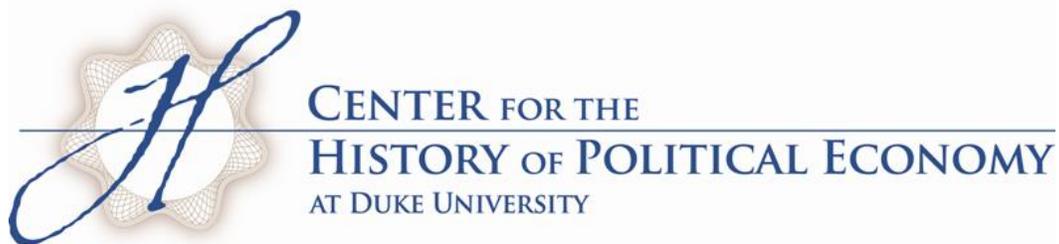


THE TRANSFORMATION OF ECONOMIC ANALYSIS AT THE  
FEDERAL RESERVE DURING THE 1960S

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## **The transformation of economic analysis at the Federal Reserve during the 1960s**

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**Abstract:** 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 economic analysis within the Federal Reserve. 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 a process internal to the discipline whereby institutionalism was displaced by neoclassical theory and econometrics. 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 paradoxically did not always 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.

## I. 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 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). 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.<sup>1</sup> François Claveau and Jeremie Dion (2018) estimate that nearly 50% of money and

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<sup>1</sup> 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.

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, p. 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).<sup>2</sup> 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

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<sup>2</sup> See also Schnidman and MacMillan (2016), who rely on an interview with Stephen Axilrod.

Committee (FOMC). Neither are economists *qua* scientists central in the works of economic historians (Bordo 2008; Feiertag and Margariaz 2016; Hetzel 2008; Monnet 2014). Their objects are the monetary policies implemented by central bankers. Allan Meltzer's history of 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; Snowden 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.<sup>3</sup>

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<sup>3</sup> 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.

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 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.

## **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 (Hetzl 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, congress committees, and the press.<sup>4</sup> 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 Commission on Money and Credit issued a report in 1961, and then launched a series of hearings in 1964.<sup>5</sup> 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.

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<sup>4</sup> Maisel (1973, pp. 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.

<sup>5</sup> Members of the commission included Marriner Eccles, Adolph Berle, David Rockefeller, Theodore Yntema. The research director was Harvard's Bertrand Fox and his deputy MIT's Eli Shapiro. Lester Chandler, Paul Samuleson and Sumner Slichter, among others, had joined the advisory board.

### *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.<sup>6</sup> 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.<sup>7</sup> 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 *Challenge*. He warned

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<sup>6</sup> Knipe to Martin, "A summary of public criticism of the Federal Reserve System, 1959-1961," February 9 1962, FRASER

<sup>7</sup> CEA to President, "Monetary Policy: High Time for Action," April 6, 1961. JFKA. See also the comments reported by Bremner (2004, p. 150)

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, 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 (*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, p. 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, p. 84).

*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, p. 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 three 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, p. 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, p. 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 (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 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, p. 40). There was however, a more fundamental attack behind the demand for transparency. The key issue, elicited by Tobin (1961, p. 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 50<sup>th</sup> 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, p. 32). It was suggested that either the chairman of 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 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, p. ix). For those policy-oriented yet academic economists involved in monetary debates, “rational foundation” meant science-based. Brunner and Meltzer (1964c, p. 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, p. 26; see also Maisel 1973, p. 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, p. 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, p. 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, p. 42-43). It was crucial that the FOMC became knowledgeable of these alternative measurements of the lags.

For Brunner and Meltzer (1964a, pp. 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.”<sup>8</sup> 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, 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” (*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

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<sup>8</sup> Franco Modigliani and Milton Friedman made similar remarks to the Board in 1965, see Rancan (2018).

*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. Institutional vs neoclassical wars also spread to microeconomics, with heated debates on the representation of the pricing behavior of firms (see Mongin 1997).

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.

### **III. 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, p. 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.

*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, p. 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.<sup>9</sup> 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,<sup>10</sup> 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-

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<sup>9</sup> 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.

<sup>10</sup> His dissertation was, nonetheless, listed in the 1949 AEA’s list of doctoral dissertations in political economy.

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, p. 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 was increasingly pointed out.<sup>11</sup> The CEA staff often complained about his lack of economic expertise. For instance, Gardner Ackley, who replaced Heller as CEA

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<sup>11</sup> Bremner (2004, 24) reports that he started a PhD in finance at Columbia in the 1930s, although he did not graduate.

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 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, p. 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.<sup>12</sup> 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

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<sup>12</sup> Bach to Modigliani, dated November 15, 1953, but the year is a typo, it was written in 1963. FMA. See Also Martin to Modigliani, February 4, 1964.

the Board a few weeks later,<sup>13</sup> and he and Meltzer participated in future meetings, which became a regular event, taking place at least once a year from then on.<sup>14</sup>

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 vs traditionalists" (Maisel 1973, pp. 215-216).<sup>15</sup> Maisel relentlessly fought for the inclusion of new indicators and formal forecasts in FOMC deliberations (Bremner 2004, p. 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.<sup>16</sup>

It was not just that the older bankers assembled around Martin did not catch on to this intellectual renewal. They actively rejected their younger colleagues' push for

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<sup>13</sup> 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.»

<sup>14</sup> Modigliani's papers include material from academic consultants meetings up to the early 1980s. G. L. Bach remained the organizer.

<sup>15</sup> 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).

<sup>16</sup> Bremner (2004, p. 230n18). Holland to Modigliani, 01/20/64.

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, 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.

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.

*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.<sup>17</sup> 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.<sup>18</sup> 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.

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<sup>17</sup> 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.

<sup>18</sup> 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.

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 four to eight, 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 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. Finally, a sizeable number of these economists pursued a PhD *while working at the DRS*. They were thus presumably more in touch with recent advances in monetary economics than the previous generation of directors. Table 8 shows that some researchers completed it ten to fifteen 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.”<sup>19</sup> 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

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<sup>19</sup> Hetzel’s oral history collection is available online at <https://fraser.stlouisfed.org/archival/4927>.

former professor at Amherst College who had also worked with Wesley Mitchell (Yohe 1990; Rutherford 2011, ch. 6).<sup>20</sup> 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.<sup>21</sup> 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.<sup>22</sup> 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).<sup>23</sup> In 1960 he was asked by the recently created Committee on Economic

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<sup>20</sup> In fact, it was Mitchell who recommended him for a position at the Fed (Yohe 1990).

<sup>21</sup> 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.

<sup>22</sup> 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).

<sup>23</sup> 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.

Stability of the Social Science Research Council to participate in 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, 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.<sup>24</sup> 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).<sup>25</sup>

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

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<sup>24</sup> This depiction of the SSRC Committee and their macroeconomic 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.

<sup>25</sup> Sherman 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.

model (De Leeuw 1965a) and additional simulation work he had published that year (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 2018). 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.

De Leeuw was a driving force behind the development of macroeconomic 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 describes. The push was supported by the establishment of an empirical seminar by Martin and DRS associate director Robert Holland in 1964.<sup>26</sup> It was also enabled by an adequate technical infrastructure. The Board had a mainframe computer since at least the late 1950s and a Division of Data

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<sup>26</sup> Holland to Modigliani, 02/04/194, *ibid.*

Processing, which grew out of the DRS, was established in 1963.<sup>27</sup> 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.<sup>28</sup> 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 members, and the staff thanks to his “persuasive intellect,” a 1964 article from *Business Week* recounts.<sup>29</sup> 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*

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<sup>27</sup> Board Minutes and Federal Reserve Bulletin, 1962, October, p. 1291.

<sup>28</sup> 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>.

<sup>29</sup> “The Fed remodels itself,” *Business Week*, May 16, 1964.

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, p. 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.<sup>30</sup> 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.’”<sup>31</sup> 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.<sup>32</sup> 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

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<sup>30</sup> 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.

<sup>31</sup> Riefler to Committee on Selection of the Rockefeller Public Service Award, August 31, 1953, WWSPIA.

<sup>32</sup> Board minutes, September 23, 1960.

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-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.

#### **IV. SCIENCE IN SUPPORT OF THE ART: A DIFFICULT CROSS-FERTILIZATION**

##### *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.<sup>33</sup>

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.<sup>34</sup> 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 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.<sup>35</sup>

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<sup>33</sup> Conference minutes, April 2-3, 1965, SSRC1.

<sup>34</sup> 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.

<sup>35</sup>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

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.<sup>36</sup> An official merger of the two projects was enacted in 1966, and the Board funded the joint model until December of 1970.<sup>37</sup> 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,

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and simple approaches to Estimating the role of money on economic activity,” 06/05/1969, Edward Gramlich papers.

<sup>36</sup> 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, p. 101) therefore did not reflect the process through which the model developed.

<sup>37</sup> The model was handed over to Wharton Economic Forecasting Associates Inc. (WEFA) for maintenance and distribution. Ando to Hickman, July 19, 1971, FMP.

who joined the Board in 1965 after completing a PhD at Yale under Tobin on the aggregate demand impact of fiscal policy (Gramlich 1997).<sup>38</sup>

The resulting “Fed-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.”<sup>39</sup> 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 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.<sup>40</sup> The influence of interest rates on state and local public expenditures also received specific attention (De Leuw 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

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<sup>38</sup> 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.

<sup>39</sup> Ando, « Introduction », *ibid*

<sup>40</sup> See Acosta and Rubin (2018) for a detailed analysis of the role of banks in the FMP model.

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.<sup>41</sup>

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.<sup>42</sup> Constraints and purposes may have been different but other interventions speak 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”:

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<sup>41</sup> 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 made it clear that the model currently at use at the Board was different from the one being distributed (p. 76).

<sup>42</sup> Ando to Brill, 01/10/1969, FMP

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.<sup>43</sup>

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.<sup>44</sup>

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 fallout 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

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<sup>43</sup> Ando, “Introduction,” *ibid.*

<sup>44</sup> 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.

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 journals as well as Fed bulletins.<sup>45</sup> 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.<sup>46</sup>

*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.<sup>47</sup> 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

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<sup>45</sup> See Rancan (2018) for an exhaustive account of the battles around the FMP and St Louis model, in particular the Andersen-Jordan equation.

<sup>46</sup> 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)

<sup>47</sup> Pierce remembers that they also worked with MIT engineers on the application of control theory to their problem (Pierce 1996a, p. 34).

need for such a system. Projections of the GNP, credit, or the money supply were totally lacking at the time" (1972, p. 176). He asked the DRS to prepare data 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.<sup>48</sup> 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 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 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, pp. 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

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<sup>48</sup> 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, 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."

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).<sup>49</sup> 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, pp. 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” (1973,

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<sup>49</sup> See for instance the bluebook for the December 15, 1970 meeting : <https://www.federalreserve.gov/monetarypolicy/files/FOMC19701215bluebook19701211.pdf>

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 blending former practices with econometrics initially appeared quite successful, though it created tensions between the econometric “technicians” and other breeds of analyst.<sup>50</sup> 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, pp. 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.<sup>51</sup> 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

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<sup>50</sup> 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, p. 43).

<sup>51</sup> Morris (1994a, pp. 8-10), Parthemos (1994, pp. 11-13), Pierce (1995, pp. 1-2), Axilrod (2011, p. 45).

20014, p. 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, p. 122) even remembered that he 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 than 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, p. 6). Black and Richmond Fed vice-president James Parthemos both considered that the chairman “ran those [FOMC] meetings like a graduate seminar” (Black 1994, p. 8; Parthemos 1994, p. 15). Burns also found the DRS much too “Keynesian” for his taste (Parthemos 1994, p. 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 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 1996a, p. 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, p. 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, p. 17) and then Burns would go for B, which was more moderate. It was all orchestrated.<sup>52</sup>

## V. CONCLUSION

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<sup>52</sup> 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).

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 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 books and underlying forecasts was resisted, by non-economists as well as by PhD economists who favored institutionalist styles of analysis. By the mid-1970s, styles of research like macroeconomics 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.

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## Annexes

The information presented in the following tables was obtained from the Federal Reserve's History website ([www.federalreservehistory.org](http://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 \*)**

<b>Apr-51</b>	<b>Apr-61</b>	<b>May-65</b>	<b>Feb-70</b>	<b>Mar-75</b>
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.**

<b>Name</b>	<b>Appointed</b>	<b>Years after PhD</b>	<b>Appointed by</b>	<b>Terminal degree</b>	<b>Institution</b>
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 \*)**

<b>Reserve Bank</b>	<b>Apr-51</b>	<b>Apr-61</b>	<b>May-65</b>	<b>Feb-70</b>	<b>Mar-75</b>
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.**

<b>Name</b>	<b>Reserve Bank</b>	<b>Appointed</b>	<b>Years after PhD</b>	<b>Terminal degree</b>	<b>Institution</b>
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**

<b>Name</b>	<b>Period</b>	<b>Terminal degree</b>	<b>Institution</b>
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**

Name	Entered 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		