Essays on the history of macroeconometric modeling and the evolution of economic analysis at the Federal Reserve

Essais sur l'histoire de la modélisation macroéconometrique et l'évolution de l'analyse économique à la Reserve Fédérale.

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- Goulven Rubin, Université Paris 1.
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Résumé

Cette thèse est composée de quatre articles qui analysent l'évolution de l'analyse économique au sein de la Reserve Fédérale et le développement des modèles macroéconométriques aux Etats Unis pendant les années cinquante et soixante. Le premier article, « Roosa et Samuelson sur l'efficacité de la politique monétaire », porte sur les différents types d'arguments utilisés par Robert Roosa (Banque de la Réserve Fédérale de New York) et Paul Samuelson (Massachussetts Institute of Technology) au sujet du rôle des banques dans l'efficacité de la politique monétaire au début des années cinquante. Roosa souligne l'importance de prendre en compte les caractéristiques du système financier des Etats-Unis et son évolution. Son argumentation se fond sur l'intuition acquise dans son activité quotidienne sur le marché monétaire à New York. Samuelson, quant à lui, transforme le débat et le réduit à la question de l'existence d'un équilibre avec rationnement sur le marché du crédit. Bien que Samuelson n'ait pas proposé de modèle mathématique, il a ainsi orienté le débat dans une direction plus proche du langage des économistes, reposant sur les concepts d'équilibre et comportement rationnel.

Dans le deuxième article, « La modélisation macroéconométrique et le comité sur la stabilité économique (CES) du SSRC, 1959-1963 », Erich Pinzón-Fuchs et moi-même discutons l'élaboration d'un modèle qui a jeté les bases de la macroéconométrie des années soixante. Nous analysons, à l'aide de l'étude du travail individuel des chercheurs impliqués et des retranscriptions de leurs réunions annuels, comment le modèle a été construit par un groupe d'une vingtaine de chercheurs. Nous signalons l'importance des liens que ce projet a institué entre les économistes, différentes agences gouvernementales, et des *think-tanks* comme la Brookings Institution.

Dans le troisième article, « Le comportement des banques dans les modèles macroéconométriques des années soixante », Goulven Rubin et moi-même étudions l'intégration du choix de portefeuille pour les banques et le traitement du rationnement du crédit dans ces modèles. Nous démontrons que le modèle de la Reserve Fédérale est plus transparent que les modèles précédents dans la

mesure où la structure du marché monétaire est plus claire. Un effort a été réalisé pour clarifier le rapport entre les équations estimées et les choix microéconomiques des banques. Par rapport au rationnement du crédit, nous soulignons l'effort des modélisateurs pour l'inclure dans les modèles malgré la difficulté à observer ce rationnement directement. Leurs efforts pour favoriser la mesure a permis d'inclure ce rationnement dans le modèle, mais avec des résultats limités en termes d'implication analytique du rationnement sur la politique monétaire.

Dans le quatrième article, « La transformation de l'analyse économique à la Reserve Fédérale pendant les années soixante », Béatrice Cherrier et moi-même utilisons les données biographiques des fonctionnaires de la Reserve Fédérale, des témoignages, et des archives, pour montrer comment la modélisation économétrique et les prévisions ont trouvé une place au sein de la Reserve Fédérale. Nous montrons, en particulier, que l'arrivée de ces méthodes a été la conséquence des pressions externes mais aussi de la volonté des fonctionnaires de la Reserve Fédérale pour explorer ces méthodes et leurs possibles usages pour guider la politique monétaire.

Abstract

This dissertation contains four papers that discuss the transformation of economic analysis at the Federal Reserve and the development of large-scale macroeconometric models during the 1950s and 1960s in the United States.

The first paper is titled "Roosa and Samuelson on the effectiveness of monetary policy." I discuss the different types of arguments used by Robert Roosa (Federal Reserve Bank of New York) and Paul Samuelson (MIT) in their discussion about the effectiveness of monetary policy in the early 1950s. Roosa emphasized the importance of lenders' willingness to lend and, in general, of taking into account the details of the evolution of the American financial system. He presented an argument based on the intuition acquired in his participation—as an official of the New York Federal Reserve— in the New York money market. Samuelson, for his part, transformed the debate by reducing it to a discussion about the existence of an equilibrium with rationing in the credit market. Although Samuelson did not provide a mathematical model, he did transform the debate into a discussion palatable for economists, based on concepts like equilibrium and rational behavior.

The second paper is titled "Macroeconometric modeling and the SSRC's Committee on Economic Stability, 1959-1963." Erich Pinzón-Fuchs and I discuss the construction of a macroeconometric model (1960-1963) that laid the bases for subsequent large-scale macroeconometric models of the 1960s. We discuss how, using an approach based on individual work together with two long annual conferences, the model was built by a team of more than 20 researchers. We also point out the important connections that the project helped establish between economists in academia, the government, and the Federal Reserve.

The third paper is titled "Bank behavior in large-scale macroeconometric models of the 1960s." Goulven Rubin and I discuss the implementation of a portfolio choice framework and the inclusion of credit rationing by banks in these models. We found that the Fed-MIT-Penn model has a more transparent structure: the structure of the money market is clearer, as is the relationship of its equations

with the microeconomic choices of banks. Regarding credit rationing, we found that modelers made important efforts to include it despite its non-observable nature and to develop a measure of it. Once a measure was found, and despite constant negative results, modelers kept trying to find a place for credit rationing in their model. These results invite a deeper reflection on the idea of microfoundations in large-scale macroeconometric models and on the role of beliefs in macroeconometric modeling.

The fourth paper is "The transformation of economic analysis at the Federal Reserve during the 1960s." Béatrice Cherrier and I use biographical data, reminiscences, and archival sources to show how econometric modeling and forecasting found a place at the Federal Reserve. We show, in particular, that the arrival of these methods was in part the consequence of external pressures, but also of the will of Fed officials interested in exploring the possible uses of these methods for monetary policymaking. There was no simple takeover by econometricians at the Federal Reserve but, instead, an equilibrium between judgmental and econometric forms of analysis emerged by the early 1970s.

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In France I was fortunate to find Goulven Rubin as an advisor for my masters thesis and later as co-advisor for my dissertation. He allowed me to find and pursue my own approach to the history of economics and has offered thoughtful and straightforward criticism throughout these years. His advice wasn't always easy to listen to, but I am convinced it has made me a better researcher. Etienne Farvaque, my second co-advisor, also provided very helpful advice. His interest in my research questions was always uplifting, providing important doses of validation that are not easy to find these days for historians of economics. My only regret is not seeking his advice with more regularity.

My 2016 and 2019 visits to Duke's Center for the History of Political Economy had a great impact on my understanding of the history of economics as a discipline and boosted my confidence in the relevance and the richness of our subject matter. I thank Bruce Caldwell, Roy Weintraub, Kevin Hoover, and Paul Dudenhefer for their advice and comments on my project. The Center's faculty, staff, and the other Research Fellows provided a wonderful intellectual atmosphere and I thank them for that. I'm particularly grateful to Erich Pinzón-Fuchs, with whom I had the chance to discuss at length my views on the history of economics during my 2016 visit, and whose research on Klein helped frame my understanding on the history of macroeconometric modeling. In addition, Erich and Emilie Lefevre, his partner in crime, made sure that my stay at the Center was as fun as it was intellectually productive. During my 2019 visit, as I finished writing my dissertation, I particularly benefited from discussions with Aurelien Goutsmedt, Chung-Tang Cheng, James Forder, and Christina Laskaridis. In addition, the writing group organized by Paul Dudenhefer provided a very helpful and therapeutic space, and I thank him and the other group members for participating in it.

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I. General introduction

The work presented in this dissertation is the outcome of an ongoing effort to understand how the worlds of central banking and academic macroeconomics in the United States interacted in the first two decades following the Second World War. Two major changes took place during this period: the Federal Reserve (the "Fed") regained its independence over monetary policy in 1951—marked most notably by 9 years of pegged rates—and a new style of doing economics became dominant, making formal mathematical modeling and a specific type of statistical analysis a defining characteristic of the discipline. These two developments were not independent, and the new breed of mathematical economists played a key role in the discussion about what the Fed should or should not do, criticizing not just particular policy decisions but the aptness of Fed officials to carry out the job. The Fed, until then led by bankers and lawyers, adapted gradually and by the end of the 1960s it had opened a space for new practices like macroeconometric modeling and forecasting. The papers included in this dissertation contribute to a characterization of this transformation in the role of economic analysis at the Board of Governors and the Federal Reserve Bank of New York, and to the history of macroeconometric modeling, a key practice that brought together Fed officials and academic macroeconomists.

Before going into the specific gaps that I have attempted to fill and the new questions that my research raises regarding the history of macroeconomics and central banking in the United States, a few comments about how I got to this point are in order, particularly regarding how I came to work on these issues and how I approached them. Interest in the history of applied economics and policy institutions, and a focus on practices rather than theories, is not completely new in the history of economics literature (e.g., Coats 1981) but it is certainly far from being what most historians of economics do: a handful of big names and an emphasis on theories still dominate the field even if new types of sources and approaches are being used to discuss them (Chassonnery-Zaïgouche, Herfeld, and Pinzón-Fuchs 2018). A brief recount of the path I have followed in my research over the past four years should thus help the reader understand the choice of topics dealt with in these four papers and the common threads that run through them.

My original project was different and more "traditional" in some ways. Before arriving in Paris for my masters in the history of economic thought, I had written a masters thesis about the 19th century plans to establish a national bank in Colombia (Acosta 2016) and had a general interest on 19th century history of central banking and monetary theories. In Paris I wrote a masters thesis on Ralph Hawtrey's first book, *Good and Bad Trade* (1913), where I explored Hawtrey's theory of the business cycle, focusing in particular on his treatment of commercial banks' behavior and his approach to "microfoundations" (Acosta 2014). When the time came to choose a subject for my dissertation my adviser suggested I work on Franco Modigliani. He is a major, yet still understudied figure in the history of macroeconomics who also played an important role in monetary policy debates and participated in the construction of a large-scale macroeconometric model for the Federal Reserve. Furthermore, he had a particular view of Keynes' message—one where money and monetary policy are very important (Modigliani 2003)—that distanced him from the preference for fiscal policy that is often associated with the Keynesian revolution. We were motivated by the contrast that Laidler (1999) had shown existed between the importance given by 19th century and early 20th century authors to the role of commercial banks in the business cycle—from Henry Thornton to Fisher and Schumpeter—and the initial work inspired by Keynes' *General Theory*, where the mechanics of the supply of money are collapsed into a single variable *M* (Hicks 1937; Modigliani 1944; Klein 1947; Patinkin 1956). Modigliani, who had worked to complexify the depiction of the money supply in his 1963 paper and in his work on the Federal Reserve Board-MIT-University of Pennsylvania (FMP) model, was an excellent vehicle to tell the story of how the commercial banking and its effects on the supply of money had been reintroduced into macroeconomics after the IS-LM model had been developed. It was a relatively traditional project in two senses. It was a project about a big name, and it was an idea-driven project. That is, the focus was on understanding how the inclusion of commercial banking and other complications changed the results of the previous models, and whether it had been done in a logically coherent way and accompanied by a consistent story.

Some parts of this original project still remain in my dissertation, in particular in the chapter about the differences between Robert V. Roosa's and Paul Samuelson's views on the effectiveness of monetary policy (chapter two), and in the chapter about the characterization of commercial banks' behavior in the large-scale macroeconometric models of the 1960s (chapter four). And I also still think that there are some analytical questions that should be explored, in particular regarding the development of the concept of credit rationing.[1] My interest in the goals of this original project, however, started dwindling early on when I discovered the works of Robert V. Roosa, an official at the Federal Reserve Bank of New York who became the face of what would come to be known as the "availability doctrine." Modigliani (1963) cited Roosa (1951a) as a key instigator of the discussion about the role of banks in the transmission of monetary policy, and since this was precisely what my project was about, it clearly made sense to look into it. Roosa's arguments about the importance of the Fed's actions in curtailing commercial banks' "willingness to lend" had been understood by economists as an argument about credit rationing, and further investigation revealed that Samuelson's (1952a) discussion of Roosa's argument had played an important role in the emergence of a distinct economics literature on credit rationing in the 1960s. I therefore decided to write a paper on Roosa and Samuelson as a prelude to the story about Modigliani. It was the first version of what is now chapter two of this dissertation, and the focus was on

[1] In addition, Modigliani (2003) did a rather poor job at telling his life story so a biography written by a more talented writer could perhaps do justice to what seems to have been an interesting life.

uncovering the logic behind Roosa's argument and Samuelson's criticism, which I argued led directly to the quest to characterize an equilibrium with credit rationing as the result of bank's maximizing behavior—a goal that Hodgman, a key initiator of this literature, had later explicitly stated (Hodgman 1960).

I finished the paper at the beginning of my second year. At the time I was a visiting fellow at Duke's Center for the History of Political Economy, where I had come to work on Samuelson's and Modigliani's archives. This was a major learning experience. First, the months I spent going trough Samuelson's and Modigliani's archives made me realize that they were, simply put, human beings: strategic and motivated but also capable of making mistakes, part of an intellectual community, and living in a specific time and place. Furthermore, seeing the figures drawn on graphing paper, the stacks of printed data series and regression results, and the purple ink of mimeographed working papers that academics used to physically lend each other (!), made it clear that I should be mindful of the time that separated me from the people I was studying. Second, my interaction with the Center's faculty and the other Fellows showed me that there was a much larger catalog of legitimate topics in the history of economics than I had previously considered and motivated me to explore further ideas and questions that had been accumulating as my research progressed but that I had deemed less important or accessory.

It was in this context that I got interested in the Social Science Research Council (SSRC) and its Committee on Economic Stability. There were many references to this Committee and its Subcommittee on Monetary Research in Modigliani's archives and in the published literature, and several short articles in ITEMS (the SSRC's magazine) summarized various projects of the Committee. It was clear that they had played a major role in the construction of macroeconometric models during the 1960s, but they had not been studied by historians of economics, so I located the records of the SSRC and ordered duplicates of several folders after returning to France in January of 2017. In the following couple of months I worked on a preliminary version of what is now chapter four. This paper, coauthored with

Goulven Rubin, was prepared for a conference on the history of macroeconometric modeling that took place in Utrecht on April of 2017,[2] and, in line with my original dissertation project, this version of the paper was heavily focused on Modigliani. Although we mentioned the existence of the Committee on Economic Stability and the Subcommittee on Monetary Research, the paper was mostly an analysis of Modigliani's work, where we tried to understand the role of banks in his IS-LM-type models of 1944 and 1963, and his work on credit rationing and the supply of money in the second half of the 1960s. It was a preliminary paper and it showed practically no awareness of the collaborative nature of macroeconometric modeling, the material circumstances in which this activity was carried out, or the evolution in the modeling choices by macroeconometric model-builders.

A first batch of duplicates from the SSRC's records arrived in May of 2017 and I spent the summer going over the material, where I found evidence of the fascinatingly complex process that was involved in the construction of a large-scale macroeconometric model. Going through the Committee's records forced me to try to broaden the scope of chapter four, which grew to include the work of people other than Modigliani and presents some of the modeling choices made by model-builders regarding the characterization of commercial banks' behavior. But the wealth of information found in the Committee's records also convinced me to tell its story. In a sense, it was a deviation from my original project, but at the same time I considered that it had to be done because the work that I was doing on Modigliani and the FMP model had been born out of this Committee. I joined forces on this project with Erich Pinzón-Fuchs, who had just finished his dissertation on Klein's pre-1960s work (2017), and wrote a history of the early years (1959-1963) of the Committee that is included in this dissertation as chapter three. We originally presented the paper at the workshop on Economics and Public Reason organized at the University of Lausanne in May of 2018 with mixed results.[3] It was not a perfect

[2] See the program: https://www.uu.nl/en/events/conference-history-of-macro-econometric-modeling.

[3] See the program: https://agenda.unil.ch/display?id=1524813499294

fit for the envisioned special issue on the subject of the conference and we had to reshape it for publication (Acosta and Pinzón-Fuchs 2019), but the workshop was a major motivation to write the paper, and the discussions at Lausanne also motivated me to explore further the connection between policymaking, the economics discipline, and the work on macroeconometric modeling at the Fed.

The referee reports of the Roosa-Samuelson paper came back at the end of May of 2017, suggesting mainly that I reframe my paper. One of the referees remarked that my main argument about the prehistory of the credit rationing literature was not as interesting as I had originally thought, and was a weak basis for the paper. This referee considered that one of the secondary points I had made, about the differences in the type of arguments put forth by Roosa and Samuelson, was much more interesting, and what I include here as chapter two is the latest revision I have made of the paper following this suggestion. The importance that Roosa and his colleagues at the New York Fed gave to the "tone and feel" of the market when thinking about monetary policy stands in contrast with the emerging breed of mathematical economists, exemplified by Samuelson, that had a hard time dealing with such vague concepts as the "availability of credit" and demanded tractable explanations in terms of rational individual behavior. This tension between different styles of economic analysis is at the core of chapter two, but it was not limited to the early 1950s and, in fact, intensified during the 1960s. Chapter five, coauthored with Béatrice Cherrier, explores how new practices like macroeconometric modeling and forecasting found a place at the Board of Governors in a context of outside pressures by the Congress and economists, as well as internal developments at the Division of Research and Statistics. This chapter allowed me to tie together all of my research interests and was informed by the work presented in the other three chapters.

Summing up, I started with the idea to write about Modigliani and commercial banks after the IS-LM model but ended up focusing on the Federal Reserve and the history of macroeconometric modeling due to the evolution of my historiographical convictions and interests, and also due to the availability of rich sources.[4] Let me now comment on how I approached these topics. While chapters two and five make use of preliminary prosopographies of macroeconometric modeling and the Federal Reserve, the vast majority of my dissertation relies on the more traditional close reading of various types of documents, including correspondence and unpublished memos besides the usual published sources.[5] It is rather my focus on practices that I want to highlight, and it is this focus that determined the type of questions I ask in every chapter—even if it appears perhaps stronger in some than in others. Calls for a "practice-based" approach in the history of economics, sometimes under the label of "historical epistemology," have been made in recent years (Maas, Mata, and Davis 2011; Maas 2014b; Stapleford 2017) and I am sympathetic to them for two main reasons: the core idea of studying what economists *actually did* and understanding their work according to what passed as good practices for their contemporaries reveals a series of important connections between the political, social, economic, and material circumstances in which scientific work is done, which allow us to build broader historical arguments and furthermore acts as a useful check on the historian's explicit and implicit biases and assumptions.[6]

Let me unpack this further. Thomas Stapleford (2017, 116) provides a useful characterization of practices: they "are comprised of three components: (1) collections of actions that are (2) linked by teleology (they can be understood as elements in a goal-oriented process) and (3) are subject to normative evaluation (they, or the overall process of which they are a part, can be done poorly or well)."

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[4] Besides my stay 2016 stay at Duke, the workshops organized by the YSI history of economics working group in Antwerp in 2017 and Madrid in 2018 contributed considerably to my historiographical culture.

[5] See Svorenčík (2018) for a discussion of the potential for prosopography in the history of economics.

[6] This is, to a certain degree, a continuation of the movement by some inside the history of economics community to adopt the methods of and engage with scholars in the history of science, technology, and medicine community. Historical epistemology has a much longer history in the history of science literature (see Feest and Sturm 2011) and practice-based approaches have been widely discussed among sociologists and in organization studies (Nicolini 2012).

Thus studying a practice such as macroeconometric modeling implies understanding what was needed to build these models, what purpose they served, and what the model builders' guiding criteria were. How is this different from doing a history of ideas or theories? In my opinion, the key difference is that focusing on practices necessarily ties economists' work to a specific historical and material context, and removes the agency sometimes given—whether consciously or unconsciously—to theories, ideas, or thoughts. A practice-based approach, however, is not limited to studying applied work like macroeconometric modeling, for doing economic theory is also a practice as defined above and should thus be studied and understood in its appropriate context. But, at the same time, a practice-based approach is better suited for studying applied work if only because it opens up the type of questions that can be asked. For many historians of economics (e.g., Lapidus 1996; Kurz 2006; Marcuzzo 2008) our field's worth is still very much seen as determined by our potential contribution to contemporary economics, a contribution that is based on the idea that we historians have the keys to a treasure trove of forgotten ideas. This may or may not be true, but it certainly puts a premium on the history of economic theory for it's unclear how the history of applied (including policy) work would fit in this framework other than by building up the case for or against a particular theory. A focus on practices frees us from this hierarchy of research topics.[7]

[7] The lingering question is whether this helps the history of economics' stance as a field of economics. There's some evidence (Duarte and Giraud 2016) that historians of economics were not being successful in their marketing campaign anyway, so a new strategy can't hurt. At the same time, I think the argument can be made that the self awareness that comes from reading a practice-based history of economics could be better appreciated by contemporary economist, in particular if it discusses issues they deal with in their everyday work: from cleaning and using data to participating in policy discussions and navigating departmental life. I'm thus somewhat more sympathetic to Trautwein's (2017) idea of the potential role of historians of economics as helpful generalists in economics departments. Anecdotal evidence from observing Beatrice Cherrier's activity on Twitter has shown me that many economists are in fact interested in the history and implications of their methods and practices, and they find the work of historians of recent economics interesting for it shows them to what extent things are different and why that matters. At the same time, whether this has any effect on their work or is merely a curiosity still remains to be seen.

At the same time, if one is to answer a question about the importance of the material infrastructure needed to build a macroeconometric model or about the uses of these models in the monetary policymaking process, it is absolutely essential to understand in detail how economists and government officials did what they did. This is not always easy or possible, and I will elaborate below on the limitations I experienced in my dissertation regarding this issue, but it is important to be mindful of the distance that separates us from our objects of study. My training as an economist was certainly helpful for understanding the technical literature, but much like the apparent similarities between Spanish and French words that have caused me several embarrassments in these past years, many *faux amis* await the trained economist that ventures into the history of recent economics. The use of models, econometrics, computers, etc., that became more and more common after the Second World War and that are the bread and butter of economists today may give the impression to the modern reader that doing economics in the 1950s and 1960s was not so different from today. The differences in the econometric and other mathematical techniques, as well as the conventions and references of papers might signal the time period in which a paper was written, but it is still easy to miss the fact that economists had a different relationship with computers at the time, that not all projections were made with econometric models, or that the availability of data and the relationship of modelers to them was different. Furthermore, if we add to this the amount of history we think we already know, slowly absorbed through footnotes and brief passages in textbooks, the modern economist can easily think what he knows more than what's actually the case, and overlook *meaningful* details about past practices. And I emphasize the *meaning* of these details and practices because, as Nicolini (2012, 9) also argues, I am not advocating simply for the cataloging of ancient practices for their value as historical curiosities. Whatever fascination they might produce, the effort to understand practices is important because it allows us to connect the history of economics to a broader history (social, political, technological, etc.). The fact that regressions used to take hours and were done not by economists themselves but by technicians is important not simply as a curiosity but because the time factor might explain the reflection behind the specification of equations, and if we take into account the fact that these computer lab assistants and programmers were often women, there is a direct connection to a story about the gender division of work. Similarly, the fact that special software had to be developed to simulate the large-scale models of the 1960s and that large databases had to be built is not simply a historical curiosity that shows us how far we've come, it is important, for example, because it determined who could afford to build this type of models and the stakes that were at play when a forecast at the Board of Governors was considerably off the mark.

My understanding of the importance of practices evolved gradually, and the papers included in this dissertation are therefore heterogeneous in terms of the importance given to them in each one of the papers. But the importance of practices is still there, and it is central to the contributions I make to the literature about the Federal Reserve and to the literature about the history of macroeconomics, to which I now turn.

1. The Federal Reserve and its relationship with economists and economics

William McChesney Martin, Jr. is the longest serving chairman in the history of the Federal Reserve System (1951-1970). His tenure brought important changes in the practices of staff economists, a development that coincided with a renewed interest in monetary theory and policy on the part of academic economists, as well as with a change in the style of doing economics that swept the discipline after the Second World War. The papers in this dissertation, and in particular chapters two and five, look at the intersection of these phenomena by studying the evolution of economic analysis at the Fed and its connection with academic economists, a theme that hasn't been sufficiently explored by historians of the Federal Reserve or by historians of economics.

Our period of interest saw the continued development of the working and institutional independence of the Federal Reserve, which by the end of the 1940s was still a young institution. It had been created in 1913 but its institutional structure had been profoundly modified in the 1930s with the creation of two of its current distinguishing features: the Federal Open Market Committee (FOMC) in 1933 and the Board of Governors in 1935. And, in 1942, it had voluntarily surrendered its discretion over monetary policy to the Treasury by pegging interest rates as a way to facilitate the government's debt management during the war (Farvaque, Parent, and Stanek 2018). The Fed had not yet become the independent and immensely powerful institution we now know, although things started moving decidedly in that direction with the Treasury-Fed accord of March 4, 1951 and the appointment of Martin as chairman shortly afterwards. The accord, engineered in part by Martin himself, who was then at the Treasury, gave discretion over monetary policy back to the Fed.[8] And once he was appointed chairman, Martin guided the Fed through a path of independence not just from Washington politics but also from the finance world and the New York Federal Reserve, whose president had traditionally had an overwhelming influence over monetary policy. As Conti-Brown (2016, 42–51) points out, Martin is responsible for shaping key aspects of the image of the modern Federal Reserve regarding, for example, what is considered conventional monetary policy—open-market operations focused on the short-term segment of the government securities market—as well as the rationale behind monetary policy itself, which Martin illustrated with metaphors still in use today like leaning against the wind of inflation or taking the punch-bowl away at the party. Not surprisingly, many commentators have discussed the history of the Fed during these two important decades, and they also consistently agree on the change towards a more modern and technocratic institution that took place under Martin's tenure, pointing in particular to the arrival of highly trained economists and the development of forecasts and econometric models (e.g., Stockwell 1989, 21;

[8] Not suprisingly president Truman considered Martin a traitor for not playing the role he had envisioned by sending a Treasury man to the Fed (Bremner 2004, 91). See Hetzel and Leach (2001) and Meltzer (2003, 699–716) for the history of the Fed-Treasury accord.

Schnidman and MacMillan 2016, ch. 3; Maisel 1973; Meltzer 2009, 498). This literature, however, often offers only brief comments and recollections that are accessory to other larger arguments. Most notably, Allan Meltzer's history of the Fed, which devotes a full volume to the 1951-1969 period, has very interesting and useful comments on the transformation of the Fed's policymaking processes and personnel, but his focus is on the policy outcomes. Comments on the changes in the type of economic analysis, the development of macroeconometric models, and the inclusion of forecasts in the policymaking process are short, sparse, and mainly brought in as accessories to a story about policy and based mostly on minutes of FOMC discussions. In a similar vein, when discussing this period monetary historians like Hetzel (2008) have focused on the policy record of the Fed and the evolution of its policymaking processes, while Wood (2009, 2014) and Binder and Spindel (2017) have dealt with the institutional structure of the Fed and its independence as well. Conti-Brown (2016), who has also discussed the independence of the Fed and the evolution of its internal governance, has highlighted the importance of staff economists and lawyers (ch. 4) but his comments focus on a later period of the Fed's history. Mehrling (2010b), for his part, has also made brief comments on the connections between academic economists and the Fed in his study of the evolution of the latter's function as dealer of last resort, but he does not go into the details of the production and uses of economic analysis at the Fed. Finally, Bremner's (2004) fascinating biography of Martin provides a detailed and well-documented account of Martin's years at the Fed and brings up the importance of staff members like Winfield Riefler, but his account does not go into much detail regarding the evolution of economic analysis at the Board or the work of the research staff.

This literature thus offers useful information and analysis that helps navigate the history of the Fed during Martin's tenure, but there's an important gap regarding the practices of staff economists. This is an important question about the institutional history of the Fed, but it is also intimately tied to the history of the economics discipline. Economists have risen to the top positions at the Board of

Governors and the Reserve Banks, and as Claveau and Dion (2018, 355) have pointed out, the Fed currently employs nearly 50% of the registered AEA economists in the field of money and banking. Furthermore, the research of economists at the Fed and other central banks is cited more than the research of economists outside central banks (Claveau and Dion 2018, 359). How can we explain the growing proximity between the Fed and the economics discipline? The change from an institution run mostly by lawyers and bankers to an institution led mainly by economists started during Martin's tenure, and understanding the internal dynamics of knowledge production at the Fed is crucial, but we must also look outside the Fed to fully understand what happened. Two issues are particularly notable.

The first is that Martin's tenure coincided with a renewed interest in monetary policy (and theory) on the part of economists. This change was registered early on by participants in the contemporary discussions—like Roosa (1951b), who spoke of a "revival" of monetary policy, and Ellis (1951), who referred to the "rediscovery" of money—and in the 1970s Milton Friedman (1975, 176) and Modigliani (1975b, 179) agreed on "the dramatic revaluation of the importance of money which ha[d] occurred in the economic profession since the Second World War." It is hardly surprising that such a renewed interest would come about after the war, mainly since the discussion around the mounting inflation and the end of the peg on interest rates brought economists, government officials, bankers, and Fed officials into a heated public debate (e.g., Congress 1949). However, the debate did not die out after the 1951 accord, and economists became key participants in it, judging the Fed's actions and suggesting changes to its structure and policymaking processes that were based on the latest developments in academic economics.

Now, academic economists have been discussing and criticizing the Fed since it's establishment, but besides the increase in the intensity of this discussion during the postwar, the second key issue to note is that the way in which economists argued—the language and tools used—changed dramatically: the interwar pluralism of methods and scientific standards in American economics gave way to the primacy of neoclassical economics during the postwar (Morgan and Rutherford 1998; Fourcade 2009, ch. 2). A new breed of economists that argued in terms of mathematical models and a particular form of statistics gradually filled academic departments and government agencies. The methods of these mathematical economists had proven very useful during the war and important connections built with military agencies remained strong afterwards, furthering the development of fields like operations research and game theory (Mirowski 2001). But the war also brought mathematical economists into the macroeconomic sphere with projects like Kuznet's National Accounts (Ozgode 2019), and later as expert advisers with the creation of the Council of Economic Advisers (Bernstein 2001).

But what about the Fed? As Bernestein (2001, 40fn2) and Fourcade (2009, 100) duly note, there had always been economists at the Fed, but what exactly did they do? And, how has this changed? Besides Yohe's (1982; 1990) work on the 1920s, the secondary literature in the history of economics doesn't say much about the Fed, although it does offer some useful comments on the arrival of mathematical economists to government in general during the postwar. A key element of the transformation of American economics, pointed out by Morgan and Rutherford (1998, 9), is that there was a change in the conception of objectivity in economics, which came to be associated with the apparently value-free methods used by mathematical economists. They further claim (15), following Goodwin (1998), that the political climate brought by McCarthism was such that economists took refuge in these new methods to avoid scrutiny and potential persecution. This idea has been categorically rejected, for the academic world at least, by Weintraub (2017), but it is still worth asking whether the changing attitudes regarding what passed as a good, i.e. scientific, basis for policy played any role in the Fed's behavior and hiring practices, and to what extent political or ideological concerns reinforced this. After all, the United States has had a long history of mistrust towards central banking, and long-time critics like congressman Wright Patman (1893-1976) fought a constant battle to reduce the independence of the Fed.[9]

Another argument, made by Goodwin (1998, 63) as well, is that the postwar brought economists into a client-type relationship with policymakers. For Goodwin, the Council of Economics Advisers exemplifies this relationship, doing whatever the president told them to do, but he also explicitly mentions the Fed as another example of an institution where this type of relationship flourished. This is an interesting argument that forces us to zoom in into the Fed's structure, carefully differentiating not just the Board of Governors and the Reserve Banks, but the research divisions and other staff members as well. And while one may see how at least part of the Fed's staff might have been put in such a position, the issue gets complicated when we think about the fact that economists became not just advisers to the prince but princes themselves as more of them were appointed as Board governors, chairmen, and Reserve Bank presidents. Furthermore, the reach of the staff should be carefully assessed. As Conti-Brown (2016, 86) has illustrated with the case of Edwin Thomas, the Board's former director of the Division of International Finance (1977-1998), his role in conjuring several foreign currency crises during the 1990s went much farther than the purely technical one.

The existing literature—on the history of the Fed and on the history of postwar economics—has thus made very limited progress towards understanding the work of economists at the Fed during Martin's tenure, who they were, and how the Fed interacted with academic economists. Studying these questions offers a new window into the history of the Fed, which cannot be understood without taking into account the transformation of American economics during the postwar, and that at the same time illuminates this transformation. This dissertation takes us a few steps forward in this direction in at least two ways. First, I characterize episodes of tension and collaboration that occurred between Fed officials, most of them trained

[9] On the other political corner, Ron and Rand Paul are contemporary examples of the "end the Fed" discourse.

economists as well, and academic economists. As I mentioned above, chapter two discusses the tension between Roosa's and Samuelson's views regarding the effectiveness of monetary policy and the role of lenders, which is an example of a more general tension between Fed officials and academics that one can also see during the 1960s. It's a tension based on different conceptions of what a good and useful argument is: while Roosa downplayed the usefulness of abstract analysis that didn't take into account the detailed institutional structure and actual functioning of the money markets, Samuelson struggled to understand such a position, demanding instead an explanation in terms of rational individuals and equilibrium positions. Similarly, as Cherrier and I show in chapter five, during the 1960s economists like James Tobin, Karl Brunner, and Allan Meltzer strongly criticized the lack of scientificity in the Fed's decision-making process, noting in particular the reliance on vague terms and the lack of empirical corroboration as the basis of its policy discussions.

At the same time, however, there were episodes of collaboration between academics and Fed officials during Martin's tenure, and a key example was the construction of a macroeconometric model for the Board of Governors of the Federal Reserve. In chapter three, Pinzón-Fuchs and I offer a preliminary history of the activities of the Social Science Research Council's Committee on Economic Stability, which was behind some of the milestones in macroeconometric modeling of the 1960s. A key issue that we bring up is the importance of the inclusion of government officials, which brought with them expert knowledge of specific sectors as well as easier access to new or not readily-available data series. For the Board, in particular, Frank de Leeuw, an economist at the Division of Research and Statistics, wrote the model of the financial sector of the Committee's macroeconometric model (De Leeuw 1965a) and then codirected the construction of the FMP model together with Albert Ando (U. Pennsylvania) and Franco Modigliani (MIT). The work involved in the construction of the FMP model is carefully discussed by Backhouse and Cherrier (2019) in a forthcoming paper. They note in particular the different priorities of Fed officials and academics, the former more interested in having a working version of the model than the latter, who instead tended to prefer perfecting the specification of equations and its consistency with economic theory. In chapter four, forthcoming in the same issue of *History of Political Economy* that includes Backhouse and Cherrier (2019), Rubin and I complement their story by focusing on the artifacts themselves. We look at the way in which model builders associated with the Committee of Economic Stability—which include the group led by Ando, Modigliani, and de Leeuw—characterized the behavior of banks in the various large-scale macroeconometric models built during the 1960s. Our analysis shows how modeling practices, in particular regarding the recursivity of models, the derivation of behavioral equations, and the role of beliefs evolved during the decade. In particular, it allows us to contrast De Leeuw's early work with that of the FMP model, influenced heavily by Modigliani's conception of the monetary transmission process.

The second and closely related contribution is precisely about the evolution of the practices of economists at the Fed. In chapter five Cherrier and I discuss how macroeconometric modeling and forecasting found a space at the Board of Governor's during the 1960s. We argue that the change happened in a context characterized by outside pressure and inside developments at the Board. In particular, the challenge by economists like Tobin and Meltzer, mentioned above, was embedded in the political pressure exercised by Lyndon B. Johnson's administration and congressman Patman's crusade against the Fed's independence. At the Board, this pressure led to efforts to establish closer ties with academic economists at the same time that economists at the Division of Research and Statistics, including its director, got involved in macroeconometric modeling projects and in the production of forecasts for the FOMC meetings. A key aspect of this contribution has to do with our sources, a critical element in the success in studying the evolution of the practices of economists at the Fed, be that at the Board or any of the Reserve Banks. Meltzer's history of the Fed was a useful guide to the material available both at the National Archives and at the Board's archives, and his sources have now been digitized and made available online by FRASER[10]. At the same time, given Meltzer's approach and objectives, his sources were only partially useful for studying the practices of the research staff. Furthermore, to this day it is unclear if the type of material we would like to have, like correspondence and memoranda of the Board's Division of Research and Statistics, is in fact available. Although all of the other primary and secondary sources suggest that a large paper trace of the activities of the Division of Research and Statistics should exist, we haven't been able to locate much of it. While some documents from this period and afterwards are available, most notably the FOMC minutes and the Federal Reserve Bulletin, the Board hasn't made any new addition to the Central Subject File at the US National Archives, which only contains information from 1913 to 1954.[11] Furthermore, the various Freedom of Information Act requests we made to the Board came up empty. Thus, we can only conclude that either the material we are looking for simply doesn't exist because it was not kept, or even if it exists, it hasn't been properly classified and thus remains hidden.

Therefore, the challenge involved in writing about the postwar Fed is to either find new sources or find uses for existing but perhaps underutilized ones. This is precisely what we did in chapter five. First, Robert Hetzel's collection of interviews, available on FRASER, is an unparalleled resource for understanding the inner working of the Board and the Reserve Banks. Hetzel's interviews are, like his book, focused on policy, but he did nonetheless question interviewees about day-to-day work and polemic episodes that also reveal some of the inner workings of the Fed. Second, some of the regular publications and minutes of the Board and Reserve

[10] FRASER is an initiative of the Federal Reserve Bank of Saint Louis. They describe themselves as "a digital library of U.S. economic, financial, and banking history—particularly the history of the Federal Reserve System" (See https://fraser.stlouisfed.org/about/). Their initial work consisted on digitizing the sources cited in Allan Meltzer's history of the Federal Reserve, but they have continued to add valuable material, making it a fascinating and still mostly unexplored resource for historians of economics.

[11] For more details on the contents of the records of the Federal Reserve System at the National Archives, see Richardson (2006).

Banks provide important biographical information on the staff, and the preliminary prosopography of the Board's Division of Research and Statistics was built using the Federal Reserve Bulletin. This is a promising source for it shows the variety of paths that led to the Board, and should allow for a more consistent analysis of the variety in terms of graduate schools and previous affiliations once more data is gathered. Third, other previously unexplored archives were used to fill in the gaps about the macroeconometric model project at the Fed. The archives of Franco Modigliani and of the Social Science Research Council were crucial to understand how the project developed and the role that economists at the Board's Division of Research and Statistics played in this and earlier modeling projects. Based on this sources and on the preliminary prosopography of the Division, we argue that contrary to what is often suggested in the secondary literature about this period, the arrival of PhD economists to the Fed is not a good indicator for the timing of the transformation of the practices at the Board. In fact, for the macroeconometric model project, the arrival of new, young economists with PhDs is a symptom rather than a cause.

Finally, even if still preliminary, the picture we have of the Board's Division of Research and Statistics invites a careful study of the type of economists that existed at the time. As Morgan and Rutherford (1998) stressed, there was considerable variability among the economists that are usually associated with the American Institutionalist movement, but our research shows that careful attention should also be given to a third category of economists who did not necessarily fit under the neoclassical or institutionalist label. These economists, which we could refer to as "business economists,"[12] were applied economists who did not necessarily have a PhD, nor held the same epistemological standards as econometricians or NBER-type institutionalist, but did nonetheless play a major role at the Fed because of their intimate knowledge of specific sectors of the economy. Furthermore, some of the staff would perhaps be better characterized with the label of "central banker" than

[12] This label was used by James Pierce, an economist at the Board's Division of Research and Statistics during the late 1960s and early 1970s.

of economist. Roosa, for example, who had a PhD ('42) and had previously worked with the NBER before joining the New York Federal Reserve, is an interesting boundary figure who doesn't seem to have had a particularly strong opinion of quantification but also didn't care much about theoretical niceties. He became an important speaker for the New York Federal Reserve, in particular in academic settings, and discussed more than just the purely technical work he oversaw. The importance of carefully looking at what these people, that we have called economists for lack of a better word, is to make explicit our implicit assumptions about what counts as an economist in a specific period of time and for whom. This should help us make sense, for example, of the fact that Arthur Burns, the first PhD economist to chair the Board of Governors, was seen as a most competent economist by Gardner Ackley of the Council of Economic Advisers, but not by James Pierce, of the Board's Division of Research and Statistics (Pierce 1996a, 11).

2. Macroeconometric modeling and the history of macroeconomics

As I have mentioned a couple times above, most of the history of economics deals with theoretical discussions and the big names associated with them. Applied work, be that at government agencies or inside academia, has received considerably less attention. This is perhaps particularly visible in the historiography of macroeconomics, were a small number of big names (e.g., Keynes, Friedman, Lucas) dominate both the historical literature and the bits and pieces of lore that students pick up during their training from textbooks and article introductions. Macroeconometric models in particular have received little attention from historians of econometrics and macroeconomics. This dissertation contributes to reducing this gap in the literature by studying the development of large-scale macroeconometric models during the 1960s.

Historians of econometrics have mostly focused on the pre-1960s period and on the development of estimation techniques (M. S. Morgan 1990; Quin 1993, 2013; Epstein 1987), and there isn't much connection to the history of macroeconomics.[13] The Brookings model (1963-1972), born out of the SSRC's Committee on Economic Stability model project (1960-1963) and a milestone in the history of macroeconometric modeling, is only mentioned in passing by Epstein (1987), who highlighted its size and the importance of the decrease in the cost of computation in making such projects possible (128). Epstein's history of econometrics is nonetheless an important reference. It is based on two guiding concerns, the policy uses of these models and the development and uses of estimation techniques, both of which I consider useful for interrogating the subsequent period. Also, Epstein's warning against oversimplifying the dispute between the Cowles Commission and the National Bureau of Economic Research (88), and his remarks on Klein's pragmatic approach to the specification of equations (115) are relevant as well. Qin (2013, ch. 1) for her part sees the 1960s as the period of consolidation of the simultaneous-equation modeling program of the Cowles Commission—a statement with which I agree—but only briefly mentions the Brookings model (20). Her previous book (Qin 1993) covered the pre-1970s history of econometrics, but it is centered on the development of estimation techniques and she doesn’t discuss the Brookings or the FMP models.

Regarding the history of macroeconomics, the gap in the literature is somewhat different, for the problems of macroeconometric models figure prominently as a cause of the transformation of academic macroeconomics during the 1970s but the models themselves are not given much attention. This can be illustrated by looking at Snowdon and Vane (2005) and De Vroey (2016a), two attempts at telling the history of macroeconomics from Keynes to the recent literature. Their histories follow closely the traditional succession of events: the emergence of the field with

[13] For useful surveys of the work done by historians of econometrics see M. Boumans and Dupont-Kieffer (2011) and M. J. Boumans (2019).

Keynes, the IS-LM model, monetarists, New Classical economics, Real Business Cycles models, the New Neoclassical Synthesis, and then some comment on the most recent work.[14] Besides De Vroey's use of the label "DSGE macroeconomics" for the whole post-Lucas period, there aren't any major surprises, and the reader will find detailed discussions of the old literature that according to the authors is the distant predecessor of the way in which macroeconomists deal with questions such as unemployment or inflation today.

These histories mostly ignore the development of macroeconometric modeling. Instead, the main character of their discussion about the early history of macroeconomics is the IS-LM model. Snowdon and Vane's chapter on the "orthodox Keynesian school" only mentions Lawrence Klein's claim in his *Keynesian revolution* that the LM curve had a "solid empirical basis" (108), and it mentions in passing the existence of the FMP model (113). At the same time, their chapter on the "orthodox monetarist school" mentions the distrust some economists felt towards large-scale macroeconometric models (195), and their chapter on the "New Classical school" begins with the following quotation from Lucas and Sargent:

> [E]xisting Keynesian macroeconometric models are incapable of providing reliable guidance in formulating monetary, fiscal and other types of policy. This conclusion is based in part on the spectacular recent failure of these models, and in part on their lack of a sound theoretical or econometric basis ... on the latter ground, there is no hope that minor or even major modification of these models will lead to significant improvement in their reliability. (Lucas and Sargent quoted in Snowdon and Vane 2005, 219)

The subsection on the Lucas critique further shows that it was an attack on "the established practice of using large-scale macroeconometric models to evaluate the consequences of alternative policy scenarios" (265), and other passages in the same

[14] Unlike De Vroey (2016a) Snowdon and Vane (2005) also cover the discussion of issues besides those typically associated with the macroeconomics (the business cycle, inflation, and unemployment) and devote chapters to growth and political economy.

subsection make it clear that at least part of Lucas and the New Classical macroeconomics' attacks were specifically aimed at macroeconometric models and not the IS-LM model. Snowdon and Vane give no precise reasons as to why they neglected to discuss macroeconometric modeling, and the reader is left only with their judgment in choosing the topics to include in the book following "what we consider to have been the major issues that emerged following the birth of macroeconomics in the 1930s" (Snowdon and Vane 2005, xv).

De Vroey (2016a) presents the same inconsistency, although he does give some more room to macroeconometric models. They are mentioned several times and this type of work is presented as a key step in the history of macroeconomics, but it is never discussed in detail, and the core of his chapter on "Keynesian macroeconomics" is the IS-LM model. Although he does allocate a subsection to discuss the Klein-Goldberger model (1955), the core of his discussion is the model's results regarding the wage-adjustment equation. De Vroey (2016a, 37) makes only passing comments about what a macroeconometric model actually is and how it is built, and these claims are not recovered in later discussions to compare them to subsequent modelers. Thus, interesting claims about a "pragmatic" approach to modeling, or the relationship between data, estimation, and theory (38) are left adrift. Similarly, De Vroey's (2016a, 41) claim that the advances in the computer industry facilitated the development of large-scale macroeconometric models during the 1960s and onwards is stated without any sources or further comment, despite the fact that this same element will be presented as an important aspect in the development of New Classical macroeconomics (165).

On almost the opposite side to these two books we can find Bodkin, Klein, and Marwah (1991), a book-length history of macroeconometric modeling in the US and other countries written by participants, including Lawrence R. Klein. Lacking other sources, this is a very useful survey of macroeconometric models, but it is limited in its scope as a history of macroeconomics. The description of the Brookings and FMP models (ch. 4), however, was a very important resource at the start of my research, despite its focus on the models' properties and on specific sectors. Some attention is given to the challenges involved in the consistent estimation of these models as well as the management of data and the simulation of the models, and some of the uses of these models are also pointed out.

In terms of the contribution to the history of macroeconomics, this dissertation moves forward in the direction of incorporating macroeconometric modeling into the mainly theory-focused history that we have so far.[15] And in this I follow recent work on Klein that has pointed out the nuances of the history of macroeconometric modeling: Pinzón-Fuchs (2017) has explored Klein's openness regarding the relationship between theory, data, and the specification of models, and Hoover (2012) has shown his concern for the relationship between individual and aggregate behavior, thus defying the myth that Lucas and the New Classical economists were the first to care about microfoundations. Chapters two and three of this dissertation discuss the construction of the macroeconometric model of the Committee on Economic Stability (1961-1963) and the characterization of bank behavior in several large-scale models of the 1960s.

The main contribution of chapter two is that it highlights the challenges involved in the construction of a large-scale macroeconometric model, and how this practice differs from the IS-LM model that dominates the narrative of the 1940s-1960s history of macroeconomics. In this chapter Pinzón-Fuchs and I discuss the establishment of the Committee on Economic Stability at the end of 1959 and the construction of their macroeconometric model (1961-1963), led by James Duesenberry and Klein, which was their main project during the first four years of the Committee. We show that despite some hesitation during the conference that led

[15] Some would simply call this theory-focused type of history an "internalist" history, including De Vroey: "My study focuses on what I view as the most salient episodes in the history of macroeconomics. I do not claim that it is exhaustive. I have chosen to give more emphasis to theoretical aspects than to empirical ones. My work is internal history and leaves aside most of the contextual dimension" (De Vroey 2016a, xvi; see also De Vroey (2016b), 150). If "context" includes anything that's not exclusively theory then I guess that puts my research on the "externalist" side. However, I find these labels difficult to understand in practice. I have a hard time understanding how one can cleanly separate "theory" and "context," and how the primacy of either one can possibly be established without looking at the other.

to the creation of the Committee, the model project was from the start committed to producing a tool that would be useful for economic policy analysis. In practice, this meant that the model would be as disaggregated as possible so that actual policy instruments could be included as parameters. This effort, however, meant that the size of the model was far beyond anything done until then—around 100 equations—and the chapter shows how small teams of one to three researchers were put to work on specific sectors of the model, meeting during weeks-long conferences in the summers of 1961 and 1962 to put the model together.

Much like the Committee's model during these years, before it was handed over to the Brookings Institution in September of 1963 for further development and management, our story is preliminary. Still, it highlights the major difficulties that building a model of this scale entailed, and what the solutions to them say about the practice of macroeconometric modeling during the 1960s. For example, the size of the model that led to the establishment of what Klein called a "federation of research projects" and the data that was needed to feed it brought academics and government officials together, both as knowledgeable contributors to a specific sector and as sources of data. This enriched the model project, but also helped build important connections with government agencies that contributed to the development of a macroeconometric model at the Department of Commerce's Office of Business Economics, and the Board of Governors of the Federal Reserve System. Similarly, the work of Franklin Fisher to devise a way to estimate these models consistently despite the large amount of variables and the small amount of observations, led to the block-recursive method of estimation that became the standard way to estimate these models. And finally, the development of the SIMULATE program by Charles Holt and his team at the Social Systems Research Institute at the University of Wisconsin - Madison, marked an important milestone in the history of econometric software that came about as part of the effort to handle solving and simulating the Committee's model.

Chapter three thus shows how many gears had to turn to make a macroeconometric model possible, and this should help historians of macroeconomics realize to what extent this was a practice that cannot simply be shunned aside as a simple "extension" of the IS-LM model. The fact that these large-scale macroeconometric models had an important conceptual relationship with the standard, theoretical IS-LM type models should not be taken as an excuse to ignore them under the assumption perhaps that they were straightforward applications. This type of history would necessarily miss the connections between different dimensions of applied work that are involved in macroeconometric modeling, including data and estimation, but also the role of theory itself. The idea that the essence of macroeconomics was the theory put forth in IS-LM type models, or in any of its components (e.g., investment and consumption functions), misses the fact that a lot of the work being done at the time was applied and involved a non-linear and not necessarily unidirectional relationship between theory and data. Macroeconometric models bring this out because the model builders incorporated what was available from the contemporary literature in the different sectors of the model, showing the variety of work available, but they also had to deal with unresolved issues and incomplete theories. As Backhouse and Cherrier (2019) have shown for the FMP model, there was a lot of messy work involved in adjusting equations and discarding variables that looked well on paper but led to poorer statistical results, or in trying to include variables that needed explanation because they appeared elsewhere in the model but for which there was no clear theory to guide model builders.

Regarding the monetary sector of large-scale models of the 1960s, reducing macroeconomics during the postwar to the basic IS-LM model offers a misleadingly simple image of the work being done at the time. The IS-LM model was certainly an important conceptual reference for model-builders, but the applied work did not always and everywhere follow Keynesian lines. As Rubin and I show in chapter four, for the specification of banks' behavior, there was an important evolution during the 1960s that led macroeconometric models from a simultaneous characterization of the financial markets where most markets and agents interacted with each other to a more reduced and recursive structure with a clear Keynesian-type money market. Furthermore, even if academics like Modigliani found the Keynesian money market structure appealing, significant work went into implementing it in the considerably more detailed framework of a large-scale macroeconometric model. This work incorporated applied work done on the reserve-management of banks and other financial intermediaries, and it prompted new applied work on the supply of money (e.g., Modigliani, Rasche, and Cooper 1970). Similarly, less conceptually clear and non-observable aspects of the transmission of monetary policy also proved challenging to incorporate in macroeconometric model. In particular, in chapter four we show how credit rationing, a popular idea at the time but with little supporting theory, consistently failed to deliver on its potential as a major channel in the macroeconometric models' characterization of the transmission of monetary policy. Still, the constant efforts to incorporate it show that non-theoretical beliefs played an important role in the construction of large-scale macroeconometric models. All of these issues, again, show the specific challenges involved in the practice of macroeconometric modeling which are hidden by the simple depiction of the money market in the basic IS-LM model.

Finally, besides simply correcting the record by making macroeconometric models and applied macroeconomics visible, there's a large question for which a careful study of this work is necessary. As I mentioned above, the standard history of macroeconomics places macroeconometric models as the punching bag of Lucas and the New Classical economists without really explaining what these model were and how they were built. But the standard theory has also neglected the fact that macroeconometric models were not simply abandoned after the Lucas critique or even after the consolidation of the New Classical framework with Kydland and Prescott. Macroeconometric model-builders worked on modifying their models, and both in government agencies and the private sector macroeconometric models continued to be used for forecasting and policy analysis. While macroeconometric models almost disappeared from academic macroeconomics—with exceptions like Fair (1994) confirming the norm—I find it difficult to simply ignore the fact that they remained very much alive at places like the Board of Governors of the Congressional Budget Office—not to mention Data Resources Inc. and Chase Econometrics, which made their owners millionaires. A history of macroeconomics that ignores this is leaving behind an important chapter on the uses of economics outside of academia, and thus also missing out on a discussion about macroeconomists' role in shaping policies and business decisions. This dissertation brings us at least a step closer towards writing this history.

3. Economists as experts and their influence on policymaking

I have mentioned at various places above that the growing importance of economists, and economists' practices and methods, in policymaking are important elements in the topics discussed in this dissertation. The arrival of economists to top positions at the Federal Reserve, and the transformation of the practices of the staff of the Board of Governors' Division of Research and Statistics, must be seen in the context of a larger movement of economists into policy related government agencies. Also, the connection between policy analysis and government agencies with the development of the large-scale macroeconometric models of the 1960s is a key component in the story told in this dissertation. I would like to close this introduction by pointing out the connection of the themes I have explored in my research with a broader literature that specializes precisely in the influence of economists in policymaking.[16] Drawing these connections has, of course, the advantage of broadening the audience of the work carried out by historians of economics, but the results of this literature can also serve as a helpful framework for historical research.

[16] This literature is itself a subgenre of a larger literature about the role of "experts"—from natural scientists to political scientists and economists—in public policy discussions. As Claveau and Prud'homme (2018) point out, the notion of expert can be rather vague if not properly framed. Their proposed definition requires the involvement in policy discussions, and economists thus figure as prominent examples of experts.

The survey by Hirschman and Berman (2014, 781), for example, offers a very useful characterization of the modes—individually or in combination—by which economists can influence policy: 1) Professional authority, which refers to the overall status of the discipline; 2) institutional position, which refers to the presence of economists in policy institutions, and 3) cognitive infrastructure, which refers to the presence of an economic style of reasoning among policymakers as well as the use of policy devises produced by economists. All three of them can be seen at play in the four chapters of the dissertation, although economists' role in shaping the cognitive infrastructure is the more salient one. From this viewpoint, the work of the model builders of the 1960s played a role in changing how economic policy could be discussed. Our discussion in chapter five only captures a few years during which these models were actually used for FOMC meetings, but further research into the uses of these models should look carefully at how much these models altered the decision-making process. Our research shows that Burns clearly felt constrained by the analysis of the Bluebook, but that he found ways around it.

The same question should be asked about the other institutions that adopted such models during the late 1960s and 1970s. As Pinzón-Fuchs and I pointed out in chapter three, the economists associated with the Committee on Economic Stability were in favor of using these models to make economic policy more rigorous, but careful analysis of the uses of these models will have to take place before we can say the extent to which they shaped the process of policy-making. And this is of course related to the presence of economists at these institutions, both at the staff and the decision making level, as well as the overall status of the economics profession. The presence of economists as experts whose testimonies are solicited in congressional hearings (e.g., the 1952 Patman hearings) and whose technical knowledge is used in ideological fights (e.g., Brunner and Meltzer's 1964 study for Patman's House and Banking Committee) evidences the discipline's standing. While economists' criticism during the early 1950s was certainly there, the Federal Reserve did not feel compelled to engage with them until the 1960s, when their position as experts at the Council of Economic Advisers an as potential allies to critics of the Fed's independence became a serious threat. As I point out in the conclusions of this dissertation (chapter six) my future work seeks to follow more closely this literature and to provide responses to the questions it poses.

II. The academic and the New York central banker: Paul Samuelson and Robert Roosa on the effectiveness of monetary policy

Juan Acosta[1]

1. Introduction

When the time came for Paul Samuelson to share his views on monetary policy, Milton Friedman had already been heard. It was the morning of March 25, 1952, and the two academics were participating in one of the many hearings organized by a subcommittee chaired by Texas congressman, and long-time critic of the Federal Reserve, Wright Patman. The subcommittee had been set up following the 1951 accord between the Federal Reserve and the Treasury, which had put an end to a difficult confrontation over the peg on interest rates that was in place since 1942. Not many details had been made public other than the Federal Reserve had regained its discretion over interest rates, so the subcommittee sent out detailed questionnaires to the Federal Reserve and the Treasury, and held hearings were officials from both agencies as well as academics and members of the financial community had the chance to discuss the future of monetary policy.[2]

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[1] This is the latest version of a paper originally submitted to *History of Political Economy* in November of 2016 and that referees considered should be (heavily) revised and resubmitted. I'm grateful to Goulven Rubin, Etienne Farvaque, the participants of the Center for the History of Political Economy's lunch seminar during the fall semester of 2016, and two anonymous referees for their comments on previous drafts of this paper. I'm also grateful to Julie Sager for helping me find relevant material about Robert V. Roosa in the archives of the Federal Reserve Bank of New York.

[2] Wright Patman was a strong advocate of low interest rates and considered that the Federal Reserve's tight money policies unduly limited economic growth. He was a constant critic of the Federal Reserve's lack of accountability during the 1950s and 1960s (see Young 2000, ch. 7, 8). Henry Murphy was the economist of the Patman subcommittee, see Murphy (1953) for details on

The published volumes of the questionnaire replies (Congress 1952a) and the hearings (Congress 1952b) offer an important snapshot of the different views on monetary policy present among academics and policymakers at the time. There were different positions among the academics that Tobin (1953, 122) captured in his review of the subcommittee publications by identifying two "schools" of monetary theory: Friedman represented a school that defended the stability of the velocity of money and thus the adequacy of the quantity theory, while Samuelson represented the school that criticized it. The subcommittee thus offered a preview of the academic debates to come, but it also gave us an example of the distance that existed between academics and Federal Reserve officials, for Tobin (1953, 122) also identified a third school. Led by "Robert V. Roosa and others," this school had "developed and spread rapidly in the recent years" and insisted on the importance of lenders and the availability of credit rather than on the cost of credit and borrowers' sensitivity to changes in the rate of interest. This view of the effectiveness of monetary policy was mainly associated with officials at the Federal Reserve Bank of New York, where Roosa was the manager of the Research Department, and would later be known as the "availability doctrine" (Scott 1957a).

Roosa did not participate in the Patman hearings, but he did write the text that popularized the views held by the officials at the New York Federal Reserve and became the face of the so called availability doctrine. Much of Samuelson's testimony was directed at this view and thus, together with their correspondence on the issue, it offers us an example of the distance that existed between an academic and a central banker on the effectiveness of monetary policy. It is an early example of the tension that would later reach a breaking point in the early 1960s, when academics like James Tobin, Karl Brunner, and Allan Meltzer criticized the lack of

how the inquiry was carried out. The dispute between the Federal Reserve and the Treasury has been well documented by Hetzel and Leach (2001), Meltzer (2003, ch. 7), Wood (2009, 218-38), and Farvaque *et al.* (2018). For contemporary assessments of the situation see Fforde (1951), Tobin (1953), and Kareken (1957a,b).

scientific rigor and the vagueness of the concepts used by the Federal Reserve in its policymaking process (Acosta and Cherrier 2019). Their criticism was not simply directed at Federal Reserve's policies but at the economics behind them, which did not conform to the criteria of contemporary academic economics. In the same vein, Samuelson's criticism of Roosa's views on monetary policy rested on his difficulty to understand Roosa's characterization of lenders' behavior, as well as the logical process that led to Roosa's predictions about the behavior of interest rates.

At the center of their differences was the fact that these two men observed the economy from very different corners. While Roosa and his colleagues at the New York Federal Reserve were actors in the money markets, Samuelson was leading the transformation of academic economics into a mathematical science. Roosa's understanding of the behavior of lenders and the effectiveness of monetary policy came from his first-hand experience during the late 1940s; he had little use for over-simplified theoretical constructs that ignored the institutional characteristics of the American financial system. Samuelson, for his part, had a record of skepticism—if not outright contempt—for market participants' explanations of economic phenomena. He demanded an explanation in terms of the rational behavior of individuals and equilibrium positions, something that Roosa was not able, or willing, to provide.

2. Roosa and the New York Federal Reserve

Paul Samuelson (1915-2009) and Robert Roosa (1918-1993) knew each other well.[3] They had met and become close friends in the early 1940s in Cambridge, Massachusetts, where Roosa had taught at both Harvard and MIT—including being Samuelson's teaching assistant—before being drafted for the army in 1943 (Roosa 1969, 7; Ackley et al. 1964, 162). Their professional careers, however, turned out to

[3] For reasons I still ignore Roosa changed the spelling of this last name from "Rosa" to "Roosa" in 1952. He appears as Rosa in the 1951 Annual Report of the New York Fed, but as Roosa from 1952 onwards. The same change is observed in his publications (see Roosa 1951b; Roosa 1952a).

be very different. Roosa had gotten his PhD from the University of Michigan in 1942, with Arthur Smithies as his advisor on paper, but he had actually written it at Harvard under the supervision of John Henry Williams, a long-time adviser to the New York Federal Reserve.[4] And after the war, it was Williams that convinced Roosa to come to the New York Federal Reserve, where he had already worked during the summers of 1941 and 1942, and where he had carried out a study of the industrial loan program for the National Bureau of Economic Research.[5] Roosa entered the New York Federal Reserve as an economist in 1946 and rose quickly through the ranks, leaving in 1961 as a Vice President with responsibilities over both the Research Department and the Open Market Operations Department.[6] He became the Treasury Undersecretary for Monetary affairs (1961-1963) and then went to the private sector as an associate of Brown Brothers Harriman & Co, although he continued to weigh in on monetary policy discussions, particularly on issues related to international finance.[7]

As I mentioned above, Roosa did not participate in the Patman hearings, nor is his name mentioned in the responses or in any of the testimonies. We know that he participated in the process of writing some of the responses to the Patman questionnaire, [8] but the reason Tobin named him as the leader of the third school he identified was a chapter Roosa had recently published as part of a volume honoring Williams (Roosa 1951a). "Interest Rates and the Central Bank"—which had been ready before the 1951 Accord, by July of 1950, but was only published the following year—gained Roosa some notoriety as it was immediately recognized as an

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[4] Williams to Leontief, November 28, 1951. Papers of John Henry Williams, Archives of the Federal Reserve Bank of New York (henceforth JHWP), box 180638, Correspondence Coombs-Roosa.

[5] Williams to Roosa, January 31, 1946; Letter to Leontief, November 28, 1951, JHWP, box 180638, Correspondence Coombs-Roosa.

[6] Roosa's positions throughout the 1950s can be tracked by looking at the Annual Report of the Federal Reserve Bank of New York.

[7] For biographical details about Roosa, see the 1969 oral history interview for the Treasury (Roosa 1969) as well as the note by Bruce MacLaury (1997).

[8] Roosa to Samuelson, November 20, 1951, Paul Antony Samuelson Papers, Rubenstein Library, Duke University (henceforth PASP),, box 63, Roosa Correspondence.

important contribution to the literature on central banking.[9] Samuelson also considered it an important contribution and, when he circulated a summary of his 1952 testimony, told Roosa that his essay had been an important influence for his views on the matter.[10]

Roosa's (1951a) essay developed the views of Williams and Allan Sproul, president of the New York Federal Reserve (1941-1956).[11] Sproul presented the essential points of this view a few years before (Sproul 1947) as well as in his own contribution to the Williams volume (Sproul 1951). And both Roosa and Sproul explicitly recognized the fundamental influence of Williams—mainly in the form of oral discussions—in shaping their ideas (Roosa 1951a, 275–76; Sproul 1951, 296). However, it was Roosa that became the face of the New York Fed's view. Sproul (1951, 322) saw Roosa's (1951a) essay as providing the development of the "theoretical aspects" of their view, and it is also likely that Roosa's previous connections to the academic world made him a more visible figure among economists. The discussion of the New York Federal Reserve's view of the effectiveness of monetary policy and the role of lenders that follows is thus based mostly on Roosa's (1951a) essay and other contemporary texts.

2.1 Uncertainty and the sensitivity of lenders

Ralph Leach, an external advisor to the Board of Governors in the late 1940s, recalled the story of a meeting of the Federal Open Market Committee where he wanted to make a "comment on the market" but was stopped on his tracks by chairman Thomas McCabe, who pointed out that "we don't have opinions on the market down here—we rely on New York for those opinions" (Hetzel and Leach 2001, 37). Such was the centrality of the Federal Reserve Bank of New York as the

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[9] See, for example, Johnson's (1953) review of the Williams volume.

[10] Samuelson to Harris, January 15, 1952, PASP, box 63, Roosa correspondence; Samuelson to Roosa, April 11, 1952, PASP, box 59, Patman testimony.

[11] For biographical information on Sproul see Lawrence Ritter's overview of his life and views on monetary policy (Ritter 1980, Chap. 1).

main connection between the Federal Reserve System and the financial markets. They are the ones in charge of implementing monetary policy by buying and selling government securities through the Trading Desk and they were at the forefront of the Federal Reserve's work in keeping interest rates stable during the peg (1942-1951). The view that emerged at the New York Federal Reserve and that was presented by Roosa was a direct reaction to this experience.[12]

The peg had been established in 1942 to guarantee that the war would be financed at stable rates of interest, and a pattern going from 3/8 of 1% (for 90 day bills) to 2 1/2% (for 20-25 year bonds) was set (Meltzer 2003, 594). These were historically low rates of interest that reflected the dreadful decade that followed the Great Depression, but more importantly, they established a fixed pattern of interest rates that created certainty over the price of government securities and made them essentially equal to cash. This made it easy and profitable for its holders, an in particular commercial banks, to switch between government securities of different maturities ("pattern-riding") and later between government securities and private loans ("debt-monetization") (Sproul 1951). The primacy of the war effort necessarily made these concerns secondary, but after the war ended the Federal Reserve had the opportunity to do something about the uncomfortable position in which it found itself: the large amount of government debt held by banks and other financial intermediaries represented a source of capital that could end up sustaining a large increase in lending to the private sector and causing inflation. As Marriner Eccles, then chairman of the Board of Governors, stated:

> As long as the Federal Reserve is required to buy government securities at the will of the market for the purpose of defending a fixed pattern of interest rates established by the Treasury, it must stand ready to create new bank reserves in unlimited amount. This policy makes the entire banking system, through the action of the Federal Reserve System, an *engine of inflation* (quoted in Hetzel and Leach 2001, 43; my emphasis).

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[12] See Roosa (1956a) for a description the tasks involved in running the Trading Desk at the New York Federal Reserve.

Sproul and Roosa agreed with Eccles on the need to do something about the latent danger for inflation that the large holdings of government securities posed. However, they both disagreed with Eccles and others at the Federal Reserve and in academia on the appropriate solution. Instead of increasing reserve requirements or establishing new reserve requirements to be kept in the form of government securities, Sproul and Roosa insisted on the need to move away from the peg and on the primacy of open-market operations. For them, the key element was the creation of uncertainty about the actions of the Fed. If the problem was that, thanks to the peg, control over the availability of credit was exclusively in the hands of lenders, then it was necessary to eliminate the certainty that the peg provided them. Solutions involving reserve requirements were bound to be ineffective because they didn't tackle the actual source of the problem.

Uncertainty mattered because lenders were sensitive to changes in the prices of government securities and, most importantly, to what those changes could mean in terms of the future behavior of interest rates and of the Federal Reserve. The reaction of interest rates to the measures that the Fed started taking in 1947 to unfreeze short-term interest rates, as well as the responses of bankers and fund managers to surveys about their actions, was presented as evidence that the New York Federal Reserve's approach was feasible (Sproul 1951, 309-311, 322). And this sensitivity of lenders, and the possibilities it opened to use uncertainty as a tool of monetary policy, were themselves consequences of the changes that the financial market had experienced since the creation of the Federal Reserve System in 1914. Roosa emphasized three key changes. First, the large increase in government debt had provided the market with a safe asset that facilitated the assessment of the liquidity and credit risks of the rest of the available securities, and that by 1948 represented half of the total debt in the United States (Roosa 1951a, 277). Second, specialized financial intermediaries had consolidated, favored by "a steadily growing popular insistence on 'security'—the avoidance of loss, at the expense of accepting lesser yields" (Roosa 1951a, 278). By 1948 these intermediaries held three quarters of the ultimate debt in the United States, operated on "relatively narrow margins, and [were] alert to small changes among yields on debt instruments that would have been considered trivial a few decades earlier" (Roosa 1951a, 278). Third, the mechanics of the market for government securities had also changed, and by the mid 1930s it had become a predominantly over-the-counter market, centered around a small group of dealers through which the Federal Reserve carried out its open-market operations. Furthermore, the high degree of specialization in this market was also evidenced by the change in the unit used to measure changes in the prices of government securities—which went from 1/32 to 1/100 (a basis point) of a dollar—and in the reduction of the spreads between bid and offer quotations—which went from 6-30 cents to around 1-2 cents (Roosa 1951a, 278-79). For Roosa,

> It is such changes in customary market practices which indicate, *more convincingly than abstract analysis*, that the increasing relative importance of Government securities, and the growing concentration of investable funds in the hands of yield-conscious institutions, have made the money markets highly susceptible to slight changes in interest rates. (Roosa 1951, 279, my emphasis)

Roosa also presented a criticism of the previous claims that had been made regarding monetary policy, which he summarized as the "main stream of analysis" from Wicksell to Keynes, and that were at fault for, essentially, failing to note several "gaps between concept and reality" that existed in its reasoning (1951a, 273). He considered that there had been "a cultural lag between the development of the theory of interest rates and of central banking, on the one hand, and the changing characteristics of the money markets, on the other" (1951a, 272). And, while most of the American writers during the 1910s and 1920s held views similar to those of Wicksell (e.g. Fisher), he considered that "[o]nly the Federal Reserve System itself, facing the concrete problems of implementation, saw the possible gaps and entertained a genuine skepticism over the feasibility of bridging them" (1951a, 273).

The three propositions which this main stream of analysis "implicitly accepted" had to do with the mechanism through which the central bank's policy was transmitted:

> First, a change in rates at the central bank would actually assure a roughly corresponding change at the commercial banks. Second, by focusing attention on "the" interest rate, most writers assumed a synchronous movement throughout the rates on comparable debt instruments of all maturities; that is, a change at the commercial banks was expected to spread throughout the short-term market and on through all other maturities. Third, once rate changes were achieved through central bank action, they would be followed by appropriate action on the part of borrowers (and, most writers would have added, on the part of savers). Only if all three of these presumed relationships were to hold would it be possible to go on further to accomplish precise objectives in terms of the money supply, the price level, and the control of the business cycle. (Roosa 1951a, 273)

This view of the working of monetary policy, built with the historical experience of the Bank of England in mind, and at a time when discount operations were the basis of central bank action, was at odds with the actual characteristics of the American financial system. In particular, Roosa highlighted the work of Randolph Burgess and Winfield Riefler during the late 1920s for showing that the links between central bank policy and commercial rates, and between short- and long-term rates, were in fact rather weak. Notably, the work of Burgess and Riefler established the idea that interest rates, and changes in interest rates, were not important determinants of the situation of credit in themselves but rather evidence of underlying changes in credit conditions (Roosa 1951a, 274).

Equally important was the exclusive emphasis on borrowers (and savers) for the transmission of monetary policy to economic activity. This had been at the center of the discussion between "the short-enders and the long-enders" (e.g. Hawtrey vs Keynes) regarding which interest rates were actually important, but its results had been rather sterile in Roosa's view (1951a, 274-775). He cited Hicks' *Value and Capital* (1939) and the Oxford Surveys (1938 and 1940) to point out that, by the end of the 1930s, changes in the rates of interest were broadly seen as being rather unimportant (*ibid*.). In the early 1940s in the United States, it was Williams, according to Roosa, who put lenders at the center of the working of monetary policy (1951a, 276).

The three changes in the financial markets discussed above had brought the actual characteristics of the market closer to the description of the Wicksellian mechanism presented by Roosa, but the underlying workings were different and reflected the contemporary characteristics of the American scenario. The broad holdings of government securities of virtually all maturities meant that, through its open-market operations, the central bank was in direct contact with not just commercial banks but with the whole capital markets, in both its short and long ends (propositions 1 and 2) (Roosa 1951a, 280-281). Regarding the influence of the cost of credit (proposition 3), Roosa accepted that recent critics had perhaps gone too far on their criticism of Wicksell, and accepted that the rates of interest did have "some importance as a cost factor" (1951a, 281). He clearly considered, however, that Wicksell had exaggerated the direct significance of changes in the rates of interest, attributing it to the fact that "the *niceties of logical refinement*, in isolating any one variable for marginal analysis, frequently result in an excess of zeal for the influence of the variable studied" (*ibid*., my emphasis).

For Roosa, it was the role of the lender, "neglected by the monetary theorists," that actually played the key role in the effect that monetary policy could have on economic activity by modifying its willingness to lend and making credit more or less available (1951a, 282). But influencing lender's willingness to lend wasn't straightforward; it demanded a close involvement of the personnel of the Trading Desk at the New York Federal Reserve:

> The pattern of lender reactions need not necessarily be the same for a change [in the rates of interest on government securities] of the same direction, or the same magnitude, at two different points in time. The one assured fact is that lenders will always be sensitive to slight changes, careful to balance the possible capital loss (or gain) resulting from a rise (or

> reduction) in rates against the possibilities of a greater (or lower) yield. But because lenders cannot always be expected to take the same steps following a given rate change, the System's open market account cannot be operated according to a formula. Operations must instead be based on continuous close study of the money markets. Achievement of a desired degree of ease or of restraint will depend heavily on the ability of the central bank officials to "play by ear." And the supreme advantage of open market operations for this purpose is that they can proceed in small steps, where appropriate; they need not be accompanied by formal announcements of intentions, with the rigidity and the possible exaggerated emphasis inherent in such announcements; and they can be readily reversed if the desired response is attained more quickly than expected, or in the event of a subsequent change in the underlying market situation. (Roosa 1951a, 286-287)

This close and permanent contact with the markets was fundamental because there was no easy and objective way to judge the underlying movements that actually determined the availability of credit at any given moment in time. The level of the rate of interest, the cost of credit, was only one variable, and in the New York Fed's view—and as Burgess, Riefler, and Williams had argued—it was a symptom rather than the determining cause of the tightness or abundance of credit. It was mainly the availability of credit that the Fed had to monitor and control.

2.2 The scope of monetary policy

The New York Federal Reserve’s view was, in a nutshell, that the effectiveness of monetary policy came from the effect that changes in the rates of interest had on lenders’ willingness to lend. Through open-market operations, the Federal Reserve could influence lenders’ decision to sell a government security to grant a private loan, or not. With out the peg in place, lenders’ were uncertain about the future behavior of interest rates on government securities. As Roosa noted after the accord:

> [L]enders that wanted to add to their loanable funds by selling Government securities could not tell how much further prices might fall (rates rise) if they unloaded upon the market. This is the kind of “uncertainty” that has been of key importance in U.S. credit control; it put a limit on the total availability

> of funds and forced lenders to ration the credit they had among various borrowers (whether the borrowers might have been willing to pay higher rates or not). (Roosa 1952a, 256-257)

But this was a time-specific phenomenon, and neither Roosa nor Sproul claimed more than that. And while they both argued that monetary policy could play a useful role in controlling inflation, they also recognized its limitations. Thus, while they argued that the broad holdings of government debt put the Fed in close contact with the capital markets, they also recognized that its ability to influence lender's willingness to lend—and thus the availability of credit in the economy—was not to be taken for granted. Sproul noted that, although lenders were still quite sensitive, by 1950 their sensitivity had already decreased in comparison to the 1947-1949 period (Sproul 1951, 321). Roosa, for his part also remarked that a movement towards disintermediation in some parts of the capital market was visible and that it could reduce the effectiveness of monetary policy in the future (1951a, 282-283).[13] In addition, for Sproul and Roosa monetary policy was not all-powerful. Sproul, in particular, had stated early on that the only real cure against inflation was the increase in the production of goods and the productivity of work (1947, 7). No grandiose results should be expected from monetary policy.[14]

Still, the core idea that uncertainty about the behavior of the rates of interest would force lenders to think long and hard before unloading on the market for government securities to grant credits to the private sector had powerful implications that added to the interest that the New York Federal Reserve's view attracted. If uncertainty was that important then lenders—if they were as sensitive as Roosa described them to be in 1950s America—would be wary of very small changes in the rates of government securities. This allowed monetary policy to bypass two big criticisms

[13] At the same time, however, he also considered that the Fed didn't have to respect the upwards-sloping yield curve that had been inherited from the 1930s and should consider its slope as another tool of monetary policy. A decade later, Roosa would help engineer "Operation Twist."

[14] For Sproul, by 1930 "[t]he extravagant ideas of credit control as the main or sole arbiter of our economic well-being which held some sway in the twenties had long since been abandoned by central bankers" (1951, 311).

that many of Roosa's contemporaries in academia and the government held.[15] First, monetary policy need not dramatically increase the cost of government borrowing, and any minor increase was likely to be canceled out by small reductions in the medium run. Furthermore, close collaboration between the Fed and the Treasury would result in a wisely chosen term structure of the government debt that could prevent unsuccessful debt subscriptions. Second, for those who feared that monetary policy could cause a recession and was likely to do so because it had to be applied with force to actually be effective, the New York Federal Reserve's view offered an alternative reasoning were high rates where not necessary to reduce the availability of credit and thus curb inflation.

3. Samuelson on interest rates and monetary policy

At the time of the Patman subcommittee Samuelson was the author of a game-changing book that laid the foundations for neoclassical economics (Samuelson 1947), but he had also participated in a heated debate about interest rates in the previous years and published two editions of an equally game-changing undergraduate textbook (Samuelson 1948; 1951). His position on the 1945 debate on the rate of interest shows us the contempt he held for market participants' views on economic phenomena, and his textbook makes clear just how little importance he gave to monetary policy in this period.

3.1 Samuelson's contempt for bankers

Samuelson (1945a) came in to the debate when he criticized the idea that higher interest rates would hurt holders of government securities, and in particular the banking system.[16] To be sure, he was strongly against such an increase and considered it would "imply enormous, unneeded, unnecessary, undesirable, and arbitrary gifts to certain investors at the expense of the Treasury" (1945a, 26). This

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[15] See in particular Villard (1948), but also Hansen (1949) and Samuelson (1948).

[16] Backhouse (2017, 496-499) also discusses this episode.

outweighed any "doubtful minor benefits" in controlling inflation, which could be controlled by other forms of banking policy, he added (*ibid.*). Rather, Samuelson took issue with the validity of the argument that increasing the rates of interest was a harmful policy, which was used by some of those arguing against such increases. His paper showed a strong lack of faith in the usefulness of monetary policy and was written in an arrogant tone, dismissive of banker's understanding of the situation.

Samuelson's (1945a) central argument was that the present value of *income* streams alone was a "false indicator" of the situation of a debt holder, like banks or any other holder of government securities; the present value of *disbursements* also had to be taken into account. He stated a theorem, which showed

> the *exact* conditions under which rates help or hurt a given person or institution: Increased interest rates will help any organization whose (weighted) average time period of disbursements is greater than the average time period of its receipts." (Samuelson 1945a, 19. My emphasis.)

The theorem was accompanied by its mathematical expression, in a footnote, where V was defined as the sum of the discounted values of revenues minus the sum of the discounted values of disbursements, and its derivative with respect to the rate of interest showed under what conditions it would be positive or negative. In other words, Samuelson was essentially saying that as long as the frequency with which a bank—or any other holder of government securities—received a dollar was higher than the frequency with which it paid one, the bank would be just fine. He was making an argument against the idea that an increase in the rates of interest would critically compromise the solvency of any particular bank or the banking system as a whole. This was most easily seen in the case of an insurance company, whose whole business was based on calculating the risks of its customers and thus the time distribution of its disbursements, but it applied just as well to universities, banks, and to any other holder of government securities whose future disbursements were sufficiently foreseeable. Furthermore, Samuelson argued that it was highly unlikely that there would be any significant or drastic change in the behavior of banks' disbursements in the postwar (1945a, 24,25).

Samuelson also argued that the collapse itself of the book-value of banks' holdings of securities was also a non-issue, independently of the usefulness or not of present value calculations (1945a, 22). This was so because the higher rate of interest, while it meant a particular government security had to be valued at a discount, also meant that it's yield (coupon divided by market value) increased. Together with the fact that cash from maturing government securities could be used to buy new issues at a higher interest rate, the increase in the rates of interest actually increased the income stream of banks, which would make up for any paper loses in just a few years. This was the gift Samuelson denounced would be bestowed upon the banking system if rates where increased.

For the purpose of this paper, the key issue I wish to highlight from Samuelson's argument is how little he cared for actual market practices and the reasoning that might be behind them. Samuelson's main point was to show how the issue *should be* thought about, with little regard for what market participants actually did or why. Thus, although Samuelson remarked that at the time fund managers of insurance companies often took an "implicit speculative position" (1945a, 18) he did not elaborate on their actual practices or on those of commercial banks. Furthermore, it was Samuelson's sense of scale, and not that of any actual participant in financial markets, that pervaded his argument. This was clearest in Samuelson's statement that, following a 1 per cent uniform increase in the whole structure of interest rates and based on the portfolio composition of commercial banks' holdings of government securities, banks would suffer a capital loss of "only 3 percent" and could recover from it in "less that three years" (1945a, 22, 23; both expression in italics in the original).

For Samuelson, the argument he had presented was actually a "secret which all wise men know but which no wise man will tell," and he ended his paper stating that the interest rate during the war should in fact have been lower, and inviting "the wise men" to comment on the subject (1945a, 26, 27). Similar to what he would later do in his *Economics* with the consensus among economists, he was presenting what he believed to be correct as common sense and an obvious truth, irrespective of how actual market participants behaved. This topped off the arrogant tone of a paper that started with the maxim that "[s] imple truths need constant repetition" (1945a, 16).

In an article in *Modern Industry* (January 15, 1945), Samuelson defended the "Yes" position in a debate around the question "Is the 'easy money' policy a sound one?" He defended lower interest rates---remarking that no "expert" doubted the war could have been financed at 1% if the Treasury had so wanted---and also argued that higher interest rates where an inadequate tool for controlling inflation. This time there was no arrogance, and he referred to Hans Christian Sonne, president of a commercial bank and charged with defending the "No" position, as a "formidable opponent" (Samuelson 1945b, 674). A couple months afterwards, however, Samuelson's disdain for bankers' intelligence resurfaced in his review for *The New Republic* (March 26, 1945) of Hansen's *America's role in the world economy* (1945). Commenting on the opposition that some bankers had expressed towards the establishment of the International Monetary Fund, Samuelson remarked that bankers "[had] not in the past always recognized the noses on their faces," and more bluntly that:

> in opposing the Fund, they do so...without ever having thought the matter through. The notion that a banker understands money or finance is a quaint one which will not stand up under empirical observation. A barber can discuss as cogently whether or not banks create money, while from time immemorial economic sophomores have had a field day at the expense of bankers' writings. It is for this reason that bankers always employ hack economists to serve as their triggermen and ghost-writers (p. 410).

This statement, and Samuelson's invitation to "wise men" to comment on the issue of higher interest rates was taken up by George Coleman (1945), an economist at the Mississippi Valley Trust Co. of St. Louis. His brief comment highlighted Samuelson's assumption of an "unrealistic interest rate structure" and a "slight error" in his calculations (1945, 672). Coleman certainly did not appreciate the patronizing and arrogant tone of Samuelson's previous comments, noting first that "[w]hile the academician will find such an error [in the computation of old/new capital ratios] trifling, to the average banker it might mean the difference between a profit and loss on the bond transaction" (*ibid.*). This remark on the distance between Samuelson and actual market participants' views was reinforced by pointing out that:

> What is somewhat surprising is that Mr. Samuelson should have taken so much pains to make a calculation of this type when even the lowliest bank clerk could have told him, even if his barber could not, that banks are endeavoring to maintain a short position in order to minimize the loss which a rise in interest rates with a longer position would produce" (Coleman 1945, 672).

It didn't matter what type of theoretical argument or example Samuelson proposed, Coleman saw little value in them and he could simply state that "the banking system is following consciously a portfolio policy which will result in as little loss as possible in the event that the structure of interest rate might rise" (p. 673). Samuelson (1945b) offered a brief response to Coleman and Seymour Harris (1945)—the other "wise man" who answered Samuelson's call. He focused on Coleman's reply, stating simply that he agreed with "many" of Harris' comments.[17] Regarding Coleman, Samuelson was unrepentant about his view of bankers. Samuelson did not give up on his appreciation that bankers did not act as they should. He did not care about the particular reasons a banker might have for not doing what logic and previous experience seemed to indicate was right:

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[17] This is odd, for Harris' main argument was that once the taxes paid by banks were taken into account their profits were not extraordinary and the war had in fact been a less than one percent war. This diminished the case for Samuelson's position and concluding remark in the paper that "it [was] time for another turn of the 'cheap money' screw" (1945b, 675).

> As Mr. Coleman remarks, every lowly bank clerk knows that banks have been speculating on or hedging against, a rise in interest rates. But not even bank presidents have been able to explain why they persisted in so odd a belief when in every year of the past decade (except for three transitory flurries) it proved to be wrong. In fact, those few banks which broke away from this obsession, repeatedly scored higher yields and capital gains by concentrating upon longer durations. A spinster who sees a man under her bed once can be forgiven. But what are we to think of the judgment of anyone who cries wolf for eleven years?" (p. 675)

3.2 The limited usefulness of monetary policy

Regarding monetary policy, Samuelson's position in his *Modern Industry* article against using higher interest rates to fight inflation in the postwar rested on four points:

> First, there may well be deflation rather than inflation. Second, there are superior alternative policies which act more directly and efficaciously. Third, a blunt policy of raising all interest rates will do a great deal of harm for the amount of good that it does. Fourth, and most important, while the need to control a boom will almost certainly be a temporary one, experience shows that our economy takes years to adjust itself again to lower interest rates (p. 116).

Additionally, while he considered that the problem of the public debt was not going to be the most important one, he did think that it made no sense to increase banks' earnings at the expense of the Treasury. The first edition of *Economics* (Samuelson 1948) didn't deviate much from this position. Samuelson was a strong supporter of the existence of Central Banks as a defense against banking crises like the ones suffered in the United States before 1913. In fact, he stated that their "primary function is to stand as a Rock of Gibraltar in time of panic, to be ready to use the full monetary powers of the government to stem collapse of the banking system," and considered their other functions to be subsidiary (Samuelson 1948, 322-23). Samuelson, however, didn't elaborate on his views on the Central Bank as a lender of last resort and instead focused on those other subsidiary functions and their limitations (ch. 14 and 15).

Regarding the effectiveness of monetary policy, Samuelson recognized that the Federal Reserve could potentially influence investment and consumption by influencing banks' utilization of excess reserves, and by lowering interest rates. However, he also emphasized that this mechanism, as had been shown in the preceding decades, was ill suited for dealing with the business cycle for various reasons (Samuelson 1945: 353-355). First, in the case of a depression, banks would not likely use excess reserves, and interest rates may already be so low that the banks and the public would be indifferent between holding idle cash and bonds, thus making it difficult for the Fed to achieve more than a minor decrease in the rate of interest. Also, some long-term commercial rates were sticky, so the Fed might not be able to reduce the effective rates of interest. Second, even if the Fed managed to lower interest rates significantly, it may all be useless since questionnaires (the Oxford and Harvard surveys) had shown that the rate of interest was not an important determinant for investment decisions. Third, monetary policy could be more effective against inflation, but its effectiveness was weakened by the independence of investment from the short-term capital market and banking interest rates. Furthermore, if the problem was a speculative boom—as had been the case in 1929—and the Fed relied exclusively on quantitative control, it might need to raise interest rates so much that it would end up producing a depression. Finally, any possibility of using monetary policy to fight inflation was significantly reduced by the amount of Government debt that had been created to finance the war: Samuelson, as was common at the time, feared that if the Federal Reserve broke away from the peg the prices of bonds would fall "as precipitously as they did after World War I" (1948, 354). He saw in the persistence of inflation for a long time the only scenario in which the authorities would face the decision to let interest rates increase (1948, 355; cf. Villard 1948). To sum up, Samuelson considered that monetary policy, though theoretically useful, was not a good weapon against the business cycle or inflation; Central Banks were useful mainly against banking panics.

There isn't much about the role of lenders in Samuelson’s story. The discussion of the factionary reserve system remained at an introductory level, with banks having as their basic behavioral trait exploiting their cash reserves to the maximum. Insofar as the Fed could affect their reserves through open-market operations it could influence the supply of money in the economy. Banks played a fairly passive role, essentially serving to multiply the monetary base. The sole caveat was that the level of reserves that banks felt comfortable keeping was not fixed; it could change dramatically, as had been the case during the Great Depression—banks were also covered by the liquidity preference analysis. Samuelson pointed out that the process of deposit creation and monetary expansion was not automatic (1948, 332), but the core of his discussion of the working of monetary policy assumed it was, and any deviation was only mentioned in passing.

The first edition of *Economics* contained an unclear relation between the aggregate-income analysis that was at the core of the book's argument (i.e. the 45 degree model) and monetary policy (see Hoover and Pearce 1995), but the second edition clarified this connection and explicitly incorporated the typical interest-rate transmission channel that is usually associated with Keynesian (IS-LM) models.[18] The limitations on monetary policy were still very much present and it remained "at best a supplement to other stabilization policies, such as fiscal policy" (Samuelson 1951, 343). However, A novelty regarding the limitations of monetary policy was also included (Samuelson 1951, 343-344). Besides (1) the small effect of increases in the money supply on the interest rate, and (2) the small effect of changes in the interest rate on investment, Samuelson pointed out that (3) the central banker, due to his preferences or to political pressure would not be willing to push monetary policy too far. This surely reflects the increasing tension between the Fed and the Treasury, which by then was approaching the tipping point.

[18] "To put matters most simply, an increase in the amount of money tends to depress the rate of interest; and a reduction in the interest rate tends to increase the flow of investment spending, thereby raising income, consumption, and production or prices" (Samuelson 1951, 339). Samuelson, however, did not include a discussion of the IS-LM model until the third edition of his textbook, and it was in the chapter about capital theory.

A more noticeable novelty was the inclusion of a new subsection, "Lower interest rates reinforced by more liberal credit rationing," where Samuelson pointed out that credit rationing was an important element to be taken into account when analyzing the effects of "easy money" policies:

> Borrowers who previously would have been considered to be just a little too risky will now be granted loans. This is because *the lender now has plenty of money on hand begging for investment opportunities; the lender will now be rationing out credit much more liberally than would be the case if the money market were very tight and interest rates were tending to rise*. The plentiful supply of money will also tend to bid up the prices of common stocks. It will be easier now to find buyers for new issues of common stocks. And business firms generally will find it somewhat easier to raise equity capital as well as to raise loan capital. (Samuelson 1951, 341. Emphasis in the original.)

The explicit inclusion of credit rationing in the second edition of Economics echoed the outcome of the inquiry carried out by the Subcommittee on Monetary, Credit, and Fiscal Policies, chaired by Senator Paul Douglas. The hearings and testimonials were carried out and collected during 1949, and the report—often referred to as "the Douglas report''—was published in 1950 (Congress 1949; Congress 1950).[19] The Douglas report marked an important moment for the Federal Reserve's cause against the peg on interest rates for it evidenced a clear acknowledgment of the presence of inflationary forces, as well as the contribution that monetary policy could provide if interest rates were allowed to increase. In addition, the report emphasized that monetary policy acted by modifying the availability of credit in the economy, and that credit rationing was a key element in the banks' response to monetary policy (Congress 1950, 21). The Douglas report itself was in line with the contemporary insistence on the importance of the availability of credit by Sproul and others at the New York Federal Reserve, although they did not use the expression ``credit rationing" before 1952.

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[19] Samuelson was sent a questionnaire for the inquiry of the Douglas subcommittee but he did not respond. However, he did sign the recommendations made on the NPA Conference of University economists, Sep 16-18, 1949, Princeton (Congress 1949, 435, 441).

Credit rationing doesn't appear in the rest of Samuelson's book. As he pointed out in the sentence following the quote above, its effect could be assumed to accompany the effects caused by movements in the rate of interest so there was no need to elaborate on it any further (Samuelson 1951, 341). As with other complexities or deviations from the main argument made in Samuelson's textbook, credit rationing was pushed aside right after being acknowledged. It is also likely that he had not given the issue much thought at the time.

4. The puzzle of credit rationing and Roosa's pragmatism

In the previous two sections I have characterized Roosa's and Samuelson's views on the effectiveness of monetary policy and shown how their viewpoints differed. First we had Roosa and his colleagues at the New York Federal Reserve—knee-deep in the implementation of policy and distrustful of over-simplified theories—who had formed a view in terms of what they had come to expect from lenders during their 1940s experience. And then there was Samuelson, whose previous participation in policy discussions had shown his contempt for the views of bankers, and whose textbook echoed the views of many of his contemporaries regarding the uselessness of monetary policy. His testimony during the Patman hearing furthermore emphasized that there was a correct way to think about these issues. Although les formal than most of his other work, he displayed the orderly way of thinking about economic issues that was central to his conception of economics, and offered a "down-to-earth realistic way of describing the mechanism of monetary policy" (Samuelson 1952a, 742). As such, his testimony offers another example of Samuelson's documented effort to bring clarity to economics by getting rid of the confusion fostered by the loose use of language (Maas 2014; Mehrling 2014; Backhouse 2017a).

4.1 Samuelson's testimony

Samuelson summarized his argument in the following way: "the real problem of monetary policy open to the central-bank authorities is the problem of its effects upon the cost and *availability* of credit to spenders" (1952, 693, my emphasis), and he further insisted on the fact that "[a]ll that a central bank can do is to *bid up or down the price of assets*; it can thereby bribe the banks and public into changing the composition of assets, but it cannot primarily affect the total of such assets" (*ibid*., my emphasis). His main targets were the arguments about monetary policy, "amply represented...both on the part of Government agencies and on the part of private voluntary answers" to the questionnaires of the Patman inquiry (1952, 693), that focused on the importance of the "demonetization of debt," the supply of money, and on controlling bank reserves in a mechanic way, without explicitly taking into account the effects of the cost and availability of credit. These elements were only important for their connection with the cost and availability of credit—what the Central Bank could affect and what the banks cared about—and therefore it made no sense to speak in terms of these other elements, or even worse to think that they were somehow independent from the cost and availability of credit. Doing so only hid what was actually going on when the Fed carried out monetary policy, and fostered the idea that it was carried out following a mechanistic "meaningless sequence" based on the deposit-reserve multiplier and a quantity equation.[20] Samuelson urged us, "as an economic theorist," to "show how each individual bank will be compelled or tempted by your central-bank policies to refuse credit to would be borrowers" (1952, 694).

Next, Samuelson focused on "a more subtle form that the doctrine now takes among us academic economists and also among the Federal Reserve System spokesmen" (1952, 694). Although he didn't name names, this "subtle form of the doctrine" was the view associated with Roosa and the New York Federal Reserve. Samuelson's central criticism was that its proponents had not understood the extent to which their view depended on the imperfect nature of the loan market:

[20] Samuelson presented this "meaningless sequence" in three steps: "(1) cut down on bank reserves, (2) apply a 5-- or 6--1 leverage factor to determine the resulting contraction in bank deposit money, (3) apply a `quantity equation' to show how the cut in total M results in a cut in prices or dollar spending." (Samuelson 1952, 742).

> The imperfect competition aspect of banking is absolutely crucial for the recently fashionable doctrine that the central bank gains its leverage not through its effects upon the cost of credit but by its effects upon the availability of credit. I would gladly trade 100 pages of the written and oral testimony before this committee for even a few paragraphs of careful analysis on this point. (Samuelson 1952, 696)

The imperfect competition element was fundamental because the sensitivity of lenders, which Samuelson saw as the main trait of the New York Federal Reserve's view, was not enough to explain why monetary policy could be effective through its effect on lenders. While Samuelson agreed that "insurance companies and banks [were] very responsive to slight changes in interest rates," upon closer examination

> you find that this elasticity works against monetary policy. The more elastic the supply in a perfectly competitive market of large financial lenders, the more is contractionary policy thwarted. You have to do more to get the same effect.
>
> Let me illustrate that by an extreme case. Suppose that the supply was so elastic on the part of all commercial banks, insurance companies and other institutions that you could not get any change of the interest rate. You see that the peg of the Federal Reserve System would then be replaced by the peg of the private free market and, therefore, there would be no leverage for you to tighten on borrowers. So, we have to go to a different aspect of this argument, which is a more subtle one, and is an ancient one, but has been resurrected in recent years---and I think properly so---namely, that the market for borrowing funds is an imperfectly competitive one. (Samuelson 1952, 695)

Samuelson's criticism is somewhat opaque in the sense that it doesn't address separately the behavior of lenders in the government securities market and its relationship with their behavior in the loan market. Furthermore, he discusses a general idea of sensitivity or responsiveness of lenders that is not the same as Roosa's: while for Samuelson this sensitivity is the usual price elasticity of supply, in Roosa's argument the sensitivity of lenders was about their behavior under uncertainty regarding the prices of government securities. Samuelson didn't take into account the uncertainty in the government securities market that Roosa insisted so much on.

The loan market is imperfect, Samuelson pointed out, mainly because of the nature of the good that is being transacted; some lenders may be bigger than others, but "monopolistic impurities" are secondary and what matters is the fact that getting a loan is the result of a negotiation with the banker and there is a great deal of uncertainty involved in the process (Samuelson 1952, 696).[21] For Samuelson, the loan market always displayed some degree of imperfection and thus there would always be some degree of credit rationing. He didn't explain why banks wouldn't immediately change the interest rates on loans to compensate for the effects of monetary policy, instead simply stating that "[t]here are good reasons why in the short run in an imperfectly competitive market you will not change your charges but simply increase the frequency with which you arbitrarily say 'No' to people" (1952, 697).

Samuelson's conclusion was that, if monetary policy acted through lenders, it was effective only in so far as it increased the degree of imperfection, thus increasing credit rationing and decreasing the availability of credit. This extra amount of imperfection, however, could only be temporary. After a rise in interest rates an individual banker would deny some of the loans he would have granted before the policy was put in place, but after a short amount of time he would start granting those loans again, at a higher rate, and the market would return to its original degree of imperfection (Samuelson 1952, 697). The banker would return to the previous degree of credit rationing and simply increase the rates of interest charged to borrowers to compensate for the increased tightness induced by the Federal Reserve. These were rather bad news for the New York view, for it meant that the only lasting effect of monetary policy would be a higher interest rate for borrowers. It also meant that the fundamental question for judging the effectiveness of

[21] "No one can read the future and therefore each lender must necessarily have a different opinion as to the credit worthiness of different borrowers" (Samuelson 1952, 696). Although he is not cited anywhere in the discussion, it is interesting to note that Albert Hart had made a similar remark in his discussion of "capital rationing" (Hart 1940a, 50).

monetary policy beyond the very short-run necessarily depended on the empirical findings of the elasticities of demand of borrowers (Samuelson 1952, 698). While this time he gave a more elaborate argument about the (limited) importance of credit rationing, Samuelson's position was essentially the same that he had already expressed in the second edition of his textbook: credit rationing existed but it went in the same direction of the effects of changes in the interest rate.

Finally, it is also noteworthy to point out that Samuelson didn't build on his previous engagement with the behavior of banks (Samuelson 1945a). Contrary to the testimony, where the banks' behavior in the government securities market is largely neglected, Samuelson (1945a) was essentially an analysis of the importance of bank's holdings of securities and the relevance of changes in the rates of interest. Later commentators referred to this argument in their criticism of the "lock-in" effect and the New York Fed's view (e.g. Smith 1956) but Samuelson didn't.[22] Other than a passing dismissal of the relevance of the lock-in effect, which was based on factual experience and not theory (Samuelson 1952, 740), Samuelson didn't mention the issue during his testimony.

4.2. Mixed views about the importance of credit rationing

It is important to note that Samuelson's published testimony is not a verbatim transcription of his speech during the Patman hearings since participants had the opportunity to elaborate on their testimonies before final publication. Samuelson in fact sent a summary of a revised version of his testimony to several economists asking for comments, and admitting that he had purposely overstated the problem of the short term relevance of credit rationing. The responses that he got show that credit rationing was quite complicated to grasp and opinions about its actual

[22] The so-called "lock-in" effect referred to the banks' unwillingness to sell securities at a loss, not so much because of the magnitude of the loss itself, but because of an idiosyncratic unwillingness to show losses on their balances.

significance for the effectiveness of monetary policy varied.[23] Among the economists that Samuelson corresponded with was Howard Ellis, who seems to have played a particularly important role in the final content of Samuelson's published testimony. In his letter to Ellis Samuelson wrote:

> I felt it desirable to elaborate explanations on a few points. The enclosed represents the principal additions.
> You will note that the insert on imperfect competition aspects on banking grew out of the few words we had together after the session. I am grateful to you for calling to my attention the need to elaborate my position.[24]

In his response Ellis included a summary of what he understood to be Samuelson's view—judging from the summary he received—and it would seem that he understood Samuelson's use of the expression "imperfect competition" to mean that the issue was about monopoly or oligopoly problems. Ellis insisted on the fact that credit was not an uniform good and thus credit-worthiness was important, on the fact that credit rationing was always present although its stringency was variable, and on the fact that interest rates were "rather inflexible" in the United States.[25] Thus, Samuelson's initial version seems to have been less clear about the character of the imperfect competition that he thought was important. Unfortunately, the original summary that was sent out was not present in the correspondence available in Samuelson's papers, so it is difficult to see exactly how important were Ellis' remarks. Given the similarity of the published testimony and of Ellis' position, however, it is clear that he played an important role in the final outcome.

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[23] PASP, box 59, Patman Testimony contains letters, of varying length and substance, from Bach, Chandler, Despres, Ellis, Friedman, Haberler, Hansen, Musgrave, Roosa, Shaw, Tobin, Wallich, and Williams. Copies of the letters sent to Seymour Harris, Arthur Smithies, and Herbert Stein are also present, although their responses are not. Samuelson also sent the summary of this testimony to Paul Douglas and received a rather insubstantial response. See PASP, box 26, Douglas Correspondence.

[24] Samuelson to Ellis, April 11, 1952, PASP, box 59, Patman Testimony.

[25] Ellis to Samuelson, April 30, 1952, PASP, box 59, Patman Testimony.

George Lee Bach, for his part, believed that Samuelson had raised an important issue, especially given "the easy acceptance of the 'availability' doctrine...in the last few years."[26] He was, however, not fully convinced by Samuelson's reasoning because he had implicitly assumed that the Federal Reserve would continue to buy securities, even if at a higher rate of interest, during a tight-money policy. The "meaningless sequence" denounced by Samuelson was not meaningless at all in this case. Furthermore, Samuelson's assumptions about the behavior of commercial banks and the reestablishment of the previous level of stringency were not "unreasonable" but they weren't "obvious" either. In fact, Bach emphasized:

> I see no convincing evidence that interest rates are the primary equilibrating mechanism in the imperfect markets between bankers and commercial borrowers; indeed most of the evidence seems to me to point in the direction of the capital rationing phenomenon being the more important one, with changes in interest rates rather more a reflection of the tightness of the market than an equilibrating mechanism between bankers and borrowers.[27]

Lester Chandler considered that Samuelson had not taken into account the importance of the expectations that could arise regarding the future behavior of the Federal Reserve. He stressed the importance of understanding the issue as a dynamic one, instead of a static situation in which the Federal Reserve simply raised the rates by any given amount "and then left the impression that the rate would remain constant at the new higher level." For Chandler, "some of the effects of expectations...collectively may be quite important," although he also recognized that he wasn't sure exactly how important they could be.[28] Similarly, Gottfried Haberler pointed out that he did not understand why the effect of credit rationing could not

[26] Bach to Samuelson, May 7, 1952, PASP, box59, Patman Testimony. This is, to the best of my knowledge, one of the first uses of the expression "availability doctrine," with which Roosa and the New York Fed's view would come to be identified with. See also Scott (1957a).

[27] Bach to Samuelson, May 7, 1952, PASP, box 59, Patman Testimony. It is worth noting that Roosa, in his review essay "The revival of monetary policy" (1951b), considered Bach's book *Federal Reserve Policy-Making* (1950) as following the same line, even if Bach was in some respects somewhat behind, that was championed at the New York Fed.

[28] Chandler to Samuelson, April 30, 1952, PASP, box 59, Patman Testimony.

be more prolonged, adding as well, however, that he did not know about the subject and that "[s]ome expert in the field of banking should be able to enlighten us on these matters."[29]

Emile Despres, who had worked at the New York Fed during the 1930s, believed that "the effects of tight money on the availability of credit are more persistent than you suggest, perhaps even permanent or nearly so." However, he also thought that this was nothing to celebrate for it disturbed "the orderly flow of goods through the production process."[30] Milton Friedman was of the same view, pointing out that credit rationing was a defect, but he was on the opposite side regarding its actual importance, which he considered to be "grossly exaggerated."[31] Wallich's response was particularly prescient and noted the difficulty economists had in understanding the New York Federal Reserve's argument:

> I think you have put your finger on an important weak spot in the "availability" doctrine. This doctrine says in effect that price is not a function of quantity, but merely a symbol. The interest rate goes up as the volume of credit is reduced, but in no specifiable proportion. This proposition is very hard to swallow for economists and needs to be reformulated somehow so as to make it fit within our framework.[32]

He considered that Samuelson's idea of explaining this using changes in the imperfection of the market was promising, but that it would need to be developed further. He thought that an "analysis of how bankers maximize their various advantages by this change in imperfection" would need to be developed. This is precisely the route that would be taken by economists during the late 1950s and the 1960s, during which credit rationing emerged as a microeconomic issue in the

[29] Haberler to Samuelson, May 7, 1952, PASP, box 59, Patman Testimony.

[30] Despres to Samuelson, April 20, 1952, PASP, box 59, Patman Testimony.

[31] Friedman to Samuelson, April 21, 1952, PASP, box 59, Patman Testimony. Milton Friedman did not address the New York Fed's view or the issue of credit rationing during his speech at the Patman Hearings.

[32] Wallich to Samuelson, May 8, 1952, box 59, Patman Testimony, PASP.

economics literature, studied on the basis of rational, profit maximizing banks (Jaffee 1971, chap. 2; Jaffee and Stiglitz 1990).

4.3 The Roosa Paradox

Samuelson wasn't the only one to find Roosa and the New York Federal Reserve's ideas troublesome or counterintuitive. Dennis Robertson (1953) also expressed his puzzlement regarding the mechanisms described in Roosa (1951a) and stated the existence of a "Roosa Paradox." Robertson, who had also contributed a chapter to the Williams volume, wrote Roosa asking him about what he considered a curious asymmetry:

> Now of course the testimony of persons of your experience and Mr. Sproul's that things do happen in this way is enormously impressive and hard to resist. But prima facie it seems extremely paradoxical that two sets of persons living in close physical contiguity and subject to the same psychological atmosphere (perhaps even to some extent consisting of the same persons working in different capacities), should react in such opposite ways—viz. (to repeat) that the demanders of credit should be predominantly influenced by the fact that its price has risen, the suppliers [lenders] by the expectation that it is going to rise further. If it is so, it is so, —but it seems a bit odd it should be so!.[33]

Roosa admitted that Robertson's observation "had kept him thinking for some time." He accepted that it was correct to assume that some borrowers could in fact be influenced by changes in the rates of interest and not the level of the rate of interest alone, but his answer was also very pragmatic:

> By making explicit a dichotomy which I had left vague, you have brought out a serious weakness in my presentation. I think that, in part, the paradox which you find so disturbing is actually a true reading of present supply and demand conditions in our money markets here. But also, in part, the situation which you intuitively suspect ought to be the case does exist.[34]

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[33] Robertson to Roosa, July 24, 1951, JHWP, box 180638, Correspondence Coombs - Roosa.

[34] Roosa to Robertson, August 22, 1951, JHWP, box 180638, Correspondence Coombs - Roosa.

While Robertson recognized the Roosa's expertise in the subject[35] and accepted the latter's explanation to the asymmetry he had pointed out, he still considered the fact that the interest rates could affect lenders and borrowers in a different way to be a paradox—the Roosa Paradox. Furthermore, he also pointed out that even if one accepted that holders of securities were influenced by their expectations about the rate of interest, it was hard to see why they didn't sell now if they expected their prices to sell even further? (Robertson 1953, 141). Roosa and Sproul's argument simply said that in the presence of uncertainty lenders would think harder about converting their holdings of securities into other commercial loans, but there was no further elaboration on the reasoning that led them to behave this way: their argument was based on them having observed this behavior in their professional experience.

4.4 Roosa and Samuelson

As I mentioned above, Samuelson also sent Roosa the summary of his testimony and explicitly acknowledged Roosa's chapter in the Williams volume had been an important influence on his thinking on the subject.[36] In his response, Roosa briefly sketched some qualifications to Samuelson's argument. His main point was that the effect of monetary policy could be of longer duration than Samuelson assumed, and that there was room for a repeat if lenders weren't sure about where the market for government securities would "hit bottom" if they tried to unload their holdings of these assets to grant new credits. Roosa insisted that it was this type of uncertainty that was fundamental for the argument he was making, something he would later repeat in print (Roosa 1952a, 256–57). In the end, however, Roosa stated that he mostly agreed with the core of Samuelson's argument:

[35] "It will be understood that I am not presuming to dispute the conclusions of such an obviously first-hand authority; but I do think they contain an element of paradox which needs dragging out into the light" (Robertson 1953, 139).

[36] Samuelson to Roosa, April 11, 1952, PASP, box 59, Patman Testimony.

> In any event, whether this qualification or some of the others that occur to me should actually bear up under the scrutiny of a closely reasoned analysis, I think the main body of your argument will still hold. when we do get the opportunity to talk these things through I feel reasonably sure that we will not find ourselves very far apart in the end.[37]

The real difference between Samuelson and Roosa was the lens through which each one of them looked at the problem. Samuelson could not fully accept Roosa's argument about the behavior of lenders and its market outcome. As he pointed out to Hansen, it was wrong to focus only on the fact that major lenders could not unload on the government securities market without depressing the price to an unknowable extent. This might be an obvious case in the new scenario where the Fed was not publicly bound to keep the rates pegged and due to the fact that the dealers' market was very "thin," but Roosa left out the behavior of the other thousands of small banks:

> Here is where Roosa goes wrong: he says that the banks as a whole cannot sell bonds unless the Fed buys them; he somehow thinks that each of the 15,000 small banks will therefore hold in its sales because it realizes how narrow the eye of the needle is in the 6-dealer bond market. This is all wrong: each small bank can try to sell, and it is attempts to sell that depress prices not transfer to securities outside the bank. Even for the big fellows, Roosa is relying on a thin day-to-day reed.[38]

The New York Fed's view that Roosa presented incorporated their firsthand experience with the market for government securities in their assessment of the effectiveness of monetary policy. And, as Roosa's response to Robertson also made clear, it reflected their reading of a very specific context, and of the behavior of lenders and the market for government securities in such a context. Roosa relied on his experience and was concerned with policy above all, and Samuelson was well aware of this:

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[37] Roosa to Samuelson, April 29, 1952, box 59, Patman Testimony, PASP.

[38] Samuelson to Hansen, January 29, 1954, box 59, Patman Testimony, PASP.

> Roosa's best defense, and he has made it in a letter to me dealing with related points, would be this: "Central Bankers live from day to day. They needn't wind (sic) permanent victories. In the long-run we're all dead. The important thing is to take advantage of each short-run and benefit from what you, Samuelson, dismiss lightly as a transient."[39]

Interestingly, some evidence suggests that despite his reservations, Samuelson came to accept that Roosa and the New York Federal people were onto something, even if they themselves could not fully understand it or convey it in an adequate way. This is clear, for example, in Samuelson's criticism of chairman Martin's "Bill's only" policy, which the New York Federal Reserve officials criticized for reducing the scope of open-market operations to the short-term segment of the market for government securities. Samuelson sided with New York, pointing out that:

> Lessening the 'Fed''s power to create uncertainty in the minds of the men in the market is to rob the New York Federal Reserve writers of one of their choicest weapons. While I am not sure that Allan Sproul, Robert V. Roosa and John H. Williams themselves always knew quite what they meant when they preached the virtues of creating uncertainty, this device may have its place in the Central Banker's arsenal and should not be thrown away or limited without careful consideration." (P. Samuelson 1956, 1470 n1)

Similarly, in the fourth edition of his *Economics*, Samuelson indicated that the size of the government debt was not necessarily a problem for monetary policy, noting that "[m]any experts, such as Dr. Robert V. Roosa" considered it made possible "extensive open-market operations of a stabilizing type and tends to enhance the effectiveness of monetary policy" (P. Samuelson 1958, 353). And, last but not least, it was Samuelson who recommended Roosa for the position of Treasury Undersecretary for Monetary Affairs to president Kennedy, on the grounds that "as far as technical competence and brightness and everything else, he's one of the best young central bankers in the world" (Ackley et al. 1964, 159). Roosa was only 43 years old when he left the New York Fed; apparently, had he been older he might have been named Secretary (*ibid.*).

[39] Letter to Hansen, January 29, 1954, PASP, box 59, Patman Testimony.

6. Conclusion

In this paper I have discussed the views on the effectiveness of monetary policy of the New York Federal Reserve officials and of Paul Samuelson. The New York view, as presented mainly by Roosa (1951a), was rooted in their experience as participants in the money and government securities markets, and it was meant to apply to the specific circumstances present in the United States at the beginning of the 1950s. Roosa's presentation was particularly critical of the usefulness of previous theories of the effectiveness of monetary policy that exhibited important "gaps between concept and reality," ignoring the specific details and functioning of the financial markets in the United States since the creation of the Federal Reserve. In the New York Fed's view, uncertainty was the key for managing lender's willingness to lend, restrain credit, and keep inflation in check. This type of reasoning was not enough for Samuelson, who at the time was not a big believer in the effectiveness of monetary policy and had previously shown contempt for the views of market participants. In his testimony for the 1952 Patman Hearings, Samuelson subjected the New York view to a preliminary analysis based on the behavior of individual, rational agents and concluded that this view crucially rested on an assumption of credit rationing by banks. Contrary to what subsequent commentators have said, Samuelson did not reject the compatibility of credit rationing with rational behavior (Baltensperger 1978, 171; Kashyap and Stein 1994, 225), nor did he restrict the existence of credit rationing to the short-term only (Jaffee 1971, 22–23; Baltensperger and Devinney 1985, 478). As he had already done in the second edition of this *Economics* (1951), Samuelson acknowledged the existence of credit rationing, but denounced the lack of a good explanation for its existence and for the importance that he considered it had in the New York Fed's view.

When the problem of credit rationing became a theoretical problem discussed in economics journals during the late 1950s and 1960s Roosa did not participate in the discussion. The path opened by Samuelson with his question about proving how individual, rational behavior could be compatible with an equilibrium with credit rationing was outside the scope of his original argument. The correspondence between Samuelson and Roosa shows that the former was never fully convinced of how Roosa and the New York Federal Reserve officials arrived at their conclusions regarding the effectiveness of monetary policy or the role of uncertainty. However, Samuelson seems to have accepted that the New York Fed view had something valuable, even if Roosa wasn't willing or able to put in term he and other economists could clearly understand.

III. MACROECONOMETRIC MODELING AND THE SSRC'S COMMITTEE ON ECONOMIC STABILITY, 1959-1963

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1. Introduction

The Committee on Economic Stability (CES) of the Social Science Research Council (SSRC) played a central role in consolidating large-scale macroeconometric modeling as a scientific practice at the frontier of macroeconomics during the 1960s.[2] The Committee was behind the initial work (1960-1963) in the construction of the model that set the bases for the Brookings Quarterly Econometric Model of the United States (1963-1972) on the one hand, and of the FRB-MIT-Penn model (1966-1974) on the other. In this paper, we use the Committee's records to document its work on the construction not only of a specific large-scale macroeconometric model of the United States, but also of the practice of macroeconometric modeling, which marked the way of doing macroeconomics in the subsequent decades. This project brought together top talent from academia,

[1] This is a slightly modified version of a paper originally prepared for the *Œconomia* conference on Economics and Public Reason, University of Lausanne, May 3-5, 2018.

[2] Following Thomas Stapleford (2017, 6), we understand practices as "collections of behavior that are teleological, subject to normative evaluation [and that] exhibit regularities across people in a constrained portion of time and space." Furthermore, a scientific practice contributes to the generation or sustainment of "formal knowledge that makes truth claims." The Social Science Research Council was stablished in 1923 and played a mayor role in channeling resources from private foundations like the Rockefeller and Ford foundations in to research into social and behavioral sciences (see Hauptman 2006 and Worcester 2001). The line between a large-scale and a not-so-large-scale macroeconometric model is not clearly defined, but there was a substantial increase in the number of equations present in the models developed in the 1960s, which quickly surpased the 100 equation mark while the biggest models of the 1950s were only at 15-20 equations.

government agencies, and private research organizations providing bridges of communication among these institutions and a common roof for those interested in the opportunities offered by macroeconometric model-building. Furthermore, as the first model-building enterprise of this size, the project's many challenges in terms of logistics, data, and computing capacity, evidence the importance of configuring a specific institutional and material context necessary to develop this new scientific practice.

In the second section of the paper, we provide a detailed account of the establishment of the Committee, which was set in motion by the motivation of several economists to understand the instability of the United States economy. Their main project during 1959-1963 was the construction of a macroeconometric model, whose constrution is discussed in the third section. In the fourth section we discuss several aspects of the model project that help us understand the challenges that needed to be overcome during the model project.

2. The establishment of the Committee on Economic Stability

The establishment of the Committee was the result of an SSRC Conference on Economic Instability held on June 17-19, 1959 at the University of Michigan. According to R. A. Gordon's account, he and other economists at the SSRC interested in the possibility of creating a committee on business cycle research proposed the conference to "get a group of economists to talk about whether such a committee seemed wise" (Gordon 1975, 31; 1959, 38). Gordon, then at the University of Berkeley, and Paul Webbink, from the SSRC and who would later oversee and handle the administrative paperwork of the Committee, were joined by 17 economists, 7 of which had positions outside academia: among the participants of the conference were people from the Brookings Institution, the Board of Governors, the Council of Economic Advisers (CEA), the National Bureau of Economic Research (NBER), the Joint Economic Committee of the Congress, and the National Planning

Association.[3] Table 1 contains the complete list of participants (column 6) and their affiliation.

Gordon opened the discussion with a short paper in which he briefly introduced the topics which would be discussed at the conference, centered on understanding what was known about the instability of the US economy and whether there were fundamental differences between pre and postwar business cycles.[4] Most notably, however, he began his remarks by noting the lack of relevance of the models put forth by theoreticians, the disconnection between theoretical work and actual policy questions, and the number of concrete questions that needed answering. Geoffrey Moore (NBER), Bert Hickman (Brookings), and James Duesenberry (Harvard) also presented papers that looked in detail at the characteristics of the cycle and at the changing role of specific elements in making the economy more or less stable (e.g. fiscal policy, financial distress, and the so-called automatic stabilizers that had been put in place in the postwar).

The paper presented by Duesenberry—co-authored by Gary Fromm (Brookings) and Otto Eckstein (Joint Economic Committee)—had been specifically commissioned by the organizers of the conference and was, notably, the only paper that contained an econometric model.[5] As Duesenberry, Eckstein, and Fromm (DEF) put it, their purpose was to consider what sources of instability were still present in the economy and to give an indication of their quantitative importance. They presented their model as follows:

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[3] See the list of participants, SSRC1, box 145, folder 801. The list in the SSRC's records shows that Paul McCracken (University of Michigan) and Arthur Burns (NBER) were originally included in the list but did not attend the conference.

[4] "Notes for the SSRC Conference on Economic Stability," SSRC1, box 145, folder 801.

[5] "Stability and instability in the American economy," SSRC1, box 145, folder 801. A revised version of the paper later appeared in *Econometrica* with a different name, "A simulation of the United States economy in recession" (Duesenberry et al., 1960). Duesenberry was the one initially asked to contribute the paper, and Eckstein and Fromm joined in afterwards. As Gordon puts it, the paper's role was to provide something "to seek our teeth into at the beginning" (1975, 31). Note the Harvard connection between the authors of the paper: Fromm and Eckstein had obtained their PhDs at Harvard and Duesenberry was a professor there.

> In order to test some of the stability properties of the American economy in recession, we have constructed a model which seeks to reflect the effectiveness of the much-vaunted automatic stabilizers, as well as the feedbacks of a downward spiral through consumption and through the reaction of inventories. We have endeavored to keep the model simple, yet provide it with sufficient detail that it can give a fair reflection of the reality of recession; the resultant model is considerably more complicated than the traditional multiplier model, but the concepts which it employs are strictly macro-economic and, on the whole, the same as the concepts used in piece-meal short-run analysis of business conditions. The model is also constructed in such a way that it can be employed as a policy model; tax rates, unemployment benefit rates, and autonomous expenditure levels are explicit parameters.[6]

It was a model purposefully limited to analyzing the behavior of the US economy in a recession and that, by including explicit policy parameters, allowed the authors to carry out fiscal policy experiments. Their conclusion, after carrying out several of these policy experiments, was that fiscal policy and the automatic stabilizers (e.g., unemployment insurance and automatic rate changes in income taxes) made it "possible to reduce the instability of the system considerably, and at relatively little financial cost to governments." However, the automatic stabilizers did not guarantee that the economy would "work itself completely out of a recession," and it was clear for DEF that "actions on the part of the government" were necessary for the economy to return to full employment.[7]

Prices were held constant in the model and DEF explicitly recognized that they had no theory of price change. DEF also left out monetary policy almost completely—mentioning it only in passing and very briefly—and focused on the behavior of inventory investment, treating non-inventory/fixed investment in a narrative way in another section of the paper, and limiting themselves to indicating "some of the major connections between private investment and the other variables in the system."[8] Clearly, as DEF acknowledged, the model had important limitations, but their 100-page paper did a great job in showcasing the type of questions that could

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[6] "Stability and instability in the American economy," 24.
[7] *Op. cit., 43, 98.*
[8] *Op. cit.*, 90

be investigated with a macroeconometric model and the opportunities for quantitative analysis of the cycle that such a model created.[9] At the same time, it allowed for a comparison between this approach and that of the NBER.

Geoffrey Moore's paper[10] used a very different methodology to discuss the characteristics of the 1957-1958 recession relative to previous ones. He proceeded by presenting the evolution of various series of data, shown in several tables and graphs, that allowed him to characterize each cycle and make comparisons among them. He concluded that many features were the same, including the duration, the severity, the scope, the shifts in the composition of output, the early decline in profit prospects and investment commitments. However, some characteristics of the 1957-58 recession were different: it had shown less financial distress, more stability of personal income and prices of commodities, and a more unstable rate of interest. Moore pointed out that although these were important differences, what was known about business cycles was still relevant. It was, however, important to "guard against over-simplification in the use of historical-perspective." He listed three aspects in particular: 1) "Don't confine comparisons to the immediately preceding recession, or even the last two;" 2) "Don't confine comparisons to an average of preceding cycles; and 3) "Be aware that current developments can fall outside the range of previous experience, but use that range as a guide to help avoid the biases we are all heir to."[11]

Hickman,[12] for his part, presented an informal discussion of the determinants of private investment in postwar United States, considering different sectors as well as the role of financial elements and monetary policy. Hickman's was, in fact, the paper that conceded most space to monetary policy considerations, concluding that while financial causes of instability had been greatly reduced—thanks mostly to deposit

[9] As Klein put it later, DEF's model played an important role in "the whetting of the appetites" for a large-scale macroeconometric model (Klein 1975, 13).
[10] "Some reflections on the 1957-58 recession and recovery," SSRC1, box 145, folder 801.
[11] *Op. cit.,* 35
[12] "The Determinants of Private Investment in the Postwar Economy, SSRC1," Box 145, folder 801.

insurance and to the fact that the Federal Reserve had done a decent job during the postwar—the problem of the timing of monetary policy and the challenge of handling both employment and price stability simultaneously were not going to go away anytime soon.

The summary of the discussion[13] shows that there was an active debate around each of the papers presented, not only on the specific elements that were considered to contribute to the stability or instability of the postwar economy of the US but also on the methodological and organizational aspects of carrying out research on this subject. The latter was a key reason for the organization of the conference and so participants had been asked to

> give serious thought in advance of the conference to [their] views about the present state of research on economic stability and instability, to questions such as the adequacy of the statistical data on which analysis must perforce be based, and to suggestions regarding the improvements in this range of research which you believe could and should be attempted.[14]

The existence of different, but not necessarily exclusive methodologies for analyzing business cycles was clear for the participants: there was the NBER's or "historic" method on the one hand and the econometric or "Keynesian" method on the other. Gardner Ackley (U. Michigan) is reported to have summarized them in the following way:

> He contrasted the method of Mitchell and the National Bureau of Economic Research, where the emphasis was on the timescale, with the Keynesian method, where the emphasis was on discovering functional relationships. The Keynesian method had been effectively refined during recent years: much data had been collected on consumption, and in the area of

[13] Discussion summary, SSRC1, box 145, folder 801. Individual opinions reported in the summary should be taken with a grain of salt, however. Interoffice correspondence indicates that while the summary would be kept for the record, it would not be sent out to the conference participants or others because it contained inaccuracies which would take a lot of work to correct and the gain from doing so may not justify the effort. SSRC inter-office correspondence, Sept 22, 1959, SSRC2, box 151, folder 1721.

[14] Webbink to the participants in the conference, May 11, 1959, SSRC1, box 145, folder 801.

> investment several experiments had been made with acceleration models and capital-accumulation models.[15]

Ackley also suggested combining both approaches by comparing the actual cyclical behavior of selected variables with the behavior produced by the estimated functional relationships and then trying to account for the difference. On a similar vein, Duesenberry is reported to have admitted the inability of the econometric method to account for structural changes and to have pointed out that

> it would be useful to integrate the econometric with the historical approach. Although the former might explain most of the observed variance, the latter could throw light on those characteristics of the cycle which combined to produce an unexpected turn of events.[16]

Also important were the data needs. In fact, the participants made remarks on the need for better and new series of data on specific variables, and for setting up procedures for making data more easily and widely available. The conference concluded with a vote in favor of the establishment of a committee at the SSRC that would fulfill several functions. As reported in Gordon's summary of the conference (1959, 39) for ITEMS, the SSRC's magazine, these functions were to:

1. Facilitate the coordination of research.
2. Help integrate current research methodologies.
3. Facilitate the collection and publication of needed data, particularly by the Federal Government.
4. Serve as a channel of communication and a facilitating agency in the field of research on problems of economic instability.

Three remarks must be made regarding these functions. The first function was specifically geared towards helping researchers working on econometric models

[15] Discussion summary, p.13, SSRC1, box 145, folder 801. Note that Ackley did not draw the distinction between the NBER and the Cowles Commission.
[16] *Op. cit.*, 3, 7.

come together. It highlighted the need for taking stock of the research available in order to avoid duplication of work and to channel efforts into disaggregation (*Ibid.*). Furthermore, Gordon pointed out that

> [i]n this way econometric business-cycle research could have much more of a cumulative effect than has been true in the past, when each investigator has started largely from scratch. It might also be possible to secure agreement on the main features which need to be built into these econometric models. (*Ibid.*)[17]

Second, it should also be noted that a fifth function, not reported in Gordon (1959) but included in the summary of the conference discussions, was that of providing information to policy-making agencies of the government.[18] Specifically, the summary reports that Henry Wallich (CEA) emphasized "the value that the model-building project could have in providing government agencies with policy recommendations" and that Duesenberry "said that simulation experiments with a model could easily be made to provide policy implications." However, and this might explain why this function did not appear in Gordon (1959), the summary also reports that "[t]here was some debate on the question of whether the task of providing recommendations for current policy would conflict with the basic research objectives of the project."[19] Unfortunately, there is no further record of the specific points that were advanced against this function during the conference.

Finally, another key difference between the discussion summary and Gordon (1959) is that the idea of building a more disaggregated model seems to have been in the air at the conference, although it is not explicitly mentioned in Gordon (1959). In the discussion summary, Duesenberry is reported as talking about the necessity to make an effort to synthesize the work being done at the time, which "would permit

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[17] In the discussion summary of the conference Duesenberry is reported as emphasising this particular point.

[18] The other four functions that the committee would fulfill were also reported in the summary of the conference discussion. The main differences are that the wording in Gordon (1959) is different and no individual points of view are communicated.

[19] Discussion summary, 15-16, SSRC1, box 145, folder 801.

the construction of a model with a considerable degree of disaggregation." Wallich's reference to a "model-building project" further suggests that the construction of a larger model was explicitly discussed by attendants. Gordon (1959, 39), however, reports only that a working conference would be a good first measure to bring together the people working on this type of research.

In any case, the proposal for the establishment of the Committee on Economic Stability was accepted in September of 1959 (Gordon 1959, 39),[20] and the initial members of the Committee were recruited in the following months. Table 2 shows the Committee's members during the early 1960s. It is noteworthy that, although Klein was one of the founding members of the Committee, he did not attend the 1959 conference and it would seem that his participation in the project was not initially guaranteed. In fact, the records show that it was considered that he might not be interested in joining the project, and the names of Irwin Friend and Robert Eisner were put as alternatives in case Klein declined the invitation.[21]

3. The macroeconometric model of the Committee on Economic Stability

> The chief value of a Committee like yours, which can presumably tap both talent and money in quantity, provided it knows how to use them, should not be to encourage small jobs of the horse-and-buggy type. Rather it should try to think of those very large enterprises which

[20] See the "Proposal for committee on economic instability," Sept 12, 1959, SSRC2 Box 151, folder 1721. In the end, though, the last word of the committee's name was replaced by "Stability," SSRC inter-office correspondence, Sept 22, 1959, SSRC2, box 151, folder 1721.

[21] SSRC inter-office correspondence, Sept 22, 1959, SSRC2, box 151, folder 1721. Although we have not been able to locate sources to clarify Klein's potential negative decision, it would make sense that his participation was not guaranteed. Indeed, Klein had had a promising but fleeting trajectory as Lecturer and director of the Quantitative Economics Research Seminar of the University of Michigan from 1949 to 1954. However, he decided to leave Michigan in 1954 and to join Oxford University only to return to the US in 1959 as a Professor of the University of Pennsylvania. His departure from Michigan was prompted by the pressure exerted by McCarthyism, the House of Unamerican Activities, and the accusations Klein received from some members of the University of Michigan such as accounting Professor William A. Paton for his short membership to the Communist Party in the mid-1940s. We ignore if Klein was invited to the conference, but it is very likely given his importance in the field. However, it wouldn't be a surprise if he had decided to decline the invitation given the presence of people like Paton at Michigan. For a detailed account of this episode see Pinzón-Fuchs (2017, chapter 2).

> individuals and small groups have rejected in the past, or perhaps never even considered, because they seemed too formidable. (Abramovitz to Webbink)[22]

Moses Abramovitz's comments on the potential of the Committee, though directed towards a different type of project—a big survey of private firms—captured well the possibilities that the existence of the Committee opened up and that would materialize with the construction of the macroeconometric model. Indeed, the Committee's activities started taking shape right after its official establishment, and at its first meeting it was decided that Klein and Duesenberry would carry out a summer institute on econometric models.[23] A group led by Duesenberry and Klein met in New York in February of 1960 to start planning the summer institute and discuss the type of model they would like to eventually produce. The group concluded that

> we want to produce a system that will be jointly useful in forecasting and policy formation. At first we should concentrate on a model of the ordinary business cycle of 8-10 years' duration. Disaggregation ought to be carried to the point where needs of policy makers are served. In government work, housing, the motor industry, agriculture, foreign trade, and finance must be treated separately as areas of policy action. Business would be interested in the maximum possible degree of disaggregation. They would be especially interested in material on inventories. We do not plan to go into regional work now, but we might have someone take up this question at our summer seminar.[24]

Thus, despite the apparent debate that took place around the idea of building a model for policy analysis at the Michigan conference, this clearly entered into their considerations of the type of model they wanted to produce. Furthermore, they were also interested in building a model that businesses could potentially find useful, thus leaving the door open to offering services like the University of

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[22] Nov. 24, 1959, box 151, folder 1721.
[23] Minutes Washington conference, December 28, 1959, SSRC1, box 147, folder 810.
[24] Meeting minutes, Feb 24, 1960, SSRC1, box 147, folder 810. A subsequent letter by Klein makes a couple of corrections based on comments by Moore: the typical duration of the cycle according to the NBER is 4-5 years, and the Adelmans' simulations of the Klein-Goldberg model favored this length; the model should aim at forecasting or identifying turning points. See Klein's letter of March 21, SSRC1, box 147, folder 810.

Michigan's Annual Economic Outlook Conference had been doing since the mid 1950s. The concern for the business public is not mentioned explicitly in subsequent minutes or correspondence of the Committee but, in any case, the concern for both policy and business usefulness evidences the importance of extra-academic interests in the Committee's macroeconometric project.[25]

At the February meeting, it was also decided that the summer institute would last six weeks and that it would take place "at a quiet retreat" where specialists would discuss the construction of the model. These plans were subsequently altered and it was agreed that two seminars would be held instead, at Dartmouth College, during the summers of 1961 and 1962.[26] The idea behind this summer institutes was to get all the people involved in the same place, thinking about the common project, and to give them enough time to actually work. The first summer institute would serve as a stepping stone of the project and it was expected that each of the specialists selected to contribute to the model would "come to the seminar with an historical summary of work done in [his] sector and [with] suggestions for new formulations."[27] Furthermore,

> [p]ersons selected to contribute papers and work at the seminar would be instructed to approach their problem with as few preconceptions as possible and to be ready to include as many variables as possible in the first instance. The overall coordinator would have the main task of trimming the parts to sizes that would fit in a workable scheme.[28]

A preliminary list of personnel—presenters and discussants for eleven sectors as well as an intervention on the statistical method and another on a historical

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[25] Many of the people involved in its work would later participate in the business of commercial econometric models. Klein, for example, would go on to lead the Wharton Economic Forecasting Associates, while Eckstein and Fromm would later establish and work at Data Resources Incorporated.

[26] Meeting minutes, April 30, 1960, SSRC1, box 147, folder 810. It's unclear why they chose Dartmouth College as the location for the summer conferences. It should be noted, though, that Dartmouth had an active group of faculty and undergraduate students working on making computers easier to use, whose work led to the development of BASIC and the Dartmouth Time-Sharing System.

[27] Meeting minutes, Feb 24, 1960, SSRC1, box 147 F810.

[28] *Op. Cit.*

summary of cycles—was agreed upon at an April meeting.[29] The steering committee of the SSRC, the Problems and Policy Committee, approved the econometrics model proposal in May[30] and in July Klein addressed his fellow Committee members with news about the project: A subcommittee composed by Duesenberry, Klein, Moore, Avram Kisselgoff (Allied Chemical Co.), and David Lusher (CEA) had been appointed to "deal with the problem of constructing an effective new econometric model of the USA."[31] Klein emphasized again that they wanted their model to have the "widest possible degree of acceptance" and that they were thus "approaching the problem with no fixed ideas on the design or scope of the model."[32] He also elaborated on the path that lay ahead for the project:

> Specialists will be expected to work during the academic year preceding the first summer session, summarizing as much as possible of the known econometric material for his designated area. This material, together with positive suggestions by each author, will be discussed at the first session. During the following year, specialists will be expected to work on data for their sectors to be presented in a more final form at the second summer session. In this stage, data will be made mutually consistent and the forms of relationships studied will be chosen so as to fit with other contributions.[33]

Naturally, this type of project would require ample funding. The National Science Foundation (NSF) provided a $105,000 two-year grant, plus a $20,000 extension in 1963. The cover letter of the original proposal, sent by the SSRC's president, Pendleton Herring, mentioned that the team would "evaluate critically" the econometric work done on specific sectors of the economy and "establish a basis for a *generally acceptable* model of the economy."[34] The Board of Governors of the Federal Reserve was also contacted, but their reception of the project was

[29] Meeting minutes, April 30, 1960, SSRC1, box 147, folder 810.
[30] Webbink's letter to Gordon, May 27, 1960, box 151, folder 1721.
[31] July 13, 1960, SSRC1, box 147, folder 810.
[32] *Op. Cit.*
[33] *Op. Cit.*
[34] Herring to Riecken, Oct 04, 1960, SSRC2, box 151, folder 1721. Our emphasis.

lukewarm. They were happy to let Daniel Brill[35] collaborate with the project, but Governor A. L. Mills felt it was not appropriate for the Board to

> finance an outside organization in a project of this kind. If the project had the promise that seemed to be expected from it, it would in a sense be similar to the Talle Subcommittee studies that the Board undertook several years ago pursuant to Congressional request, and if that were the approach the study perhaps should be focused entirely in the Board.[36]

While the Board did not commit any funds at the time, it did authorize the staff to discuss the matter further with the Committee. Webbink reported that

> [f]urther discussion with Jack Noyes [Director of the Board's Division of Research and Statistics] has made it clear that getting financing from the Federal Reserve would require a more specific statement of plans and anticipated results. It would probably be better to err on the modest side of this rather than on the expansive side, but it might also be necessary to make some contention that what will be accomplished is something that the Federal Reserve otherwise, sooner or later, would have to do, or at least ought to do, with its own staff. [37]

The idea of obtaining funds from the Board was eventually dropped on the ground that it was very uncertain and that convincing its members would take too much work.[38] In addition, both Webbink and the members of the Committee were quite confident that they would obtain the funding from the National Science Foundation, as it effectively happened.[39] It is interesting to note, however, that there would seem

[35] At the time Brill was an Associate Adviser at the Division of Research and statistics, and had been chosen as the specialist in charge of the monetary sector. Brill had arrived at the Board of Governors in the late 1940s as part of the team led by Morris Copeland to build the flow-of-funds accounts, and become the director of the Division of Research and Statistics in 1963. He played an important role in the introduction of macroeconometric modeling at the Federal Reserve and supported the development of the Federal Reserve-MIT-University of Pennsylvania model (see chapter five).

[36] Minutes of the Board meeting of September 23, 1960, 4ff. The minutes of the Board meetings are available at https://fraser.stlouisfed.org/title/821. The only explicit objection recorded in the Board minutes is by Governor A. L. Mills Jr. Since no contrary points of view were raised, we can assume that there were no strong opinions in favor or against the project other than the favorable words mentioned regarding the prestige of the SSRC and of the economists associated with the project.

[37] Webbink to Gordon, Oct 05, 1960, SSRC2, box 151, folder 1721.

[38] *Op. Cit.* See also Gordon to Webbink, Oct 10, 1960, SSRC2, box 151, folder 1721.

[39] On the approval of the NSF grant see Fouraker to Klein, June 16, 1961, SSRC2, box 151, folder 1721. There is no further mention of the project in the Board's minutes before the December 12, 1960

to be at least some agreement between both parties–the Committee and the Board–regarding the potential usefulness of the project. Furthermore, Webbink's proposed strategy of emphasizing the inevitability of the project is an example of the push for the use of quantitative tools for policy analysis that was part of the Committee's ethos.

A planning meeting took place in February of 1961 and participants in the summer institute got to interact with each other and plan their contributions for the summer. The minutes of the meeting report that "[t]he main point taken up [...] was the division of the economy into sectors, and an attempt to reach some preliminary agreement within the group on the work to be done by each sector specialist in preparation for the first summer seminar."[40] A discussion initiated by Edwin Kuh led to the reaffirmation that their objective was to build a model that was useful for solving policy questions and not a pure forecasting model. This was followed by a discussion about the type of variables that would be needed to build a model that shows the flexibility with which the model as a whole was being considered:

> There was a general discussion of the approach to policy uses. Jim Duesenberry suggested that we make up a list of policy variables and be sure that each sector specialist includes some work on these if relevant to his sector. Franklin Fisher noted that our list should include a number of policy variables suggested by intuition and theory. Karl Fox raised the question of the influence of the end use of the system on the degree of disaggregation, and the choice of *targets* or *instruments*. We agreed generally that the system should, at first, be left open so that any variables of potential importance could be included.[41]

The discussions of the specialists about their individual sectors also show how their work constrained and was constrained by the rest of the model. For example:

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meeting (pp. 4-5). By then the project had already been funded and the matter was closed at the Board.

[40] Meeting minutes, Feb 3, 1961, SSRC1, box 147, folder 810.

[41] *Op. cit.*, 2-3. The terms "targets" and "instruments" were emphasized in the original. Note that they correspond to Tinbergen's usage.

> Dan Brill, who will study the money market, raised important questions as to the coordination of this work with that in other sectors on saving (business saving and personal saving including residential construction) and with the financial variables that appear in the various behavior equations of the system. From the other sectors, we listed the following possible financial variables: consumer credit terms, mortgage rates and terms, long term rates, short term rates, share prices, gold stock, foreign liquid balances, and corporate balances. This brought Dan's work into focus. At the same time we asked of him a precise statement and listing of the control variables in the monetary sector. He plans to study these under the broad headings of treasury debt management, open market operations, reserve requirements, and government corporations. We asked him to show how specific control variables under these general headings are (structurally) related to the monetary variables appearing in the equations of the other sectors of the economy.[42]

3.1 The Dartmouth conferences

The first of the two summer institutes took place at Dartmouth College during August 7-25, 1961. The meeting brought together the team of researchers directly involved in the model project, as well as some guests and research assistants, to discuss the reports that had been written since the February planning meeting and to start structuring the model. Table 1 (columns 7 and 8) lists the participants in the two Dartmouth conferences. As Klein noted in his summary of the conference, it was a format that enabled the functioning of the new model-building strategy that the Committee was inaugurating:

> The subcommittee recommended a new approach to model building. The limited scope of most other efforts in this field can be attributed to the fact that they have basically been "one-man" jobs. At best a small, closely knit research team with not more than one or two or three principal investigators have undertaken the task of constructing an economy wide model. The subcommittee suggested that a large research group be assembled for periodic meetings with private research being conducted by individuals between meetings [...] Instead of the small, closely knit research team, we decided upon *a federation of major research projects* united at periodic conferences and held together by two coordinators.[43]

[42] *Op. cit.*, 3.
[43] "The Dartmouth Conference on an Econometric Model of the United States," August 7-25, 1961, SSRC1, box 147, folder 810. Our emphasis. A slightly reduced version of this summary appeared in ITEMS as Klein (1961).

Such a format allowed "investigators to meet in a common discussion where each separate research effort could be adjusted towards fitting in a systematic whole," a necessary counterbalance to the main disadvantage that such a team effort implied:

> [T]he possibility of heterogeneity and lack of complete research discipline and coordination [are the main disadvantages]. Each separate investigator may be inclined to attach too much importance to many small points within his sector. These small points may not be significant when considered from the point of view of an over-all model of the economy.[44]

Klein also highlighted the broad knowledge of the participants:

> [W]e had an unusual array of talent. Each person knew economic theory, statistical theory, and realistic description of behavior associated with his own sector. Many of the people knew other sectors well, and criticism was highly constructive. New ideas about model construction came out of the discussions.[45]

The discussions were productive and actual work—even if still exploratory in nature—was carried out during the conference. The reports prepared by the researchers, many of which contained exploratory calculations, were complemented with additional calculations made on the computers of the Dartmouth College and of the Board of Governors. Data, however, was clearly a pressing constraint on the project, and Klein's summary evidences the importance of being in contact with people from the agencies that could alleviate such constraints:

> Data problems arose frequently, and we discussed practical means for obtaining assistance in getting necessary data from government agencies. The visit of George [J]aszi to our conference in connection with other meetings of the Committee on Economic Stability was fortunate. We were able to discuss with him the obtaining of special series for our purposes from the National Income Division of the Department of Commerce.[46]

[44] *Op. cit.*, 4.
[45] *Op. cit.*, 3.
[46] *Op. cit.*

The issue of the level of disaggregation was particularly important. Aggregate variables like “government expenditure” or the “supply of money” would be replaced by variables that represented actual variables relevant for monetary of fiscal analysis, like the military and industrial construction activity or the reserve requirement on time deposits. This type of finer analysis was one of the main objectives of the project, but dividing sectors into subsectors could easily get out of hand, and explanatory variables included in a particular sector might need to be endogenized in the model as a whole. In the end, it was agreed that they would work on three increasingly disaggregated models, setting a 30-sector model as the arrival point, although they considered it was unlikely that they would get there by 1962.[47] Following the conference, researchers were expected to work on preliminary versions of their sector models: it was “expected that participants [would] appear next year at the research conference with a tentative set of equations for [their] sector[s] and series of prepared data.”[48] Duesenberry and Klein, in their role of coordinators, would be in charge of writing the proposed models of the whole system for each of the three levels of disaggregation.

A preliminary outline of the aggregative model was ready by November 1961,[49] and an interim meeting took place on February 22-23, 1962 at the Brookings Institution.[50] Reports on individual sectors were presented and the initial aggregative model was modified to incorporate the researchers’ new work. Researchers were asked to send Charles Holt and Franklin Fisher—who were in charge of studying the properties of the model and the details of its estimation,

[47] This discussion on the level of disaggregation of the model led to the development of a way to combine input-output matrices with more traditional econometric modeling, a distinguishing feature of the Brookings model (Bodkin et al. 1991, 99).
[48] *Op. cit.*
[49] Klein to Webbink, Oct 19, 1961, SSRC2, box 151, folder 1721. See also Klein’s letter of November 1, 1961, SSRC1, box 147, folder 810
[50] Minutes, Washington meeting, Feb 23-24 1962, SSRC1, box 147, folder 810.

respectively—information on the mean and variance of their series, preliminary OLS estimations if they had them, or their "best guesses" if they did not.[51]

The second Dartmouth conference took place during August 6-17, 1962 (Klein 1962). As for the previous year's meeting, Klein's summary emphasized the importance of the criticism offered by the team of researchers on each individual sector. And, again, actual work got done, but this time there was much more to work with in terms of preliminary results:

> For the 1962 conference we had nearly complete presentations of single-equation least-squares estimates of the relations that will be taken into account in the aggregative model. Some of these were known in crude form at the 1961 conference; some were available at our interim meeting in February; but most were put before the group for discussion for the first time this past summer. (Klein 1962, 39)

As a result, a clearer picture of the structure of the model as a whole emerged. While there were still "some loose ends in the system," and some equations and identities had not yet been decided upon or adequately specified, the team was able to put together a "nearly complete" flow diagram of the aggregative model (Klein 1962, 39; see Figure 1).[52] The exact number of equations would depend on how loose ends and the especification of individual equations were dealt with. The aggregative model was around 100 equations and the next stage in disaggregation would take it to around 300 equations, the main difference being the number of production sectors included in each version. Although modifying the model, improving or augmenting it in any sense, was a complicated job potentially involving work on several sectors of the model, Klein "hope[d] to have a *living* model that will be kept up to date, continuously improved, and explored for the possibility of incorporating

[51] *Op. cit.*

[52] The diagram was also presented at the December meetings of the Econometric Society, unfortunately it wasn't present in the SSRC records. The version of the diagram included as a Figure 1 comes from Hickman (1965). This is a previous version of the diagram included in Duesenberry *et al.* (1965) and presented here as Figure 2. These flow diagrams likely played a role in helping the team make sense of the model and as an explanatory device for outsiders.

further sector detail" (Klein 1962, 38). This, however, was the work to be undertaken in the next stage of the model project:

> At the conclusion of the second summer conference we held an organizational meeting, at which it became clear that the participants in our project had indeed found a fruitful basis of research cooperation that we want to continue indefinitely into the future. With the two models being planned, we still have much to do and can readily conceive of specialized or more refined studies along the same lines continuing as far ahead as we care to look. We therefore agreed that the committee should seek means of perpetuating its project and continuing our joint research effort. We do not plan to meet in the summer of 1963 but will reconvene as a group when a model has been fully estimated and applied.
>
> By 1964 or 1965, we should be ready for this stage. We agreed that a permanent research base should be sought for the model, where it could be maintained and extended by a small permanent staff. In that case special projects could be undertaken by members of the larger research team, and periodically the group as a whole could consider the entire model. The group would include the present collaborators, but additions or retirements would be possible. (Klein 1962, 40)

3.2 The Brookings model

The next home of the model would be the Brookings Institution. Although it is unclear from the available records whether any alternative site was seriously considered, Klein provided an explicit argument in favor of choosing the Brookings Institution over a university to host the project:

> We selected the Brookings Institution as a highly desirable site because it removes the model from any particular school of thought in economics. The diversity of views among the members of our team and the wide acceptance throughout the profession that we are seeking for this model suggests that it should not be in any particular university where it may eventually become dominated by a small group of economists who tend to think along similar lines. We want it near data sources, and we want it in an establishment with known research

> facilities. To us, the Brookings Institution seemed to be an ideal locale, and we were pleased to have an enthusiastic reception for this idea from the Brookings staff. [53]

In any case, many members involved in the Committee's work would continue to be involved in this new stage at the Brookings Institution. Most notably, Klein would be the principal investigator of the project, Fromm would be the project's staff director, and Duesenberry would chair an advisory committee that would help steer the project.[54] At first sight, this transition to the Brookings Institution seems to have been an easy one, but judgment should be reserved until more research has been done on this new stage of the model at Brookings.

After the second Dartmouth conference, the team worked hard to "get the model into shape" before handing it over to the Brookings Institution.[55] Thus, by September of 1963, the team had centralized the source data, transferred individual series to punch cards, and had estimates the most of the equations in individual sectors. Estimates of the small model as a whole, however, were expected to be available only by December of the same year. Many of the individual sectors had been presented at academic conferences and a volume describing the work carried out from 1961-1963 was being put together.[56] This is the volume that would later be published as "The Brookings Quarterly Model of the United States" (Duesenberry *et al.* 1965). Despite the name, this volume encapsulated mainly the work undertaken by the Committee's project members between 1961-1963. In particular, the volume assembled the papers written by the sector specialists—discussed and reworked during and since the Dartmouth conferences—and offered a version of the aggregative model of around 150 equations.

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[53] See the NSF grant proposal "An econometric model of the United States economy," January 30, 1963, 7, BIA. The grant proposal notes explicitly that Klein wrote the section from which this quote was taken. Klein's argument is also noteworthy because both the Brookings model and a version of the FRB-MIT-Penn model would end up being housed by Pennsylvania's WEFA in the 1970s.
[54] *Op. cit.*, 10ff.
[55] Klein to Webbink, January 23, 1963, SSRC2, box 151, folder 1722. Since the original NSF grant ended on July of 1963 the Committee had to ask for, and effectively got, a new grant to support the work during the summer. See Klein to Webbink, May 29, 1963, SSRC2, box 151, folder 1722.
[56] See the "Report on the econometric model project of the Committee on Economic Stability, Social Science Research Council," SSRC1, box 147, folder 811.

As Duesenberry and Klein pointed out in the introduction to the 1965 volume, each paper could "stand on its own merits as a piece of independent research," but, taken as a whole, the work presented in the volume represented a "complete model which 'explains' the variations in *GNP* and its major components, as well as major price movements, employment, and wage rates" (Duesenberry and Klein 1965, 3). Furthermore, the "general outline of the model reflect[ed] a consensus [...] on the best set of working hypotheses about the nature of the economy" (*ibid*). To be sure, the work on the model had not ended, and an important message conveyed in the volume's introduction was that it presented "only the first stage of a continuing effort" (*ibid.)*. The new stage of the model at the Brookings institution showcased an important element of macroeconometric modeling as a practice, which involved the continuous, collective, and institutionalized working and reworking of the model as a whole and of its sectors, of the specification and re-specification of its equations, and of the estimation and re-estimation of the parameters, taking into account new information and research about the economy as it became available. In this sense, the objective of the project was not to "produce once and for all a fixed model of the American economy," but to "bring together in a continually [and collectively] revised model all tested research results in the field of aggregate economics" (Duesenberry *et al.* 1965, vii). The model project provided a flexible, yet systematic "place to put things"—all the "knowledge about fine-grained sectors" produced by economists—thus contributing as well to making econometric work a cumulative process (Duesenberry and Klein 1965, 9).

Further analysis of the development of the model's new stage at the Brookings Institution is beyond the scope of this paper. Suffice it to say that years of intense work laid ahead for the team and were documented in several Brookings Institution volumes describing the subsequent work done on the model. The journey would not be easy nor would it go as expected. As Griliches's (1968) highly skeptical review of the 1965 volume showed, there was much work left to do to convince economists that such a large-scale project could produce useful results. The Brookings stage of the project, therefore, should be the subject of further study.

4. Towards the consolidation of a practice

In the last two sections we have documented the establishment of the Committee and its work on the macroeconometric model project during 1959-1963. With this general overview as a basis, we can now comment further on some of the project's characteristics that should be taken into account to understand the consolidation of macroeconometric modeling as a practice during the 1960s.

4.1 Managing the Federation of research projects

The Committee's model project was in a different league of its own regarding size and complexity. Klein's work during the 1940s and 1950s at the Cowles Commission and at the Univesity of Michigan was an important reference point at the time, but these models were significantly smaller: Klein's 1950 mark III model had 15 equations and the Klein-Goldberger model of 1955 had 20. [57] In particular, there was no precedence for a project involving more than 20 researchers, located in different types of institutions and in different geographical places, whose work had to be steered towards a common goal. The Committee's project was not just a technically difficult enterprise, but its logistic challenges were significant as well.

And yet, there is practically no evidence in the Committee's records of any major personal, logistic, or administrative problem regarding the model-building project nor any of the other activities of the Committee.[58] To be sure, the paper trace of

[57] Klein's own work was of course inspired by.the work of Jan Tinbergen, who had built a model of the Dutch economy and another one of the US economy in the mid-1930s. For an account of Tinbergen's work see Morgan (1990, chapter 4) and Boumans (1999). For a detailed history of the origins of the Cowles Commission see Grier (2005). See Christ (1956; 1994), and Hildreth (1985) and Morgan (1990) for a discussion of the influence of the Cowles Commission in the history of economics. See Pinzón-Fuchs (2017, chapter 2) for a detailed account of Klein's development of macroeconometric models at the University of Michigan.

[58] While the model-building project was by far the main activity of the Committee during the early 1960s, other possible projects were continually being discussed and a conference on quantitative policy analysis was organized on August of 1963 (see below).

discussions on these matters can never be perfect and it is certainly possible that critical matters might have been dealt with in person or by telephone. Furthermore, our sources have so far been mostly limited to the Committee's archives and it is possible that the archives of individual participants might allow for an alternative interpretation. In any case, the lack of evidence of any major problems in the internal correspondence of the Committee is noteworthy given the scale and complexity of their project.

There are a couple of factors that might explain why the Committee and the macroeconometric model project ran with relative smoothness. The first factor has to do with the important institutional infrastructure that supported the Committee: the SSRC's support, embodied in Paul Webbink, who oversaw the Committee's activities. The SSRC managed the grant funds of the Committee's projects, reducing the administrative burden on researchers. Furthermore, by providing information and at times demanding information from researchers, sending out reminders, and occasionally offering logistic advise, Webbink and the SSRC provided important administrative expertise that certainly contributed to the adequate working of the Committee. At the same time, working as an SSRC's Committee possibly helped the team obtain the funds needed to carry out their work, although more research is needed on the National Science Foundation's funding of economics projects during the 1950s and 1960s to throw light on this issue.

A second important characteristic was the summer institutes format that the Committee adopted. The two summer institutes at Dartmouth College, during 1961 and 1962, were essential for the progress of the project and allowed researchers to come together under the same roof for a period of time long enough to allow for actual and communal work to be done. It is difficult to say whether the "federation of research projects" approach would have worked without such opportunities for the whole group of researchers to come together and work on the model. While the Committee did meet at least a couple times a year—sometimes after meetings of the American Economic Association, sometimes at the Brookings Institution in

Washington, and sometimes at the SSRC's offices in New York—it would have been impossible to schedule (and afford) regular meetings for more than 20 people. It is also unclear, however, that such a different format would have actually improved the Committee's functioning, since the effectiveness of the summer institutes consisted in the provision of enough time to individual researchers to carry out work on their own and then to discuss it with the rest of the team for a couple of weeks. The origin of the summer institute idea is, unfortunately, not clear. We cannot tell yet with certainty whether this was an original idea or if it was adopted from previous experiences in economics or other disciplines, or from previous experiences in other SSRC committees.

Yet, some characteristic elements of the functioning of the Committee probably found their inspiration in Klein's experience at the Research Seminar in Quantitative Economics (RSQE) of the University of Michigan, and in the Conference on the Economic Outlook organized yearly since 1953 (Pinzón-Fuchs 2017, chapter 2). The weekly RSQE was "really based around this project team research effort" (Klein and Goldberger 1955, 1), and was understood as an ongoing project where Klein and Arthur Goldberger built what came to be known later as the Klein-Goldberger model. The Economic Outlook Conference was the yearly event attended by economists from different companies where the use of earlier versions of the model was unveiled, providing informative forecasts about the behavior of the economy for the following year. Daniel Suits describes the way in which the seminar worked in the early 1950s:

> The seminar would tool up in September when the students arrived and the assignment was to take the model apart and see where it had functioned poorly last year and what should be done about it to improve it, with the notion that come the second or third week of November [...] somebody had to stand up in front of that Conference on the Economic Outlook and produce a forecast from this model.[59]

[59] Suits, quoted in Brazer, "The Economics Department of the University of Michigan: a centennial retrospective," TUMA, box 5, p. 143.

Finally, the personal qualities of the people involved must also be taken into account. Besides Webbink's outstanding role, the two chairmen of the Committee during our period of interest, Robert Gordon and Bert Hickman, seem to have done things right in terms of supporting ongoing research and keeping communication fluid with Webbink and the rest of the Committee members. As an experienced team worker and a coordinator of the model project, however, it is Klein that probably deserves the most praise in this respect. Duesenberry's involvement in the project seems to have been intermittent at certain times, and the fact that almost all of the correspondence related to the model in the Committee's archives is from Klein strongly suggests that he was the main overseer of the project.[60]

4.2 The role of the NBER

As already mentioned in section two, George Moore of the NBER participated in the 1959 Michigan conference,[61] presenting a paper that showcased the NBER's approach to the analysis of business cycles. And participants to seem to have considered that the two available approaches, the "historical" and the "econometric"—showcased by Duesenberry, Eckstein, and Froom—could complement each other. Thus, the establishment of the Committee seemed like a good opportunity for the collaboration between the NBER and the econometricians to flourish. Alas, this collaboration does not seem to have really taken up, at least in any direct way. In fact, although Moore was a member of both the Committee and

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[60] Duesenberry's intermittent participation is the only noteworthy difficulty that appears in the Committee's records, but even this does not seem to have been a major cause of concern, most likely because Klein provided a sturdy backbone for the project. Duesenberry's apparent lack of commitment to the project emerged most notably in the discussion to choose a replacement for Gordon when he announced he had to cut back on his responsibilities due to an illness. Webbink made his appreciation of Klein's work explicit: "[L]et me say [...] that the economics community owes you a very large quantity of gratitude for the skill and devotion with which you have directed the project, and that I am personally grateful for the experience of dealing with someone so thoroughly responsible and systematic. I hope that your graduate students are absorbing these qualities from you." Webbink to Klein, August 5, 1963, SSRC2, box 151, folder 1722.

[61] It is interesting to note again that the list of participants in the Committee's records points out that Arthur Burns was also invited but did not attend. See SSRC1, box 145, folder 801.

the macroeconometric model Subcommittee,[62] he did not play a major role in shaping the agenda of the Committee and did not contribute to the model project beyond the first Dartmouth conference.

The only project that Moore presented for consideration by the Committee was the co-sponsoring of a "time series encyclopedia" that the NBER wanted to prepare and publish.[63] Moore considered that the Committee's involvement would make it easier for the NBER to find the funding necessary for the project, and the Committee agreed it was a valuable initiative. The NBER did, however, organize a conference on economic planning in 1964, a year after the Committee organized a conference on quantitative policy analysis that, incidentally, did not include anyone from the NBER.[64] If there was ever a potential for complementarity between both institutions it was not being developed through these projects.

Regarding the macroeconometric model, at the February 24, 1960 meeting it was decided that it would be useful to have a contribution on the "historical summary of the main features of individual cycles" for the next year's summer institute.[65] It was initially considered that Abramovitz would work on this, but by the time of the planning meeting of February 3, 1961, that task was most likely going to be taken up by Moore. According to the minutes of the meeting, Moore's paper "would indicate the type of features that ought to appear, explicitly or implicitly, in our final model if it is to give a faithful representation of American business cycles, as measured by the National Bureau."[66] Moore effectively participated in the first Dartmouth conference, but there was no contribution on this topic at the second Dartmouth conference. Similarly, no chapter dealing with a historical analysis of the US

[62] The records show that he attended the meetings of the Committee and the Subcommittee regularly for 1959-1961, that he did not attend the two Subcommittee meetings of 1962, but that he did attend again the 1963 Committee meeting.
[63] See the minutes of the meetings of December 28, 1959, and December 28, 1960, SSRC1, box 147, folder 810.
[64] The proceedings of the NBER conference were published as (Millikan 1967); the proceedings of the Committee's conference were published as Hickman (1965a).
[65] Meeting minutes, SSRC1, box 147, folder 810.
[66] SSRC1, box 147, folder 810.

business cycles was included in Duesenberry *et al.* (1965a), although the possibility of validating the model by comparing its output against the NBER's characterization of business cycles was very briefly mentioned by Charles Holt (1965, 640) in his chapter on the simulation work done on the model thus far. It is possible that this was due to the fact that initial simulations of the complete model were not available before the project was handed over to the Brookings Institution, but no historical chapter was included in the Fromm and Taubman (1968) volume or in any of the subsequent volumes describing the work on the Brookings model. It is also important to note that while the initial thought was that the NBER's approach could *complement* the results of the econometric model, the relationship quickly passed to seeing the NBER's work as a way to *validate* and not extend or complement the econometric model. This should be seen as a continuation of the debates held between Klein and Friedman in the late 1940s and early 1950s on the role that NBER methods, such as "naive-models" should play as standards to measure the performance of large-scale macroeconometric models, notably in the context of Carl F. Christ's (1951) work at the Cowles Commission.[67] It could also have been a reaction to the work done by Irma Adelman and Frank L. Adelman (1959) on the Klein-Goldberger model.[68]

4.3 The connection with government agencies

The Committee's model project was successful in bringing together academics and people from government agencies. This was important on at least three fronts: the expert knowledge that these people brought to the project, the direct access to the data from their agencies, and the potential use that these agencies could make of the results of the model. All of these elements contributed to carrying out the project in line with the goals laid out by the Committee, and also with its ultimate aim of increasing the understanding of the *actual* US economy. Since we lack sources related to the planning of the 1959 Michigan conference, we do not know how the contact with these government agencies and researchers was initially made, but

[67] See Pinzón-Fuchs (2017, chapter 4) for a discussion on the debate between Klein and Friedman.
[68] Holt (1965) cites their work.

looking at the academic background of the participants (see table 1) we can see that all but one[69] of the five government-affiliated participants held a PhD. Similarly, and although our data still has some gaps, we can see that at least four (out of seven) of the government-affiliated participants at Dartmouth 1, at least three (out of seven) of the participants at Dartmouth 2, and four (out of five) of the government-affiliated contributors to Duesenberry *et al.* (1965) held PhD. degrees. We should be cautions when assesing this information due to the variability in time and in university departements of the PhD degrees, but this information suggests that the Committee established a connection with the people who could understand the technical discussions involved in the model project, or that at the very least were interested in hearing about them. Thus, it would seem, the actual connections between the model building project and government agencies were built through these technically oriented staff and not directly with people high up the decision-making ladder. This was certainly the case with the Board of Governors, whose representatives in the project were the future director and staff members from the Division of Research and Statistics. Sherman Maisel, who was the expert in charge of non-business construction and became a Board Governor in 1965, was a Professor at UC Berkeley during the period he was involved in the Committee's model project.[70]

The participation of some members of the staff of the Council of Economic Advisers in the Committee's activities is particularly interesting given the Council's high standing and influence. Both Henry Wallich and David Lusher attended the 1959 Michigan conference, and Lusher became the expert in charge of the Government revenues and expenditures sector.[71] The Committee approached James Tobin and Walter W. Heller early on with a rather open invitation to discuss and see if the

[69] It is not clear whether Louis Weiner effectively graduated from his Ph.D. at Harvard.

[70] Maisel later supported the use of forecasts as part of the decision-making process at the Federal Reserve (see chapter five).

[71] Lusher worked with Louis Weiner on this sector and they participated in the two Dartmouth conferences. The chapter for Duesenberry *et al.* (1965) on this sector, however, was written by Albert Ando, Cary Brown, and Earl Adams, Jr. The Treasury helped Lusher in his work and Klein was glad they were showing interest in their work. Klein to Webbink, July 1962, SSRC2, box 151, folder 1721.

Council would be interested in the Committee's work, getting an enthusiastic response from both of them.[72] It would seem that a meeting took place on May 17, 1961 but unfortunately there is no further evidence about any other contacts with the CEA before 1964.

The Committee and the model project also established an interesting relationship with the Department of Commerce. Not only was the Department—as the producer of the national accounts—a major source of data, but a group of their officials was interested in obtaining help from the Committee in kick-starting its own econometric research group at the Department's Office of Business Economics (OBE).[73] The OBE had taken up and updated Klein's quarterly model (Klein 1964), and had the intention of doing further work on econometric policy analysis. Researchers at the Department wanted the Committee to help them guide their research agenda and find adequate personnel.[74] This is another clear example of the type of technically oriented people from government agencies that were attracted to the Committee's activities.

Finally, the organization of a 1963 conference on quantiative planning should also be considered. As we discuss in Acosta and Pinzón-Fuchs (2019), another set of members of the Committee organized a conference aimed at showcasing the experiences of Japan, France, and the Netherlands with various forms of quantiative policy analysis. This was a calculated effort to publicize the rigurouness that quantitative methods could bring to policymaking among US economists and government officials, and as such it renforced the connections built between the Committee and various government agencies during the model project.

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[72] See Gordon's memos of April 7 and April 19, 1961, as well as the minutes of the Committee's meeting of December 28, 1960, SSRC1, box 147, folder 810. Tobin had been initially considered as a candidate to take over the work on consumption for the model. It would seem that he was officially invited, and declined, but there is no further evidence on this in the Committee's records. See Klein's letter of invitation to collaborate on the model project, July 13, 1960, SSRC1, box 147, folder 810.
[73] Gordon to Webbink, August 28, 1961; Gordon to Webbink, October 16, 1961, SSRC2, box 151, folder 1721.
[74] See the minutes of the meeting between the OBE team and the Committee, November 5, 1963, SSRC1, box 147, folder 811.

4.4 Data, estimation, and simulation

Econometric models need data, and one as large, complex, and disaggregated as the Committee's needed a lot of it. The importance of data for this particular project had been foreseen since the 1959 Michigan conference and the government agencies that collaborated with the Committee's project played a key role in supplying it. Yet, not all the series that the model team needed existed at the time and so an important part of the project's work went into producing new data series, in particular for their preliminary work on the disaggregate, 30+ sector model. Thus, series of employment, wages, capital stock, GNP, and price deflators for each of these sectors were produced (Klein 1962, 40). Even if the existence and availability of data had not been an issue, the massive amount of data alone presented some important challenges for the team. In particular, to be able to estimate the complete model it would be necessary to have all the data series available in one place. This involved getting source data from all individual researchers and transferring it to punch cards or magnetic tape. An initial process of centralization of both data and preliminary estimates of individual equations took place at the University of Pennsylvania during 1962-1963. Similarly, once the project was handed over to the Brookings Institution, a similar central repository of data was created, [75] although it was still far from the experience of integrated data retrieval and analysis that modern software allows.

The model, of course, had to be estimated as well. This represented an important challenge and Franklin Fisher played a key role in this aspect of the project. Every individual researcher could provide ordinary or even two-stage least squares estimates for their sectors, but this was a preliminary result since estimates of the parameters were likely to change once the model as a whole was estimated. The estimation of the model as a whole, however, was difficult given the high degree of interdependence and the extremely low amount of observations relative to the

[75] See Klein (1962, 39) as well as the "Report on the econometric model project of the CES, SSRC," September 26, 1963, SSRC 1, box 147, folder 810.

number of variables and lags.[76] Fisher worked on evaluating the most adequate estimation methods for such a system and in the implementation of a recursive-block strategy that allowed portions of the model to be estimated independently without sacrificing consistency.

Finally, for the model to be useful for policy analysis—a key goal of the whole project—the computer program SIMULATE was devised at the University of Wisconsin that could solve and simulate the model (Holt 1965). The program was built in parallel to the model, and thus Holt used previous, smaller models to test and improve the program during the 1961-1963 stage of the project. Unfortunately, we have been unable to locate a copy of the manual of the first version of program SIMULATE, but we do have the manual of the second version, Program Simulate II (Holt et al. 1967). A careful analysis of the development of the software and its functions is beyond the scope of this paper, but we would like to point out at least a key element related, again, to the size of the model: an important feature of the program was that it allowed to automatically find the recursive-block structure needed to solve and simualte the model. Keeping track of changes in equations and variables may seem like a basic function, but there is a big difference between doing this for a basic IS-LM model, a 15 or 20 equation model, and a 100 or more equation model.[77] In the same way that electronic computers made it possible to invert large matrices in a reasonable amount of time, this type of software also made it possible to handle the modifictaion of large-scale models in similarly practical and less time-consuming way.

5. Concluding remarks: Towards a history of empirical/applied macroeconomics

[76] Duesenberry and Klein (1965) and Fisher (1965) explain in detail the challenges involved in the estimation of the model.

[77] "Anyone who has tried to study a sizeable model of an economy or other complex system is impressed with the volume of sheer dogwork involved in manipulating the model and data, and in obtaining the mathematical solutions of large systems of nonlinear equations" (Holt et al. 1967, 1).

The history of macroeconomics is usually told in terms of both schools of thought, key authors, texts, theories, ideas, and methodological dicta, and of the policy conclusions derived from these theories and ideas (e.g., Snowdon and Vane, 2005; De Vroey, 2016a). This view of the history of macroeconomics, although enlightening and perhaps pedagogically useful, has traditionally downplayed the importance of applied work done by practicing macroeconomists, as well as the institutional context in which this work was carried out and the tools needed to do so. An interesting and somewhat paradoxical characteristic of this approach to the history of macroeconomics is that, while New Classical Macroeconomics is shown to emerge out of a criticism of large scale macroeconometric models, it is the IS-LM model that is put at the center of the narrative regarding the Keynesian developments in the 1940s and afterwards. Left unexplained, the lack of discussion of the type of work involved in the construction of large-scale macroeconometric models—to which Robert E. Lucas's (1976) criticism was pointed at—could be interpreted as implying that it was a straightforward extension of the theoretical, IS-LM-type models produced to interpret Keynes's message.[78] A large-scale macroeconometric model, however, is a different type of object. It takes a different type of work to build and use one, and it is meant to be used to answer much more specific quantitative questions. This type of model, together with the macroeconometric modeling practice that evolved around it, brought about a new way of producing macroeconomic knowledge.

In this paper we have looked at how the macroeconometric model of the Committee on Economic Stability was built. It is a contribution to our understanding of what it took to build a large-scale macroeconometric model in the early 1960s. We have

[78] As historians of econometrics have shown, however, the relationship between theoretical results and estimation was far from simple and involved debates over the uses of econometrics that shaped the development of the economics discipline. Neglecting the role played by large scale macroeconometric models in the decades following the postwar misrepresents the actual practices of macroeconomists at the time, which also contributes to overestimating the relative importance of theory in the evolution of macroeconomics. The history of econometrics and the history of macroeconomics have been usually written without making much emphasis on their interconnections and concomitant evolution. For accounts on the history of econometrics, see for example Morgan (1990), Epstein (1987), and Louçã (2007).

shown how a team of researchers worked together to build the model and overcome the coordinative, administrative, data, computing, funding, and institutional challenges they faced. Further studies are necessary on the subsequent development of the model within the Brookings Institution, which would clarify the actual role played by this model in informing concrete policy decisions. Judging by the subsequent publications, it seems that the Brookings project did not become the infallible tool used to make policy recommendations, but that it became rather a sort of "laboratory" where economists would learn the practice of macroeconometric modeling and how to concretely build a large-scale macroeconometric model (Klein 1975). At the Brookings Institution, economists would have first-hand access to the teamwork and institutional dynamics of such an ambitious enterprise, to the methodological, theoretical, and practical difficulties of putting together a 30+ sector macroeconometric model, and of building in as much detail as was needed. Similarly, more research on the individual trajectory of the participants in the Committee's activities is necessary, for it might allow us to have a better understanding of the way in which networks of economists were built across different institutions, allowing for the dissemination and continuous adaptation and evolution of macroeconometric modeling. The study of these individual figures might also help us understand specific difficulties that do not appear in the records of the SSRC.

Appendix to chapter III

Table 1: People involved with CES activities

Name	Age 1959	Affiliation[1]	PhD		Michigan	Dartmouth		CES Member 1959-65	1965 Vol.
			Institution	Year		1961	1962		
Abramovitz, Moses	47	Stanford	Columbia	1939	Yes	Yes		Yes	
Ackley, Gardner	44	U. Michigan	Michigan	1940	Yes				
Alexander, Sidney	43	MIT	Harvard	1946	Yes				
Conard, Joseph	48	Swarthmore College	Berkeley	1956	Yes				
Denison, Edward	44	Committee for Economic Development	Brown	1941	Yes				
Duesenberry, James	41	Harvard	Michigan	1948	Yes	Yes	Yes	Yes	Yes
Eckstein, Otto	32	Joint Economic Committee, US Congress	Harvard	1955	Yes				
Fels, Rendigs	42	Vanderbilt	Harvard	1948	Yes				
Friend, Irwin	44	U. Pennsylvania	American University	1953	Yes				
Fromm, Gary	26	Harvard	Harvard	1961	Yes	Yes			Yes
Gordon, R. A.	50	UC Berkeley	Harvard	1934	Yes			Yes	
Hickman, Bert	35	Brookings Institution	Berkeley	1951	Yes	Yes	Yes	Yes	
Lusher, David		Council of Economic Advisers	Harvard	1942	Yes	Yes		Yes	
Moore, Geoffrey	45	NBER	????		Yes	Yes		Yes	
Roose, Kenneth	40	Oberlin College	Yale	1948	Yes				
Shaw, Edward	51	Stanford	Stanford		Yes				
Wallich, Henry	45	Council of Economic Advisers	Harvard	1944	Yes				
Webbink, Paul	56	SSRC	No		Yes				
Weiner, Louis	49	Board of Governors, DRS	Harvard	1936[2]	Yes	Yes			
Adams Jr., Earl		Amherst College					Yes		Yes
Ando, Albert	30	U. Pennsylvania	Carnegie Institute	1959			Yes		Yes
Archibald, Christopher	33	London School of Economics	No				Yes		
Babcock, Jarvis		Iowa State				Yes	Yes		
Boissonneault, Lorette	48	IMF	No						Yes
Brill, Daniel		Board of Governors, DRS				Yes	Yes		
Bristol, Ralph		Department of Treasury	Yale	1956			Yes		
Bronfenbrenner, Martin	45	U. Minnesota	Chicago	1939				Yes	
Brown, E. Cary	43	MIT	Harvard	1948			Yes		Yes
Darling, Paul		Bowdoin College	Columbia	1954		Yes	Yes		Yes

[1] At the moment they got involved with the project.
[2] It's unclear if he finished his PhD.

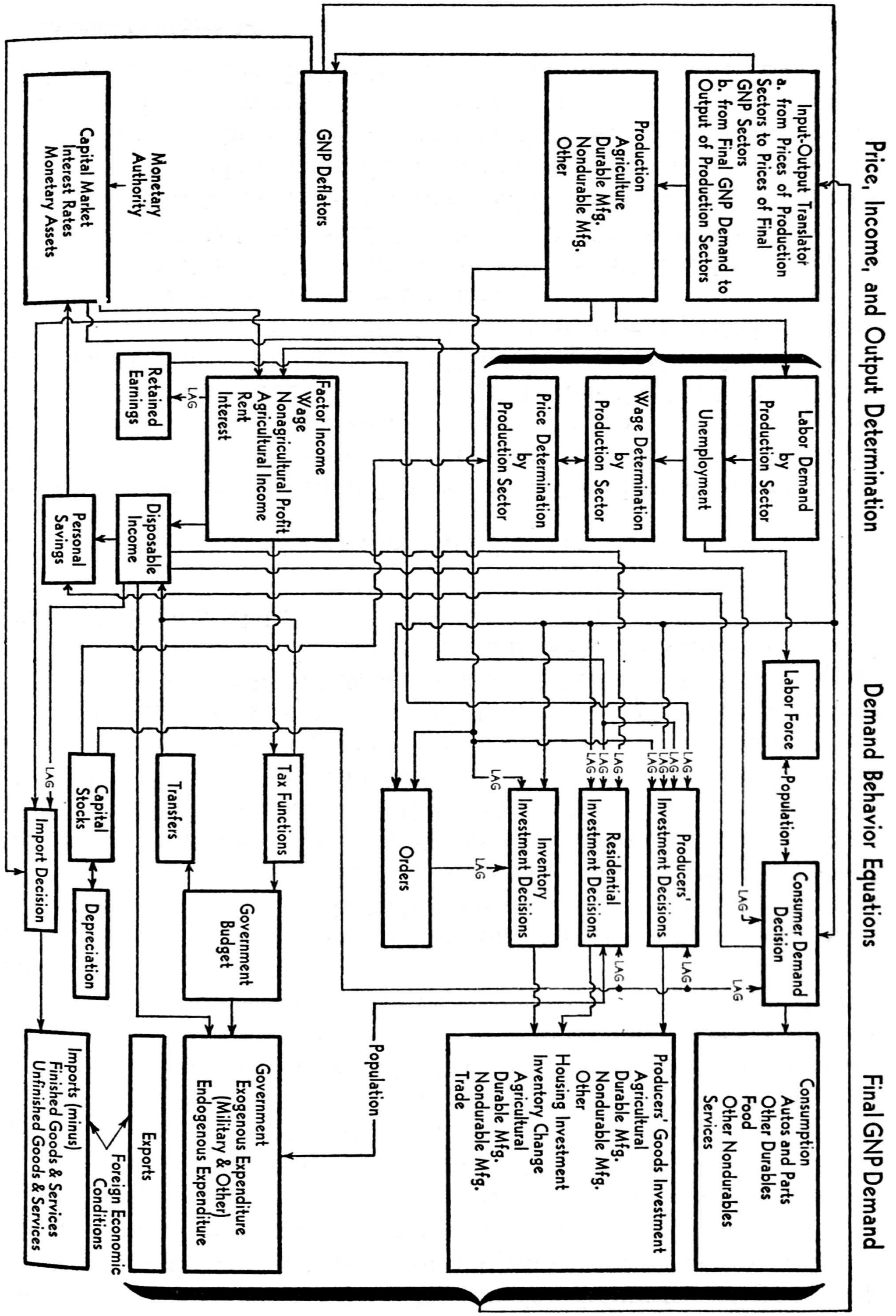
De Leeuw, Frank	29	Board of Governors, DRS	Harvard	1965			Yes		Yes
Dutta, Manoranjan	34	Rutgers	Pennsylvania	1962					Yes
Edwards, Frank		Harvard				Yes			
Eisner, Robert	37	Northwestern	Johns Hopkins	1951		Yes	Yes		Yes
Fisher, Franklin	25	MIT	Harvard	1960		Yes	Yes		Yes
Fox, Karl	42	Iowa State	Berkeley	1954		Yes	Yes	Yes	Yes
Griliches, Zvi	29	U. Chicago	Chicago	1957		Yes			
Hines, Howard		National Science Foundation					Yes		
Holt, Charles	38	U. Wisconsin	Chicago	1955		Yes	Yes		Yes
Jaszi, George	44	Department of Commerce, OBE	Harvard	1946		Yes			
Jorgenson, Dale	26	UC Berkeley	Harvard	1959		Yes	Yes		Yes
Kaitz, Hyman		Department of Labor					Yes		
Kisselgoff, Avram	52	Allied Chemical Co.	Columbia	1950		Yes	Yes		
Klein, Lawrence	39	U. Pennsylvania	MIT	1945		Yes	Yes	Yes	Yes
Kuh, Edwin	34	MIT	Harvard	1955		Yes	Yes		Yes
Lebergott, Stanley	31	Wesleyan	No			Yes	Yes		Yes
Liu, Ta-Chung		Cornell	Cornell	1940			Yes		
Lovell, Michael	29	Carnegie Institute of Technology	Harvard	1959					Yes
Lundberg, Erik		U. Stockholm + UC Berkeley							
Maisel, Sherman	41	UC Berkeley	Harvard	1949		Yes	Yes		Yes
Modigliani, Franco	41	MIT	New School	1944				Yes	
Nakamura, Mitsugu		U. Pennsylvania				Yes			
Prais, Sigburt									
Rhomberg, Rudolf	36	IMF	Yale	1959		Yes	Yes		Yes
Schultze, Charles	35	US Bureau of the Budget	Maryland	1960		Yes			Yes
Schwartz, Herbert		Board of Governors, DRS				Yes			
Shinkai, Yoishi		Osaka					Yes		Yes
Smith, Thomas		Department of Treasury					Yes		
Sparks, Gordon		U. Michigan	Michigan	1965			Yes		Yes
Suits, Daniel		U. Michigan	Michigan	1949		Yes	Yes		Yes
Tims, W		Netherland's Central Planning Bureau				Yes			
Tryon, Joseph	32	Georgetown + National Planning Association	Harvard	1961			Yes		Yes

Table 2: Members of the Committee on Economic Stability, 1959-1963

Name	Affiliation	59-60	60-61	61-62	62-63
Klein, Lawrence	U. Pennsylvania	X	X	X	X
Duesenberry, James	Harvard University	X	X	X	X
Hickman, Bert	Brookings Institution	X	X	X	X!
Gordon, R. A.	UC Berkeley	X!	X!	X!	X
Moore, Geoffrey	NBER	X	X	X	X
Lusher, David	CEA	X	X	X	X
Abramovitz, Moses	Stanford	?	X	X	X
Bronfenbrenner, Martin	U. Minnesota			X	X
Franco Modigliani	MIT				X
Fox, Karl	Iowa State University				X

Figure 1

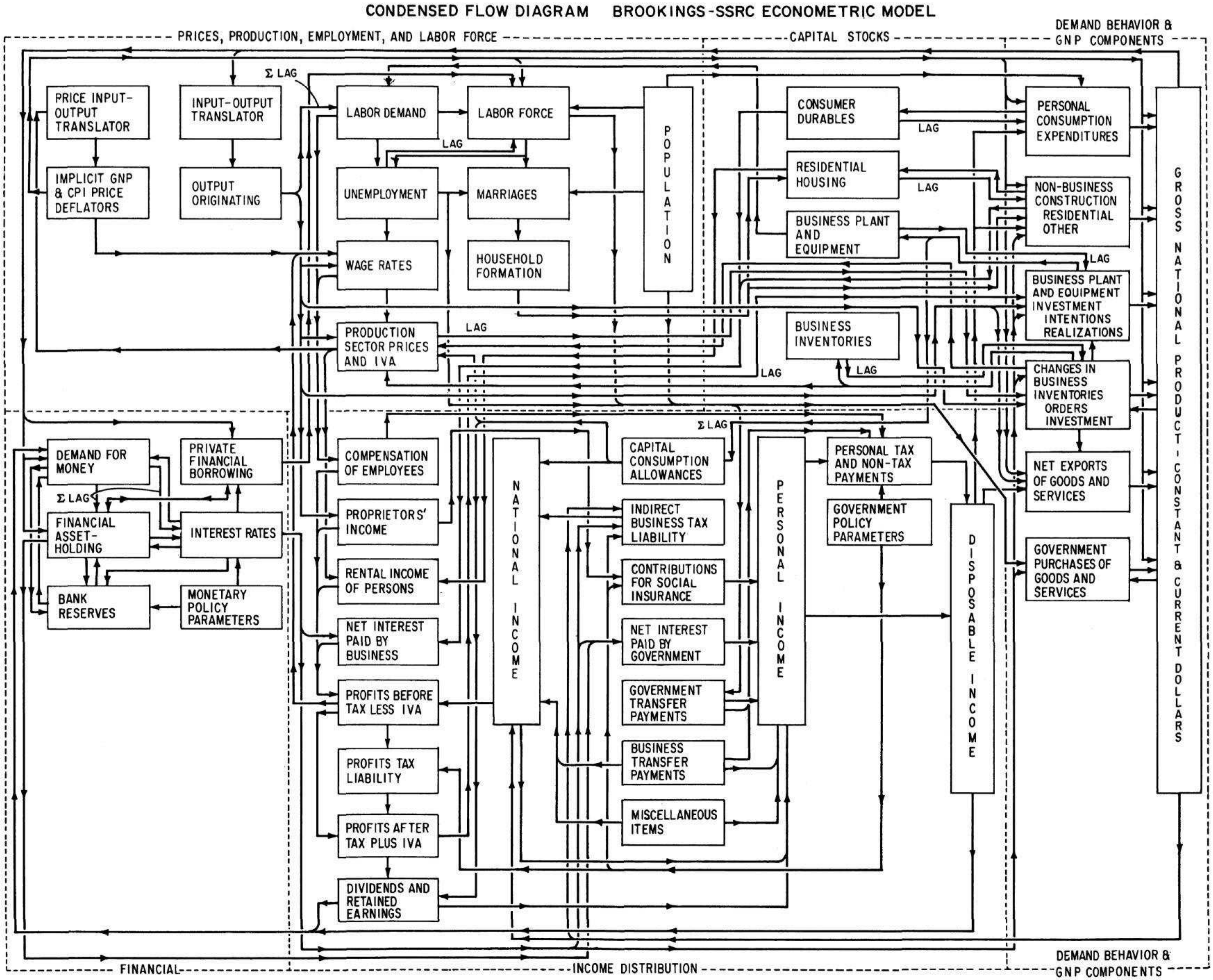
FIGURE 3-6. *Flow Diagram of Brookings-SSRC Econometric Model*[a]



[a] This is a preliminary diagram prepared in 1963. A revised version appears in J. S. Duesenberry, G. Fromm, L. R. Klein, and E. Kuh, eds., *The Brookings-SSRC Quarterly Econometric Model of the United States*, to be published by Rand McNally (Chicago) and North-Holland (Amsterdam) in early 1965.

Source: Hickman (1965a).

Figure 2



Source: Duesenberry et al. (1965a)

IV. Bank behavior in large-scale macroeconometric models of the 1960s

Juan Acosta and Goulven Rubin (Université de Paris 1)[1]

1. Introduction

The 1960s saw the consolidation of large-scale macroeconometric modeling in the United States. Teams of researchers built larger, more disaggregated models than ever before. A major aim of these economists was to develop new tools to assess monetary policies. This required modeling a more detailed financial sector and, in particular, a more elaborate representation of the banking sector.[2] We focus here on economists associated with the Committee on Economic Stability (CES) of the Social Science Research Council who elaborated a succession of models of the financial sector that became part of the Brookings model and of the Federal Reserve-MIT-Pennsylvania (FMP) model. From 1961 to 1963 Frank de Leeuw, an economist at the Division of Research and Statistics of the Board of Governors of the Federal Reserve System, built the financial sector of the CES model (De Leeuw 1965).[3] In 1963, Franco Modigliani joined the Committee and later organized a Subcommittee on Monetary Research co-chaired with James

[1] This paper is forthcoming in the *History of Political Economy* supplement on the history of macroeconometric modeling. We would like to thank Beatrice Cherrier, Erich Pinzón-Fuchs, an anonymous referee, and the participants in the 2017 Utrecht conference on the history of macroeconometric modeling for their helpful comments on previous versions of this paper.

[2] "If one is interested in policy questions it is clearly relevant to examine bank behavior since the strength of monetary policy is mediated, in part, by the investment decisions of the commercial banks." (Goldfeld 1966, 4-5).

[3] The work on the model during its phase as a project of the Committee on Economic Stability and its first year at the Brookings Institution was published as Duesenberry *et al.* (1965). Daniel Brill, also of the Division of Research and Statistics of the Board, was originally in charge of the model (Klein 1961, 8) but De Leeuw took over early on. The Committee's model became the Brookings model when it was handed over to the Brookings Institution for maintenance and improvement September of 1963. For details son the Committee's model project see Acosta and Pinzón-Fuchs (2018).

Duesenberry. In this context, Modigliani worked with De Leeuw, Albert Ando, and several other economists to develop what became the FMP model (1966-1970). De Leeuw's work served as the starting point for its financial sector together with the work of Stephen Goldfeld (1966), a former PhD student of Albert Ando at MIT.[4]

These works are important because, in total contrast with the impression that the IS-LM literature conveys (Modigliani, 1944 and 1963; Patinkin, 1956), they show that the post-war mainstream in macroeconomics studied carefully the role of commercial banks. By ignoring the actual content of macroeconometric models, standard histories of macroeconomics have made this aspect of postwar economics invisible.[5] For various reasons, monetary policy attracted more attention in the 1960s and was the subject of fierce debates between the Monetarists and the Keynesians (Rancan, this issue). But the policy instruments in the hands of the central bank affected the economy only through their effect on the behavior of commercial banks. Understanding the behavior of banks was thus crucial.

The paper analyzes the successive efforts of De Leeuw, Goldfeld, and the FMP team to model the banking sectors in their respective macroeconometric models. These economists pursued two main strategies to assess the importance of banks within the transmission mechanism of monetary policy. They tried to model the behavior of banks as the reflection of portfolio choices, and they also tried to show the importance of credit rationing, a phenomenon believed to increase the traction of monetary policy. Our analysis reveals two trends in the evolution from De Leeuw (1963) to the published version of the FMP model's money supply equations (Cooper *et al.* 1970). We first note a

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[4] Goldfeld's advisory committee also included Modigliani and Edwin Kuh. Submitted in 1963 but published in 1966, his dissertation proposed a small-scale macroeconometric model with a detailed banking sector. Together with De Leeuw's work, it was acknowledged as having "suggested the feasibility of, and laid out useful foundations for" the work that started in the summer 1966 (Modigliani 1966, 8).

[5] The behavior of banks was the subject of a growing literature in the early 1960s. The mimeographed chapters of a course later published as *Money, Credit, and Capital* given by James Tobin at Yale and containing a portfolio choice theory of commercial banking circulated among the specialists. Books stressing the importance of financial intermediaries by Gurley and Shaw (1960) or Meigs (1962) had appeared. But these works did not incorporate banks into full-fledged macro-models with the level of detail found in the Brookings or the FMP models.

contrast between the first versions of the financial sector and the latter ones and propose to interpret it as the consequence of a search for "transparency," after an expression used by the FMP team in a memo to the Board of Governors.[6] This search took the form of a progressive clarification of the microfoundations of the banking sector equations together with an alignment of the overall structure of the financial sector along the line of an LM equation. Second, the belief that credit rationing was an important phenomenon was incorporated in these models despite its non-observable nature and the lack of clear guidance from theory. A succession of proxy variables was used and the belief in the importance of credit rationing was not abandoned despite continuous negative results. All this shows how priors and theory informed the work of macroeconometric model builders, particularly the FMP team under the leadership of Modigliani.

2. From the portfolio choice of banks to the structure of the financial sector

Modeling the financial sector of the economy within a macroeconometric model involved the definition of behavior equations for various agents on different markets. Economists had to face a trade-off between, on one hand, the level of institutional detail and the degree of comprehensiveness of their model, and the intelligibility of the transmission mechanism of monetary policy on the other. They also had to justify their behavioral assumptions and this involved a clarification of the relation of their empirical models to existing microeconomic theory. Going from De Leeuw and Goldfeld's versions of the financial sector to the successive versions developed by the FMP team, one can observe how the latter put a stronger premium on simplicity over comprehensiveness and on consistency with microeconomics.

De Leeuw built the financial sector of the Committee on Economic Stability's model (De Leeuw 1965) in the open-minded and empirical spirit prevailing in this group under the

[6] "In addition, partly because of the personal preferences of those of us responsible for the project, and partly because we can now draw on the results of earlier models, the theoretical structure of this model is somewhat more transparent and easily understood by other economists." Board presentation, November, 1968. FMP, box RW 15, "Notebook 1968-1969" folder.

influence of Klein (Acosta and Pinzón-Fuchs 2018, 10). His model was composed of "behavioral" supply and demand equations based on the following principle. Agents "desired" a certain relation between the composition of their portfolios and interest rates. In general though, actual magnitudes differed from desired ones and agents would close a fraction of the gap during each quarter. In De Leeuw's typical equation for agent behavior the change in the holdings of an asset depended on its lagged holdings, the rates of return, and current and lagged short-run constraints. On this basis he developed a nineteen-equation financial sector showing how five groups of "transactors" interacted in seven financial markets.[7] This type of partial adjustment framework was kept by future modelers.

De Leeuw was explicitly concerned by the connection between his behavioral equations and economic theory. The notion of "desired relationship" was supposed to be consistent with "maximizing net worth" and a footnote referred to the works of Tobin (1956, 1958). Another footnote referred to other articles dealing with commercial banks portfolio choices (Meigs, 1962; Morrison, 1962) or the term structure of interest rates (Meiselman, 1962; Wood, 1962). The use of a weighted average of "recent values of GNP" as a constraint on the choices of the public was justified by reference to the work of Milton Friedman on permanent income. But, beyond this general philosophy, the absence of any justification of the form taken by each particular equation is equally striking. De Leeuw did not define the "desired relationships" that he invoked. His presentation, began with the following caveat suggesting that, in fact, his approach was only loosely connected to theory:

> The area of behavior which the model covers is one where theoretical foundations are weak and earlier econometric work skimpy. Most earlier work has been confined to the demand for money and the behavior of banks. Apart from this topic, there is very little to build on. […] *The equations are no more than a set of preliminary empirical explorations of financial behavior*. (De Leeuw 1965, 466. Our emphasis.)

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7 The markets considered were: Bank reserves, currency, demand deposits, time deposits, US government securities, an aggregate market of private securities, and an aggregate market of savings and insurance. The transactors included were: Banks, nonbank financial institutions, the Federal Reserve, the Treasury, and the public.

The representation of commercial banks' behavior illustrates this general pattern. De Leeuw did not discuss this behavior systematically. He only offered a hint of his general understanding in discussing their short run constraints by noting that "total deposits serve as a measure of the 'wealth' of the banking sector" (1965: 468). This suggest that De Leeuw saw banks as managing a portfolio based on the amount of deposits they received. The equations of the model show that this portfolio was reduced to three categories of assets: US securities (an aggregate of short term and long term Treasury bonds), private securities (an aggregate also) and reserves. This implied that their portfolio choice would be affected by a mix of short term and long run interest rates. These assets were financed by demand and time deposits and by borrowings from the Fed. Banks were supposed to make six decisions concerning five markets. Concerning their portfolio they decided the amounts of private and US securities and their levels of excess reserves and borrowings from the Fed. De Leeuw stressed that the demand equation on the market for private securities had the rate of interest as the dependent variable and he explicitly said that banks set the yield on time deposits, their sixth decision. The model contained no equation for the supply of deposits.

As a consequence of this representation of the banking sector, the overall structure of the financial sector remained complex. De Leeuw was concerned by intelligibility of the "structure" of his macroeconometric model taken as a whole:

> Considered as a descriptive device, the system of nineteen simultaneous equations just listed is somewhat unwieldy. To aid in the understanding of the mechanism implied in the equations, the following paragraphs will indicate verbally the way the model "works" (…). (1965: 482)

It was not enough to test the system of equations. The reader needed to understand its "working". This referred to the transmission mechanism of monetary policy. De Leeuw proposed to establish a correspondence with a "simpler hypothetical model" where each market was defined by a supply, a demand and a price. He thus explained how his equations defined supply and demand conditions on his seven markets. In so doing he singled out the "reserve-currency-deposit complex" that would be replaced by a market for money in the FMP. The market for reserve was a key market since it introduced unborrowed reserves in the model, the key decision variable of the central bank. This part of the text ended with a thought experiment considering the consequences of an increase of the supply of unborrowed reserves. In retrospect, it reveals the absence of recursivity in the structure of De Leeuw's model. The economist cannot trace the consequences of the shock he studies in a simple way because various endogenous variables can accommodate it on the market for reserve:

> An increase in the supply of unborrowed reserves and currency, for example, affects initially either bank holdings of excess or required reserves or the public's holdings of currency (or a combination of the tree). (1965: 485)

The comprehensiveness of De Leeuw's equations made them all interdependent. As a result, he could not trace the multiplier mechanism and how it affected long-term rates in a simple way.

Stephen Goldfeld was a 26 years old assistant professor at Princeton when his *Commercial Bank Behavior and Economic Activity* (1966) was published. The book was an even more ambitious project than De Leeuw's in that Goldfeld's 32-equation system was a complete model that included not only an analysis of the monetary side of the economy but also of investment and consumption decisions. Besides, Goldfeld's focus on commercial banking was more obvious than De Leeuw's: "If one is interested in policy questions it is clearly relevant to examine bank behavior since the strength of monetary policy is mediated, in part, by the investment decisions of the commercial banks" (1966, 4-5). Goldfeld presented his aim as quantifying the portfolio behavior of commercial banks in order to evaluate the effects it had on the supply of money and the transmission of monetary policy.[8] The full model, furthermore, could illuminate the working of traditional monetary instruments (1966, 1-2).

Goldfeld's approach to microfoundations went further than De Leeuw. The longest and most detailed chapter of the book dealt with bank's portfolio decisions, starting with a discussion of the sources of uncertainty faced by banks and how they determined their holdings of liquid assets. Following Tobin's 1959 analysis of the subject in a manuscript

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[8] In line with De Leeuw, Goldfeld also stated that "[w]hile much has been written about bank portfolio management, little of this has been quantitative in nature" (1966, 1).

that circulated widely and also Meigs (1962), he stressed that banks' choices rendered the money supply partially "endogenous". Goldfeld drew from both academic and non-academic sources, including publications from the Board of Governors, speeches by the chairman and other Board members, and textbooks on bank management to justify the behavioral equations of his model. But like De Leeuw, he did not derive his equations mathematically and agents' desired magnitudes were not part of these equations.

If Goldfeld announced a "money supply formulation" in the introduction of his book, the overall structure of his financial sector resembled the structure of De Leeuw's model in some respects. The behavior of banks was captured by a set of equations corresponding to the various magnitudes they had to choose. Adding to the level of institutional detail, Goldfeld introduced two new categories of assets in their portfolio: long term securities and municipal bonds. Besides, he distinguished country and non-country banks. But Goldfeld's notation, simpler that De Leeuw's, was taken up in the FMP and he actually gave a recursive form to his system of equations. Goldfeld noted that "the chain of events implicit in the model is the textbook one" (1966: 175) where monetary policy influenced financial variables and then investment and consumption. A step forward in the direction of the FMP was made by abandoning the equations for the US securities or the market for Treasury bonds. A money market emerged from the model with a clearly identified "demand for money" and a supply that could be derived from the components of banks portfolio behavior. This subsystem could determine the three-month Treasury bill rate and the long-term rates would be determined by the term structure and other equations. Goldfeld added some detail in the model but he simultaneously initiated the simplification of its overall structure.

Drawing on the works of De Leeuw and Goldfeld, the elaboration of the financial sector of the FMP spanned the years 1966 to 1970.[9] This was a collective effort but the name of Modigliani can be attached to all the parts of the sector and, in particular, to the elaboration of the equations concerning the banking sector. Commercial banks were a crucial element of the FMP model for a simple reason. The model was designed to

[9] See Backhouse and Cherrier (this issue) for a discussion of the work involved in the construction of the model.

discuss the "effects of monetary policy on income and prices."[10] Its builders conceived monetary policy as the control of four instruments: "non borrowed reserves, reserve requirement ratios against demand and time deposits, the discount rate at the New York Fed, and ceiling rates on bank time deposits" (*ibid.*). Those four instruments influenced the supply of deposits and commercial loans by weighting on the portfolio choices of commercial banks:

> Speaking roughly, we can say that banks respond to monetary policy variables and to interest rates by adjusting their reserve positions, their total investments, and the rates they charge on loans and pay on time deposits; while the public responds to its income and to interest rates by adjusting its borrowing and its holding of money, time deposits and other assets.
>
> With non borrowed reserves acting to limit the total of deposits and reserves, these adjustments by banks and the public drive short-term interest rates—in our model, the Treasury bill rate and the commercial paper rate—up or down enough to bring these various forces into balance.[11]

This quotation illustrates how the model builders understood the structure of their model. The decisions of the Fed concerning its policy instruments influenced banks' supply of demand deposits. This was the core of the model or what they called the "money supply mechanism." This supply on the part of banks was also a function of the three-month Treasury bill rate, a crucial determinant of their portfolio choices. This rate was also an important determinant of the demand for demand deposits. As a result, the interactions between the supply and the demand for money (currency being taken apart) were assumed to determine the rate of interest on Treasury bills or the short-term interest rate of the economic system. The short-term rate in turn would contribute to the determination of the long-term rates of the system. The connection between short and long-term rates was established in line with the work done by Modigliani and Sutch (1966). The long-term rates in turn determined the various components of investment and consumption. The model was recursive with a typically Keynesian LM relation or money market at its heart, a property that was stressed in various documents published by the FMP team.

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[10] De Leeuw, Board presentation, FMP, box RW15, "Notebook …" folder.

[11] *Ibid.*

This recursive structure was rapidly established. Ando, Modigliani, and De Leeuw presented the first version of the FMP model to the Board in November 1966. Their discussion of the "supply of money", as it was then called, remained extremely sketchy at this stage. They already summarized the portfolio choice of banks with one equation determining the supply of demand deposits. But this equation still depended on long term and short-term rates so that all the equations of the financial sector were interrelated. The structure was simplified in the 1967 presentation of the financial Sector by Modigliani and Robert Rasche, and then in a 1970 article published in the *Journal of Money, Credit and Banking* by Modigliani, Rasche, and Cooper. The title of the later "Central Bank Policy, the Money Supply, and the Short-Term Rate of Interest" illustrates their focus. As the introduction explained, the paper presented a "model of short-term interest rate determination" (Cooper *et al.* 1970, 167). This recursive structure was allowed by a drastic simplification of commercial banks' portfolio choices. Banks now chose between three assets only, as in De Leeuw, but these assets no longer mixed long-term and short-term securities. Banks first made a decision concerning the rate on commercial loans and accommodated the demand on the part of the public. The amount of commercial loan being fixed, they had to decide how to allocate their remaining resources between short-term Treasury bills and free reserves. Long-term rates were no longer involved in the decision regarding the supply of demand deposits. Note that this modification is not justified in the archival material or in the published texts at our disposal. In fact, in a discussion in 1975, Tobin criticized the unsystematic character of the model's structure (Tobin 1975, 566). For this reason, we cannot but see it mainly as a reflection of the model builders' search for transparency of the monetary mechanism in contrast with the original models of De Leeuw and even Goldfeld.

Another striking feature of the financial sector of the FMP model is the progressive clarification of the portfolio choice backing the behavioral equation of the banking sector. The 1966 presentation to the board introduced equations to define the levels of excess reserves and of borrowings from the Fed desired by Banks. These equations were ad hoc but they were used to derive mathematically the final behavioral equations in line with the principles of De Leeuw (1965). In 1967, the numerous targets of banks' choice in De Leeuw (1965) and Goldfeld (1966) were replaced by a decision concerning Free Reserves only or the difference between excess reserves and borrowed reserves. The balance sheet identity of banks could then be used to derive their level of investment (in Treasury bills) from which their supply of deposits was deduced. At this stage, the equation defining the desired level of free reserves was justified verbally as a way of facing "the lack of synchronization between inflows and outflows of deposits (both of which are at least partly in the nature of stochastic variables)."[12] The authors assumed that it would increase with the amount of demand deposits, increase with the rediscount rate, that is the cost of borrowing to the Fed, and decrease with the Treasury bill rate, that is, the opportunity cost of free reserves. The equation explaining the effective variation of free reserves was elaborated in various stages upon this founding relation. Finally, the 1970 article showed how optimal investments and free reserves could be derived mathematically from the maximization program of a bank faced with stochastic variations of its deposits and commercial loans. The result was an optimal level of free reserves dependent on three rates of interest: the cost of borrowed funds, the return on reserves and the return on investments. This theoretical equation was then adjusted in different stages to fit the American context (nature of the interest rates) and the quarterly data. The bridge with theory was complete. The paper referred to the 1959 course of Tobin of which the authors offered an extension and an econometric application.[13]

The end result of this work was an equation showing the relation between free reserves and the instruments available to the Fed: required reserve ratios, un-borrowed reserves, and the discount rate. Estimation of the model showed that "one third of an increase in unborrowed reserves is utilized for the expansion of assets and deposits within the same quarter while the remaining two-third goes temporarily to swell free reserves" (Cooper *et al.* 1970: 200). Errors of forecast on the part of banks slowed the impact of monetary policy. Interestingly enough, nowhere in these 1970 money supply equations was credit rationing to be found.

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[12] "A survey of the financial sector of the MIT-FRB model," 28, FMP, box RW17, "Structure of the model …" folder.

[13] This aspect of the FMP model was mentioned approvingly by Tobin (1975).

3. The challenge of credit rationing

Credit rationing—also referred to as *non-price* credit rationing—denotes a situation where the demand for credit is greater than the supply at a given rate of interest, and where banks use mechanisms other than adjusting the rate of interest to allocate credit among their customers. Some clients would thus be rejected, given a smaller loan, or given a loan with a shorter maturity. This issue, although present in earlier literature—sometimes under the name of "capital rationing"—had reemerged in the discussions about monetary policy in the early 1950s, in particular around the discussion of the so-called “availability doctrine” (Samuelson 1952; Scott 1957; Jaffee 1971, Chap. 2). In addition, a formal microeconomic literature that explored whether a situation of equilibrium with credit rationing was compatible with rational behavior consolidated in the early 1960s (Jaffee 1971, Chap.2; Baltensperger 1978). Central bankers and many economists saw credit rationing as a complement to the traditional cost of capital channel of transmission for monetary policy. In particular, rationing could increase the effectiveness of monetary policy (Modigliani, 1963; 100). All the modelers we’ve discussed were aware of the potential importance of this phenomenon and tried to include it in their models. The non-observable nature of credit rationing, however, made this difficult, and a series of proxy variables were used in order to capture its effects with very limited success. The insistence on trying to include a phenomenon that seemed to escape statistical corroboration, however, illustrates the importance of beliefs in macroeconometric modeling.

De Leeuw's model, as mentioned above, characterized banks’ behavior as the choice over the interest rates set on time deposits and the "terms on which they extend[ed] loans" (De Leeuw 1965, 507). These terms were captured by the interest rate set on loans and it was assumed that it was this variable—and not the amount of loans outstanding—that was controlled by banks. Although De Leeuw added a proviso indicating that this variable should be interpreted broadly, as representing "a whole range of contract terms and changes in screening procedures rather than interest charges alone" (De Leeuw 1965, 508n), this meant that the model's specification effectively ruled out any explicit non-price rationing. Still, it is noteworthy to find a reference to a broader interpretation of the rate of interest as it shows that he was aware of the issue, even if he did not provide a more adequate solution. Lack of adequate information regarding exactly how and to what degree credit rationing operated was also important, De Leeuw added (*ibid.*).

Goldfeld (1966) provided a longer discussion of the challenge involved in incorporating non-price elements into the characterization of the behavior of banks. He didn't elaborate on why banks might resort to non-price rationing measures, but he did cite the work of Kane and Malkiel (1965), who argued that banks might choose to provide shorter or smaller-than-desired loans to their customers—instead of flatly denying them credit—so as to not antagonize them.[14] It was an important issue, but not much could be done:

> If market responses are due to a varying mix of the change in rates and the rigidity of non-price limitations, we will mis-specify our system by relying on the loan rate alone. Unfortunately, it is impossible to deal with this matter empirically as there is no satisfactory quantitative estimate of the degree of credit rationing. We must simply rely on the loan rate. (Goldfeld 1966, 64)

Thus, much like De Leeuw had done before—although in this case for a separate commercial loan market—banks' behavior was represented as the choice over the loan rate. Goldfeld did provide, however, a brief comment on why this was not necessarily as limiting as it seemed: If the additional stringency caused by credit rationing was only temporary, as Samuelson (1952a) had argued, its effects would eventually be reflected on the loan rate. This suggested that the loan rate was usable, but that there might be lags in its response to demand pressures (*ibid.*). Accordingly, a variable for the demand for loans and a one period lag were included in the commercial loan rate equations.

Instead of modeling credit rationing directly, and lacking a measure, Goldfeld included variables that could at least capture some of its importance in other equations. On the demand side of the commercial loan market Goldfeld explicitly included a variable that was meant to capture "some of the nonprice elements of the set of loan terms." His equation for the demand of commercial loans included changes in "potential deposits," calculated as the maximum amount of deposits that banks could create and determined by

[14] This bank-customer relationship played an important part in the available literature on credit rationing since Hodgman (1961) had brought it up. See Jaffee (1971, Chapter 2) and Baltensperger (1978).

the reserve requirements on time and demand deposits, and by open market operations (Goldfeld 1966, 162). Assuming that tight monetary policy led banks to ration credit, the effect of changes in this variable could be seen as capturing the effect of non-price elements on the demand for loans. Goldfeld also included changes in total commercial loans as an explanatory variable in the equations for fixed and inventory investment as a way to capture the influence of non-price rationing on investment decisions.

Lacking a better alternative, Ando and Goldfeld (1968) followed an almost identical path:

> What we would have liked to assume is that there exists a category of bank "customers" whose loan requests are necessarily serviced and to whom banks feel committed. As the concept of a "customer" is unobservable at the aggregate level, the only reasonable alternative was to assume that banks view the entire volume of commercial loans largely as a constraint. It was not assumed that banks supply a given quantity of loans, but, rather, that they set a loan rate which is gradually adjusted toward an optimum (given demand conditions). This, it is recognized, does not adequately cope with the problem of credit rationing. However, as nonprice effects are allowed for on the demand side, this seems to be a reasonable first approximation. (Ando and Goldfeld 1968, 230)

Their equation for the commercial loan rate thus included an explanatory variable that corresponded to the ratio of loans to deposits and that was interpreted as a constraint determined by the bank-customer relationship. This was only slightly different to Goldfeld (1966), who had included the total volume of loans instead of a loan-deposit ratio. Ando and Goldfeld also included the changes in total commercial loans as an explanatory variable in each investment equation (plant, non-plant, and inventory). Regarding the demand for commercial loans, Ando and Goldfeld proceeded in the exact same way as Goldfeld (1966) and included potential deposits as an explanatory variable in the equation of the demand for commercial loans.

Overall, the results in Goldfeld (1966) and Ando and Goldfeld (1968) were only suggestive of the importance of credit rationing. They obtained mostly statistically significant results—except for the equations for inventory investment—but they had no explicit measure of credit rationing and had to rely on various proxy variables that could be interpreted as capturing at least some of the effect of credit rationing. The initial meetings of what would later become the Committee on Economic Stability's Subcommittee on Monetary Research in 1964 and 1965 showed that De Leeuw, Ando, and Goldfeld were not the only ones having difficulties wrapping their heads around credit rationing.[15] Summaries of the discussions of these meetings show that participants made no reference to the available theoretical literature, were not sure how to specify a framework that would allow them to identify and measure it, and did not know if the required data existed.[16]

And yet, the majority of assistants considered that credit rationing was important and should be taken into account. This was particularly clear in the reactions to Donald Hester's 1962 study on commercial lending presented at the 1965 meeting. Hester had tested whether credit rationing occurred when interest rates were high. His conclusion was that the terms of the loans granted—rate of interest, size, maturity, and whether it was secured or not—did not change between October of 1955 and October of 1957, when rates where significantly higher. Hester presented his results as evidence against the existence of credit rationing, and the minutes note that attendants were "generally impressed with Hester's work and showed interest in having it extended. However, *much sentiment* for the view that credit rationing exist[ed] remained."[17] Attendants would not give up easily. Modigliani indicated that he would study "small business investment and the availability thesis. He want[ed] to construct a proxy for non-price terms to small business borrowers."[18]

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[15] These meetings, as well as a summer institute organized at MIT in 1965 had as a main objective taking stock of, testing, and comparing the main theories available regarding the influence of monetary variables on investment. See Modigliani (1964, 1966).

[16] March 13-14, 1964 conference minutes, 11ff, SSRC2, box 151, folder 1724; April 2-3, 1965 conference minutes, 14ff, SSRC1, box 147, folder 812. See Jaffee (1971, Chap.2) and Baltensperger (1978) for an overview of what was available at the time. Donald Hodgman, one of the key contributors to the then emerging literature on credit rationing, attended the 1964 conference.

[17] April 2-3, 1965 conference minutes, 16, SSRC1, box 147, folder 812. Our emphasis.

[18] *Op. cit.*, 18. This was in line with comments he had made the previous year. He had pointed out that, since credit rationing could not be measured directly, it might be possible to use "some measure of general tightening of the commercial banking sector (which might be associated with rationing." March 13-14, 1964 conference minutes, 11. SSRC2, Box 151, Folder 1724. Note that Modigliani had included credit rationing in the 1963 reformulation of his original IS-LM model (1963, 97ff).

This work was, at least initially, taken over by John Hand, Modigliani's PhD student at MIT.[19] It's unclear if by the time Hand presented his work at the next year's meeting he had already decided to use factor analysis and principal components to extract a measure of credit rationing—the core of this dissertation (Hand 1968)—but the minutes do mention a "Hand index."[20] They don't mention exactly how it was constructed, but the characteristics and the interpretation of his index are not far from the work done in his dissertation:

> The Hand index is constructed from data obtained from the quarterly survey of interest rates on short-term business loans at large banks. Movements in the index largely reflect changes in the proportion of large business loans to total loans granted during the survey period, and to changes in the proportion of loans granted at the prime rate. These proportions both have prominent cyclical components, which Hand interprets as indicative of changes in the intensity of rationing at commercial banks.[21]

Hand's was the most careful attempt so far at building a proxy for credit rationing and his work was seen as a potentially useful contribution, but the lack of explicit reference to supply and demand factors was also remarked. It wasn't clear how much of the cyclical swings were produced by supply, demand, or a combination of both factors, and attendants to the meeting considered a more careful analysis was necessary. [22]

Later that year, when work on the FMP model had officially started, its team leaders recognized at their first presentation to the Board that evidence on the existence and importance of credit rationing was weak. Thus, and although only a rough sketch of the model existed at the time, the work followed its predecessors in representing the behavior of banks in the commercial loan market as their choice over the rate of interest. It was a pragmatic compromise pending further evidence about credit rationing:

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[19] Hand got involved with the Subcommittee's work in the summer of 1965 and his work was discussed at the May 13-14, 1966 meeting of the Subcommittee on Monetary Research. See Modigliani to Webbink, June 01, 1965. SSRC2, Box 151, Folder 1724 and Hand (1968, 141).

[20] These were indirect methods of estimation developed to obtain measures of directly unobservable characteristics based on observable ones, so they were particularly well suited for tackling the problem of constructing a measure of credit rationing (Hand 1968, 56ff).

[21] May 13-14, 1966 meeting minutes. SSRC1, box 147, folder 812.

[22] *Op. cit.*, 2. Our emphasis.

> This formulation does have some appeal in terms of the institutional arrangements, but it would be unsatisfactory to the extent that any non-price rationing of loans is practiced by banks. However, estimates of the Ando-Goldfeld model appear to be the only direct evidence in our possession which support the existence of rationing, and they are very weak evidences [sic]. A survey conducted by a New York brokerage firm in cooperation with SSRC Subcommittee on Monetary Mechanism appears emphatically to deny the existence of rationing, even in the summer of 1966. Pending a second survey directed at smaller firms, we propose to adopt the extreme formulation referred to above [banks choosing the loan rate], and then investigate in some detail to see if we can find any evidence that this formulation fails in the periods when the rationing was likely to have existed. [23]

The April 1, 1967 update of the list of equations didn't bring any changes regarding credit rationing.[24] Neither the equations for the commercial and industrial loan market nor the investment equations had any variable meant to capture any sort of non-price rationing. An accompanying memo written by Modigliani and Rasche indicates that the team was still actively looking into incorporating credit rationing since "in principle [it] could affect every component of investment."[25] Unfortunately, the section dealing with this was not present among Modigliani's papers, although it likely contained preliminary work done by Modigliani and Dwight Jaffee, his other PhD student at MIT. The same memo did contain, a short section on the effects of credit rationing on the demand for demand deposits as a possible refinement to the model. Modigliani and Rasche used the "Hand" measure to test for the possibility that credit rationing would alter the velocity of money but reported that "[t]o our disappointment, the variable had an entirely insignificant coefficient (and with the wrong sign at that)."[26] This was yet another negative result, and this section was eventually taken out of the following draft of the paper and of Cooper *et al.* (1970). [27]

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[23] Presentation to the Board, November 31, 1966, 41, FMP, box RW 17, "Structure of the model ..." folder.

[24] FMP, box 17, "Structure of the model ..." folder.

[25] A survey of the financial sector of the MIT-FRB model. FMP, box 17, "Structure of the model ..." folder.

[26] *Op. cit.*, 19.

[27] FMP, box RW 12, "FMPS Central bank policy ..." folder, contains a copy of the intermediate draft.

The results of the survey that had been mentioned in the November 31, 1966 presentation to the Board were available by mid-1967.[28] The report of the survey indicated that "bank credit rationing elude[d] measurement but [was] clearly substantial."[29] However, the papers describing the structure of the model that were made available in 1968 show that the team had not managed to incorporate it in the model as part of the description of the behavior of banks or in the investment equations. Instead, the papers by De Leeuw and Gramlich (1968, 18, 19) and Rasche and Shapiro (1968, 127) indicated that the model team was looking into the effect of credit rationing on housing starts. This ended up being a successful path and credit rationing on the mortgage market was presented as one of the main channels of transmission of monetary policy in De Leeuw and Gramlich (1969, 482). It was introduced in the same way that had been tried on previous occasions with investment equations. Thus, the equations for housing starts included a term that measured the relative availability of deposits at savings institutions and that was meant to capture non-price rationing as these institutions would ration credit when their deposits were relatively low. It was a key channel given the importance of the housing sector in the overall economy, but De Leeuw and Gramlich reported that they still considered the same phenomenon to be present elsewhere:

> The prerequisites for credit rationing—sluggish lending and deposit rates, little predictability of deposit flows, little short-run control over asset composition—are by far more prevalent in the mortgage-housing area than in most other credit markets. Nevertheless, we are not convinced that they are unimportant in other markets, and we feel further work on representing and testing for rationing effects might prove fruitful. (De Leeuw and Gramlich 1969, 483)

When the Board's funding of the FMP model project stopped in 1970, however, the importance of credit rationing still remained exclusive to the mortgage market. As reported by Modigliani (1975), the 1972 version of the model only contained credit

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[28] Timely review of 1966 credit shortage effects on business financing and spending decisions. Enclosed in Edwards to Webbink, July 21, 1967. SSRC2, box 152, folder 1729.

[29] The short paragraph explaining the result read: "A frequent explanation of spending reduction by medium to smaller companies was: 'We talked our plans over with our bankers before asking for a loan. He discouraged us and indicated such a loan application might not be approved, so that we never formally requested the money.' This probably largely explains why, with 28% of the yearend survey companies having reduced spending at least in part because of lack of available credit, only 13% sought and were not fully granted bank credit upon their initial 'request.'" *Op. cit.*, 4.

rationing in the housing market.[30] This is intriguing as the work of Jaffee (1968, 1971) and Modigliani (Jaffee and Modigliani 1969) on the commercial loan market showed very promising results. They produced a theory of credit rationing based on banks' grouping of clients into separate customer classes—instead of calculating an interest rate for each individual client—and they modeled both the demand and the supply side of the commercial loan market. Most notably, they included an equation that determined explicitly the amount of credit rationing and managed to estimate a new series for credit rationing. The fact that credit rationing still did not make it into the FMP model would seem to imply that even this new measure did not produce good results on the investment equations. However, it is also unclear how their equations would have been made compatible with the work on the supply of demand deposits presented in Cooper *et al.* (1970).[31]

4. Conclusions

Our discussion about the characterization of bank behavior in large-scale macroeconometric models contributes to a more detailed and nuanced characterization of these models. This is particularly important given their neglect in standard histories of macroeconomics (e.g. Snowdon and Vane 2005; De Vroey 2016) and, as Hoover points out, the "stigmatization" of these models in the mainstream narrative (Hoover 2012, 45). In particular, our results show that, as Hoover (2012, 39-45) pointed out in his description of Klein's microfoundational program, the modelers we have discussed had a clear concern with the microeconomic behavior behind the equations they estimated. As we have shown, however, this connection was made much more explicit in the FMP model than in De Leeuw (1965) and Goldfeld (1966). In line with Klein's desire for greater disaggregation (Hoover 2012, 41), De Leeuw (1965) offered a significantly more

[30] Note that, under the sponsorship of the SSRC, the model continued to be developed by the group when the contract with the Board ended. It was later handed over to Wharton EFA Inc. for maintenance and distribution. See Ando to Hickman, October 19, 1971, FMP, box CO1, "Ando MPS" folder.

[31] Jaffee continued to work on credit rationing and coauthored an important paper where credit rationing was explained using Akerlof's imperfect information framework (Jaffee and Russell 1976).

exhaustive characterization of the monetary sphere of the economy than previous models (e.g. Klein 1964) and Goldfeld (1966), for his part, treated city and country banks separately. This distinction, however, was lost in the FMP model, which, as we have argued, moved towards a simplification of the structure of the money market and the determination of the money supply. Most notably, compared to De Leeuw (1965) and Goldfeld (1966), the number of assets considered in banks' portfolio choice was reduced and the determination of the long-term rate simplified. Thus, instead of pursuing greater disaggregation or exhaustiveness in the equations determining the supply of money, the team behind the FMP model privileged a simpler, more "transparent" specification that was closer in structure to the IS-LM model.

Our results thus suggest that, while Klein's microfoundational program is visible in the characterization of bank behavior in the models we have discussed, there were also notable differences among them. It is difficult not to think that Modigliani's strong convictions about the way in which money mattered in the economy—which he depicted in his 1944 and 1963 IS-LM models and kept until the end of his life (Modigliani 2003)—were a key driver behind the shifts observed in the FMP model, even more so given his direct involvement in all aspects related to the financial sector of the model.[32] The study of other segments of large-scale macroeconometric models, like investment decisions or the relationship between wages and prices, should be the subject of future research as they could reveal further nuances and choices in the construction of these models.

[32] Memorandum by Ando, De Leeuw, and Modigliani. April 1, 1967, FMP, box 17, "Structure of the model ..." folder. For James Pierce, an economist at the Board's Division of Research and Statistics at the time, Modigliani "dominated" the group (Pierce 1996, 39).

V. The transformation of economic analysis at the Federal Reserve during the 1960s

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1. Introduction

The 2018 appointment of Jerome Powell, a trained lawyer, as chairman of the Board of Governors of the Federal Reserve System is a throwback to a time where non-economists ran the Fed. Up until the early 1970s most of the Board governors and the Regional Bank presidents were not economists but bankers, lawyers, or businessmen: among the first 9 chairmen, 6 were bankers (William Harding, Roy Young, Eugene Meyer, Marriner Eccles, Thomas McCabe, William Martin) and 3 had a background in law (Charles Hamlin, Daniel Crissinger, Eugene Black). Academic credentials were much less valuable than practical experience in the business and banking world, either in the private sector or at the Federal Reserve System. A successful, self-made banker with no college education like Marriner Eccles could become chairman of the Board of Governors. Since the 1960s, however, the number of trained economists serving as Board governors and Regional Bank presidents has increased substantially, and of the last Board chairs—from Arthur Burns (1970-1978) to Janet Yellen (2014-2018)—only George Miller (1978-1979) had neither a MA nor a PhD in economics.

The postwar transformation of the Fed is not restricted to decision-makers.[2] François Claveau and Jeremie Dion (2018) estimate that nearly 50% of money

[1] This paper was submitted to, and is now being revised for, the *Journal of the History of Economic Thought*.

and banking economists listed by the *American Economic Association* work in the research departments of Regional Banks and the Board; they publish a growing share of academic papers, and these tend to have a greater impact than those published by economists outside central banks (see also Fox 2014, Bordo and Istrefi 2018, Ban 2018). This closer relationship between central banks and academia has been interpreted by historians and sociologists as contributing to a "scientization" of central banking, a "process by which explicit, abstract, intellectually calculable rules and procedures are increasingly substituted for sentiments, tradition, and rules of thumb" (Wrong 1970 quoted in Marcussen 2009, 375). Protagonists generally agree that the 1960s were a pivotal moment in the march toward a more modern and technocratic institution (Maisel 1973, Stockwell 1989; Axilrod 2011; Meltzer 2010).[3] How this transformation was engineered, by whom, and how it unfolded, however, remain a blind spot of the flourishing literature on central banking.

Histories of the Fed span several genres. One, mentioned above, is popular among sociologists, political scientists and international relation specialists. It deals with how central bankers have shaped the postwar social, economic and financial international order, and how the institutional and legal foundations of their operations have been transformed (Ban 2018; Lebaron 2012; Baker *et al.* 2017; McGregor and Young 2013). In these works, the economic identity of major protagonists matters in that it structures their policy views and agency. Their contribution to economic knowledge is secondary. The focus is not on the models they build and test, or on their controversies, but on how they vote as members of the Federal Open Market Committee (FOMC). Neither are economists *qua* scientists central in the works of economic historians (Bordo 2008; Feiertag and Margariaz 2016; Monnet 2014). Their objects are the monetary policies implemented by central bankers. Allan Meltzer's history of

[2] The FOMC, who presides over open market policy, consists of the Board of governors, the president of the New York Fed, and four of the eleven presidents of regional banks, who serve on a rotating basis. The board of governors set the discount rate and reserve requirements. Throughout this paper, we will therefore use "Fed" to designate the Federal Reserve System as a "whole" and the Board and the FOMC respectively to designate the two bodies who set monetary policy.

[3] See also Schnidman and MacMillan (2016), who rely on an interview with Stephen Axilrod.

Federal Reserve (2003; 2009), despite its detailed discussions and broad coverage, only discusses changes in the tools and practices of economic analysis briefly, as accessories to a story centered mostly on the Fed's policy actions. Economists are fully restored as monetary model builders in histories of macroeconomics (Hoover 1990, De Vroey 2016; Snowdon and Vane 2005), but that several protagonists were affiliated with central banks stands in the background, if mentioned at all. Two exceptions to this separation between institutional and intellectual histories are Conti-Brown (2017) and Mehrling (2010), but these work are more specific in scope: they are concerned with the history of the central bank's independence and dealer of last resort ideas respectively.

Another popular genre is biographies and autobiographies: Bernanke's (2015) memoirs is only the last one of a series that includes reminiscences by Maisel (1973), Axilrod (2011), Stockwell (1989). Mallaby's 2016 prized biopic of Alan Greenspan succeeds Bremner's 2004 landmark biography of chairman William McChesney Martin. They provide lively daily accounts of the intellectual climate, the debates and the types of work economists were tasked with at the Fed, but information on why they were hired in the first place, allowed to research and build models, and challenged has to be extracted and reconstructed from their memories.[4]

In this paper, we build on data on Fed officials, oral history repositories and hitherto under-researched archival sources to unpack the torturous path toward crafting an institutional and intellectual space for postwar developments in theoretical and empirical macroeconomics within the Fed. We show that growing attention to new macroeconomic research was a reaction to both mounting external criticisms against the Fed's decision-making process and an oft-described process internal to the discipline whereby institutionalism was displaced by new forms of analysis (Morgan Rutherford 1998). We argue that the rise of the number of PhD economists working at the Fed is a symptom rather

[4] Maisel (1973) sports a whole chapter on the introduction of formal forecasts at the Fed in the 1960s, but it focuses on the uses rather than the making of economic knowledge.

than a cause of this transformation. Key to our story are a handful of economists from the Board of Governor's Division of Research and Statistics (DRS) who paradoxically did not always held a PhD, but envisioned their role as going beyond mere data accumulation and got involved into large-scale macroeconometric model building. We conclude that the divide between PhD and non-PhD economists may not be fully relevant to understand both the shift in the type of economics practiced at the Fed and the uses of this knowledge in the decision making-process. Equally important was the rift between different styles of economic analysis.

2. The Fed under pressure

William McChesney Martin, chairman of the Board of Governors of the Federal Reserve System between 1951 and 1970, took office in April 1951. A month earlier, as assistant secretary of the Treasury, he had negotiated a landmark agreement between the Treasury and the Fed (Hetzel and Leach 2012). The March 4, 1951 accord officially ended the peg on interest rates that the Fed had maintained since 1942 as part of the war effort, and Martin was therefore eager to reassess the Fed's newfound ability to pursue independent monetary policy. Throughout his tenure Martin worked to distance the Fed from the political pressures of Washington and to make the Board of Governors the center of the Federal Reserve System—thus reclaiming the spot from the Federal Reserve Bank of New York, which Martin considered to be too close to the financial community (Meltzer 2009, 55ff). By the end of the 1950s, however the Fed faced mounting criticisms from governmental bodies, the financial community, congress committees, and the press.[5] The Council of Economic Advisers sent weekly memos to Kennedy complaining about high interest rates (Cherrier 2018). The Joint Economic Committee of the Congress published a critical 1959 report largely authored by Otto Eckstein, and two more followed in 1960. The

[5] Maisel (1973, 27-29) describes some of these pressures. New economic phenomena, in particular mounting inflation from 1966 onward and international imbalances and pressures on the dollar added to the challenges.

Commission on Money and Credit issued a report in 1961, and then launched a series of hearings in 1964.[6] The Fed's policy orientations, as well as the decision process which led to them, were disparaged on a daily basis. Though sometimes highly political, most of these attacks were also fueled by academic economists who essentially faulted the Fed for not relying on the latest advances in monetary economics.

2.1 Dissatisfaction with policy orientations

A first line of criticisms targeted the Fed's policy choices. James Knipe, a special consultant to the Board's chairman, wrote a digest of "the public criticism of the Federal Reserve system" for Martin in 1961.[7] He explained that monetary policy was seen as lacking effectiveness in controlling expenditure on capital equipment and business inventories, but was "too effective" in restraining small businesses. In addition, critics considered that the Fed was "stunting national economic growth" by maintaining interest rates too high, and that this was the consequence of the undue influence of private banking interests. These criticisms focused on the choice of targets as well as the choice of instruments.

The Council of Economic Advisors formed by John F. Kennedy—Minnesota tax expert Walter Heller, Yale macroeconomist James Tobin, and budget specialist Kermit Gordon—were especially outspoken with regard to mis-specified targets. They flooded the president with memos explaining that "monetary policy has made no significant contribution to economic recovery." "Short-term rates have been kept from falling to protect our gold stock," they complained.[8] Tobin, whose research stood at the frontier of monetary economics, was even willing to go public. In January of the same year, he published a vocal critic of the Fed in

[6] Members of the commission included Marriner Eccles, Adolph Berle, David Rockefeller, Theodore Yntema. The research director was Harvard's Betrand Fox and his deputy MIT's Eli Shapiro. Lester Chandler, Paul Samuleson and Sumner Slichter, among others, had joined the advisory board.

[7] Knipe to Martin, "A summary of public criticism of the Federal Reserve System, 1959-1961," February 9 1962, FRASER (https://fraser.stlouisfed.org/files/docs/historical/martin/21_04_19620209.pdf)

[8] CEA to President, "Monetary Policy: High Time for Action," April 6, 1961. JFKA, https://www.jfklibrary.org/Asset-Viewer/Archives/JFKPOF-073-003.aspx. See also the comments reported by Bremner (2004, 150)

Challenge. He warned about building tensions between the Fed and both the administration and Congress on inflation, growth, and the management of the debt. The essential problem was the price stability fetish at the Fed: "the heavy reliance placed on monetary restraint over the past eight years is one of the reasons that, relative to GNP, consumption has grown while investment has fallen," he explained (Tobin 1961, 26). He also condemned the Fed's belief that deviating from monetary restraint would result in a "collapse." "[T]he economic logic of this prejudice [was], to say the least, obscure," he chaffed (*ibid)*.

Tobin did not merely fault the Fed for its narrow economic target, but also for rejecting the idea that price stabilization could be achieved by a combination of easier monetary policy and *fiscal* restraint. He also denounced the Fed's neglect of the cost of the debt for the Treasury when choosing the combination of reserve requirements and open market operations used throughout the business cycle (Tobin 1961, 27). In a study commissioned by the House Committee on Banking and Currency, monetarist economists Karl Brunner and Alan Meltzer (1964a,b,c) instead criticized the Fed's use of free reserves as an indicator of the degree of liquidity of the market and of individual banks, and its failure to distinguish between individual banks and system-wide changes on free reserves. In line with their monetarist leanings, they argued that "[t]he desired growth rate of the money supply should be explicitly chosen for a 6-month or longer period and policy operations should be directed towards achieving that growth rate by explicit choice of a growth rate for the monetary base" (1964c, 84).

2.2 Dissatisfaction with the Fed's policy decision-making process

These economists also criticized *how* the Fed arrived at its policy decisions. Brunner and Meltzer (1963a, viii) made it clear that it was their main concern: "we believe that there is a more important series of questions that has not been asked very often: are the procedures for making monetary policy adequate? Does the Federal Reserve have adequate information in sufficient time to make appropriate decisions?," Brunner and Meltzer stated at the beginning of their study. First, they found the decision-making process plagued with short-termism. The FOMC met every 3 weeks, which was too often and led them to focus too much on short-run phenomena and to rely on the "tone and feel" of the market rather than serious quantitative analysis. Second, the FOMC was monitoring too many variables, reflecting vague and contradictory definitions of concepts like "credit" and "availability." Brunner and Meltzer (1964a) identified "a variety of magnitudes or entities reflecting the behavior of banks or the operations on credit markets … some refer to free reserves, some to short-term rates; others point to reserves, required reserves, 'credit,' long-term yields, short-term yields, liquid assets" and concluded that "the very mixed nature of these criteria reveals the absence of a coherent conception" (1964a, 4).

Short-termism and lack of structure resulted in a lack of direction, Brunner and Meltzer concluded, echoing other critics. A specific longstanding bone of contention was the content of the directive that the FOMC issued to the manager of the Fed's Trading Desk—located at the New York Fed—to implement open-market policy. The directive was initially loose, of the "ease or restraint" type. This left a huge discretionary power to the New York Fed, which Martin early on attempted to reduce. How to draft a more specific directive was discussed and re-discussed throughout the 1960s and 1970s. This "indecision" also stemmed from the absence of real debates and confrontation of alternative frameworks. While active and at times "heated" discussions took place during FOMC meetings, these did not necessarily "contribute to the formation of rational monetary policy … [as a] variety of unsubstantiated judgments and unsupported opinions replaces analysis and evidence as the basis for policy operations," Brunner and Meltzer (1964c, 92) bemoaned. Their suggestion for reform was consequently radical: "serious consideration should be given to replacing the FOMC with a single administrative official," they wrote (p93).

Another line of criticism was the Fed's lack of transparency. Both the Joint Economic Committee and the Monetary Commission complained that the lack of communication on the reasons for major policy decision and actions on the Fed's policies resulted in "a tendency to seize upon even the most outlandish rumors as significant" (Knipe 1962, 40). There was however, a more fundamental attack behind the demand for transparency. The key issue, elicited by Tobin (1961, 24) in his *Challenge* article, was how the board had used its freedom since 1951. The independence of the Fed had become a "heated issue, a symbol of irresponsible power to some, and to others the last citadel protecting the dollar and the country from disaster," he warned.

Thwarting this newfound independence was precisely the agenda of the chairman of the Commission on Money and Credit, Texas congressman Wright Patman. Dubbed "the populist scourge of the Fed," he was a staunch opponent of high interest rates and of the separation of monetary and fiscal policy, which he constantly challenged after 1951 (Conti-Brown 2017, 274-276; see also Young 2000, ch. 7, 8). In 1964, he proposed to hold four-month hearings for the 50th birthday of the Federal Reserve Act, hoping that the Congress would eventually agree to get rid of the FOMC, reduce the budgetary autonomy of the Fed, and restore the primacy of the Treasury over the definition of monetary policy. Johnson's intervention, at Martin's request, thwarted Patman's plans, but the criticisms outlined in the Brunner-Meltzer report emanating from the commission would have a lasting influence. Again, the report echoed the 1959 diagnosis of the Joint Economic Committee: monetary and fiscal policy should be better coordinated, under the oversight of the executive or the Congress (Knipe 1962, 32). It was suggested that either the chairman of CEA or the Secretary of the Treasury sit on the Board of Governors.

2.3 Dissatisfaction with the lack of scientific underpinnings

Underlying these criticisms of monetary policy was thus the shared notion that "after 50 years the Federal Reserve ha[d] not yet provided a rational foundation for policymaking" (Brunner and Meltzer 1964a, ix). For those policy-oriented yet academic economists involved in monetary debates, "rational foundation" meant science-based. Brunner and Meltzer (1964c, 83) were explicit that the Fed "should develop and test a theory incorporating the essential elements of the money supply process" (*ibid.).* The insistence on solid theoretical *and* empirical foundations echoed almost verbatim Tobin's earlier complaint that the Fed's decisions "rel[y] more on a general faith that virtue pays than on careful empirical and theoretical analysis" (Tobin 1961, 26; see also Maisel 1973, 168).

The theory the FOMC needed was a systematic and testable "understanding of the mechanism connecting monetary policy operations with the money supply" (Brunner and Meltzer 1963a, viii). The 1950s and 1960s were decades in which monetary theories were debated, without any consensus in sight. A 1962 survey of the literature opens with the remark that "the past decade has witnessed a resurgence of controversy over the perennial issues of monetary policy. What methods should it employ? What are the channels or processes through which it influences economic activity? By now, opinion has become so sharply divided…as to almost defy classification" (Ritter 1962, 14). Economists did not agree on the channels whereby monetary policy influenced the real economy, and hence they could not agree on the adequate instruments for policy. Brunner, Meltzer, Tobin, Samuelson others faulted the FOMC for not understanding that financial agents were making rational choices in the currency and assets they wanted to hold (in line with Tobin's portfolio theory which treated money as a riskless asset, see Acosta and Rubin 2018). The monetarists wanted more focus on the monetary base, while the CEA Keynesians argued that the key role of monetary policy was to finance public debt. Restrictive fiscal policy would slow growth down if necessary. There was also a wealth of research on the scope of lags between the Fed's decision and the shift in the effective money supply, and the additional lag to changes in credit availability or shifts in short and long-term interest rates on the market, and eventually to effects on production and unemployment (Knipe 1962, 42-43). It was crucial that the FOMC became knowledgeable of these alternative measurements of the lags.

For Brunner and Meltzer (1964a, 2-3), the lack of consensus was not an issue. It was the competition between rival yet scientifically informed conceptions of monetary policy that would create rational policy decisions. The confrontation would be solved through discussion but also through *empirical testing*, academic economists insisted. What the Fed needed was a systematic framework for "continuous appraisal, reappraisal and comparison [of] alternative conceptions."[9] The problem, they argued, was not that the Fed lacked in-house research facilities, but that the latter was not doing the right kind of science. The Board had a Division for Research and Statistics (DRS) and the regional banks were also staffing up their research departments. But these bodies were largely devoted to the collection of data on banks and credit markets, which explained the Fed's excellent record in identifying the turning point of the business cycle—which even Brunner and Meltzer acknowledged (1964a, viii). "But the relevance of this mass of data cannot be judged in the absence of a coherent conception systematically weaving this information into a meaningful pattern," Brunner and Meltzer (1964a, 3) claimed. "Collection and preparation of data not guided by an explicit analytical frame often leads to a pointless accumulation of data" (*ibid.)*.

The accusation of "pointless accumulation of data" resonated with profound fault lines among economists. In the postwar decades the profession was pervaded with fundamental methodological debates, some best encapsulated in the "Measurement without Theory" controversy which erupted between the neoclassical researchers associated with the Cowles Commission and the institutionalists of the NBER (see Morgan and Rutherford 1998, Mirowski 1989, Rutherford 2011). In 1946, Cowles vice-director Tjalling Koopmans (1946) published a scathing review of *Measuring Business Cycles* by NBER associates Arthur Burns and Wesley Mitchell. He called the painful data collection and resulting identification of regularities the "Kepler stage" of economics, one surpassed by the estimation of systems of simultaneous equations through probabilistic-based econometric methods he had developed with some colleagues. Economics had entered a "Newton age" characterized by a "fuller utilization of the concepts and hypotheses of economic theory" rather than "naïve empiricism," Koopmans concluded. Institutionalist vs neoclassical wars also spread to microeconomics, with heated debates on the representation of the pricing behavior of firms (see Mongin 1997).

[9] Franco Modigliani and Milton Friedman made similar remarks to the Board in 1965, see Rancan (2018).

Martin, the board and the FOMC could not stay deaf to the swelling tide of criticisms their decisions elicited. Yet staffing up and giving greater agency to economists would create new questions and divisions. Inside the Fed as elsewhere in economic circles, the two linked debates over the quantification of economic variables and relationships and the relative merits of inductive vs deductive empirical analysis played out full volume.

3. Inside the Fed

A year after leaving the Fed, former Board Governor Sherman Maisel (1965-1972) clearly outlined the mid-1960s ethos: "the Fed found itself on the defensive. Specific accusations of bad judgment could no longer be countered by generalities. The system had to develop a more comprehensive theory of monetary policy and clarify its own views" (Maisel 1973, 26). Martin was acutely aware of these external pressures. Yet, the composition of the FOMC and the Board's staff was shifting in these years, which resulted in growing divides on how the Fed should respond.

3.1 Changing views of monetary policy-making at the FOMC

Kennedy and Johnson were, Heller reflected after completing his term as chairman of the Council of Economic Advisors, "the first modern economists in the American presidency" (Heller 1967, 37). Tables 1 and 2 in the annex display the consequences of these presidents' inclination towards economics: four of the five Governors they appointed were economists, and three of them had a PhD. Kennedy's first appointment was George Mitchell, a tax economist with a BA from Wisconsin who at the time was the vice president of the Chicago Fed. A handful of economists had served as Governors before, but the first economics PhD had been appointed by Eisenhower in 1955. Canby Balderston, aged 57 at the time of his appointment, had a PhD from Pennsylvania (1928) and was the director of the Wharton School of Finance and Commerce at the time.[10] After Mitchell, who was also 57 when he was appointed, Kennedy and Johnson , subsequently appointed three younger economists, all of them trained at Harvard as the Keynesian revolution was being absorbed in graduate curricula: James Dewey Daane, who graduated in 1949 with a doctorate in public administration,[11] had previously worked at the Richmond and Minneapolis Reserve Banks as well as the Treasury; Sherman Maisel, who also graduated in 1949, was a professor at Berkeley; and Andrew Brimmer, who became the first Afro-American to serve on the board, had graduated in 1957, then worked at the New York Fed and the US Department of Commerce.

The tables 3 and 4 list information on the Regional Bank presidents from 1950 through the mid 1970s. When Martin was appointed as chairman there was only one Reserve Bank president with a PhD, Alfred H. Williams (Pennsylvania, 1924). The other presidents were professional bankers that had climbed the ladder in the private sector or made a career at the Federal Reserve System. For example, New York Fed's Allan Sproul, a widely recognized master of the art of central banking, had originally studied pomology (the science of fruit growing) at Berkeley and had been appointed a year later as head of the San Francisco Fed's Division of Analysis and Research despite knowing "little about banking and nothing about central banking" (Sproul quoted in Ritter 1980, 4). By the time Burns was appointed chairman of the Fed in 1970, however, there were five PhDs serving as Regional Bank presidents, and by 1975 they had become a majority.

Being trained in economics was, however, increasingly seen as a prerequisite for sound policy-making decisions. The kind of career chairman Marriner Eccles (1934-1948) built after taking over his father's businesses right after high school was becoming an exception. And the fact that Martin himself—a trained lawyer, banker, and former president of the NYSE—had no background in economics

[10] We are excluding here Paul E. Miller, who served as Governor for two months during 1954, was a trained (BS, MSc) in agriculture, and received the honorary degree of Doctor in Economic Science from the University of Ireland in 1951.

[11] His dissertation was, nonetheless, listed in the 1949 AEA's list of doctoral dissertations in political economy.

was increasingly pointed out.[12] The CEA staff often complained about his lack of economic expertise. For instance, Gardner Ackley, who replaced Heller as CEA chairman, later remarked that "Martin was absolutely zero as an economist. He had no real understanding of economics" (Ackley 1974, 5; see also Maisel 1973, 122-123). This momentum resulted in the nomination of Arthur Burns, whose work on business cycles was widely recognized, as chairman in 1970. Of him Ackley said: " [he] is a first-rate, intelligent economist. He talks about things much in the same terms that I do; and even if we often disagree, at least there is communication at a professional level" (Ackley 1974, 11).

Martin lacked academic credentials in economics, but he supported the Board's interaction with economists. In 1964, he complained to G.L. Bach that "[t]he Board feels that … it … has not had very effective contacts with academic economists on monetary issues" and asked him to organize an "Academic Consultant Meetings" series akin to what Seymour Harris had set up at the Treasury in 1960.[13] The founding director of the Carnegie School of Industrial Administration, Bach activated his wide network to invite some of the most renowned macroeconomists: Stanford's Edward Shaw, Yale's James Tobin, Harvard's James Duesenberry, and MIT's Franco Modigliani, among others, participated in the first meeting. Milton Friedman visited the board a few weeks later,[14] and he and Meltzer participated in future meetings, which became a regular event, taking place at least once a year from then on.[15]

At the same time, even if the academically trained members agreed that there was more to monetary policy than a scientific framework, the shift in the FOMC's demographics created a rift between "two bands: younger, Keynesian-laden staff

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[12] Bremner (2004, 24) reports that he started a PhD in finance at Columbia in the 1930s, although he did not graduate.

[13] Bach to Modigliani, dated November 15, 1953, but the year is a typo, it was written in 1963. FMA, Box PS1, folder "Academic consultants meeting 1964." See Also Martin to Modigliani, February 4, 1964, same folder.

[14] Bach to Modigliani, 01/08/1964, same folder. Topics included « lags and signals for monetary action ,» « the demand-time deposits mix problem » or « the quality of credit problem.»

[15] Modigliani's papers include material from academic consultants meetings up to the early 1980s. G. L. Bach remained the organizer.

vs traditionalists" (Maisel 1973, 215-216).[16] Maisel relentlessly fought for the inclusion of new indicators and formal forecasts in FOMC deliberations (Bremner 2004, 253), and Mitchell enthusiastically pushed for the dissemination of empirical research, declaring that by 1967, "the 'new economics' was firmly in the saddle" at the Board.[17]

It was not just that the older bankers assembled around Martin did not catch on this intellectual renewal. They actively rejected their younger colleagues' push for quantification and rationalization. Martin believed that financial markets were characterized by uncertainty and complex psychological individual and collective phenomena, so that he had very little faith in the value of attempting to quantify Federal Reserve policy. In his opinion, measurement was dangerous, if not impossible: numbers obtained would not accurately reflect real conditions and the Fed could do best by carefully evaluating events in the financial markets (Maisel 1973, 118). The chairman had to be skilled in reading the "tone and feel" of the market, and all his colleagues concurred that Martin was exceptionally gifted in the matter. Maisel (1973, 170) again aptly summarized Martin's paradoxical stance:

> Chairman Martin led the group who felt that Federal Reserve policy had to remain an art rather than a science. However, while he opposed the introduction of any specific analytical framework, he did believe in research and knowledge. He allowed and even encouraged the staff to explore new techniques, but at the same time he adhered to his belief that real quantification was impossible, that it would downgrade judgment and intuition, and therefore would lead to greater errors on the part of the Federal Reserve

Whether enthusiastically or reluctantly, all eyes were thus set on the in-house pool of economists explicitly tasked with channeling new economic research to the Board: the Division of Research and Statistics.

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[16] Maisel, one of the staunchest supporters of quantification, acknowledged that "[b]efore my appointment to the Board of Governors of the Federal Reserve System in 1965, I had spent nearly twenty years studying and teaching monetary economics. I thought I understood what the Fed did and how it affected the economy. I soon discovered how little I knew" (Maisel 1973, ix).

[17] Bremner (2004, 230n18). Holland to Modigliani, 01/20/64, same folder.

3.2 The analysts: the Division of Research and Statistics

Unlike the Board of Governors and the FOMC, the Division of Research and Statistics (DRS) had hired and been directed by economists since its establishment in 1918.[18] Since the DRS is less visible than the FOMC, systematic information is more difficult to gather. Tables 5 and 6 provide background information on DRS top-advisors up to 1975.[19] They show no overwhelming trend toward an academicization of the DRS akin to what is visible within the FOMC, but rather the continuation of a pre-war trend: 47% (8/17) top DRS officials who have been recruited at the Fed in the pre-Martin era held a PhD. Twenty years after, the number has risen to 65% (13/20). By the time Burns took over, an economics PhD had become a prerequisite to work at the DRS. There was, however, more diversity among PhD programs than at the FOMC, dominated by a Harvard pipeline.

These tables also highlight other interesting evolutions. First, the DRS had substantially grown in size, suggesting its role had expanded within the Fed. In the fifties, the number of top officials grew from 4 to 8, then remained stagnant in the first half of the 1960s. In the wake of external challenges to monetary policies, the DRS head was staffed up to 14 in 1970 and 18 by March 1975. Second, with the exception of Ralph Young and Arthur Marget, who were directly hired in top positions, and Guy Noyes, whose promotion was expedited, new recruits climbed the ladders within the DRS. Before 1960, reaching a top position took an average of 16 years. For those hired after 1965, it only took 5.4 year on average, meaning that DRS top officials were considerably younger by the time Martin resigned. Finally, a sizeable number of these economists pursued a PhD *while working at the DRS*. They were thus presumably more in touch with recent

[18] The DRS resulted from the merging of the Office of the Statistician and the Division of Analysis and Research in 1923. It was not the only one that carried out research. The Division of International Finance also hired economists and carried out research. We have left the DIF out of the picture, because the intellectual context for its operations – international economics – have hitherto been less researched. Also, the academic and professional communities involved were slightly different. See Yohe (1990) for a discussion of research at the Board in the 1920s.

[19] These include associate and assistant directors, advisers, and assistant and associate advisers. We don't know exactly how many economists worked at the DRS in any given moment in time during our period, only the names of the people at the top of the hierarchy of the DRS were listed in the monthly Federal Reserve Bulletin so our discussion only includes them.

advances in monetary economics than the previous generation of directors. Table 8 shows that some researchers completed it 10 to 15 years after recruitment. This challenges the usual separation between academic economists and practitioners (or central banking economists).

The DRS was, like the FOMC, pervaded by disagreements. This is revealed by the oral history project conducted by Robert Hetzel between 1994 and 2003 and personal archives, which present a more nuanced picture than what is suggested through counting heads."[20] What mattered was not so much whether researchers had received some graduate training or of what kind. To be sure, many DRS directors had strong connections to the institutionalist movement, starting with Walter Stewart, a former professor at Amherst College who had also worked with Wesley Mitchell (Yohe 1990; Rutherford 2011, ch. 6).[21] Woodlief Thomas had graduated from the Brookings Graduate School (Rutherford 2011, ch. 7), and Ralph Young had been the director of the NBER's Financial Research Program before entering the DRS.[22] Daniel Brill was initially recruited entered as Morris Copeland's main assistant in his project to build the first flow-of-funds accounts in the late 1940s.[23] Key to the DRS mission was thus the data intensive approach several directors had inherited from their contact with the Mitchell-NBER research agenda.

And yet, it was Daniel Brill, who had been trained before the war, did not boast a PhD, and fully endorsed Copeland's institutionalism, who became a pivotal figure in the DRS's endorsement of econometrics during the 1960s (Meltzer 2009, 493).[24] In 1960 he was asked by the recently created Committee on Economic Stability of the Social Science Research Council to participate in their macroeconometric

[20] Hetzel's oral history collection is available online at https://fraser.stlouisfed.org/archival/4927.
[21] In fact, it was Mitchell who recommended him for a position at the Fed (Yohe 1990).
[22] See the Federal Reserve Bulletin for a short profile of Young (March, 1967, 388). See Saulnier (1947) for a description of the Financial Research Program. Young stayed at the Board for a few more years as adviser to the Board and as director of the Division of International Finance after he left the DRS.
[23] See the Federal Reserve Bulletin for a short profile on Brill (December, 1963, 1653-4) and Copeland's recommendation letter for the Rockefeller Public service Award, October 21, 1953, WWSPIA. On Copeland, see Rutherford (2011, ch. 4).
[24] Brill did do some graduate work at the American University in Washington in 1937-38 but didn't get a PhD. Similarly, the AEA dissertations list shows that Guy Noyes attended graduate school at Yale but did not graduate.

model project. The purpose of the Committee was to develop a larger version of the kind of multi-equation representation of an economy initially developed by Jan Tinbergen before the war and then adapted to the US economy by Lawrence Klein in a Keynesian framework. The team of more than 20 researchers, led by Klein and Duesenberry, wanted to reach a wider degree of disaggregation, and entrusted various participants with writing blocks of equations meant to describe aggregate consumption, investment, the financial sector, the housing sector, among others.[25] Brill, then an associate advisor of the DRS, had been selected as the expert in charge of the financial sector of the model. Early on, however, he requested the help of a recent DRS recruit, Frank de Leeuw, who eventually took over Brill's work and wrote the published version of the financial sector for the Committee's model (De Leeuw 1965).[26]

After a Harvard Master of Public Administration ('53), Frank de Leeuw had joined the San Francisco Fed before transferring to the DRS in 1956. He initially worked on the demand for capital goods, but he took a leave of absence in 1964 to complete a dissertation at Harvard (De Leeuw 1965b) that included his work for the Committee's model (De Leeuw 1965a) and additional simulation work he had published that year (De Leeuw 1964). Under the "general direction of Daniel Brill" (1965a, n533), De Leeuw had crafted a nineteen-equation model spanning seven financial markets and five groups of agents. It was considerably larger and more disaggregated than any previous depiction of the financial sector, and allowed useful simulation experiments by including explicit parameters for actual monetary policy instruments—such as unborrowed reserves or reserve requirements on demand and time deposits. It also presented an early implementation of a portfolio choice framework of behavior for all the financial agents included in the model (Acosta and Rubin 20018). The dissertation—which shared the title of his chapter for the Committee's model, "A model of financial behavior"—was defended in June 1965 and signed by Duesenberry and

[25] This depiction of the SSRC Committee and their macroeconometric model project relies on Acosta and Pinzón-Fuchs (2018). See also Bodkin *et al*. (1991) and Pinzón-Fuchs (2017). The Committee's model was entrusted to the Brookings Institution in 1963 and became the Brookings Quarterly model.
[26] Sherman Maisel was the expert in charge of the non-industrial construction sector of the model. See list of experts, Box 147, Folder 810, SSRC1, and Acosta & Pinzón-Fuchs (2018) for more details.

John Lintner, but De Leeuw also thanked his colleagues at the DRS and at the Committee model project. His work thus fully represented a synthesis between academic debates and central bank research.

De Leeuw was a driving force behind the development of macroeconometric expertise at the DRS. "All the young people at the Fed then felt he walked on water," Edward Gramlich (2004) remembers. "In the back rooms of the research division, experimental work with the new science of econometric modeling was going on. This was mind-stretching work for Frank de Leeuw and the rest of the staff involved," Stockwell (1989, 22) likewise describes. The push was supported by the establishment of an empirical seminar by Martin and DRS associate director Robert Holland in 1964.[27] It was also enabled by an adequate technical infrastructure. The Board had a mainframe computer since at leas the late 1950s and a Division of Data Processing, which grew out of the DRS, was established in 1963.[28] The Board had software for multiple regression analysis but a devoted staff also wrote additional FORTRAN programs to suit its needs. Ann Walka did all the computer work related to De Leeuw's work on the financial mode up to 1965. Other programmers who helped De Leeuw included Helen Popkin and Enid Miller.[29] De Leeuw could thus carry part of the computation for his project at the Fed, though it is not clear whether such equipment was a driver or a result of the growing interest in macroeconometrics.

The work of Brill and De Leeuw was supported by the two successive directors of the DRS, Ralph Young (1949-1960)—who stayed as secretary of the FOMC afterwards—and Guy Noyes (1960-1963), and this might have been the most significant transformation. Throughout the 1950s, the work of the DRS had been shackled by the vision of the assistant to the chair and secretary of the FOMC Winfield Riefler. A former graduate of the Brookings Graduate School (Rutherford 2011, ch. 7), Riefler had a strong influence on Martin, Board

[27] Holland to Modigliani, 02/04/194, ibid.
[28] Board Minutes and Federal Reserve Bulletin, 1962_10, p. 1291
[29] The July 15 and August 23, 1963 issues of the Newsletter of the Committee on Computers in Research, Federal Reserve System present Ann Walka's work on a program to carry out transformations of variables. Some issues of the Newsletter are available at http://www.emelichar.com/ProgrammingNote.html.

members, and the staff thanks to his "persuasive intellect," a 1964 article from *Business Week* recounts.[30] He had established a "Riefler rule" whereby the Federal Reserve "didn't make or discuss forecasts" (Meltzer 2009, 45, 498). When Riefler retired in 1959 and was succeeded by Young, "there [was] an unleashing of staff brainpower," *Business Week* journalists remarked. More staffers entered policy debate, the article continues, and Meltzer adds that "the methods taught in graduate schools such as econometric forecasts and economic models" were introduced (2009, 498).

An unsuccessful memo Noyes and Young drafted in 1960 to stir the board toward funding the SSRC model highlights the reason why the institutionalist directors supported macroeconometrics.[31] According to the minutes, the memo "pointed out that the Board's flow of funds accounts might well provide the statistical framework for much of the analysis." Young and Noyes may thus have seen in the project an opportunity to use the hitherto under-used flow of funds accounts they had been building since Copeland's 1947-52 project. The Board had done some work to use them for projections in the early 1950s and Riefler stated in 1953 that Brill "ha[d] been most ingenious and original in making the new material [the flow-of-funds accounts]'talk.'"[32] Indeed, in heir memo, the two officials advertised the SSRC project as a projection tool: "the project would undertake to explore the potentialities of econometric methods of projecting the economy's future performance and assess the utility of these methods as a supplement to other approaches to economic projection," they wrote.[33] The report therefore minimized the policy analysis goal of the project. Also, the reasons why Noyes and Young used the "projection" rather than hitherto anathema "forecast" language are unclear. It could have reflected a misunderstanding of the orientation of the SSRC model, or a deliberate strategy from DRS officials to stir the board toward greater use of up-to-date econometric techniques and the systematic use of forecasts for FOMC decisions. By the mid-

[30] "The Fed remodels itself," *Business Week*, May 16, 1964.
[31] The Board's discussion on the Committee's proposal for funding is summarized in the September 23, 1960 minutes of the meetings of the Board of Governors.
[32] Riefler to Committee on Selection of the Rockefeller Public Service Award, August 31, 1953, WWSPIA.
[33] Board minutes, September 23, 1960.

1960s, external and internal pressures had warmed the FOMC up to these new ideas. The combination of mechanical forecasts and judgments, of science and art would however prove a difficult endeavor.

4. Science in support of the Art: a difficult cross-fertilization

4.1 The development of a macroeconometric model

After the Committee's model was handed over to the Brookings Institution for further development, economists at the DRS sensed that the enhanced understanding of the transmission mechanisms whereby monetary policy decisions influence output and the systematic forecasts necessary to improve the Board's decision-making process required yet another macroeconometric model. During a 1965 conference on investment organized at the board, Brill reported that:

> the Federal Reserve is currently pursuing a comprehensive research project on linkages between monetary policy and the general economy. Working groups in the Federal Reserve have been formed to study: (1) the entire linkage process from Federal Reserve actions to spending decisions, (2) the linkages among money market variables, such as between open market operations and member banks reserves, and (3) the linkages between money market variables and more basic financial variables, such as between bank reserves and the money supply.[34]

The DRS economists were, however, not alone in believing that another model should be developed. According to University of Pennsylvania macroeconomist Albert Ando, the newly-funded SSRC Subcommittee on Monetary Research had come to the same conclusion.[35] Ando was then involved, with his former Carnegie colleague Modigliani, in a heated academic exchange with Friedman and his student David Meiselman over the respective influence of fiscal and

[34] Conference minutes, April 2-3, 1965, box 147, folder 812, SSRC1.

[35] Ando, "Introduction," undated but probably 1968, Box RW15 folder 'notebook," FMP. The Subcommittee was part of the Committee on Economic Stability. Initial meetings of what came to be the Subcommittee were held at and funded by the Board of Governors.

monetary policy over consumption and the business cycle. Monetarist ideas were gaining traction, which called for a detailed empirical reexamination of the influence of monetary variables on the real sector, and Ando and Modigliani were eager to direct such project from MIT and Penn.[36]

Settling an academic dispute through empirical work was arguably a different motive from improving Fed decision-making. But the minutes of the 1965 conference on investment attest that both Brill and Modigliani were aware of the complementarity of the two groups. "Because the SSRC working group [...] is already investigating the linkages between basic financial variables and final spending decisions, the Federal Reserve is presently concentrating most of its resources on the earlier linkages in the process," Brill explained. "Modigliani proposed the closest cooperation between the two groups," the minutes then read.[37] An official merger of the two projects was enacted in 1966, and the Board funded the joint model until December of 1970.[38] A Special Studies Section had been created at DRS to house their econometric work, and De Leeuw, its leader, was chosen to co-direct the new model project with Ando and Modigliani. He was seconded by Edward Gramlich, who joined the Board in 1965 after completing a PhD at Yale under Tobin on the aggregate demand impact of fiscal policy (Gramlich 1997).[39]

The resulting "Fed-MIT-Penn" or "FMP" model was different from the cohort of other macroeconometric models developed in the 1960s in that it "contains many more policy variables that can be used directly to represent the policy

[36]In a 1969 talk, DRS economist Edward Gramlich likewise insisted that "a hot dispute currently rages as to the importance of money in influencing econ activity … The FRB-MIT econometric model … originated in this controversy…. [Modigliani and Ando] were spurred on in an attempt to resolve their inconclusive interchange with Friedman-Meiselman in the 1965 *American Economic Review.*" In **"**Complicated and simple approaches to Estimating the role of money on economic activity," 06/05/1969, Box 1, EGP.

[37] Conference minutes, April 2-3, 1965, box 147, folder 812, SSRC1. Modigliani's *ex post* assertion that "the Fed wanted the model to be developed outside, the academic community to be aware of this decision, and the result not to reflect its ideas on how to operate" (2001, 101) therefore did not reflect the process through which the model developed.

[38] The model was handed over to Wharton Economic Forecasting Associates Inc. (WEFA) for maintenance and distribution. Ando to Hickman, July 19, 1971, box CO1, folder "Ando," Franco Modigliani Papers, Duke.

[39] See Backhouse and Cherrier (2018) for a detailed account of each group's perspective and on the organization of the (not so) joint work in developing the model.

actions of the monetary and fiscal authorities of the federal government."[40] It exhibited the usual blocks of equations (consumption, investment, financial sector, housing), but with twists that allowed for more numerous and refined transmission mechanisms that the cost of capital effects previous macroeconometric models relied on. The consumption equations allowed for detailed wealth effects and credit rationing in the mortgage market was also taken into account.[41] The influence of interest rates on state and local public expenditures also received specific attention (De Leuuw and Gramlich 1969).

The goals and modeling practices of the two groups were nonetheless different enough so that two models were initially worked out. Even after they were merged in 1968, several versions coexisted (Backhouse and Cherrier 2018). Brill had indeed warned Modigliani that he wanted to "preserve the identity of our [The Fed's] contribution to the project," suggesting that it would help them increase the staff devoted to model-building and remain autonomous in operating their version. He also need distinct identity so that the Board would not be publicly associated with strange model outputs: "Obviously it would have to be made clear that the did not necessarily reflect the views of the Board or the staff....I can foresee the possibility of distinct embarrassment to the System from widespread publicity given to strange results of early simulation runs of an untested model," he wrote Modigliani.[42]

The differences in purposes and approaches showed up on many occasions. At the beginning of 1969, Ando explained that the "academic side" wanted to postpone "the work involved in putting together the model and concentrated on improvements of each equation." On the contrary, the Fed team "must have a functioning system as soon as possible." They had already started operating their model in November 1967, almost a year before the MIT-Penn economists.[43] Constraints and purposes may have been different but other interventions speak

[40] Ando, « Introduction », *ibid*
[41] See Acosta and Rubin (2018) for a detailed analysis of the role of banks in the FMP model.
[42] Brill to Modigliani, July 6, 1966. Box 151, folder 1725, SSRC2. The note that appeared on the January, 1971 issue of the *Federal Reserve Bulletin* reporting the agreement reached with WEFA made it clear that the model currently at use at the Board was different from the one being distributed (p. 76).
[43] Ando to Brill, 01/10/1969, Box RW15, FMP

to a shared "macroeconomics" identity and how blurred boundaries between academic and policy-circles were. All participants understood the FOMC's reservations with "mechanical" forecasts. As he presented the model to the Board in 1968, Ando emphasized the complementarity between macroeconometrics and "experts' judgments":

> it is not necessary to use the model mechanically for the purpose of forecasting…it is easy to insert into the model judgmental forecasts made by experts for housing expenditures, and rerun the model to obtain the conditional forecast of all other variables… Thus, it is hoped that the model will perform many of the routine chores currently performed by the experts, and free them to concentrate on more crucial and difficult aspects of econ analysis.[44]

The next year, Gramlich repeated the same argument to the Committee on Banking and Credit Policy: "Model forecasts can increase the mechanical advantage of judgmental forecasters … models can take care of the major economic relationships and allow judgmental forecasters to worry exclusively about specific developments," he explained.[45]

Conversely, Fed economists were painfully aware that, beyond the intrinsic challenges of communicating the model's simulation to a skeptical FOMC, they had to cope with the fallouts of the debates between Keynesians and Monetarists *within* the Fed. The 1960s saw the emancipation of the Regional Banks' research departments, hitherto devoted to data collection. In the Federal Reserve Bank of St. Louis, Jerry Lee Jordan took over the practice of writing simple single-equations models, as Friedman and his PhD advisor Brunner favored (Rancan 2018). Together with Leonall Andersen, Jordan wrote a single-equation model which correlated the levels and differences in money supply and expenditures with income to assess the relative importance and speed of fiscal and monetary policy. It threw macroeconomists into years of theoretical, empirical, methodological, and policy intertwined debates. Modigliani, Ando, Brunner, Meltzer, and De Leeuw participated into a controversy played out in academic

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[44] Ando, "Introduction," *ibid*.
[45] Gramlich, "Recent experience with the FRB-MIT model," Presented to the committee on Banking and Credit Policy, New York, 11/06/1969, Edward Gramlich papers.

journals as well as Fed bulletins.[46] In Minnesota, staff economists Thomas Muench, Arthur Rolnick, William Weiler, and specific advisor and Minnesota professor Neil Wallace were working on a devastating assessment of the prediction generated by the FRB and Michigan Quarterly models.[47]

4.2 Bringing economic analysis to the board: the making of the Green and Blue Books

Around the time the Fed decided to build its own macroeconomic model, the internal pressure to rationalize the FOMC decision-making process resulted in concrete changes in procedures. First, Martin agreed to establish a second committee to reform the directive in the late 1960s. It was led by FOMC Secretary, and future Board Governor, Robert Holland, Governor Maisel, and DRS's James Pierce.[48] Second, Maisel credited himself with "the inauguration of a formal forecasting system […] The first memorandum I wrote after being appointed to the Board suggested the vital need for such a system. Projections of the GNP, credit, or the money supply were totally lacking at the time" (1972, 176). He asked the DRS to prepare data outlining recent past and "projected" evolutions of 9 series of variables, from monetary instruments, monetary and fiscal variables to real and nominal output, prices unemployment, and balance of payment variables.[49] Each table was accompanied with paragraph detailing the probable sources for recent evolutions (for instance negotiations in the steel industry explained some price changes), and the whole was assembled in a document titled "Current Economic and Financial Conditions" sporting a green cover. This so-called "Greenbook" was first distributed in advance of the June 10, 1964 FOMC meeting. An updated version was then circulated before every

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[46] See Rancan (2018) for an exhaustive account of the battles around the FMP and St Louis model, in particular the Andersen-Jordan equation.

[47] The paper was published in 1974, but it was under way in the late 1960s already (interview of Rolnick by Cherrier, June 2018, University of Minnesota)

[48] Pierce remembers that they also worked with MIT engineers on the application of control theory to their problem (Pierce 1996a, 34).

[49] Given the lag with which national statistics were made available at the time, what was being "forecasted" was in fact the present, and the short-term future, one or two quarters ahead (see Pierce 1995). Axilrod (2001, 41-42) explained that the term "projection" was purposely chosen over "forecast" because "the former term seemed more professional and less likely to raise questions about whether they did or did not represent satisfactory outcomes."

meeting, and the forecasts were more substantially updated every 3 months or so.

By the end of the year, Brill, now head of the DRS, seems to have asked staffer Stephen Axilrod to transform a chart of money indicators into an outline of possible scenarios for monetary policy operation the FOMC might choose from (Axilrod 2001, 45-46). These would be based on carefully chosen money parameters (interest rates, but also reserve measures), with the purpose of "quantifying," thus making more specific the instructions in the directive the FOMC transmitted to the New York desk manager after each meeting. The "Bluebook," originally implemented for the November 2, 1965 meeting, quickly grew in size. At the end of the discussion, "possible directive language" was presented as a set of 3 indicative policy alternatives (A, B and C).[50] A "Redbook" summarizing economic conditions by district was added in 1970.

These forecasts or "projections" were initially purely judgmental. DRS econometrician James Pierce later recounted that they were provided by what econometricians called "judgmental economists" or "business economists… really expert about what was going on in their sector […] You just stare at the wall and figure out what's going to happen—that's how the [Greenbook] forecasts were made" (1995, 31-32). A major issue was that this practice paradoxically left no role for monetary and financial variables to play. As they became available around the end of 1967, forecasts resulting from the FRB model's simulation did not *replace* these judgmental forecasts. DRS officials knew Martin and other board members' resistance to "mechanical forecasts," and they personally seemed to adhere to the idea that monetary policy operations required a blending of science and art. Brill, Axilrod, Pierce, and then Charles Partee and Lyle Gramley thus tried to blend judgment and econometric models. Maisel insisted that "policy is not based on a literal acceptance of any specific model [but] develops from...debate which allow[s] for the inclusion of judgments about the economy and the model and value judgments as to goals"

[50] See for instance the bluebook for the December 15, 1970 meeting : https://www.federalreserve.gov/monetarypolicy/files/FOMC19701215bluebook19701211.pdf

(1973, 180). To Pierce, the addition of model forecasts "forced a discipline" in that the monetary and real sector got better integrated.

The DRS's cautious approach to blending former practices with econometrics initially appeared quite successful, though it created tensions between the econometric "technicians" and other breeds of analyst.[51] It was, however, thwarted by their 1968 failure to predict the economic consequences of the tax surcharge implemented by Johnson to curb inflation. The decrease in spending predicted by the staff, which would support a pause in the tightening of monetary policy, failed to materialize and put the Fed in the uncomfortable position of implementing a restrictive policy and potentially suffocating the economy or waiting and potentially letting inflation mount (Bremner 2004, 252-254). Virtually all accounts by Fed protagonists consistently identify this forecasting debacle as a turning point, and it likely contributed to Brill's resignation in 1969.[52] Although Martin also leaned towards ease then, the 1968 failure contributed to his distrust of the staff's forecasts. As inflation intensified in 1969, Martin, now pushing for restraint, criticized the staff's confidence in their forecasts despite their previous failures. He and some Board members felt the staff and its projections had misled them, and Martin later reportedly reaffirmed that he would rather "dispense with the kind of analysis presented in the Bluebook" (Bremner 20014, 271) and added that "there is a disease called statisticalitis that could kill us" (p273).

Paradoxically, the internal purchase of econometric methods did not improve with Burns' nomination as chairman in 1970. Though he was the first Fed chairman to hold a PhD, and a widely renowned academic at that, "he was an institutionalist … more than anything else," Holland highlights. "He consumed economic data in big volumes," he added, in line with Richmond Fed's president Robert Black's recollections: "he believed in distilling—because of his work at the National Bureau—huge amounts of empirical information and drawing conclusions from that." Maisel (1973, 122) even remembered that he

[51] Pierce profoundly disliked being called a technician, which was done to dismiss any of his expertise besides the purely technical aspects of the model (Pierce 1995, 43).
[52] Morris (1994a, 8-10), Parthemos (1994, 11-13), Pierce (1995, 1-2), Axilrod (2011, 45).

supplemented Board's staff work with his own sources of information. In the NBER tradition, Burns believed that his own judgmental interpretation of this mass of data was more reliable that the output of econometric models. His self-confidence was also, by all accounts, psychological. Unlike Martin, often pictured as a "consensus-seeker," Burns was hailed as an "old autocrat" (Black 1994, 6). Black and Richmond Fed vice-president James Parthemos both considered that the chairman "ran those [FOMC] meetings like a graduate seminar" (Black 1994, 8; Parthemos 1994, 15). Burns also found the DRS much too "Keynesian" for his taste (Parthemos 1994, 16). Finally, he understood monetary policy making as a scientific, artistic, but also highly political endeavor.

Burns' style led to further marginalization of macroeconometrics in the decision-making process. Pierce, who became a major antagonist to Burns in this period, explained that the prioritization of short-term judgmental forecasts allowed the chairman to rely on conservative projections that vindicated his desire to tighten monetary policy in response to growing inflation. The econometrician acknowledged that these judgmental forecasts were more accurate that short-term econometric forecasts (see also Maisel 1973, 1981-2), but he believed econometricians were better able to predict if the economy might slip into recession with time (Pierce 1996a, 21). Burns did not, however, dismiss the Bluebook, as Martin had been tempted to do. He instead bent it to fit his own policy agenda. Pierce remembers that scenario B, the middle position, was toughly negotiated with Burns:

> The one they were supposed to vote on was B. And B was the thing that Burns wanted them to do, so he'd get together with Axilrod and he'd tell Axilrod what he wanted. And then we were supposed to come up with stuff that matched that. … It was all sort of a sham. The decisions were made ahead of time" (Pierce 1995, 22)

Former DRS advisor Peter Keir's recollections are consistent with Pierce's. He explained that then DRS director and Board advisor Charles Partee "would be very aggressive for, say, … typically for lowering the funds rate, which I think was alternative C in the Bluebook" (Keir 1994, 17) and then Burns would go for B, which was more moderate. It was all orchestrated.[53]

5. Conclusion

The claim that the history of central banking is characterized by a trend toward "scientization" is consensual. It is one seen in quantitative accounts of the transformation of the background of Fed recruits across time, and their growing contribution to academic journals. Yet, the story of how economic analysis was gradually embedded in the Fed's decision-making process outlined in this paper belies the idea of a linear irresistible takeover by newly minted PhD economists. To some extent, mounting criticisms of monetary policy operations since the 1950s spurred a gradual replacement of lawyers, bankers and businessmen with academically trained economists at the Board and the Regional banks. Symptomatic of this shift was the nomination of Burns, the first PhD-economist as chairman in 1970.

This transformation should no, however, be interpreted as a *replacement* of old style data and intuition-based evaluation of the economic situation by sophisticated large-scale macroeconometric models. At the DRS, which had always been directed by professional economists, several styles of research cohabited, were blended, or clashed. "Judgmental" and "mechanical" economics were combined in documents carefully crafted to appeal to FOMC members with diverse backgrounds. Furthermore, the shift toward new forms of analysis was engineered by economists who either had no PhD, or completed one during their career at the DRS. It was not their training that was key to their endorsement of macroeconometrics, but their participation into collective endeavors (for Brill) or the external pressures they faced (for Martin and FOMC members). Finally, the use of the books and underlying forecasts was resisted, by non-economists as well as by PhD economists who favored institutionalist styles of analysis. By the

[53] Holland explained that Burns relied on Partee and Lyle Gramley, whom he trusted as "analysts," to "filter the works of the rest of the econometrics modeling staff" (Holland 1994, 45).

mid-1970s, styles of research like macroecoeconometrics were already challenged in the academia, but they were still influential in the Fed's decision-making process. The road toward scientization was a long and bumpy one.

Appendix to chapter V

The information presented in the following tables was obtained from the Federal Reserve's History website (www.federalreservehistory.org), the *Federal Reserve Bulletin*, and the lists of dissertations published by the American Economic Association. When possible, the catalogues of university libraries were used to corroborate, or in some cases correct, the information present in the other sources.

Table 1: Board of Governors, 1951-1975 (PhD economists identified with *)

Apr-51	Apr-61	May-65	Feb-70	Mar-75
Martin [c]	Martin [c]	Martin [c]	Burns* [c]	Burns* [c]
Eccles	Balderston* [vc]	Balderston* [vc]	Robertson [vc]	Mitchell [vc]
Szymczak	Szymczak	Robertson	Mitchell	Coldwell*
Evans	Mills	Shepardson	Daane*	Holland*
Vardaman	Robertson	Mitchell	Maisel*	Sheenan
Norton	Shepardson	Daane*	Brimmer*	Bucher
Powell	King	Maisel*	Sherrill	Wallich*

Table 2: Appointments of Board members after first PhD economist in the period.

Name	Appointed	Years after PhD	Appointed by	Terminal degree	Institution
Balderston	1954	26 (1928)	Eisenhower [R]	PhD	Pennsylvania
Shepardson	1955		Eisenhower [R]	MSc (Ag., 1924)	Iowa State
King	1959		Eisenhower [R]	BS (1941)	Louisiana State
Mitchell	1961		Kennedy [D]	BA (1925)	Wisconsin
Daane	1963	14 (1949)	Kennedy [D]	PhD	Harvard
Maisel	1965	16 (1949)	Johnson [D]	PhD	Harvard
Brimmer	1966	9 (1957)	Johnson [D]	PhD	Harvard
Sherrill	1967		Johnson [D]	MBA (1952)	Harvard
Burns	1970	36 (1934)	Nixon [R]	PhD	Columbia
Sheenan	1972		Nixon [R]	MBA (1960)	Harvard
Bucher	1972		Nixon [R]	JD (1956)	Stanford
Holland	1973	14 (1959)	Nixon [R]	PhD	Pennsylvania
Wallich	1974	30 (1944)	Nixon [R]	PhD	Harvard
Coldwell	1974	22 (1952)	Ford [R]	PhD	Wisconsin

Table 3: Regional Bank presidents, 1951-1975 (PhD economists identified with *)

Reserve Bank	Apr-51	Apr-61	May-65	Feb-70	Mar-75
Atlanta	Bryan	Bryan	Bryan	Kimbrel	Kimbrel
Boston	Erickson	Ellis*	Ellis*	Morris*	Morris*
Chicago	Young	Allen	Scanlon	Scanlon	Mayo
Cleveland	Gidney	Fulton	Hickman*	Hickman*	Winn*
Dallas	Gilbert	Irons*	Irons*	Coldwell*	Baughman
Kansas	Leedy	Clay	Clay	Clay	Clay
Minneapolis	Peyton	Deming*	Galusha	Galusha	MacLaury*
New York	Sproul	Hayes	Hayes	Hayes	Hayes
Philadelphia	Williams*	Bopp*	Bopp*	Bopp*	Eastburn*
Richmond	Leach	Wayne	Wayne	Heflin	Black*
San Francisco	Earhart	Swan	Swan	Swan	Balles*
St. Louis	Johns	Johns	Shuford	Francis*	Francis*

Table 4: Presidents appointed between 1960 and 1975.

Name	Reserve Bank	Appointed	Years after PhD	Terminal degree	Institution
Clay	Kansas	1961		Law degree (NA)	Missouri
Ellis	Boston	1961	11 (1950)	PhD	Harvard
Wayne	Richmond	1961			
Swan	San Francisco	1961		BA (1932)	Berkeley
Scanlon	Chicago	1962			
Shuford	St. Louis	1962		Law degree (NA)	S. Meth. School of Law
Hickman	Cleveland	1963	26 (1937)	PhD	Johns Hopkins
Galusha	Minneapolis	1965		BA (NA)	Pennsylvania
Patterson	Atlanta	1965		Law degree (1928)	Harvard
Francis	St. Louis	1966		BA (Ag., NA)	Missouri
Kimbrel	Atlanta	1968		BA (Business, 1936)	U Georgia
Coldwell	Dallas	1968	16 (1952)	PhD	Wisconsin
Heflin	Richmond	1968		Law degree (1936)	Virginia
Morris	Boston	1968	13 (1955)	PhD	Michigan
Eastburn	Philadelphia	1970	13 (1957)	PhD	Pennsylvania
Mayo	Chicago	1970		MBA (1938)	Washington
MacLaury	Minneapolis	1971	10 (1961)	PhD	Harvard
Winn	Cleveland	1971	20 (1951)	PhD	Pennsylvania
Balles	San Francisco	1972	21 (1951)	PhD	Ohio State
Black	Richmond	1973	18 (1955)	PhD	Virginia
Baughman	Dallas	1974		MS (Ag., 1941)	Minnesota

Table 5: Directors of the Division of Research and Statistics, 1918-1977

Name	Period	Terminal degree	Institution
Willis	1918-1922	PhD (1897)	Chicago
Stewart	1922-1926	BA (1909)	Missouri
Goldenweiser	1927-1945	PhD (1907)	Cornell
Thomas	1945-1949	PhD (1928)	Brookings GS
R. Young	1949-1960	PhD (1930)	Pennsylvania
Noyes	1960-1963	BA (1934)	Missouri
Brill	1963-1969	MA (1937)	Columbia
Partee	1969-1974	MBA (1949)	Indiana
Gramley	1974-1977	PhD (1956)	Indiana

Table 6: Top DRS officials, 1950-1975

	Entered						
Name	**Fed**	**Top DRS**	**Left Fed**	**Term. degree**	**Institution**	**Entrance-PhD**	**Top-entrance**
Garfield	1929	1950	1966	NA	NA		21
Robinson	1934; 1956	1956	1946; 1961	PhD (1937)	Michigan	-3	22
Burr	1935	1951	1960	PhD (1925)	Stanford	10	16
Dembitz	1935	1956	1965	NA	NA		21
Youngdahl	1943	1952	1954	PhD (1949)	Minnesota	-6	9
Wernick	1945;1953	1967	1951;1974	BA (NA)	Emoklyn C.		22
Young	1946	1946	1967	PhD (1930)	Pennsylvania	16	0
Koch	1946	1955	1968	NA	NA		9
Brill	1947	1960	1969	MA (1937)	Columbia		13
Solomon	1947	1963	1976	PhD (1952)	Harvard	-5	16
Sigel	1947	1965		PhD (1953)	Harvard	-6	18
Weiner	1947	1968	1974	BA (NA)	Harvard		21
Noyes	1948	1950;1952	1965	BA (1934)	Missouri		2
Partee	1949;1962	1964	1956;	MBA (1948)	Indiana		15
Holland	1949	1961	1976	PhD (1959)	Pennsylvania	-10	12
Marget	1950	1950	1961	PhD (1926)	Harvard	24	0
Williams	NA	1950	1974	NA	NA		
Smith	1950	1965		MA (NA)	Colorado C.		15
Wendel	1951	1974		PhD (1966)	Columbia	-15	23
Axilrod	1952	1965		MA (NA)	Chicago		13
Keir	1953	1968		BA (NA)	Harvard		15
Eckert	1953	1967		PhD (1947)	Cornell	6	14
Taylor	1953	1970	1985	MBA (1949)	Columbia		17
Gramley	1955;1964;1980	1965	1962;1977;1985	PhD (1956)	Indiana	-1	10
Peret	1956	1975		PhD (1962)	Harvard	-6	19
Fisher	1958	1975		PhD (1958)	Columbia	0	17
Garabedian	1959	1970		MBA (NA)	American U.		11
Shull	1965	1968		PhD (1958)	Wisconsin	7	3
Lawrence	1965	1973		PhD (1963)	Michigan	2	8
Thomson	1965	1974		PhD (1966)	Chicago (GSB)	-1	9
Zeisel	1966	1969		PhD (1968)	American U.	-2	3
Pierce	1966	1970	1975	PhD (1964)	Berkeley	2	4
Kichline	1966	1974		PhD (1968)	Maryland	-2	8
Ettin	1968	1971		PhD (1962)	Michigan	6	3
Chase	NA	1971		PhD (1960)	UC Berkeley		

VI. Conclusion

In this dissertation I have made a contribution to both the institutional history of the Federal Reserve and the history of macroeconomics. I have studying related episodes of the US postwar that showcase the transformation of economic analysis at the Federal Reserve and the development of macroeconometric models. Chapter two, "Robert Roosa and Paul Samuelson on the effectiveness of monetary policy," discussed the differences between Paul Samuelson's and Robert Roosa's early 1950s views on the behavior of lenders—mostly commercial banks—and their importance for the effectiveness of monetary policy. I used journal articles and other published sources, including congressional hearing records, together with correspondence to show that Roosa's and Samuelson's arguments were constructed from different perspectives, with different ingredients, and under different criteria of goodness. Roosa, an official from the Federal Reserve Bank of New York, offered a money-market insider's view. He insisted on the importance of the ability of the Federal Reserve's Desk's officials to "read" the market and carry out open-market operations accordingly, and he criticized previous, overly theoretical approaches to the analysis of monetary policy that did not take into account the detailed institutional characteristics of the financial system. Samuelson, by then already a major figure of the new mathematical economics, did not offer a model but did set the problem in terms of individuals, rational behavior, and equilibrium positions, and reduced the discussion to the compatibility of these elements with credit rationing. Samuelson was relatively well informed about the monetary institutions and the practices of the participants in the monetary markets, but he suspected market participants' own understanding of monetary phenomena and read evidence through the lenses of price theory. The differences in their arguments are also an early example of the type of tension that existed—and would intensify during the 1960s—between mathematical and econometric arguments on the one hand, and verbal arguments based on such things as the "tone and feel" of the market on the other. The latter approach dominated the Federal Reserve until well into the 1960s when, as we discussed in chapter five, it came under pressure by economists.

The next two chapters focused on the 1960s and on how large-scale macroeconometric models were built. Chapter three, "Macroeconometric modeling and the SSRC's Committee on Economic Stability, 1959-1963," dealt with the construction of a large-scale macroeconometric model where officials from the Federal Reserve's Board of Governors participated and that is a direct predecessor of the Board's first macroeconometric model. Erich Pinzón-Fuchs and I used the records of the Social Science Research Council (SSRC) to provide a detailed account of the establishment of the SSRC's Committee on Economic Stability and the construction of its macroeconometric model during the early 1960s. The Committee's work set the bases for the subsequent construction of the Brookings Model (1963-1972) and the Federal Reserve Board-MIT-University of Pennsylvania model (FMP, 1966-1970), and it also helped large-scale macroeconometric modeling consolidate as a scientific practice at the frontier of macroeconomics in the 1960s. It was the first model-building enterprise of this size—involving a team of more than 20 researchers working from different places and on different sectors of the model—and the project's many challenges in terms of logistics, data, and computing capacity evidence the importance of configuring a specific institutional and material context necessary to develop this scientific practice. In addition, we showed that the Committee was successful in bringing together academics, economists working at think-tanks, and government officials—including people from the Federal Reserve Board's staff.

In chapter four, "Bank behavior in large-scale macroeconometric models of the 1960s," Goulven Rubin and I discussed how the behavior of banks was modeled in a series of large-scale macroeconometric models. We focused on two aspects in particular: (1) the implementation of a portfolio choice framework, and (2) the challenges involved in incorporating credit rationing. The list of models considered is not exhaustive. Instead, we followed the work done by researchers associated with the Committee on Economic Stability of the SSRC. This includes the Committee's macroeconometric model (1960-1963), the models by Goldfeld (1966) and Ando and Goldfeld (1968), and several preliminary versions of the FMP model (1966- 1970). This comparative strategy produced two main conclusions. First, the specification of the equations that implemented the portfolio choice framework in the FMP model was intentionally and significantly more “transparent”—a term used by the FMP project directors—in its connection with theory than was the case for the previous models. This can be seen both in the clearer derivation of equations from a maximization program as well as in the overall simplification of the model’s structure to make it more recursive. Second, the belief that credit rationing was an important phenomenon for the effectiveness of monetary policy was incorporated in the first models despite its non-observable nature and the lack of clear guidance from theory. A series of proxy variables was used to capture some of the effect of credit rationing, but these efforts had very limited success. These efforts, however, show the importance of the modeler’s belief in the importance of credit rationing, even if it escaped statistical confirmation.

Despite the pressure that economists’ criticism put on the Federal Reserve, these two papers show that collaboration among academic economists and some government officials was possible. To be sure, the extremely open approach that Lawrence R. Klein and James Duesenberry—the coordinators of the Committee's model project—followed contrasts with the much tighter grip that Franco Modigliani seems to have had over the FMP model. But the common belief in the usefulness of econometric modeling for policy analysis seems to have been enough to make communication and work towards a common goal possible. The challenges that the macroeconometric model-builders faced when dealing with credit rationing also offer a continuation of the story I presented in chapter two, and they show that the role of banks in the effectiveness of monetary policy remained an interesting but difficult issue to assess.

The last chapter is in many ways an outcome of the other three and ties everything together. In chapter five, "The transformation of economic analysis at the Federal Reserve during the 1960s," Béatrice Cherrier and I discussed the evolution of economists and economic analysis at the Board of Governors and the Federal Open Market Committee (FOMC) during 1960s and early 1970s to elucidate the complex context in which new methods found a place at the Federal Reserve. We used data on Federal Reserve officials and previously unused primary sources to complement the view that can be constructed about this period from previous commentators, most of them participants in this episode of the history of the Federal Reserve. We argue that the arrival of econometric modeling and forecasting were the outcome of both external and internal developments. Economists, both Keynesian and Monetarists, criticized the Federal Reserve's expertise in monetary policy and the Board of Governors responded by creating spaces where Fed officials and academics could meet. Also, the Board's Division of Research and Statistics pursued macroeconometric modeling, collaborating with outside economists of the SSRC's Committee on Economic Stability and later the Subcommittee on Monetary Research in the construction of the FMP model. Forecasts entered monetary decision-making in the mid 1960s in the form of two documents, the Greenbook and the Bluebook. A frequent interpretation of the changes that the Fed went through is that the arrival of trained PhD economists was the main driver of the adoption of new methods. Our research shows that it was rather the opposite. The arrival of PhD economists trained in econometrics reinforced a process that had already been started and supported at the Division of Research and Statistics in the early 1960s by people like Daniel Brill and Frank de Leeuw. Our research also showed that the adoption of forecasts was by no means straightforward. They had found a place at the Board by the late 1960s, but bad forecasts made chairman Martin and others at the Board distrust them. The next chairman, Burn, did not care much for forecasts, but found a way to use them to his advantage despite that.

These four chapters exemplify the type of results that a concern for the practices of economists and government officials can produce, and it suggests that the marginal gain of further studies into the history of economic analysis at the Federal Reserve and of macroeconometric modeling is high. I thus intend to continue on this path, and my current research agenda develops the topics studied in this dissertation. The first promising question is the evolution of the Board's Division of Research and Statistics from the late 1920s to 1960, which would fill the gap between the work carried out by Yohe (1990) and chapter five. As I mentioned in the introduction, contrary to the 1960s, there is abundant archival material up to the mid 1950s that will greatly facilitate the task. Particularly promising is the archival material related to the two directors of the Division during this period, Emanuel Alexander Goldenweiser (1927-1945) and Ralph A. Young (1945-1960), which should help us better understand the type of work done during this period, its uses, and its connection to the type of work carried out by the National Bureau of Economic Research.

Similarly, Robert V. Roosa deserves a closer look. After leaving the New York Reserve Bank in 1961 he was an important influence at the Treasury for a couple of years before going to the private sector, although he remained an important voice in monetary discussions well into the 1970s. But beyond his notoriety, his life can tell us about the type of work carried out by the Research Department at the New York Federal Reserve and the Treasury, and about the particular approach to monetary policy that emerged at the New York Federal Reserve. Furthermore, he can offer an interesting contrast to the trajectories of other monetary economists of his generation like James Duesenberry, Franco Modigliani, and James Tobin, who developed their careers in academia, and help us understand better the differences between academics and Fed economists at the time. I accumulated a large amount of archival and published material related to Roosa during my research for chapter one and I'm in the process of organizing it and plan to turn it into an intellectual biography.

Regarding the history of macroeconomics, the next step is essentially to continue the story of the Committee on Economic Stability and its related projects during the

1960s and early 1970s. In a forthcoming paper, Erich Pinzón-Fuchs and I (2019) have discussed the role that the Committee's activities played in peddling the use of macroeconometric models for policy analysis, and we are currently working on a paper about the Brookings model (1963-1972) based on the archival records of the Brookings Institution. At the same time, I have only partially explored the archival material related to the Committee's Subcommittee on Monetary Research, whose main project was the construction of the FMP model but that also organized supplementary work and discussions that informed the model project.

As a whole, further work on the Committee could illuminate at least two big questions in the history of economics. First, we simply don't know enough about these models. As I noted in the introduction and in chapters three and four, these models have been largely neglected by historians, so in and of itself there's a net contribution to be made by further work into understanding how these models were built. Furthermore, a better understanding of the material conditions in which these models were built, who built them, and what for, would throw light on the dynamics of the profession and its relationship with government agencies and private businesses. Almost no attention has been paid to the legions of economists and government officials that participated in the construction of models for the Federal Reserve, the Department of Commerce, and the Congressional Budget Office, nor have forecasting services providers like Data Research Inc. or Chase Econometrics been studied. Similarly, and despite its monumental importance, not much attention has been paid to the study of the development of econometric software besides Renfro (2004; 2009). There is thus an enormous work to be done regarding our understanding of macroeconometric models, and studying the work of the Committee is an important and necessary step in this direction given its centrality for the consolidation of macroeconometric modeling.

Besides casting light on a segment of the history of macroeconomics that has simply not been given enough attention, there's another big question regarding the story that macroeconomists like to tell themselves about the 1970s and the rise of new classical macroeconomics. This story, in which the large-scale models were abandoned after their demonstrated failure to deal with 1970s inflation and the Lucas critique, is too centered on academic debates and essentially neglects the fact that these models continue to exist until today. Of course, large-scale macroeconometric models had to be adapted, but they were not completely abandoned. A careful study of these models will help historians move beyond the participant's history that dominates the profession understanding of their field.

Finally, I would like to point out, again, that we should not loose sight of the bigger picture in which all of these research questions fit. They all deal with the influence that economists have had in shaping economic policy, and as such they would benefit from the vast literature on this topic for it can help frame historical research by offering examples and useful generalizations (see Hirschman and Berman 2014). As historians of economics we should deploy our detailed and nuanced knowledge of the development of the profession to engage with and contribute to this literature. And in the case of the Federal Reserve, in particular, an important challenge is to work towards a dehomogenization of the “economist” label. My research in this dissertation shows that this question matters and that it’s difficult to put economists of different generations and with different education in one bag—the variety of PhD programs, generations, and specialties at the Board of Governors being an important example. The Federal Reserve has not always kept pace with academic economics and we should look closely at how the differences in practices and criteria of scientificity between economists, and between economists in academia and government, have shaped the policy discussion.

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BIA Brookings Institution Archives, Economic Studies Program Project Files, 1960-1982, box 16.

EGP Edward Gramlich papers, Bentley Historical Library, University of Michigan.

FMP Franco Modigliani papers, Rubenstein Library, Duke University.

JFKA John Fitzgerald Kennedy online archives.

JHWP Papers of John Henry Williams, Federal Reserve Bank of New York.

PASP Paul A. Samuelson papers, Rubenstein Library, Duke University.

SSRC1 Records of the SSRC, Record Group 1, Accession 1, Series 1, Subseries 19. Rockefeller Archive Center.

SSRC2 Records of the SSRC, Record Group 2, Accession 2, Series 1, Subseries 23. Rockefeller Archive Center.

TUMA The University of Michigan Archives, Bentley Historical Library. Michigan Historical Collections.

WWSPIA Woodrow Wilson School of Public and International Affairs Records, box 35, folder 17. Seely G. Mudd Manuscript Library, Princeton University.

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