THE TRANSFORMATION OF ECONOMIC ANALYSIS AT THE BOARD OF GOVERNORS OF THE FEDERAL RESERVE SYSTEM DURING THE 1960S

BY JUAN ACOSTA AND BEATRICE CHERRIER*

Abstract

In this paper, we build on data on officials of the Federal Reserve System, oral history repositories, and hitherto under-researched archival sources to unpack the tortuous path toward crafting an institutional and intellectual space for postwar economic analysis within the Board of Governors. 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 to the spread of new macroeconomic theories and econometric techniques. We argue that the rise of the number of PhD economists working at the Fed is a symptom rather than a cause of this transformation. Key to our story are a handful of economists from the Board of Governors' Division of Research and Statistics (DRS) who did not hold a PhD but envisioned their role as going beyond mere data accumulation and got involved in 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.

* Universidad de los Andes and CNRS-THEMA, University of Cergy Pontoise.

This “preprint” is the peer-reviewed and accepted typescript of an article that is forthcoming in revised form, after minor editorial changes, in the *Journal of the History of Economic Thought* (ISSN: 1053-8372), issue TBA. Copyright to the journal's articles is held by the History of Economics Society (HES), whose exclusive licensee and publisher for the journal is Cambridge University Press (<https://www.cambridge.org/core/journals/journal-of-the-history-of-economic-thought>). This preprint may be used only for private research and study and is not to be distributed further.

The preprint may be cited as follows:

Acosta, Juan and Beatrice Cherrier. The transformation of economic analysis at the Board of Governors of the Federal Reserve System during the 1960s. *Journal of the History of Economic Thought* (forthcoming). Preprint at SocArXiv, osf.io/preprints/socarxiv

I. INTRODUCTION

In 2018, Jerome Powell, a trained lawyer who spend his career in finance and politics, was appointed chairman of the Board of Governors of the Federal Reserve System. This represents a throwback to a time when non-economists ran the “Fed.” Up until the early 1970s most of the Board’s governors and the presidents of the Federal Reserve Banks were not economists but bankers, lawyers, or businessmen; among the first nine chairmen, six were bankers (William Harding, Roy Young, Eugene Meyer, Marriner Eccles, Thomas McCabe, William Martin) and three had a background in law (Charles Hamlin, Daniel Crissinger, Eugene Black).¹ Academic credentials were much less important than practical experience in the business and banking world, either in the private sector or at the Federal Reserve System (Maisel 1973, Stockwell 1989; Axilrod 2011; Meltzer 2009). 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 governors and presidents has increased substantially, and of the last Board chairmen—from Arthur Burns (1970-1978) to Janet Yellen (2014-2018)—only George Miller (1978-1979) had neither an MA nor a PhD in economics.

The postwar transformation of the Fed was not restricted to top decision-makers.² François Claveau and Jérémie Dion (2018) estimate that economists at the

¹ Note that the position of chairman of the Board of Governors exists only since the 1935 Banking Act. The pre-1935 officials named above had the position of Governor of the Federal Reserve Board. Similarly, before 1935 the Reserve Banks had governors instead of presidents.

² The Federal Open Market Committee, which presides over open market policy, consists of the seven members of the Board of Governors, the president of the Federal Reserve Bank of New York, and four of the eleven presidents of regional banks, who serve on a rotating basis. The Board of Governors sets the discount rate and reserve requirements. Throughout this paper, we will 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.

research departments of the Board and the Federal Reserve Banks today represent nearly 50% of the money and banking economists listed by the *American Economic Association* (p. 353); they publish a growing share of academic papers (p. 355), and these tend to have a greater impact than those published by economists outside central banks (p. 362; see also Fox 2014, Bordo and Istrefi 2018, Ban 2018). This closer relationship between central banks and academia has been tied by historians and sociologists to a “scientization” of central banking. The polysemic term sometimes refers to the rationalization of the decision-making process and the growing use of standardized, calculable rules by a committee rather than a single chair (see Marcussen 2009) or their reliance upon “cutting edge economic thinking” (Mudge and Vauchez 2016, 153).³ It sometimes refers to changes in the training and career of central bankers themselves (Conti-Brown 2016), or to the growing number of PhDs in the staff (Claveau and Dion 2018, Lebaron and Dogan 2016) after the PhD had become the marker of economic technical mastery in the mid-1960s (see Fourcade 2009). Participants narratives have also pointed out the connection—sometimes disconnection—between academic macroeconomics and economic analysis at the Fed (Mankiw 2006; Woodford 2009),

These accounts of the “scientization” of central banks however often rely on collective biographies, and on a top-down approach which centers on the transformations of the policy-making decision process and emphasizes the visions of chairpersons and board members. Yet any “scientization” process implies that some space was created inside the Fed for economic analysis to be developed and

³ See also McGregor and Young (2013) on Regional Fed presidents’ growing reliance on macroeconomic research in their public speeches.

disseminated. What kind of space was created, where, by whom, in short, the prehistory of this trend toward scientization, however remains a blind spot of the flourishing literature. In this paper, we thus build on data on Fed officials, oral history repositories, and hitherto under-researched personal archival sources to unpack the tortuous path toward crafting such an institutional and intellectual space for postwar developments in theoretical and empirical macroeconomics within the Board of Governors. As of 2019, it is not possible to access any internal Board archive on the development of in-house economic analysis and its use by the Board, so that our purpose is *not* to document its possible influence on the monetary policy decision-making process. Rather, it is to track how and why economic knowledge developed at the Board, under what set of constraints and affordances, and for which purposes.

We show that growing attention to new macroeconomic research and practices at the Board during the 1960s was the combined result of the mounting external criticisms of the Fed's decision-making process, an internal push for quantification, and the staff's interest in macroeconometric modeling. This transformation in the type of economic analysis produced at the Board only partially fits the oft-described process whereby mathematical and econometric modeling displaced previous forms of analysis (Morgan Rutherford 1998). While the development of new practices at the Board's Division of Research and Statistics (DRS) involved a break with the existing institutionalist tradition at the Board, the result was not a takeover of "old" types of empirical analysis by new ones reflective of a generational clash. Instead, macroeconometric modeling was welcomed by researchers of diverse backgrounds, and its forecasts were integrated with a longstanding tradition of judgmental analysis and scenario writing. Science did not replace art, and econometrics did not replace

institutional analysis. Rather, these perspectives were initially blended into the Greenbook and the Bluebook prepared for FOMC meetings. Finally, we show that the variety in the staff's training (in terms level of education, period, and university) suggests that the divide between PhD and non-PhD economists, as well as between academic and central bank researchers, is only partially relevant to understand the shift in the type of economics practiced at the Board and the efforts to carry it to the FOMC.

II. 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, p. 55ff). By the end of the 1950s, however the Fed faced mounting criticisms from governmental bodies, the financial community, congressional committees, and the press.⁴ Though sometimes highly political, most of these attacks were also fueled by academic economists who essentially faulted the Fed for not relying on economic science to guide their policymaking.

⁴ Maisel (1973, pp. 26-29) describes some of these pressures, which continued into the 1960s. New economic phenomena, in particular mounting inflation from 1966 onward and the deterioration of the Bretton Woods system added to the challenges.

Dissatisfaction with policy instruments and objectives

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 1962. He explained that monetary policy was lacking effectiveness in controlling expenditure on capital equipment and business inventories, yet was "too effective" in restraining residential construction, public projects, and small businesses (Knipe 1962, p. 20). More generally, critics considered that the Fed was "stunting national economic growth" by maintaining interest rates too high under the influence of private banking interests (p. 64).

The Joint Economic Committee of the Congress published a critical report in 1959 and two more in 1960, and another one was issued the next year by the Commission on Money and Credit.⁵ President John F. Kennedy's Council of Economic Advisers (comprising Minnesota tax expert Walter Heller, Yale macroeconomist James Tobin, and budget specialist Kermit Gordon) also relentlessly criticized the choice of targets. They flooded the president with memoranda 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.⁶ The same year, Tobin published a vocal critic of the Fed in *Challenge*. The essential problem was the

⁵ The Commission was established by the Committee on Economic Development, a nongovernmental institution focused on public policy research. Members of the Commission included Marriner Eccles, Adolph Berle, David Rockefeller and Theodore Yntema. The research director and his deputy were Harvard's Bertrand Fox and MIT's Eli Shapiro. Lester Chandler, Paul Samuelson and Sumner Slichter, among others joined the advisory board (Knipe 1962; Brunner 1961).

⁶ CEA to President, "Monetary Policy: High Time for Action," April 6, 1961. JFKA. See Cherrier 2018 for a full account. See also the comments reported by Bremner (2004, p. 150).

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, p. 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 (p. 26). 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.

In a study commissioned by the House Committee on Banking and Currency, 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 future monetarist precepts, 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, p. 84).⁷

Dissatisfaction with the Fed’s policy decision-making process

These economists also criticized *how* the Fed arrived at its policy decisions. Brunner and Meltzer (1964a, p. viii) emphasized that “there is a more important series of questions that has not been asked very often: are the procedures for making monetary

⁷ As detailed in Rancan (2020, p. 15), Friedman took the opportunity of the establishment of the Academic Consultant Meetings described in the next section to voice similar arguments to the Board.

policy adequate? Does the Federal Reserve have adequate information in sufficient time to make appropriate decisions?” The FOMC met every three weeks, they noted, which was too often, 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. It was also monitoring too many variables, from yields to liquid assets, rates and required reserves, reflecting vague and contradictory definitions of concepts like “credit” and “availability.” “The very mixed nature of these criteria reveals the absence of a coherent conception,” they concluded (1964a, p. 4).

“Heated” discussions took place during FOMC meetings, but they 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, p. 92) bemoaned. The result was indecision and a lack of direction. For instance, the open-market policy directives that the FOMC issued to the manager of the Fed’s Trading Desk—located at the New York Fed—were very loose, of the “ease of restraint” type. This left a huge discretionary power to the New York Fed, which Martin attempted to reduce. A consensus on how to draft more specific directives, however, proved difficult to reach. Brunner and Meltzer’s suggestion for reform was consequently radical: “serious consideration should be given to replacing the FOMC with a single administrative official,” they wrote (p. 93).

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 of the reasons for the Fed’s major policy decisions and actions resulted

in “a tendency to seize upon even the most outlandish rumors as significant” (Knipe 1962, p. 40). There was, however, a more fundamental attack behind the demand for transparency. The key issue, highlighted by Tobin 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,” Tobin (1961, p. 24) noted.

Suppressing this newfound independence was precisely the agenda of the chairman of the House Committee on Banking and Currency, Texas congressman Wright Patman. Dubbed “the populist scourge of the Fed,” he was a staunch opponent to high interest rates and to the separation of monetary and fiscal policy, which he constantly challenged after 1951 (Conti-Brown 2017, 274-276; Young 2000, ch. 7, 8). In 1964, he proposed to hold four months of hearings for the 50th anniversary of the 1913 Federal Reserve Act (Congress 1964), 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 regarding the definition of monetary policy. President Lyndon Johnson’s intervention, at Martin’s request, thwarted Patman’s plans, but the criticisms outlined in the Brunner-Meltzer report, commissioned by the Committee, 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, p. 32). It was suggested that either the chairman of the CEA or the Secretary of the Treasury sit on the Board of Governors.

Dissatisfaction with the lack of scientific underpinnings

Underlying these criticisms of monetary policy was the shared notion that “after 50 years the Federal Reserve ha[d] not yet provided a rational foundation for policymaking” (Brunner and Meltzer 1964a, p. ix). For those policy-oriented yet academic economists involved in monetary debates, “rational foundation” meant science-based. Brunner and Meltzer were explicit that the Fed “should develop and test a theory incorporating the essential elements of the money supply process” (1964c, p. 83). This 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, p. 26; see also Maisel 1973, p. 168).

Monetary theories were being hotly debated at the time, with no consensus in sight.⁸ 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. There was a wealth of research on the scope of lags between the Fed’s decision and the shift in the effective money supply, the changes in credit availability or in short and long-term interest rates on the market, and the effects on production and unemployment (Knipe 1962, p. 42-43). For Brunner and Meltzer (1964a, pp. 2-3), however, 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 and *empirical*

⁸ A 1962 survey of monetary economics opened with the statement 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, p. 14).

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 it was not doing the right kind of science. The Board had a Division for Research and Statistics (DRS) and the Federal Reserve 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 points of the business cycle—which even Brunner and Meltzer acknowledged (1964a, p. 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, p. 3) claimed. “Collection and preparation of data not guided by an explicit analytical frame often leads to a pointless accumulation of data” (p. 3).¹⁰

Martin, the Board, and the Fed in general could not ignore this swelling tide of criticisms. Economists with opposed views of the transmission channels and monetary policy nevertheless converged in their criticism of the Fed, and their arguments were powerful ammunition for the those in Congress that constantly threatened the Fed’s

⁹ Franco Modigliani and Milton Friedman made similar remarks to the Board in 1965, see Rancan (2019, p. 451).

¹⁰ The accusation of “pointless accumulation of data” echoes the “Measurement without theory” debate, prompted by the scathing review of NBER’s Arthur Burns’s *Measuring Business Cycles* (1946) published by Tjalling Koopmans (1947) from the Cowles Commission. See Morgan and Rutherford (1998), Mirowski (1989), and Rutherford (2011, pp. 277-282) for a discussion of this controversy. Brunner’s criticism of pure data gathering, however, must not be taken to be an endorsement of the Cowles Commission approach to the relationship between theory and empirics. See Hoover (2018) for a discussion of Brunner’s approach to empirical work.

independence.¹¹ A year after leaving the Board, former Governor Sherman Maisel 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, p. 26). Martin was acutely aware of these external pressures, yet the Fed lacked a unified vision on how to respond: Kennedy and Lyndon Johnson appointed younger Keynesian macroeconomics-trained Board members more favorable to newer empirical techniques than other members. Staff members exhibited a growing variety of approaches, no matter what their age or training was, which resulted in fragmented views of the knowledge which should be produced and presented to the FOMC.

III. INSIDE THE BOARD

Change and resistance at the FOMC

Kennedy and Johnson were, Heller reflected after completing his term as chairman of the Council of Economic Advisers, “the first modern economists in the American presidency” (Heller 1967, p. 37). Table 1 displays the consequences of these presidents’ inclination towards economics: four of the five Governors whom they appointed were economists, and three of them had PhDs. But this did not mean that they brought a standardized view of how monetary policy should be conducted.

¹¹ As former Governor Sherman Maisel pointed out, Patman “always had a bill to do away with the powers of the Fed. There was a general recognition that you had to handle the Congress in such a way that Patman wouldn’t get his way” (2009, p. 27)

Table 1: Appointment 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

Source: Federal Reserve's History website (www.federalreservehistory.org), the *Federal Reserve Bulletin* (various issues), and the lists of dissertations published by the American Economic Association in the *American Economic Review* (various issues).

Kennedy's first appointment was George Mitchell (app. 1961), a tax economist with a BA from the University of Wisconsin and who at the time was the vice president of the Federal Reserve Bank of Chicago. A handful of economists had served as Governors before, but the first economics PhD had been appointed by Eisenhower in 1954. Canby Balderston (app. 1954), aged 57 at the time of his appointment, had a PhD from the University of Pennsylvania (1928) and was the Dean of the Wharton School of Finance and Commerce at the time.¹² 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 (app. 1963), who graduated in 1949 with a doctorate in public administration,¹³ had previously worked at the Federal Reserve Banks of Richmond and Minneapolis as well as the Treasury; Sherman Maisel (app. 1965), who also graduated in 1949, was a professor at Berkeley; and Andrew Brimmer (app. 1966), who became the first African American to serve on the Board, had graduated in 1957, then worked at the New York Fed and the US Department of Commerce.

¹² 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.

¹³ His dissertation was, nonetheless, listed in the 1949 AEA's list of doctoral dissertations in political economy. It is also noteworthy that Daane, a Fed insider supported by Martin, was not the first choice. The Treasury wanted Seymour Harris, whose priorities aligned more with the Kennedy's administration (Daane 2006, p. 14).

Table 2: Reserve Bank 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

Source: Federal Reserve’s History website (www.federalreservehistory.org), the *Federal Reserve Bulletin* (various issues), and the lists of dissertations published by the American Economic Association in the *American Economic Review* (various issues).

Table 2 lists information on the presidents of the Federal Reserve Banks from 1950 through the mid 1970s. When Martin was appointed as chairman there was only one 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 at Berkeley and had his career at the Fed 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, p. 4). By the time Burns was appointed chairman of the Fed in 1970, however, there were five PhDs serving as presidents of Federal Reserve Banks, and by 1975 they had become a majority.

The fact that Martin himself—a banker and former president of the New York Stock Exchange—had no background in economics was increasingly pointed out.¹⁴ 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, p. 5; see also Maisel 1973, p. 122-123). This momentum contributed to the

¹⁴ Bremner (2004, 24) reports that he started a PhD in finance at Columbia in 1935, although he did not graduate.

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, p. 11).

But PhD credentials did not fully determine chairs’ attitude toward economic knowledge or the conduct of monetary policy. If Martin lacked academic credentials in economics, he nevertheless 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 1960.¹⁵ 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 Massachusetts Institute of Technology’s Franco Modigliani, among others, participated in the first meeting. Milton Friedman visited the Board a few weeks later, and he and Meltzer participated in future meetings, which became a regular event, taking place at least once a year from then on.¹⁶

What Martin and the older bankers at the FOMC would not endorse was their younger colleagues’ push for quantification and rationalization. For the shift in the FOMC’s demographics created a rift between “two bands: younger, Keynesian-laden

¹⁵ Bach to Modigliani, dated November 15, 1953, but the year is a typo, it was written in 1963. FMP. See Also Martin to Modigliani, February 4, 1964.

¹⁶ Bach to Modigliani, 01/08/1964. Topics included “lags and signals for monetary action,” “the demand-time deposits mix problem” or “the quality of credit problem.” Modigliani’s papers include material from academic consultants meetings up to the early 1980s. G. L. Bach remained the organizer.

staff vs traditionalists” (Maisel 1973, pp. 215-216). Maisel relentlessly fought for the inclusion of new indicators and formal forecasts in FOMC deliberations (Bremner 2004, p. 253). 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 Martin countered 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, p. 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, p. 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.

¹⁷ Bremner (2004, p. 230n18). Holland to Modigliani, 01/20/64. The push for quantification did not mean that the new economists at the Board disregarded the complexities of monetary policymaking. 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, p. ix).

By the mid-sixties, this blend of enthusiasm, suspicion, and external pressures was opening a space whereby new forms of economics analysis could enter the policymaking process at the Fed. Absent a clear vision about how to proceed, occupying such space was left to the Board's staff, itself increasingly knowledgeable of the latest developments in academic macroeconomics.

The Board's 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 The DRS being less visible than the FOMC, systematic information was more difficult to gather. Tables 3 and 4 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 had been recruited in the pre-Martin era held a PhD, and by the end of Martin's tenure the number had risen to 65% (13/20). There was, however, more diversity among PhD programs than at the FOMC, which was dominated by a Harvard pipeline.

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—has 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.

Table 3: Directors of the Board's 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

Source: Federal Reserve's History website (www.federalreservehistory.org), the *Federal Reserve Bulletin* (various issues), and the lists of dissertations published by the American Economic Association in the *American Economic Review* (various issues).

Table 4: Top DRS officials, 1950-1975

Name	Entered Fed	Top DRS	Left DRS/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		

Source: Federal Reserve's History website (www.federalreservehistory.org), the *Federal Reserve Bulletin* (various issues), and the lists of dissertations published by the American Economic Association in the *American Economic Review* (various issues).

These tables also highlight that, assuming the number of top officials is positively related to the total size of the DRS, the latter had substantially grown in size. This suggests that its role had expanded within the Fed. In the fifties, the number of top officials grew from four to eight, then stagnated 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 1975.²⁰ With the exception of Ralph Young (dir. 1949-60) and Arthur Marget, who were directly hired in top positions, and Guy Noyes (dir. 1960-3), whose promotion was expedited, new recruits climbed the ladders within the DRS. Before 1960, reaching a top position took an average of sixteen 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. The data also highlights that a sizeable number of these economists obtained a PhD *while working at the DRS*. Table 4 shows that some of the early DRS economists completed it ten to fifteen years after recruitment; for the later period most got their PhDs a couple of years after entering the Fed, which most likely means they were hired right after finishing their coursework. This challenges the usual separation between academic economists and practitioners (or central banking economists), and suggests that DRS economists in the postwar were

²⁰ We don't have detailed information about the size of the staff employed at the DRS at all points in time during the 1960s. In 1966, however, Brill reported that of the "approximately" 180 members of the DRS, "about" half were economists or senior research assistants, "the bulk" of which carried out "current analysis and/or more basic research projects" and "about" a third worked on maintaining key time series. Brill to Brimmer, March 8, 1966, p. 1, AFBP.

knowledgeable of the latest development in monetary theory and empirical techniques as taught in professionalized PhD programs.

As with the FOMC, however, counting PhDs does not allow to grasp the scope and nuances and the transformation that took place at the DRS during the 1960s. Oral histories and personal archives show that it was not one characterized by a generational replacement, but more by a handful of key researchers slowly transitioning between different styles of economic thinking.²¹ 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).²² 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.²³ Finally, Daniel Brill, director throughout much of the 1960s, initially entered the DRS as Morris Copeland's main assistant in his project to build the first flow-of-funds accounts in the late 1940s.²⁴ Key to the DRS mission was thus the data intensive approach several directors had inherited from their contact with the Mitchell-NBER research agenda.

²¹ There are two major collections of oral histories. First, there's Robert Hetzel's oral history collection, which comprises interviews carried out between 1994 and 2003. It is available online at <https://fraser.stlouisfed.org/archival/4927>. Second, this year the Board released its oral history project, carried out to commemorate the centenary of the Federal Reserve's establishment. Interviews are available online at: <https://www.federalreserve.gov/aboutthefed/centennial/federal-reserve-oral-history-interviews.htm>

²² In fact, it was Mitchell who recommended him for a position at the Fed (Yohe 1990, p. 470).

²³ 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.

²⁴ 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. On Copeland, see Rutherford (2011, ch. 4).

And yet, it was Brill, who was trained before the war, did not have a PhD, and had a deep connection to American Institutionalism, who became a pivotal figure in the DRS's endorsement of macroeconomic modeling during the 1960s.²⁵ In 1960 he was asked by the recently created Committee on Economic Stability of the Social Science Research Council (SSRC) to participate into their macroeconomic 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, aimed at reaching a deeper 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.²⁶ Brill, then an associate advisor of the DRS, had been selected as the expert in charge of the financial sector of the model, but early on, DRS economist Frank de Leeuw took over Brill's work and wrote the published version of the financial sector for the Committee's model (De Leeuw 1965).²⁷ Brill did not get his hands dirty with writing down equations, but throughout his tenure as DRS director he acted as an advocate of macroeconomics models and transformed the staff and its culture to allow for this type of analysis to be carried out at the Board.²⁸

²⁵ 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, the DRS's previous director, attended graduate school at Yale but also did not graduate.

²⁶ This depiction of the SSRC Committee and their macroeconomic model project relies on Acosta and Pinzón-Fuchs (2018). See Pinzón-Fuchs (2017) for a discussion of Klein's approach to macroeconomic modeling, and Bodkin *et al.* (1991) for a survey of macroeconomic modeling in the US and the centrality of the SSRC-Brookings model.

²⁷ Maisel was the expert in charge of the non-industrial construction sector of the model. See list of experts, SSRC1, and Acosta & Pinzón-Fuchs (2018) for more details.

²⁸ Edward Ettin (2006, pp. 3-4) credits Brill with starting a change towards "a culture where people were seriously interested in economics and were respected for what they were able to do on the economic side." See also Gramley (2007, p. 5), Enzler (2013, p. 35), Kichline (2007, p. 5), Gramlich (2007, p. 2), and Meltzer 2009, 493.

Frank de Leeuw had joined the San Francisco Fed in 1953 as he obtained a Master's in Public Administration from Harvard, and transferred to the DRS three years later. His initial work was on the demand for capital goods, but his work for the SSRC in the early 1960s led him to specialize in monetary economics. In 1964 he took a nine month leave of absence to comply with the residency requirements for a doctorate at Harvard. His dissertation (De Leeuw 1965b) comprised his work for the Committee's model (De Leeuw 1965a) and additional simulation work he had published the year before (De Leeuw 1964). Under the "general direction of Daniel Brill" (1965a, p. 465n1), 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 2019). 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 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.

While Brill was the advocate, De Leeuw was the mastermind 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, p. 22) likewise writes.²⁹ The push was supported by the establishment of an empirical seminar by Martin and DRS associate director Robert Holland in 1964.³⁰ That the Board was equipped with a mainframe computer since at least the late 1950s also helped. A Division of Data Processing was established in 1963, and the Board developed software for multiple regression analysis and additional FORTRAN programs.³¹

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). This represented a significant break with prior DRS culture. 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 graduate of the Brookings Graduate School (Rutherford 2011, ch. 7), Riefler had a strong influence on Martin, Board members, and the staff thanks to his “persuasive intellect.”³² He had established a “Riefler rule” whereby the Federal Reserve “didn’t make or discuss forecasts” (Meltzer 2009, p.45, p. 498). When Riefler retired in 1959 and was succeeded by Young, “there [was] an unleashing of staff brainpower,” *Business Week* journalists remarked: his retirement allowed the introduction of

²⁹ Patrick Hendershott, who worked with De Leeuw at the DRS during 1964-1967 considers he got “a second PhD” by working with him [Telephone conversation with Juan Acosta, August 13, 2019].

³⁰ Holland to Modigliani, 02/04/194, *ibid*.

³¹ Board Minutes and Federal Reserve Bulletin, 1962, October, p. 1291. Computer facilities then seemed to fall behind (Enzler 2013, p. 6,7,17). The production of statistics was given priority, so that modelers sometimes had to buy computer time outside of the board. Ann Walka did all the computer work related to De Leeuw’s work on the financial model up to 1965 (see July 15 and August 23, 1963 issues of the Newsletter of the Committee on Computers in Research <http://www.emelichar.com/ProgrammingNote.html>). Enid Miller, a programmer, and Helen Popkin also worked with De Leeuw in the second half of the 1960s.

³² “The Fed remodels itself,” *Business Week*, May 16, 1964.

“methods taught in graduate schools such as econometric forecasts and economic models” (Meltzer 2009, p. 498).

Institutionalists’ support for macroeconometrics can be explained by the fact that 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. In an unsuccessful 1960 memorandum meant to stir the Board toward funding the SSRC model, they pointed out “that the Board’s flow of funds accounts might well provide the statistical framework for much of the analysis.”³³ In 1953 already, Riefler had remarked of the attempts to use the flow of funds accounts to make projections that Brill “ha[d] been most ingenious and original in making the new [flow-of-funds] material ‘talk.’”³⁴ Likewise, Young and Noyes’s memo endorsed the SSRC macroeconomic model as “explor[ing] 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.”³⁵ The reasons why Noyes and Young used the “projection” rather than hitherto anathema “forecast” wording 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. But as more FOMC members found scientifically produced forecasts an important resource, the combination of large-scale models and judgment, of science and art, proved difficult to implement and communicate.

³³ 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.

³⁴ Riefler to Committee on Selection of the Rockefeller Public Service Award, August 31, 1953, WWSPIA.

³⁵ Board minutes, September 23, 1960.

IV. JUDGMENT, MODELS, AND POLICY DECISIONS: A DIFFICULT CROSS-FERTILIZATION

The development of a macroeconomic model

After the model of the SSRC's Committee on Economic Stability was handed over to the Brookings Institution for further development in 1963, 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 macroeconomic 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.³⁶

³⁶ Conference minutes, April 2-3, 1965, SSRC1. This work was "largely under the immediate direction of Frank de Leeuw," Brill reported to incoming governor Brimmer. March 10, 1966, p. 2, AFBP.

The DRS economists, however, were not alone in believing that another model should be developed. According to University of Pennsylvania macroeconomist Albert Ando, the newly-founded SSRC's Subcommittee on Monetary Research had come to the same conclusion.³⁷ Ando was then involved, together 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 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.³⁸

Settling an academic dispute through empirical work and improving Fed decision-making were different goals, but 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 during a 1965 conference.³⁹ The two projects were officially merged in 1966, and the Board funded the joint model until

³⁷ Ando, "Introduction," undated but probably 1968, 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.

³⁸ In a 1969 talk, DRS economist (and future Governor) 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*." "Complicated and simple approaches to Estimating the role of money on economic activity," 06/05/1969, Edward Gramlich papers.

³⁹ Conference minutes, April 2-3, 1965, box 147, folder 812, SSRC1. "Modigliani proposed the closest cooperation between the two groups," the minutes read. 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, p. 101) therefore does not reflect the process through which the model actually developed.

December of 1970.⁴⁰ A Special Studies Section had been created at the 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 effects of fiscal policy on aggregate demand (Gramlich 1997).⁴¹

The resulting “FRB-MIT-Penn” or “FMP” model was different from the cohort of other macroeconomic models developed in the 1960s in that it “contains many more policy variables that can be used directly to represent the policy actions of the monetary and fiscal authorities of the federal government.”⁴² It exhibited the usual blocks of equations (consumption, investment, financial sector, housing), but with twists that allowed for more numerous and refined transmission mechanisms than the cost-of-capital effects that previous macroeconomic models relied on. The consumption equations allowed for detailed wealth effects, and credit rationing in the mortgage market was also taken into account. The influence of interest rates on state and local public expenditures also received specific attention.⁴³

That several versions of the model coexisted even after the merger (Backhouse and Cherrier 2019) speaks to the difficulty of developing a model suitable both for

⁴⁰ We unfortunately do not have records of the discussions regarding the funding of the project by the Board. We do know that, though very expensive, Brill considered it to be “one of the most exciting adventures in which [the DRS] ha[d] embarked.” Brill to Brimmer, March 10, 1966, p. 6, ADBP. In 1971 a copy of the current version of the model was handed over to Wharton Economic Forecasting Associates Inc. (WEFA) for maintenance and distribution. Ando to Hickman, July 19, 1971, FMP. The Board continued to develop their own version of model throughout the 70s and 80s. See Brayton *et al.* (1997) for a brief discussion of the macroeconomic models developed at the Board.

⁴¹ See Backhouse and Cherrier (2019) for a detailed account of each group’s perspective and on the organization of the (not so) joint work in developing the model.

⁴² Ando, “Introduction,” FMP.

⁴³ De Leuw and Gramlich (1969) offer a succinct description of the model. See Acosta and Rubin (2019) for a discussion of the role of banks in the FMP and other contemporary large-scale macroeconomic models.

academic and policy purposes. Brill had indeed warned Modigliani that he wanted to “preserve the identity of our [the DRS] contribution to the project,” suggesting that it would help them to increase the staff devoted to model-building and to remain autonomous in operating their version. He also needed a distinct identity so that the Board would not be publicly associated with strange model outputs: “Obviously it would have to be made clear that these results 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 to Modigliani.⁴⁴ At the beginning of 1969, Ando then 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 DRS 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.⁴⁵

In spite of diverging constraints and purposes, a shared “macroeconomics” identity nonetheless emerged, blurring the boundaries between academic and policy-circles. DRS economists were painfully aware that FOMC’s skepticism toward the model’s simulation might worsen as the debate between Keynesians and Monetarists spread *within* the Fed. The 1960s saw the emancipation of the Reserve 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-equation models. Together with Leonall Andersen they wrote a single-equation model which

⁴⁴ Brill to Modigliani, July 6, 1966, SSRC2. The note that appeared on the January, 1971 issue of the *Federal Reserve Bulletin* reporting the agreement reached with WEFA also made it clear that the model currently at use at the Board was different from the one being distributed (p. 76).

⁴⁵ Ando to Brill, 01/10/1969, FMP

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 Gramlich participated in a controversy played out in academic journals as well as Board bulletins and Reserve Banks' reviews.

Reciprocally, academics and DRS economists alike labored to overcome 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.⁴⁶

The same argument was soon reprised by Gramlich : "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 to the Committee

⁴⁶ Ando, "Introduction," *ibid.*

on Banking and Credit Policy.⁴⁷ Combining formal/mechanical and judgmental forecasts was also considered so important by Brill and De Leeuw that, in 1966, the latter had asked to be transferred to the National Income Section of the DRS, which was responsible for the judgmental forecasting work. “A closer interchange between the formal model-builders and those using essentially judgmental techniques will produce a much more reasonable and useful model, enabling us to incorporate more systematically the wealth of institutional knowledge and understanding of the numbers our staff possesses,” they argued.⁴⁸

Bringing forecasts to the FOMC: the making of the Green and Blue Books

It was the changes in procedures resulting from the pressures to rationalize the monetary decision-making process that offered the opportunity to bring the mix of mechanical and judgmental forecasts nurtured by the DRS to the FOMC. 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.⁴⁹ Second, “a formal forecasting system” was inaugurated, a development Maisel (1972, P.176) took credit for. He asked the DRS to prepare data

⁴⁷ 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.

⁴⁸ Brill to Brimmer, March 3, 1966, p. 3, AFBP.

⁴⁹ Pierce remembers that they also worked with MIT engineers on the application of control theory to their problem (Pierce 1996, p. 34). Duarte (2009) relates how Poole drew on control theory to introduce a quadratic loss function to guide monetary policy at the turn of the 1970s. Though disagreement on the right instrument to guide monetary policy loomed large since the 1960s, Tom Sargent, Neil Wallace as well as Keynesian Arthur Okun all agreed that relying on such well specified and computable function would provide an objective way of defining “loss” and of guiding monetary policy. Though some of these discussions were carried under the auspices of the Minneapolis and New York Feds as well as the Board, Duarte provides no hints that such approach was either discussed or implemented by Board members or in-house research staff.

outlining recent past and “projected” evolutions of nine series of variables, from monetary instruments, monetary and fiscal variables to real and nominal output, prices unemployment, and balance of payment variables.⁵⁰ Each table was accompanied with a 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 meeting, and the forecasts were more substantially updated every three months or so.

By the end of the year, Brill, now head of the DRS, seems to have asked staff economist 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 2011, pp. 45-46). These would be based on carefully chosen monetary parameters (interest rates, but also reserve measures) with the purpose of “quantifying,” and thus making more specific, the instructions in the directive the FOMC transmitted to the Trading Desk manager at the New York Fed after each meeting. This “Bluebook,” originally implemented for the November 2, 1965 meeting, quickly grew in size. At the end of each discussion, “possible directive language” was presented as a set of 3 indicative policy alternatives (A, B and C).⁵¹ A “Redbook” summarizing economic conditions by district was added in 1970.

⁵⁰ 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 (2011, pp. 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.”

⁵¹ See for instance the Bluebook for the December 15, 1970 meeting : <https://www.federalreserve.gov/monetarypolicy/files/FOMC19701215bluebook19701211.pdf>

These forecasts or “projections” were initially purely judgmental. 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, pp. 31-32). As they became available around 1968, forecasts resulting from the FRB model’s simulation were combined with these judgmental forecasts, in line with DRS economists’ aforementioned approach to forecasting. 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” (1973, p. 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 the introduction of new practices was initially quite successful, though it created tensions between the econometric “technicians” and other breeds of analysts.⁵² It was, however, thwarted by their failure to predict the economic consequences of the 1968 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, pp. 252-254). Virtually all accounts by former Fed officials consistently identify this forecasting

⁵² Pierce profoundly disliked being called a technician, which he felt was done to dismiss any of his expertise besides the purely technical aspects of econometric modeling (Pierce 1995, p. 43).

debacle as a turning point, and it likely contributed to Brill's resignation in 1969.⁵³ 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, p. 271) and added that "there is a disease called statisticalitis that could kill us" (p. 273).

The internal purchase of econometric methods did not improve with Burns' nomination as chairman in 1970. Despite being the first chairman to hold a PhD and a widely renowned academic, "[he] believed in distilling—because of his work at the NBER—huge amounts of empirical information and drawing conclusions from that," former Richmond Fed's president Robert Black recalled (1994, p. 7; see also Holland 1994, p. 23). Burns, furthermore, had a completely opposite style to Martin: While the latter was a "consensus seeker," Burns was an "old autocrat" who "ran those [FOMC] meetings like a graduate seminar" (Black 1994, pp. 6, 8; Parthemos 1994, p. 15).⁵⁴ 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

⁵³ Frank Morris (1994a, pp. 8-10), James Parthemos (1994, pp. 11-13), Pierce (1995, pp. 1-2), Axilrod (2011, p. 45). Brill went on to work in the private sector. Frank de Leeuw also left the Board in 1969 and went to the Urban Institute.

⁵⁴ Burns also found the DRS much too "Keynesian" for his taste (Parthemos 1994, p. 16). See Rutherford (2011, ch. 9) for an account of Burns and the NBER's brand of institutionalism.

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 than short-term econometric forecasts (see also Maisel 1973, pp. 181-2), but he believed econometricians were better able to predict if the economy might slip into recession with time (Pierce 1996, p. 21). Burns did not dismiss the Bluebook, as Martin had been tempted to do. Rather, he 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, p. 22)

Former DRS advisor Peter Keir likewise explains 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, p. 17) and then Burns would go for B, which was more moderate. It was all orchestrated.⁵⁵

V. CONCLUSION

⁵⁵ 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, p. 45).

Existing accounts of the “scientization” of the Fed often rely on quantitative accounts of how newly minted PhD economists took over the FOMC and the staff. Yet, our story of how economic analysis was gradually embedded in the Fed’s decision-making process outlines a long transitional period in which the training and degrees held by protagonists mattered much less than their encounters and the vision of the importance of art, science, judgment, and quantification in the monetary policy decision-making process that they each forge in response to external pressures.

This transformation of Fed operations should not be seen as a *replacement* of old-style data and market intuition by sophisticated large-scale macroeconomic 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 macroeconomics, but their participation into collective endeavors (for Brill) or the external pressures they faced (for Martin and FOMC members). Finally, the use of the Green and Blue books and underlying forecasts was resisted, by non-economists as well as by PhD economists who favored institutionalist styles of analysis.

In the 1960s and early 1970s, then, the purchase of new forms of economic analysis largely hinged on the constantly changing power dynamics, personalities, and visions of FOMC members. Our story is thus not one of the rising influence of modern

economic analysis at the Fed yet. It is merely one of creating some space for it, through the development of a large-scale macroeconomic models and the Green and Blue books. Ironically, it was only at the moment macroeconomics entered a state of crisis in the mid-1970s that it gained clearer traction in monetary policy decisions. The road toward scientization was a long and bumpy one.

REFERENCES

Archival:

AFBP: Andrew F. Brimmer Papers, box 125, folder 2. Baker Library, Harvard University.

EGP: Edward Gramlich Papers, Bentley Historical library, University of Michigan.

FMP: Franco Modigliani Papers, Rubinstein Library, Duke University.

JFKA: John Fitzgerald Kennedy Online Archives.

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

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

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

Published:

“The Fed Remodels Itself.” 1964. *Business Week*, May, 65–76.

Ackley, G. 1974, “Interview by Joe Franz”, 7 March, *Oral History Transcript*, Lyndon

B. Johnson Library Oral Histories.

Acosta, J. Pinzon-Fuchs, E. 2018. "Macroeconometric Modeling and the SSRC's Committee on Economic Stability, 1959-1963," *CHOPE working paper*, https://papers.ssrn.com/sol3/Papers.cfm?abstract_id=3175933

Acosta, J. Rubin, G. 2019. "Bank behavior in large-scale macroeconometric models of the 1960s." *History of Political Economy* 51 (3): 471- 491.

Axilrod, Stephen H. 2011. *Inside the Fed*. MIT Press.

Backhouse, R. Cherrier, B. 2019. "The ordinary business of macroeconometric modeling: working on the MIT-Fed-Penn model (1964-1974)." *History of Political Economy* 51 (3): 425-447.

Ban, Cornel. 2018. "The Professional Politics of the Austerity Debate: Comparing the European Central Bank and the International Monetary Fund." CIYPERC working paper.

Black, Robert. 1994. "Interview by Robert Hetzel, June 6." Available at <https://fraser.stlouisfed.org/archival/4927>

Bodkin, Ronald G., Lawrence Klein, and Kanta Marwah. 1991. *A History of Macroeconometric model-building*. Aldershot, Hants, England; Brookfield, Vt., USA: Edgar Elgar

Bordo, Michael, and Klodiana Istrefi. 2018. "Perceived FOMC: The making of hawks, doves, and swingers." Hoover Institution, Economics Working Paper 18108.

Brayton, Flint, Andrew Levin, Ralph Lyon, and John C. Williams. 1997. "The Evolution of Macro Models at the Federal Reserve Board." *Carnegie-Rochester Conference Series on Public Policy* 47 (December): 43–81.

Bremner, Robert P. 2004. *Chairman of the Fed: William McChesney Martin Jr., and the Creation of the Modern American Financial System*. Yale University Press.

Brunner, Karl. 1961. "The Report of the Commission on Money and Credit." *Journal of Political Economy* 69 (6): 605–20.

Brunner, Karl, and Allan Meltzer. 1964a. *Some General Features of the Federal Reserve's Approach to Policy*. Subcommittee on Domestic Finance, Committee on Banking and Currency of the House of Representatives.

Brunner, Karl, and Allan Meltzer. 1964b. *The Federal Reserve's Attachment to the Free Reserve Concept*. Subcommittee on Domestic Finance, Committee on Banking and Currency of the House of Representatives.

Brunner, Karl, and Allan Meltzer. 1964c. *An Alternative Approach to the Monetary Mechanism*. Subcommittee on Domestic Finance, Committee on Banking and Currency of the House of Representatives.

Burns, Arthur F. 1946. *Measuring Business Cycles*. New York: National Bureau of Economic Research.

Cherrier, Beatrice. 2019. "How to Write a Memo to Convince a President: Walter Heller, Policy-Advising, and the Kennedy Tax Cut." *Æconomia. History, Methodology, Philosophy* 9 (2): 315–35.

Claveau, F. Dion, J. 2018. "Quantifying Central Banks' Scientization: Why and How to Do a Quantified organizational History of Economics." *Journal of Economic Methodology* 25 (4): 339-366.

Congress of the United States of America. 1964. *The Federal Reserve after fifty years: Hearings before the Subcommittee on Domestic Finance of the Committee on Banking and Currency, House of Representatives*. Washington: US Government Printing Office.

Conti-Brown, Peter. 2017. *The power and independence of the Federal Reserve*. Princeton University Press.

Daane, Dewey. 2006. "Interview for the Federal Reserve Oral History Project, June 1." Available at <https://www.federalreserve.gov/aboutthefed/files/j-dewey-daane-interview-20060601.pdf>

DeLeeuw, Frank. 1964. "Financial Factors in Business Cycles : a Simulation Study." *American Economic Review* 54 (3): 309-323.

De Leeuw, Frank. 1965a. "A Model of Financial Behavior." In James Duesenberry, Gary Fromm, Lawrence Klein, and Edwin Kuh, eds., *The Brookings Quarterly Econometric Model of the United States*. Chicago, Ill: Rand-McNally

DeLeeuw, 1965b, "A Model of Financial Behavior," *dissertation*, Harvard University.

DeLeeuw, F. Gramlich, E. 1969. "The Channels of Monetary Policy." *Federal Reserve Bulletin* June: 472–91.

Duarte, Pedro Garcia. 2009. "A Feasible and Objective Concept of Optimal Monetary Policy: The Quadratic Loss Function in the Postwar Period." *History of Political Economy* 41 (1): 1–55.

Enzler, Jared. 2013. "Interview for the Federal Reserve Oral History Project, June 26." Available at <https://www.federalreserve.gov/aboutthefed/files/jared-enzler-interview-20130626.pdf>

Ettin, Edward. 2006. "Interview for the Federal Reserve Oral History Project, March 10." Available at <https://www.federalreserve.gov/aboutthefed/files/edward-c-ettin-interview-20060310.pdf>

Feiertag, Olivier and Michel Margariaz, 2016. *Les banques centrales et l'État-nation*.

Les presses de Sciences Po.

Fox, Justin. 2014. "How economics PhDs took over the Federal Reserve." *Harvard Business Review Online* (February 3), accessed August 12, 2019, <https://hbr.org/2014/02/how-economics-phds-took-over-the-federal-reserve>.

Gramley, Lyle. 2007. "Interview for the Federal Reserve Oral History Project, May 14." Available at <https://www.federalreserve.gov/aboutthefed/files/lyle-e-gramley-interview-20070514.pdf>

Gramlich, E. 1997. Reflections of a policy economist, *American Economist* 41 (1): 22-30.

Gramlich, Edward M. 2004. *The Board's Modeling Work in the 1960s*. Delivered at the Federal Reserve Board Models and Monetary Policy Conference, Washington, D.C. March 26. Accessed August 12, 2019, <https://www.federalreserve.gov/boarddocs/speeches/2004/20040326/default.htm>.

Gramlich, Edward. 2007. "Interview for the Federal Reserve Oral History Project, August 8." Available at <https://www.federalreserve.gov/aboutthefed/files/edward-m-gramlich-interview-20070808.pdf>

Hetzel, R. Leach, 2012. "The Treasury-**Fed** Accord: A New Narrative Account." *FRB Richmond Economic Quarterly* 87 (1): 33-56.

Heller, W. W. 1967. *New Dimensions of Political Economy*. New York: Norton.

Holland, Robert. 1994. "Interview by Robert Hetzel, July 13." Available at <https://fraser.stlouisfed.org/archival/4927>

Hoover, Kevin. 2018. “Karl Brunner’s Philosophy of Science: Macroeconomics through the Lens of Logical Empiricism.” *Center for the History of Political Economy Working Paper* 2018-15.

Keir, Peter. 2001. “Interview by Robert Hetzel, July 17.” Available at <https://fraser.stlouisfed.org/archival/4927>

Kichline, James. 2007. “Interview for the Federal Reserve Oral History Project, September 21.” Available at <https://www.federalreserve.gov/aboutthefed/files/james-l-kichline-interview-20070921.pdf>

Knipe, James. 1962. “A summary of public criticism of the Federal Reserve System, 1959-1961.” Available at https://fraser.stlouisfed.org/files/docs/historical/martin/21_04_19620209.pdf.

Koopmans, Tjalling C. 1947. “Measurement Without Theory.” *The Review of Economics and Statistics* 29 (3): 161–72.

Lebaron, Frédéric, and Aykiz Dogan. 2016. “Do Central Bankers’ Biographies Matter?” *Sociologica*, January.

Maisel, Sherman. 1973. *Managing the Dollar*. Norton.

Maisel, Sherman. 2009. “Interview for the Federal Reserve Oral History Project, July 8.” Available at <https://www.federalreserve.gov/aboutthefed/files/sherman-j-maisel-interview-20090708.pdf>

Mankiw, N.G., 2006. The Macroeconomist as Scientist and Engineer. *Journal of Economic Perspectives* 20, 29–46.

Marcussen, Martin. 2009. “Scientization of Central Banking.” In Kenneth Dyson and Martin Marcussen, eds., *Central Banks in the Age of the Euro: Europeanization, Convergence, and Power*. Oxford University Press, pp. 373–390.

McGregor, Roy, and Warren Young. 2013. "Federal Reserve Bank presidents as public intellectuals," *History of Political Economy* 45 (suppl.): 166-190.

Meltzer, Allan H. 2009. *A History of the Federal Reserve, Volume 2*. University of Chicago Press.

Mirowski, P. 1989. "The measurement without theory controversy : defeating rival research programs by accusing them of naive empiricism" *Economies et Sociétés* 11.

Morgan, M. Rutherford, (eds) 1998. *From Interwar Pluralism to Postwar Neoclassicism*, special Issue of *History of Political Economy* 30 (suppl.): 1-26.

Morris, Frank. 1994a. "Interview by Robert Hetzel, March 10." Available at <https://fraser.stlouisfed.org/archival/4927>

Mudge, Stephanie L., and Antoine Vauchez. 2016. "Fielding Supranationalism: The European Central Bank as a Field Effect." *The Sociological Review Monographs* 64 (2): 146–69. <https://doi.org/10.1002/2059-7932.12006>.

Parthemos, James. 1994. "Interview by Robert Hetzel, June 10." Available at <https://fraser.stlouisfed.org/archival/4927>

Pierce, James. 1995. "Interview by Robert Hetzel, April 27." Available at <https://fraser.stlouisfed.org/archival/4927>

Pierce, James. 1996. "Interview by Robert Hetzel, April 10." Available at <https://fraser.stlouisfed.org/archival/4927>

Rancan, A. 2019. "Empirical Macroeconomics in a Policy Context: The Fed-MIT-Penn Model versus the St. Louis Model, 1965–75." *History of Political Economy* 51 (3): 449-470.

Rancan, A. 2020. "The 'place of the Phillips curve' in macroeconometric models: the

case of the Fed Board's macroeconometric model (1966-1980s)," Available at SSRN: <https://ssrn.com/abstract=3517231>.

Ritter, L.S. 1962. "Official Central Banking Theory in the United States, 1939-61: Four Editions of the Federal Reserve System: Purposes and Functions." *Journal of Political Economy* 70(1): 14-29.

Ritter, Lawrence S., ed. 1980. *Selected Papers of Allan Sproul*. Federal Reserve Bank of New York.

Rutherford, M. 2011. *The Institutionalist Movement in American Economics, 1918-1947: Science and Social Control*. Cambridge and New York: Cambridge University Press.

Stockwell, Eleanor, ed. 1989. *Working at the Board: 1930s-1970s*. Washington: Board of Governors of the Federal Reserve System.

Tobin, J. 1961. "The Future of the Fed." *Challenge* 4(9): 24-28.

Woodford, M., 2009. Convergence in Macroeconomics: Elements of the New Synthesis. *American Economic Journal: Macroeconomics* 1, 267-279.

Yohe, William P. 1990. "The Intellectual Milieu at the Federal Reserve Board in the 1920s." *History of Political Economy* 22 (3): 465-488.

Young, Nancy Beck. 2000. *Wright Patman: Populism, Liberalism, and the American Dream*. Southern Methodist University Press.