group left IMPA in that decade, including Fernandez and Schechtman.

4. Consolidation of a community: the mathematical economics graduate program at IMPA

4.1. Araujo and Scheinkman in the American universities and their return to IMPA in the late 1970s

During the 1970s, while IMPA was organizing the graduate program in statistics with some links with mathematical economics, Araujo and Scheinkman were becoming relevant actors in the American community of mathematical economics. Although Araujo decided to pursue his PhD in statistics at Berkeley, the environment he would find in the Evans Hall

[158] In the following years she authored with Gale and Alvin Roth multiple papers on the subject, and an influential book, *Two-Sided Matching* (Roth and Sotomayor 1990).

would keep his research interests linked to mathematical economics.[159] In the department of statistics, Araujo shared the same building of economists such as Gerard Debreu, Daniel McFadden, George Akerlof, and Andreu Mas-Colell. Indeed, at Evans Hall, mathematical economics became a topic of interest shared by faculty of other departments. The conversion of Smale to mathematical economics was of particular importance. After Debreu reached Smale at Berkeley to discuss some mathematical problems in general equilibrium theory, the mathematician became increasingly interested in the subject (Smale 1984). Doing mathematical economics was the first experience of Smale in applied mathematics – in the 1980s he also made contributions to algorithms and computer science. He became a member of the advisory board of the *Journal of Mathematical Economics* at the time of its creation in 1974 - the first volume of the new publication had four research papers written by him.[160] When Debreu (1993) reviewed the work of Smale in mathematical economics, he emphasized the productivity of the mathematician while he was interested in the subject. Between 1973 and 1981, Smale published fifteen articles addressing problems "at the core of [general equilibrium] theory" (Debreu 1993, 131). At the height of this interest in mathematical economics, Smale made his second trip to IMPA, in April 1976. During the same year, he was appointed a professor of economics at Berkeley with a 0% appointment (Moore 2007).

In the dynamical cross-disciplinary space at the Evans Hall, Debreu hosted regular summer seminars on mathematical economics that attracted economists from many different locations to discuss general equilibrium theory and other topics. A regular participant in the seminars was Scheinkman, who was himself working with mathematical economics at the Department of Economics of the University of Rochester. Scheinkman's visits to Berkeley rendered a co-authorship with Araujo on a paper published in *Econometrica* exploring properties of dynamic programming in infinite dimensional spaces, applying it to optimal growth and turnpike theory (Araujo and Scheinkman 1977).

At Berkeley, Scheinkman met William Brock, a PhD in mathematics under the supervision of Gale. They started a research in the summer of 1973 during the seminars that would lead to results for optimal control systems used in growth theory (Brock and Scheinkman 1976). Scheinkman completed his PhD at the University of Rochester in 1974,

[159] Araujo (2019) reports having contact with applications of mathematics in the social sciences before arriving at Berkeley by reading a paper by Debreu and the works of R. Duncan Luce in mathematical psychology (see, for example, Luce 1956).
[160] Other members of the advisory board were Kenneth Arrow, Robert Aumann, Gerard Debreu, David Gale, Leonid Hurwicz, Edmond Malinvaud, Boris Mityagin, Roy Radner, Herbert Scarf and Lloyd Shapley.

his research themes focusing on global stability results of turnpike theory models with discounted utility. In the published paper of the dissertation, he acknowledged financial support from Ford Foundation and IMPA during his PhD (Scheinkman 1976). IMPA was also a sponsor of Araujo's PhD fellowship (Letter from Dias to Araujo, 1971, IMPA archives). After their graduation, both had to spend some time at IMPA as a counterpart for the funding. Before returning to Brazil, however, Scheinkman moved to the University of Chicago on a postdoctoral fellowship.[161] The mathematical dynamics he had learned at IMPA proved useful in a time when economists were aiming to build dynamical models in macroeconomics. With Lawrence Benveniste, another PhD student of Gale whom he met on a workshop at the University of Pennsylvania in 1975, Scheinkman worked on an application of the envelope theorem to dynamical programming (Benveniste and Scheinkman 1979) and on the application of duality theory in dynamic optimization models (Benveniste and Scheinkman 1982).[162] With these and other work in progress (Jones and Scheinkman 1977, Scheinkman 1978), he received tenure as professor of economics at the University of Chicago in 1977 and became a fellow of the Econometric Society in the following year.

After graduating in 1974, Araujo remained at Berkeley as a postdoctoral fellow in the Department of Statistics (Araujo 2003, 8). An economist who played an important role for his return to economics was Mas-Colell. They had followed similar career paths: both started their college studies as undergraduate students of economics, then moved to mathematics for their graduate education (Araujo at IMPA and Mas-Colell at the University of Minnesota), and then back again to economics (or a mathematics-economics hybrid), with a detour in statistics to Araujo. In collaboration with Mas-Colell, Araujo published his second research paper in economics (Araujo and Mas-Colell 1978) on the smoothing of aggregate demand functions, a problem initially posed by Debreu (1972). For most of the 1970s, Araujo was still sharing his research time between statistics and economics.

In the beginning of 1978, Araujo joined Scheinkman as a visiting professor at the Department of Economics of the University of Chicago. Together at Chicago they planned their visiting period at IMPA. Araujo visited IMPA in the second semester of 1978, but still remained in Chicago for the whole year of 1979. In Rio de Janeiro, he taught two courses

[161] By the influence of Arnold Harbeger, Chicago was home for several Brazilian students during that time. Scheinkman recalls meeting Afonso Pastore, Cláudio Haddad, Fernando Holanda and Antonio Carlos Gonçalves during his time at Chicago University (Scheinkman 2003, 173).

[162] Although this work was published in 1982, Benveniste and Scheinkman started working on it at least before 1976, as it is cited in Scheinkman (1978) as an unpublished manuscript.

from August to November, *Dynamical Economics* and *Microeconomics*. Araujo has credited this period as crucial for his decision to come back for a career in Brazil in 1980, stating that he returned to Chicago in 1979 "with the intention of coming back" (Araujo 2003, 8). Even though he was concerned with being isolated from the international community, he believed IMPA "created the conditions of doing science far away from established centers" (ibid, 9).

Scheinkman visited Rio de Janeiro from September 1978 to December 1979.In contrast to Araujo who returned in 1980 for a career in Brazil, Scheinkman returned for his tenured position at the University of Chicago and built his academic career in the United States. Therefore, this period in Rio de Janeiro was crucial for Scheinkman to develop networks with the Brazilian academic community. Scheinkman held visiting positions at IMPA and at EPGE (FGV). Scheinkman was invited to EPGE by director Carlos Langoni, a 1970 PhD graduate in economics from the University of Chicago (Scheinkman 2003, 174). Scheinkman's time at EPGE coincided with the return of Simonsen to academic affairs. Since 1974 acting as Brazil's Minister of Finance, Simonsen resigned from office in August 1979 and returned to his academic duties at EPGE. With his return to academia, Simonsen and Scheinkman met for the first time.[163] They were participants in the *I Encontro Brasileiro de Econometria* (EBE, 1st Brazilian Meeting in Econometrics) in 1979, a meeting organized mostly by economists who had recently returned from their graduate studies in prestigious universities in the United States. The meeting marked the creation of the *Brazilian Econometric Society* (SBE), a first institutionalization of the scientific community of mathematical economists in Brazil. [164] SBE remains an important association of economists in the context of Brazilian academia, which promotes the use of quantitative methods in economic research.

During his time at IMPA, Scheinkman interacted with the group of young students at the institute who had sprouted around Schechtman. In the second semester of 1979, Scheinkman taught the course *Introduction to Economic Theory* to IMPA students. Interaction with Schechtman sparked a joint work published in *The Review of Economic Studies* on

[163] Scheinkman reported being surprised by how Simonsen was aware of very recent research while serving as minister for six years. He also mentions a later encounter with Simonsen, during the creation of the Santa Fe Institute in 1987, when the former minister was representing Citibank, an important sponsor of the institute (Scheinkman 2003, 175-177).

[164] Among the organizers of the event were the members of the first SBE presidential office: Joanílio Rodolpho Teixeira (PhD Kent, 1975), Edmar Bacha (PhD Yale, 1968), Fernando de Holanda Barbosa (PhD Chicago, 1975), Roberto Macedo (PhD Harvard, 1974), José Luiz Carvalho (PhD Chicago, 1972), and Adriano Dias (PhD Vanderbilt, 1976). See Teixeira (1984, 145).

general equilibrium theory under the existence of storage (Scheinkman and Schechtman, 1983). In 1980, Scheinkman returned to the University of Chicago, while Araujo took the reverse path, leaving Chicago to become professor at IMPA. Back in the institute as professor, he organized the before planned graduate program in mathematical economics. Although it remained small in numbers, the mathematical economics group at IMPA had an enduring impact in the shaping of a scientific community of economists working on general equilibrium, game theory, auction theory, and other related topics. Those fields of research found an easier path of international dissemination to Brazil through a mathematics institute rather than connections between economics departments.

4.2. Consolidation of a scientific community: The creation of the mathematical economics graduate program at IMPA

The mathematical economics program at IMPA began at a very small size. In the 1980s, a total of six students received master's degrees, and one received a doctorate degree from the program. The program expanded in the 1990s, granting 19 master's degrees and 6 doctoral degrees, but continued to operate on a small scale and being elitist.[165] In 1981, IMPA moved to a larger site, to its current headquarters in the building located near the Tijuca Forest. With more space, the institute could receive visitors more frequently and offer them better office spaces. Proportionally to the small size of the graduate program, visiting researchers became an important part of the teaching of mathematical economics at IMPA (see Table 3.3 for a selected list of visiting appointments in the mathematical economics program). Moreover, visiting economists offered an important opportunity to those graduate students of the program desiring to apply for a PhD abroad. In 1980, Hugo Sonnenschein visited IMPA and met student Sergio Werlang, later his PhD student at Princeton University. In the following year, Ricardo Paes de Barros met his future advisor at the University of Chicago, James Heckman, who came to IMPA on a short-period visit. In the next years, Araujo's co-authors would become frequent visitors to IMPA, such as Alberto Holly and Mario Pascoa.[166] Another very frequent visitor during the 1980s was Scheinkman, who often

[165]The number of graduates in mathematical economics (List of Graduates in Mathematical Economics, IMPA archives) is a conservative estimation of the total number of economists who attended classes at IMPA. Some interested economists could attend classes as non-enrolled students, and others would not complete their degree, due to opportunities abroad, for instance. IMPA openness to this kind of informal attendance to classes limits a complete quantitative assessment since these students are not listed in the documents in the archives.

[166] Pascoa had an appointment at the *Universidade Nova de Lisboa* that has been described (Bastien 2000, 176) as the university most aligned with American economics in Portuguese academia.

spent his summers at IMPA. The situation changed in the 1990s when Scheinkman became more demanded abroad, such as when he became the Chairman of the Department of Economics at Chicago in 1995-98.

Werlang and Carlos Ivan Simonsen Leal were the first students officially enrolled in the mathematical economics graduate program.[167] They were engineers who studied together at EP and attended some classes in mathematics at IMPA before joining the master's program in mathematical economics (Alberti, Sarmento and Rocha 2002, 240). After completing the master's at IMPA in 1982, both were accepted in the economics PhD program at Princeton University. They concluded their studies in 1986, doing research in game theory (Leal 1986, Werlang 1986). With their doctorate degree in hand, they returned to Brazil becoming professors of economics at FGV (EPGE) in Rio de Janeiro.

Table 3.3: Number of visits to IMPA (1980-1999)

Visitor	**Affiliation**	*1980-84*	*1985-89*	*1990-94*	*1995-99*
J. A. Scheinkman	University of Chicago	5	2	3	0
Hugo Sonnenschein	Princeton	3	1	0	0
Andreu Mas-Colell	UC Berkeley	2	0	0	0
James Heckman	University of Chicago	1	0	0	0
Alberto Holly	Lausanne	1	1	4	4
Mario Pascoa	Nova Lisboa	0	1	3	5
Costas Azariadis	University of Pennsylvania	0	1	0	0
Edward Prescott	University of Minnesota	0	0	1	0

Sources: Annual Reports of Activities (1980-1999), IMPA Archives

The third student to enter the mathematical economics program at IMPA was Paulo Klinger Monteiro. He was the first student to receive a doctorate degree in mathematical economics at IMPA, in 1988. Monteiro was an early exception, as the next student to receive such a degree did it only in 1994. During its first years, the program remained specialized as a springboard for PhDs in prestigious American universities, which was not the case of Monteiro. Nonetheless, he was able to engage with the global community of mathematical economics. Trained among mathematicians, Monteiro dealt primarily with more abstract theory in his work (for instance Monteiro 1987 on the existence of utility representations for preferences defined on path-connected spaces). In 1990-91, he went for postdoctoral studies

[167] Leal was the nephew of Mário Henrique Simonsen, who had a major influence on his career towards economics (Klüger 2017, 705).

at Harvard, where he collaborated with Mas-Colell. From 1994 to 1999, Monteiro taught microeconomics in the mathematical economics program at IMPA.

Although the mathematical economics program at IMPA was organized mostly around Araujo, other professors were hired to complete the economics offerings at the program. Initially there were temporary positions occupied by recent PhDs returning from the United States (such as Paulo Guedes, returning from the University of Chicago in 1980). In 1983, after completing his PhD in economics at LSE, Valls Pereira was appointed assistant professor at IMPA, teaching in the program until 1986. Paulo César Coutinho was appointed professor at IMPA in 1984. Coutinho had completed in that year his PhD in economics at the University of Pennsylvania, his thesis supervisor being Costa Azariadis. At IMPA, Coutinho taught general courses in mathematical economics and more specific classes in contract theory. He supervised a total of six master's students at IMPA, while all other mathematical economics students had Araujo as dissertation supervisor. Coutinho was absent from IMPA for visiting positions in the departments of economics of Princeton University in 1988 and the University of Pennsylvania in 1989. He left IMPA to the department of economics in the University of Brasilia in 1993. Monteiro replaced him and was appointed professor at IMPA in 1994, where he continued teaching until 1999.

Graduate textbooks are important elements on standardization of curricula and consolidation of a department position (Teixeira, 2014). Araujo sought to organize his teaching at IMPA through textbooks that provided material in Portuguese for students. His textbooks reflected mostly topics discussed in classes. In 1983 he published *Introdução à Economia Matemática* (Introduction to Mathematical Economics), a textbook reportedly inspired in Arrow and Hahn (1971), Varian (1978), and Ekeland (1979) – the latter being a visitor to IMPA in the same year of the publication of Araujo's textbook. The book was structured in two parts, isolated behavior, including consumer and firm theory under certainty and uncertainty, and collective behavior, with Arrow's impossibility theorem, general equilibrium and Pareto efficiency, and game theory. Araujo published another textbook in 1993, *Introdução à Economia Dinâmica* (Introduction to Dynamical Economics), reportedly inspired by Stokey and Lucas (1989). The book covered models of optimal economic growth, as the Ramsey-Cass-Koopmans model and turnpike theory, and methods of recursive solutions using dynamic programming. It also included an appendix on incomplete markets written by Monteiro. Both books carried a mathematical style typical of other IMPA's

productions: theorems stated with clean and direct proofs, with very few space dedicated to discuss economic intuition – which explains the short number of pages of both books, Mathematical Economics with 122 pages and Dynamical Economics with 98 pages.

Although hosted within a mathematics institute, the mathematical economics program was able to establish connections to other Brazilian economics departments, especially FGV (EPGE). In 1982, EPGE director Mario Henrique Simonsen invited Araujo to teach mathematical economics in the economics department. Sharing time between IMPA and EPGE, Araujo was a central actor in the close relations between the two departments. In 1984, Simonsen returned once again to IMPA to teach a class in macroeconomics. In that period the departments conducted research focused on different topics, and even the targeted audiences differed, as recalled by Araujo in an interview:

> At EPGE there were more engineering students with the ambition to work professionally with economics; they got their master's degree, had excellent training, and then looked for a job (…) While at IMPA students usually were pure mathematicians, without major interest in economics; they came from mathematics, and doing mathematical economics was a big step toward applications (…) It was not a competition, IMPA never joined Anpec, and the research I did was associated with pure mathematics (…) EPGE had a strong interest in inflation, macroeconomics, the big topics that Simonsen worked on. (Araujo 2003, 12)

Since the 1980s, however, EPGE has turned into an important milieu for research in mathematical economics (or economic theory), stemming from this close connection to IMPA. In the 1990s, several former IMPA students joined Araujo as professors at EPGE. Leal chaired the EPGE department in 1994-97 and has been the president of FGV since 2000. Other former IMPA students were appointed professors at EPGE, as Werlang in 1993, Monteiro in 2000, and Humberto Moreira in 2002. The influence of this scientific network weaved at IMPA made mathematical economics a major topic of research at EPGE.[168]

Araujo (2019) said in an interview to the author that he envisioned the mathematical economics program at IMPA to do frontier research in Brazil and, to reach this goal, it was necessary to expand the PhD program. A total of ten students graduated as PhDs under

[168] In order to assess this claim based on published works, I have constructed an economics department ranking based on Debreu's bundle of journals (Debreu 1987) for the period 2008-2018 using Tilburg University Economics Ranking sandbox. By this standard, EPGE ranks in the top 50 world list of leading departments. However, it does not appear in the top 100 list using the standard bundle of journals provided by the Tilburg Ranking.

Araujo's supervision in the 2000s at the institute, more than the total number of PhDs graduated in the previous decades. A feature of this expansion was an increasing internationalization of the enrolled students. The program received candidates from other South American countries, such as Chile, Colombia, Peru, Bolivia, and Uruguay. Indeed, eleven of the twenty four PhDs in mathematical economics granted by IMPA are from those countries, a high rate of international students in comparison to the average Brazilian economics graduate programs.

Table 3.4: Publishing in Selected Journals: IMPA graduates (master's and PhD)

Researcher	PhD	Current Department	*Econometrica*	*REStud*	*IER*	*JET*	*JMA*
Aloisio Araujo	Berkeley, 1974	IMPA/EPGE	6	0	2	9	10
J. A. Scheinkman	Rochester, 1974	Columbia	5	5	1	9	1
Sergio Werlang	Princeton, 1986	EPGE	1	0	0	4	3
P. K. Monteiro	IMPA, 1988	EPGE	1	0	0	5	20
Alvaro Sandroni	IMPA, 1994	Northwestern (Kellogg)	3	2	0	3	2
Humberto Moreira	IMPA, 1996	EPGE	0	0	0	2	2
L. I. de Castro	IMPA, 2004	IMPA	2	0	0	4	1
José Heleno Faro	IMPA, 2005	INSPER	0	0	0	3	2
Ana Fostel	Yale, 2005	Virginia	1	0	0	1	0
Eduardo Faingold	Pennsylvania, 2006	INSPER	2	1	0	0	0
Gustavo Manso	Stanford (Finance), 2006	Berkeley (Haas)	2	0	0	2	0

Despite the small scale of the program, IMPA has been able to include its students into the global scientific network of mathematical economics. Table 3.4 quantifies the insertion of IMPA's former students in the main journals in mathematical economics. Following a list of top journals where research on the topic is published organized by Debreu (2008), the table lists the number of published articles of a list of selected former IMPA students in the *Journal of Economic Theory* (JET), *Journal of Mathematical Economics* (JMAE), *Econometrica*, the *Review of Economic Studies* (REStud), and the *International Economic Review* (IER). The table also includes the current affiliation of the researchers. Former graduates at IMPA have taken jobs not only at EPGE and INSPER[169] as stated in the table, but in various other universities in Brazil. A smaller group have followed their careers in academic institutions in the United States or other Latin American countries.

[169] INSPER is a private university located in São Paulo that is currently directed by economist Marcos Lisboa. Lisboa took classes from Araujo at IMPA before moving to the University of Pennsylvania for his PhD studies in the 1990s. He is listed as an enrolled student in some of Araujo's classes in the late 1980s, while he was an undergraduate student at UFRJ (IMPA archives).

5. Concluding remarks

Coats (1996, 3) described "the internationalization of economics [as] a complex, multifaceted process." In this process, not only national borders are crossed but also disciplinary boundaries. IMPA was in the middle of an intricate web of academic connections that included economics and mathematics departments. The institute grew under the influence from both national (USP, EP, EPGE) and American institutions (Berkeley, Chicago, Princeton, Rochester). Following American patterns, this dissemination was made possible by multiple patronages from higher education, government and foundations (Goodwin 1998).

As an institute of mathematics that created an economics graduate program very recently, IMPA has been usually overlooked as an important institution in the development of economics in Brazil (Loureiro 1996, Szmrecsányi and Coelho 2006, and Klüger 2017). This chapter adds to the history told by these works by including the influence of IMPA for the development of mathematical economics in Brazil. However, the scope of influence of IMPA is not limited to South America. The work done by Araujo (at IMPA) and Scheinkman has also been influential in the American scientific community of economics, as both are the only Brazilian economists in the rather exclusive group of the National Academy of Sciences.[170] IMPA continues to contribute to the formation of economists in Brazil today, as the mathematical economics program is still active. Among many temporary collaborators, Araujo continued his work as the centerpiece of the program. In May 2019, IMPA announced the hiring of former student Luciano Irineu de Castro as a researcher in the field. His inaugural class was named "Dynamic quantile models of rational behavior".[171]

Within an institution dedicated to advanced pure mathematics, the applied field, important to justify funding, flourished thanks to economics. An unintended consequence of the foundation of IMPA was the emergence of an important nest for mathematical economics. As the internationalization of the economics discipline to the Third World was crowded with development economics during the Cold War years, the more abstract mathematical economics took a turnpike through the post-war internationalization of mathematics to reach Brazil. This was made possible by the interaction of multiple academic institutions, private

[170] Their profiles are available in NAS website at < http://www.nasonline.org/member-directory/members/20012697.html> and < http://www.nasonline.org/member-directory/members/2541395.html>.

[171] The announcement was made in the institute website (in Portuguese): https://impa.br/noticias/luciano-de-castro-e-o-novo-pesquisador-do-impa/

foundations, governments, mathematicians, economists, and scholars traveling between both disciplines.

CONCLUSION

This dissertation explored the history of the growing of mathematical economics in the twentieth century through the history of its expansion at three specific institutions: Stanford and Berkeley in the United States, and IMPA in Brazil. The local histories told here are intertwined with the general process of the transformation of economics in the postwar period. They are not only particular cases of an all-encompassing process, just like the transformation of economics cannot be reduced to a sum of local processes. In each place studied, the development of mathematical economics was constrained and shaped by the macro process of transformation, as well as it shaped and constrained such process. Moreover, they showcase the complexity of such process of transformation. The histories document the multiplicity of actors mobilized in order to transform economics in the twentieth century, not limited to mathematical economists, but also of university administrators, private foundations, scientists from other disciplines, and many others - along with a measure of randomness.

In part I, I examined how mathematical economics developed within the departments of economics of the two Californian universities. The trajectories of the subject at each institution had their differences. At Stanford, the department of economics initially played a minor role within a university focused on research in engineering and technology. Such institutional structure promoted some early connections between the department of economics, hosted in the social sciences division, and the school of engineering. The creation of the Food Research Institute (FRI) in order to use a technical approach to assess the allocative problems of food production in the aftermath of World War I spurred pioneering research on mathematical economics at Stanford. Central actors of the scientific community of mathematical economics in the interwar period began their research careers within the academic space created at the FRI. The interwar experience of mathematical economics at the University of California, Berkeley was rather different, since it was mainly limited to the Department of Mathematics. The arrival of Griffith Conrad Evans as professor and head of that department in the early 1930s spurred interest in the subject among graduate students in mathematics. However, there was almost no exchange between the Department of Economics and the mathematical economics community that grew around Evans. The lack of institutional support for cross-disciplinary exchanges combined with an established tradition of economics at Berkeley that saw little or no advantages in mixing economic theory and mathematics contributed to such an outcome.

Research on and interest in mathematical economics went through remarkable changes in the 1950s. In that decade, the Ford Foundation channeled a large sum of funds to Stanford as part of its effort to develop the so-called behavioral sciences. Within this context, the Institute for Mathematical Studies in the Social Sciences (IMSSS) was established, led by faculty from multiple departments, to advance mathematical training and research in the application of mathematical methods to the social sciences such as economics, management, and psychology. Despite its small size, the IMSSS played a crucial role for the expansion of mathematical economics at Stanford. If the department of economics offered few vacancies for new faculty from the 1950s to the 1960s, the IMSSS supported the expansion of research positions to mathematical economists. The cross-disciplinary character of the IMSSS allowed researchers with backgrounds in other disciplines such as Hirofumi Uzawa, Samuel Karlin, and Herbert Scarf to interact with the mathematical economics community of Stanford. Moreover, the institute offered the economics graduate students a dynamic environment of research, fostering interest in mathematical economics among a new generation of economists.

At Berkeley, the postwar period marked the entrance of mathematical economists in the department of economics, but not without resistance from the established faculty. A first group of mathematical economists led by Robert Dorfman were appointed as professors in the department in the early 1950s. However, a conflictive environment nudged the group out of Berkeley before the middle of that decade. This impasse between mathematical economists and the established faculty would be untangled by the arrival of Andreas Papandreou to head the department in the second half of the decade. Papandreou was able to curb internal tensions among faculty during his administration. In order to maintain course offering in mathematical economics after the departure of Dorfman's group, he built networks with researchers with interests in the subject from other departments, as well as recruited a group of young mathematical economists. The "Andy's Boys", as that group became known, were responsible for making Berkeley in the 1960s an important milieu of mathematical economics in the United States. If by the beginning of the 1950s the climate for mathematical economics at Berkeley's economics department was bad enough to scare away researchers on the subject from the department, in the 1970s the department even received office spaces to be given to their mathematical economists in the new campus building dedicated to the mathematical sciences – the Evans Hall.

Such developments in the mathematical economics community in the United States were not confined to its national borders. In part II, the dissertation focused on the history of the dissemination of mathematical economics to Brazil. Also, it moved away from studying departments of economics to analyze mathematical economics at a research institute in advanced mathematics. Founded in the early 1950s in the context of a nascent higher education system, IMPA was able to gain recognition in the global mathematical community through the presence of members of the Bourbaki group in Brazil as visiting professors in the aftermath of World War II. This insertion of IMPA in the global community of mathematics allowed some young Brazilian students to pursue their graduate studies in mathematics abroad, mainly in the United States. Those students built networks abroad that, as they returned home, solidified IMPA's position within the global network of mathematics. One case of particular importance was the connection of the then recent PhDs Stephen Smale and Mauricio Peixoto that was crucial for the work of the former in the field of dynamical systems. When Smale became professor in Berkeley in the 1960s, connections between that university and IMPA were strengthened. A considerable group of Berkeley graduates became professors at IMPA, most of them advised by Smale.

IMPA played an important role in the growth of Brazilian mathematical economics through some particular connections. Beginning in the late 1950s, the engineer turned economist Mario Henrique Simonsen offered classes in linear programming using recent developments on mathematical economics being done in the United States. In the following decade, Simonsen left IMPA to organize one of the first graduate schools in economics in Brazil, where mathematical literacy was a fundamental skill that an economist had to master. In the 1970s, IMPA began to expand its course offerings, creating graduate programs on a number of topics associated with applied mathematics. In the statistics and operations research program, mathematical economics reappeared at the institute, under the guidance of Jack Schechtman, a former graduate student of David Gale at Berkeley's Department of Industrial Engineering and Operations Research. Then at the end of the decade, the institute inaugurated a graduate program in mathematical economics. Aloisio Araujo, formerly a Master's student of mathematics at IMPA who completed his PhD at Berkeley, was responsible for organizing such program. Through the international connections mobilized by IMPA, Master's students from the program were able to pursue their PhDs in economics in

prestigious universities in the United States, and later to follow academic careers in Brazil and abroad.

The expansion of mathematical economics in each place studied required the mobilization of multiple actors from different social spaces. At Stanford, the cross-disciplinary space created by the increased investment in the behavioral sciences during the postwar period became the major center of support to research in mathematical economics. The demand for research in the behavioral sciences opened positions for mathematical economists outside the department of economics. Also, the research opportunities generated by the behavioral science centers were able to push graduate students of economics into mathematical economics. At Berkeley, mathematical economics had to conquer its space within the department of economics. This was made possible through the mobilization of the university administration to manage conflicts with established faculty and to carry out the genesis of a mathematical economics group within the department. The dissemination of mathematical economics to Brazil through IMPA mobilized the intellectual networks drawn by the global community of mathematics. Those multifarious connections that have supported the dissemination of mathematical economics illustrate the complexity of the process of transformation of economics.

Another important feature of the transformation of economics into a mathematical science in the middle of the twentieth century that is displayed in the histories studied in this work is that such process can hardly be classified as a scientific revolution in a Kuhnian sense. If mathematical economics research was able to stabilize its place in elite journals in the 1950s (Backhouse 1998), still into the 1960s it was not more than a scientific niche within top departments of economics. Adopting a local approach to the history of economics builds a history where success and failure were at some point equally feasible. There was never any rolling out of a red carpet to mathematical economics. It needed to overcome various resistances to carve its space within the economics community. At Stanford, mathematical economists required collaboration with other disciplines to expand its group. At Berkeley, it was only in the 1960s that a group of mathematical economists could get grip of space in the economics department. Indeed, a full dominance of mathematical economics in the 1960s is not observable even in more mathematically-oriented departments such as the MIT (see Duarte 2014, 89). So a history of postwar dominance of mathematical economics must take

account of this smoother transition that can be seen when we trace the history of departments of economics in the period.

Furthermore, there was no "mathematization of economics program" put forward by the mathematical economists. They rarely engaged in defenses of mathematical over non-mathematical economics, often only if nudged into some debate (such as in the 1954 volume of the *Review of Economics and Statistics* cited in the introduction's epigraph). They welcomed cross-disciplinary projects if it seemed more promising than engaging with economists (such as the CASBS project at Stanford). Plurality of approaches to economics remained a feature of departments in the immediate postwar period, even when a mathematical economist came to hold office as head of department (Arrow at Stanford in the 1950s, Radner at Berkeley in the 1960s).

As explored in the introduction, the changing meaning of mathematical economics to the economics community illustrates how the subject was more a niche rather than a new orthodoxy for economics in the immediate postwar period. Then, mathematical economics was a blanket term for diverse applications of mathematics to economics, ranging from the axiomatization of general equilibrium theory to applied econometrics. When mathematical economics indeed became dominant within economics, it lost its former meaning (since the anti-group of non-mathematical economists became much smaller) to denote a particular way of applying mathematics to economics. During the 1980s (when the mathematical economics graduate program was created at IMPA) mathematical economics had such different meaning compared to what it meant in the 1950s.

This dissertation is just an example of how research in the history of economics departments can contribute to enhancing our historical understanding of the transformation of economics in twentieth century. The contribution I strived here was to locate the production and reproduction of mathematical economics by exploring the diverse social networks and connections constructed within academic spaces and that support the historical unraveling of scientific change. Nevertheless, there are many other historical questions that can be explored by looking at economics being done in a specific department. Some have been hinted in this work, such as the role played by university administration in the developing of departments, how clusters of new researchers can gather around some dissertation advisors, how graduate programs can be affected by associated research institutes, and how other departments can

influence the economics department of a specific university. Moreover, this dissertation has only focused on three spaces, still a grain of salt in an ocean of possible academic spaces to be studied. Therefore, this work also wants to encourage new research in the history of departments of economics.

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