

What does Jerome Powell know that William McChesney Martin
didn't—and what role did academic research play in that?

by

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It is indeed a pleasure for an old LSE alumnus like me (M.Sc., 1968) to return to help mark the 50th anniversary of the Money Macro and Finance Research Group. I actually have on my bookshelf a copy of Harry Johnson's 1972 book, *Macroeconomics and Monetary Theory* (Johnson, 1972), which was based on his lectures at LSE during the 1969-1970 academic year. I presumably heard some of these lectures as a student two years earlier. But fifty years is a long time and memory fails.

The world changes in 50 years. In 1969, the New York Jets won the Superbowl of American football and the New York Mets won the World Series of baseball—two huge upsets in the same year! The former has never reoccurred; the latter has been repeated only once. The year 1969 is perhaps most famous for astronaut Neil Armstrong's walk on the moon—a remarkable feat that was accomplished five more times, but not since 1972.

However, some aspects of the world don't change much in a half-century. The Boeing 747 took its maiden flight in 1969 and is still flying. The British government demonetized the half-penny in 1969 and has not brought it back. Prince Charles was officially crowned Prince of Wales by his mother in 1969 and still wears that same crown.

These observations frame my theme. Thinking about both the *theory* and the *practice* of monetary policy, I want to ask what has endured since 1969 and what has proven to be fads or fancies. As my title puts it, what is different about the way Jerome Powell thinks about monetary policy today versus the way William McChesney Martin thought about it in 1969? And what role did we academics play in those changes? My concentration on the Federal Reserve may seem a bit parochial here in London, and I suppose it is. But that's the central bank I know best. Besides, I think the basic ideas, though not the specific names and dates, are

generic. Charles Goodhart could probably write a fine version of this lecture with Leslie O'Brien and Mark Carney replacing Martin and Powell.

I. The Goals of Monetary Policy

A logical place to start the comparison is with the *goals* of monetary policy. First, a quick look back before 1969.

The Federal Reserve in 1913, not to mention the Bank of England in 1694, was not designed for monetary policy—a concept that was unknown at the time. The chapeau of the original Federal Reserve Act (FRA) delineates its main purposes as “to furnish an elastic currency, to afford means of rediscounting commercial paper, to establish a more effective supervision of banking in the United States, and for other purposes.” In contemporary parlance, you can make a cogent argument that the Fed was designed to preserve *financial stability*. You cannot make a case that it was designed to conduct *monetary policy*. So, naturally, neither low inflation nor high employment was listed as a goal of the new central bank. Rather, as the Fed's official history puts it succinctly, “The paramount goal of the Fed's founders was to eliminate banking panics.” (Wheelock, 2013).

The Fed stumbled into monetary policy in the 1920s—and not very gracefully, as its miserable performance during the Great Contraction attests. Milton Friedman and Anna Schwartz (1963, p. 253) characterized the portion of the Fed's *Tenth Annual Report* (1923) that dealt with monetary policy as offering “little beyond glittering generalities instructing the men exercising the judgment to do the right thing at the right time.”

The first glimmer of a mention of monetary policy in the Federal Reserve Act came in the June 1933 amendments that created the Federal Open Market Committee (FOMC). It read in

part: “The time, character, and volume of all purchases and sales of paper described in ... this Act ... shall be governed with a view to accommodating commerce and business and with regard to their bearing upon the general credit situation of the country.”¹ *Accommodating commerce and business* could be read--perhaps charitably--as “leaning against the wind.” In any case, that idea became central to the Fed’s mission during Martin’s chairmanship.

However, Martin believed that low inflation, not high employment, should be the Fed’s main goal (Romer & Romer, 2004). And unlike Friedman and other monetarists, Martin saw at least as much inflationary danger in fiscal deficits as in excessive money growth.

But what inflation rate should policymakers shoot for? In 1969, Friedman (1969) had just caused a stir in academia by arguing for an inflation rate *negative* enough to make the *nominal* interest rate zero—so perhaps a 2-3% rate of *deflation*. It seems unlikely that Martin paid any attention to this argument for deflation. He was not an economist, had little interest in economic theory, and remembered the Great Depression all too well. Nor did the majority of the economics profession sign on to Friedman’s optimal rate of *deflation*.

Rather, inflation hawks both inside and outside central banks settled on the more obvious inflation target: zero. After all, zero would make money a stable measuring rod, like the yardstick. In a letter to the Senate Banking Committee in 1989, Alan Greenspan expressed support for “approximately zero” inflation, but said he could wait “several years” for it (Kilborn, 1989). He was still paying lip service to zero as late as 1995, when he—at least putatively—supported a bill in Congress, the Mack-Saxton bill, that would have replaced the Fed’s dual mandate with a single goal: price stability. The bill itself, which never came close to passage,

¹ From Section 12(A), which was rewritten in 1935.

left the numerical definition of “price stability” to the Fed’s discretion. But as I recall (I was Vice Chairman of the Fed at the time), Senator Mack and Congressman Saxton did not have 2% inflation in mind. They wanted zero.

The FOMC devoted a good deal of time at its September 1995 meeting to discussing how, if at all, it should respond to congressional efforts to change the dual mandate into an inflation-only mandate. The committee was sharply divided, and Chairman Greenspan made no effort to bring it to an agreement. Then, in July 1996, with the threat of congressional action a thing of the past, the FOMC held a full-fledged debate over whether it should have a numerical inflation target and, if so, what that target should be. Well before that debate, the Reserve Bank of New Zealand had established its own 0-2% target range, and Stanley Fischer (1994), in his famous Bank of England Tercentenary paper, had suggested a 2% goal—which many central banks had adopted. So 2% was a natural focal point.

Toward the end of that debate, then-Governor Janet Yellen asked Greenspan for his preferred definition of “price stability”. This interchange followed:

CHAIRMAN GREENSPAN. Price stability is that state in which expected changes in the general price level do not effectively alter business or household decisions.

MS. YELLEN. Could you please put a number on that? [Laughter]

CHAIRMAN GREENSPAN. I would say the number is zero, if inflation is properly measured.

MS. YELLEN. Improperly measured, I believe that heading toward 2 percent inflation would be a good idea, and that we should do so in a slow fashion, looking at what happens along the way.

Yellen's view clearly commanded a consensus at that meeting, a consensus that did not, however, include Chairman Greenspan.² The next day (it was a two-day meeting), he admonished the FOMC to keep the consensus for 2 percent inflation a closely-guarded secret: "I will tell you that if the 2 percent inflation figure gets out of this room, it is going to create more problems for us than I think any of you might anticipate." Remarkable, from a modern standpoint.

Prior to that Fed debate in 1996, most inflation-targeting central banks had adopted a target near (or centered on) 2% *measured* inflation, and almost everyone seemed content with the choice until the Great Recession struck and the effective lower bound (ELB) on nominal interest rates (often incorrectly called the *zero* lower bound) became a binding constraint on monetary policy. Had the inflation target been set higher back in the 1990s, central banks would have had more latitude to cut rates, and the slump would have been less severe.³

Jay Powell and other contemporary central bankers now understand something that Bill Martin either did not understand or would have viewed as fanciful: Inflation can get too low as well as too high. The main reasons are familiar: Upward biases in official price indexes, wage-price stickiness that can prevent relative prices from adjusting downward when necessary, and the dangers of encountering the ELB in a weak economy.

Economic analysis has contributed a great deal to our understanding of the first of these: biases in the price index. In particular, if he ever thought about it, Martin would probably have

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³ See, for example, Williams (2009) and Blanchard, Dell'Ariccia, and Mauro (2010).

thought first, and maybe last, about substitution bias—an obvious implication of downward-sloping demand curves. However, we now understand that substitution bias is small compared to such matters as inadequate quality adjustment and lags in the introduction of new products.⁴

The second reason to eschew zero inflation, wage-price stickiness, dates all the way back to Keynes (1936) and was emphasized, for example, by Akerlof, Dickens, and Perry (1996). The supply shocks of the 1970s and early 1980s demonstrated just how important price stickiness can be. When OPEC forced up the *relative* price of oil, inflation was a necessary part of the adjustment because non-oil prices simply would not fall. Inflation, it turned out, is not *always* a monetary phenomenon.

The last reason to avoid zero inflation may be the main reason behind the uncomfortable fact that central banks seem to be better at pushing inflation *down* when it is too high than at pushing inflation *up* when it is too low. I believe the ELB problem is so severe, and so likely to recur, that if the central banks of the world could turn back the clock to the 1990s, they would settle on a higher target inflation rate—perhaps 3% or 4%. But it is not possible to rewrite history on a blank slate. A decision to abandon 2% *now* would be an admission of failure and a blow to central bank credibility. Some academics may see that as a good idea. Central bankers do not.

The Federal Reserve, of course, has a second, coequal goal: “maximum employment,” which the Fed interprets as pushing the unemployment rate down to its natural rate—a parameter that must be estimated and can change over time. In a literal sense, the dual

⁴ Boskin, Dulberger, Gordon, Griliches, and Jorgenson (1996) is the classic reference.

mandate dates only to 1977 amendments to the Federal Reserve Act. But the employment goal was lurking in the background long before that because the Employment Act of 1946 committed the entire US government “to use all practicable means.... [to create] conditions under which there will be afforded useful employment opportunities... for those able, willing, and seeking to work.” The Employment Act was not aimed at the Fed in particular; no one in 1946 thought monetary policy was that important in determining employment. But the Fed was part of the US government, and Martin knew that.

Still, as late as 1994, I got branded an “outlier” on the FOMC when I had the temerity, in a short speech at Jackson Hole, to call attention to the employment part of the Fed’s dual mandate.⁵ Imagine that. The Fed’s Vice Chairman was pledging allegiance to the Federal Reserve Act! Yet my talk initiated a media frenzy that Greenspan refused to quell, despite being queried about it by several journalists. You can’t imagine that happening now. At least since Ben Bernanke’s chairmanship, fealty to the dual mandate has been mother’s milk at the Fed—and it’s not just lip service. This is another respect in which Jay Powell’s world differs dramatically from Bill Martin’s.

One final point on the Fed’s legal mandate: Ironically given the Fed’s origins, the monetary policy mandate now found in the Federal Reserve Act does not list financial stability as a goal. That was a slightly strange omission in 1977, made even stranger by the fact that the Dodd-Frank Act of 2010, despite making many changes to the FRA, did not add financial stability to the mandate. But, of course, preserving financial stability has been part of the Fed’s mission since 1913. Powell and Martin stand on equal footing here.

⁵ Blinder (1994). For a detailed journalistic account of the episode, see Starobin (1994).

So when it comes to monetary policy goals, do things look more the same or more different today than in 1969? I'd say a bit of each, but more similar than different—although the enunciation of the Fed's goals is far clearer today. And academic research certainly played a significant role in these changes.

II. The Monetary Policy Instrument

The ultimate goal variables of monetary policy are not controlled directly by the central bank. Rather, the authorities must use one or more control *instruments* to move the goal variables toward their targets—subject, of course, to control errors, stochastic or otherwise.

In Chairman Martin's day, the Fed's main instrument was "net free reserves" (what we would now call excess reserves) or "non-borrowed reserves" (total reserves less borrowings from the Fed), either of which the trading desk in New York could control tightly through open-market operations that provided or drained reserves. Such open-market operations were, in turn, used to push around money market interest rates. So it seems a matter of little import whether one says the Fed's main policy instrument back then was the short-term interest rate or some measure of bank reserves. All this would sound quite natural to Chairman Powell today—except the notion that excess reserves are negligible. There is far more continuity than change here.

But in the 1960s and 1970s, monetarists argued that the focus on interest rates was fraught with peril. For example, holding a *nominal* interest rate constant as inflation rises or falls will lead to falling or rising *real* interest rates—just when you don't want that to happen. Better to use M1 or M2 as the policy instrument, they argued, and let interest rates go where they will. Notably, the monetarist assault on the *status quo* came from the academy, not from

inside the Fed.⁶ It received major assists from Milton Friedman's sharp pen and legendary debating skills, but also from rising inflation—for which monetarists successfully blamed Keynesian policies. However, the Federal Reserve never really converted to monetarism. (See below.)

In one of those great historical ironies, the same inflation that helped monetarism rise in intellectual and central banking circles also sowed the seeds of monetarism's destruction. The perverse interactions of high inflation with regulatory limits on *nominal* interest rates led both to waves of financial innovation (often outside banks) and to financial deregulation. The latter, in turn, opened the door to even further innovations *inside* banks.

The lesser irony here is that it was an important scholarly paper by William Poole (1970), himself a monetarist, that explained why money-supply-targeting would be inferior to interest-rate targeting if, say, financial innovations rendered the demand for money unstable. Poole made his argument with a few simple equations. But the intuition is straightforward. If money *demand* grows at a roughly constant, or at least predictable, rate, then it makes sense for the central bank to make the money *supply* do so as well. But if growth in the demand for money (by some definition) becomes erratic, it makes little sense to try to stabilize the growth of the supply of money (by that definition). That compelling intuition shows why Poole's basic conclusion held up to many subsequent extensions and complications.⁷

Financial innovations in many countries during the 1970s made money demand *very* erratic, which in turn made the choice between money growth targeting and interest rate

⁶ Monetarism may have been more influential inside some other central banks, such as the Deutsche Bundesbank.

⁷ It seems largely to have escaped notice that Sargent and Wallace's (1975) famous paper on monetary policy ineffectiveness began as an effort to extend Poole's analysis.

targeting a no-brainer. As Bank of Canada Governor Gerald Bouey famously told his parliament in 1983, “We didn’t abandon M1, M1 abandoned us.” (Canada, 1983, p. 12).

Despite these evident problems, Paul Volcker, then the newly-installed Chairman of the Fed, claimed to adopt monetarism in the fall of 1979. The backdrop was dramatic. US inflation was historically high; many Americans saw it as “out of control.” President Jimmy Carter, though mindful of the political peril to his presidency, decided that the country needed a determined inflation fighter to run the Fed. Thus Volcker, who didn’t need much encouragement in that regard, was put at the Fed’s helm with a mandate to fight inflation.

Shortly after assuming office, Volcker heard Arthur Burns, at the IMF meetings in Belgrade, advance the idea that social forces prevent central banks from bringing inflation down (Burns, 1979). He found the thought appalling. Far from giving up, Volcker and his colleagues applied a heavy dose of monetarist medicine—at least putatively. They would slow down the growth rates of money, regardless of the consequences for interest rates—just what Friedman and other monetarists had been calling for. History records that the medicine worked in terms of lowering inflation. However, *both* money growth *and* interest rates were extremely erratic during America’s monetarist experiment (1979-1982), just as Poole’s analysis had predicted, but not as monetarists had claimed.

So was this a case of academic work affecting monetary policy profoundly? I think not. In fact, scholarly research had demonstrated that money demand was quite unstable (see, for example, Goldfeld (1976)), which, by straight application of Poole (1970), showed that monetarist policy could create disaster. Besides, Volcker was no fan of academic research. Rather, as he recounts in his recent memoir, he saw monetarism as a convenient sales pitch in

two dimensions. First, the simplicity of Friedman’s dictum that “inflation is always and everywhere a monetary phenomenon” helped Volcker explain the new approach to the American people. Second, “that approach enforced upon the Federal Reserve an internal discipline that had been lacking... We were ‘lashed to the mast’ in pursuit of price stability” (Volcker, 2018, p. 118). Modern economics talks of “commitment devices.” This was an unusual one.

By the summer of 1982, the lash was getting too tight. Perhaps even more important, inflation was coming down. Volcker took this opportunity to abandon monetarism without abandoning the Fed’s hard-earned anti-inflation credibility. The FOMC returned to using short-term interest rates as its monetary instrument, much as Martin had before. Later, during the 1990s, the Fed backed away the monetary aggregates even further. During my time as Vice Chairman (1994-1996), the exercise of setting target ranges for the Ms, though required by law, resembled kabuki more than serious monetary policy.

The virtually exclusive focus on short-term interest rates—controlled, albeit with some error, by manipulating the federal funds rate—lasted through 2008. Discount rate lending was minimal during that quarter century, and the Fed never changed required reserve ratios after 1990. The funds rate was *the* policy instrument (Bernanke & Blinder, 1992). Then, in December 2008, the FOMC encountered the effective lower bound,⁸ which left it with a stark choice: either give up on easing monetary policy further or search for unconventional instruments.

⁸ The FOMC decided it would not literally go for zero. It posted, instead, a 0-25 basis point range for the federal funds rate, which it wound up maintaining until December 2015.

Chairman Ben Bernanke, an astute scholar of the Great Depression, did not hesitate over the choice.

Was pre-existing academic research—which Bernanke knew extremely well—of help here?

Well, maybe a little. In a short paper summarizing a 1999 Federal Reserve conference on low inflation (Blinder, 2000), I had laid out seven options for a central bank stuck at what was then thought of as the zero lower bound.⁹ They were, in rough order of relevance to the U.S.

situation in 2008:

1. Create massive amounts of bank reserves via open market operations
2. Move open market operations “out the yield curve” into longer maturities
3. Conduct open market operations in private assets
4. Pay a negative interest rate on bank reserves
5. Post a higher inflation target
6. Depreciate the exchange rate
7. Conduct a Friedman-style “money rain”

Today, we call the first three of these options aspects of “quantitative easing,” a term taken from the Japanese experience. The Fed has refused to consider the fourth option, although many other central banks have used it by now. The fifth option, which was mentioned earlier, encounters a potentially fatal credibility problem when inflation is extremely low and the central bank is having trouble raising it. The sixth option is a terrible idea, and not really operational, when the whole world is struggling with the ELB at the same time. (Back in 1999, it was just Japan). Competitive devaluations would be classic “beggar thy neighbor” policies. Finally, the last option, as I noted at the conference, is really fiscal policy—not something the Fed can do on its own.

⁹ I didn’t invent these options; I just listed and discussed them.

I offer this 1999 menu because Jay Powell has read the paper, absorbed it, and referred to it several times (Powell, 2019b). Bill Martin, of course, could not have. But when I went back to review Blinder (2000) in preparing this lecture, I was surprised to learn that my list did not include forward guidance. Never mind that particular term of art, which was not in use then. My list did not even *mention* the possibility of “talking down” the long end of the yield curve. I suppose that was because the Fed was so tight-lipped at the time that I couldn’t imagine Alan Greenspan giving the sort of forward guidance that has become commonplace since. *Mea culpa*.

But 2019 is not 1999. Central banks today view quantitative easing, forward guidance, and negative interest rates as their three principal unconventional policy instruments. Since the Fed eschews negative rates, it has only two.

Regarding QE, Ben Bernanke famously quipped that it works in practice but not in theory.¹⁰ What he meant is that the expectations theory of the term structure of interest rates insists that the shape of the yield curve depends *only* on expectations of future short rates. Perfect arbitrage ensures that relative supplies of short versus long bonds cannot matter. That purist view had dominated intellectual circles for decades prior to the world financial crisis; I’m guessing it was part of the canon in LSE’s Money Macro and Finance Group. By contrast, the so-called *preferred habitat* theory (Modigliani & Sutch, 1966, 1967), which allows a role for relative demands and supplies, was a subject for history-of-thought classes, if that. Yet the efficacy of QE relies on the preferred habitat theory: The central bank is supposed to push long rates down by buying long bonds, thereby shrinking their supply to the private sector. So any evidence that

¹⁰ Stated verbally at a Brookings conference in 2014. See Ahamed and Bernanke (2014).

QE worked is evidence against perfect yield curve arbitrage.¹¹ We have here a case in which state-of-the-art academic thinking in 2009, had it been believed and followed by the Fed and other central banks, would have hamstrung monetary policy.

The opposite, however, is closer to the truth when it comes to forward guidance. I will return to central bank communication in more detail later. But for now, let me just say that, long before the crisis, a number of academics had emphasized the importance of “central bank talk” in managing expectations of future interest rates.¹² Eggertsson and Woodford (2003) had even proposed a specific form of central bank commitment on future interest rates as a way to overcome the ZLB. So I think it’s fair to say that, in the case of forward guidance, academic thinking had a profound and salutary influence on real-world monetary policy.

A survey of central bank governors’ attitudes conducted in 2016 by Ehrmann, de Haan, Jansen, and me (2017) found that a plurality (41%) of the 34 respondents thought that QE in government securities should remain in their toolkits after the crisis. Only 21% thought it should be discontinued. However, 38% deemed it too early to judge. Given that QE in Treasuries amounts to no more than doing conventional open-market operations on a grand scale, this level of support seems underwhelming. Furthermore, support for QE dwindled to an even split when we switched the question to asking about QE in assets other than government securities (29% to 29%, with 42% saying it is too early to judge).

¹¹ This is a bit of an exaggeration. There are other channels via which QE might work. See, for example, Krishnamurthy and Vissing-Jorgenson (2011).

¹² Blinder (1998), lectures which were first written and presented in 1995, may have been the first.

Forward guidance, which of course comes in many varieties, was considerably more popular with the central bankers. Fully 72% thought it should be continued after the crisis, and none thought it should be discontinued. (The remaining 28% said it was too early to judge.) But opinions differed on which *type* of forward guidance is best, with no clear consensus. The top choice, favored by about 39% of respondents, was giving *purely qualitative* guidance, such as the Fed has done lately. Some 27% preferred *data-based* (or state-contingent) guidance (e.g., “we will not raise interest rates until the unemployment rate drops below 6.5%”), and 14% preferred *calendar-based* (or time-contingent) guidance (e.g., “we will not raise rates before the third quarter of 2014”). Another 15% preferred something else.¹³

Based on these responses, we should expect to see a greater reliance on forward guidance than on QE in the future, although the Fed may be more sanguine about QE than the average central bank. But returning to my main theme, when it comes to conducting monetary policy at the effective lower bound, there have been huge changes since 2008 (though few between 1969 and 2008). Jay Powell knows a great deal more about this subject than Bill Martin could have, and academic research is one important reason.¹⁴

III. Monetary Transmission: “The Model”

Once a central bank has been given its goals (y) and decided on its main instrument or instruments (x), it next needs *quantitative estimates* of the impact of the instruments on the goals (dy/dx). A purely theoretical model that delivers only the *signs* of such derivatives is of little use. The bank needs *quantitative* estimates of the impacts of its policy instruments as they

¹³ About 12% favored no guidance at all, and 21% said it was too early to tell. The total exceeds 100% because the central bankers were allowed to check more than one box.

¹⁴ A great deal of research on these questions has also been done inside central banks.

filter into the economy via the so-called *monetary transmission mechanism*. And these impacts are not just numbers; they follow dynamic paths. Estimating such paths requires, of course, a quantitative model or models. Nowadays, there are new-fangled tools like DSGE models and Bayesian VARs. But in 1969, apart from the simple quantity theory approach, it was pretty much large-scale Keynesian macroeconometric models.

The tradition actually began much earlier, with Jan Tinbergen's (1939) pioneering work—which estimated systems of equations by single equation methods. Trygve Haavelmo (1944) pointed out a basic technical flaw: Such macro models—he might have called them **dynamic, stochastic, general equilibrium models**, for they were all of these—implied simultaneity and therefore required simultaneous equations methods of estimation. Thus began the “Cowles Foundation” tradition, whose first entry was the Klein-Goldberger model (Klein & Goldberger, 1955), consisting of 20 simultaneous equations estimated on US data for the years 1929-1952. From this small beginning, ever larger and more complex models grew, culminating perhaps in the giant Brookings model, in which Lawrence Klein (among many others) had a hand. Even before macro models reached this enormous size, simultaneous equations methods became too unwieldy to use.

It is possible to trace a nearly straight intellectual line from those early academic models to the Fed-MIT-Penn (FMP) model, which was developed in the late 1960s and early 1970s by teams of top-flight economists at MIT (led by Franco Modigliani), the University of Pennsylvania (led by Albert Ando), and the Federal Reserve Board, and was used and maintained by Fed staff

for many years thereafter. But there is no reason to believe that Martin ever paid any attention to the FMP model. I wonder if he even knew of its existence.¹⁵

The Lucas critique (1976) stopped this constructive program of model building in its tracks. Working on large macro models became a disreputable activity in American academia, and economists in leading universities basically shunned the enterprise.¹⁶ One result was that, as Christopher Sims (2002, p. 23) observed 17 years ago, “academic research in these areas has paid very little attention to the central problems of modeling for macroeconomic policy in real time.” He could repeat those same words today, I believe. Nowadays, you find the descendants of the early macroeconometric model builders mainly inside central banks and in private consulting firms like Moody’s Analytics, Macroeconomic Advisers, and IHS Markit. On a few occasions, I have made use of one or more of these big models to address policy questions (Blinder & Yellen, 2001; Blinder & Zandi, 2010; Blinder & Zandi, 2015). On each such occasion, I knew I was crossing a line that virtually guaranteed that the work would be ignored in academia—as it was. But, tellingly, these papers attracted considerable attention *outside* the academic world.

A major redirection of macro modeling efforts at the Federal Reserve occurred in 1996, when the board’s staff replaced the FMP model by the FRBUS model, which it still uses. One main purpose of the overhaul was to deal better with rational, or rather “model consistent” expectations. While the FRBUS model is by no means immune to the Lucas critique, it takes rational expectations seriously and gives expectations major roles. That said, if Klein,

¹⁵ The FMP model became the MPS model when the Fed dropped its support and the Social Science Research Council picked it up.

¹⁶ One major exception, perhaps the only one, was Ray Fair of Yale.

Modigliani, and other pioneers returned to earth today, they would probably feel right at home with the FRBUS model. Jay Powell, who is not trained in technical economics, probably does not. But unlike Bill Martin in 1969, he knows about the model and sees results from it regularly—as, for example, in the Fed’s Tealbook.

While Fed staff and a few practical macroeconomists were working on the FMP model, academic attention turned toward dealing with the Lucas critique, which was interpreted as undermining the validity of conventional macroeconometric modeling—even though its empirical importance was never demonstrated.¹⁷ I am old enough to remember when shouting “Lucas critique” in a crowded seminar room was enough to stop the show.

Into this void stepped a few determined and talented econometricians like Thomas Sargent and Lars Hansen in the late 1970s and early 1980s. Their basic idea was that “deep structural parameters” characterizing tastes and technology are impervious to policy changes and, therefore, econometricians should try to estimate them rather than hybrid objects like the marginal propensity to consume or the slope of the Phillips curve (Hansen & Sargent, 1980). This was pretty demanding work, however, and I doubt that anyone ever thought it could be done on a 100-equation macro model.

In one sense, this line of research just fizzled out. Maybe there were not enough people as smart and hard-working as Hansen and Sargent. In another sense, however, you can view “deep parameter excavation,” as it was called, as a precursor to today’s DSGE models. The two have in common that they lean heavily on *a priori* theory based on representative maximizing agents.

¹⁷ For example, what evidence ever established that Lucas critique problems were typically larger than, say, the simultaneity biases highlighted by Haavelmo (1944)?

That's where, I think, Larry Klein would have abandoned ship. More important, it made this line of research—impressive as it was—of dubious value to central bankers, who must stick closer to reality.

That last word brings up Christopher Sims' famous paper "Macroeconomics and Reality" (1980). Sims attacked the large-scale modeling tradition from an entirely different angle:¹⁸ They relied on too many "incredible" (his word) identifying assumptions. In stark contrast to Lucas (1976), however, Sims (1980) offered a constructive alternative: vector autoregressions (VARs). These unstructured, atheoretical models are not immune to the Lucas critique, of course. Indeed, since they are, in a sense, "super-reduced" forms, they are super-vulnerable to the critique.¹⁹ But VARs had one clear advantage over, say, the Hansen-Sargent approach (other than being easy to implement): Rather than squeeze the data into Procrustean theoretical beds, VARs allow the data to speak for themselves. VARs might not be able to answer deep theoretical questions, but they do reveal the dynamic auto- and cross-correlations that are present in the data.

There is at least one other important difference between the Lucas and Sims approaches. The Lucas critique leads investigators to try to uncover or invent policy "rules" for the central bank, and it conceptualizes a "change in monetary policy" as a change in the rule. The Sims approach, by contrast, focuses attention on the effects of monetary policy "shocks," that is, on movements in policy variables that differ from what the VAR (that is, past patterns) would predict.

¹⁸ In conversation, Sims assured me that the VAR methodology was *not* a reaction to the Lucas critique. He had been thinking along these lines before Lucas (1976).

¹⁹ A few years later, "structural" VARs started to offer a kind of compromise.

The notion that they were following a “rule” probably seemed strange to most central bankers in the 1970s and 1980s—and probably still does today. I was Vice Chairman of the Fed in 1994-1996, and if we had a policy rule then, no one ever bothered to tell me about it.²⁰ But the notion that the central bank might want to deviate from “normal” in response to some unusual data or event seemed natural to most central bankers—and still does. Indeed, that’s probably the main reason why monetary policy decisions are made at periodic meetings of human beings. It also may be why the VAR methodology proved so useful to central bank staffs.

Rational expectations had profound effects in the theoretical world as well. The celebrated Sargent and Wallace (1975) paper that made the policy ineffectiveness result famous gave birth to a new class of so-called New Classical models in which anticipated changes in money had no real effects. To move real variables, the central bank had to surprise agents—which is not easy to do under rational expectations. These New Classical models were *new* in their techniques and methodology, but old in that they concluded (or was it assumed?) that the *classical* economists had it right all along: Keynesian economics was a misleading diversion based on poor (or no) theory. Remember, it was only in the early 1970s that Friedman and Phelps’ *a priori* theoretical arguments had demolished the previous empirical Phillips curve tradition (Phelps, 1968). New Classical economics tried to do the same with Keynesian economics as a whole.

Theory was one thing. What about evidence? Robert Barro’s (1977) much-cited (at the time) paper purported to show that changes in money growth affected real GDP in the US *only*

²⁰ We did, however, see the results of many versions of the Taylor rule as part of the huge packet of inputs to each FOMC decision. Commonly, while we were debating over 25 basis point changes, the range between the highest and lowest Taylor-rule instruction was 100-200 basis points.

if they were unanticipated—just as in the Sargent-Wallace (1975) model. I remember being surprised by his findings and wondering if they would hold up. They didn't (Mishkin, 1982; Gordon, 1982). But that was far from the end of New Classical models, which staggered on for decades. Eventually, however, many of the former New Classical economists accepted wage-price stickiness as a reality, incorporated it into their models (Christiano, Eichenbaum, & Evans, 2005; Golosov & Lucas, 2007), and the New Classical tradition sort of morphed into the New Keynesian school. New Keynesians accept the rationality of expectations and seek microfoundations for behavior. But because of price stickiness, they reach characteristically Keynesian conclusions about monetary policy.²¹ New Keynesian models, in turn, morphed into today's DSGE ("dynamic, stochastic, general equilibrium") models, which are actually used at some central banks such as the Fed and the ECB.

So changes in the way economists in the academy looked at monetary policy between 1969 and 2019 were frequent and large. Larry Klein, Franco Modigliani, and others would probably find the structure of today's DSGE models strange—even before you told them that the empirical parameters are "calibrated," not estimated. For their part, most modern macroeconomists look back at the big macroeconometric models as intellectual dinosaurs that were wiped out by a meteor strike named Lucas and are best left extinct.

But neither Martin nor Powell was/is a devotee of models. (Were any Fed chairs? Bernanke and Yellen came closest.) As Allan Meltzer (2009, p. 6) put it early in his two-volume *A History of the Federal Reserve*, "There is often not a close connection between academic research findings and recommendations and Federal Reserve actions." An understatement. While the

²¹ Mankiw and Romer (1991) is a two-volume compendium of articles.

story in academia is one of constant change, the story in central banks is more one of continuity. Numerous academic fashions have come and gone during the 50-year lifespan of the Money Macro and Finance Group.²² But Martin believed in 1969 and Powell believes today that the FOMC's main job is to manipulate the short-term nominal interest rate so as to keep inflation low and employment high—always mindful of the short-run tradeoff between the two.

IV. Central bank communication

When it comes to communicating with the markets and the public, however, the story at central banks is dominated by change, not continuity. Prior to my 1995 Marshall lectures at Cambridge, which were subsequently enlarged and revised into my 1996 Robbins lectures here (Blinder (1998)), academics had paid hardly any attention to central bank communication. The Federal Reserve and many other central banks, for their part, still felt far too comfortable with Montague Norman's (in)famous dictum: "Never explain, never excuse."²³ Paul Volcker liked to blow smoke—both literally and figuratively—in his congressional testimonies. In his early years as Fed Chairman, Alan Greenspan (1988) boasted that he had learned to "mumble with great incoherence," and famously told an audience that "if you think what I said was clear and unmistakable, I can assure you you've probably misunderstood me." (Blinder, Goodhart, Hildebrand, Lipton, & Wyplosz, 2001, p. 66). But since those days of either silence or deliberate obfuscation, there has been a veritable revolution in central bank talk (Blinder, 2004).

The slow-motion revolution at the Fed actually began under Greenspan. In February 1994, he suggested issuing a statement, under his own name (not the FOMC's), announcing the first

²² I have skipped over the Real Business Cycle school because its message to monetary policymakers was, basically: You are irrelevant.

²³ The main (only?) exceptions were the inflation-targeting central banks. Indeed, greater transparency was one of the arguments for inflation targeting.

interest rate hike in years. After a spirited discussion, the committee adopted his suggestion— with many members (including Greenspan) insisting unrealistically that this first-ever statement should not set a precedent. It did, of course.

Later that year, at its August meeting, the Fed offered its first bit of forward guidance— although that term would not appear until years later. In announcing that it was raising both the discount rate and the federal funds rate by 50 basis points, rather than the expected 25, the committee added that “these actions are expected to be sufficient, at least for a time, to meet the objective of sustained, noninflationary growth.” You may be surprised to see an interest rate *hike* advertised as a way to sustain growth; but that was FedSpeak at the time. The new phrase was, “at least for a time,” and it was a departure from norms. The words were intended to moderate market reactions to the surprisingly large policy move. The markets interpreted “for a time” to mean three months, though no such concrete interpretation existed inside the FOMC.²⁴

The far bigger landmark came in 1999, when the FOMC implemented something that the “Blinder Committee” had recommended in 1995,²⁵ but the FOMC had resoundingly voted down: It began releasing short explanatory statements at the conclusion of *every* meeting, even when it decided *not* to change interest rates.²⁶ It also began to reveal the “bias” or “tilt” in its thinking about the near-term future of interest rates. Previously, in a slightly comical portion of each FOMC meeting, the committee would debate, sometimes vociferously, whether it was

²⁴ Full disclosure: I had something to do with this statement. See Woodward (2000, p. 128).

²⁵ The Fed virtually always has a committee on disclosure and communications, headed by its Vice Chairman.

²⁶ The decision to do so had actually been made at the December 1998 FOMC meeting. It was Greenspan’s suggestion.

“biased toward easing,” “biased toward tightening,” or neither—but then keep the decision secret, so no one in markets knew!

Why such big changes in 1999? There is no crisp answer. Times had changed since 1995, and Greenspan apparently had changed with them. There was considerable talk about increasing transparency in the late 1990s, both inside and outside the Fed. The brand-new ECB had recently opened for business with more transparency than the Fed ever dreamed of, including press conferences (which the Fed would not start until 2011). It appears that the intellectual case for increased disclosure prevailed. Imagine that!

Nothing major changed on the communications front from 1999 until after Ben Bernanke took over in 2006. Although Bernanke was a true believer in transparency, the changes didn’t happen right away. Alan Greenspan was still viewed as almost God-like in 2006, and the rookie chairman from academia was more interested in projecting continuity than in demonstrating how different he was. The mild forward guidance found in Greenspan’s last statement read, “The Committee judges that some further policy firming may be needed to keep the risks to the attainment of both sustainable economic growth and price stability roughly in balance.” The first Bernanke statement used *exactly* those same words. Only gradually did the FOMC’s phrasing change—and its statements get longer.

As time went by, the Fed and the entire central banking world found itself battling a frightening financial crisis—which virtually *required* a quantum leap in communication. There was so much more to explain! Yet the federal funds rate remained the exclusive focus of every FOMC statement until the one that followed an emergency telephonic meeting in March 2008 (the Bear Stearns month), when the Fed announced the leading edge of what would become an

alphabet soup of lending facilities. The Term Securities Lending Facility obviously required a great deal more explanation than a mere cut in the funds rate. Things then went comparatively quiet on the communications front as rates were reduced again and again.

Only when the funds rate hit its effective lower bound (which the Fed decided was 0-25 basis points) in December 2008, did communications have to go up another notch. It was after that FOMC meeting that the Fed (a) stated that it would “employ all available tools to promote the resumption of sustainable economic growth and to preserve price stability,” (b) announced what would come to be called QE1, and (c) amped up the forward guidance to: “the Committee anticipates that weak economic conditions are likely to warrant exceptionally low levels of the federal funds rate for some time.” The phrase *for some time* got peoples’ attention. It seemed that no one in the markets remembered that the FOMC had used a strikingly similar phrase (*for a time*) 14 years earlier.

Lengthy (by Fed standards) statements continued to be the norm as lending facilities came and went, and as one episode of QE gave way to another through 2012. By 2013, markets were beginning to wonder when the Fed would start exiting from its hyper-expansionary policy stance, or at least slow down asset purchases. So were some members of the FOMC, including, as we now know from the published transcripts, then-Governor Jerome Powell.

In his May 2013 congressional testimony, Chairman Bernanke dropped a hint that the Fed might start reducing the pace of asset purchases “in the next few meetings.” Like Horton the elephant, he meant what he said. But markets, as is their wont, interpreted that deliberately-vague phrase as “at the next meeting,” which was in June. When that did not happen, markets threw what came to be called the “taper tantrum.” Bond prices fell and Bernanke was blamed

for poor communication. In December 2013, the FOMC finally announced that it would begin tapering the next month.

As all this was going on, the Fed was also experimenting with various forms of forward guidance. The FOMC started with *calendar-based* forward guidance in August 2011, when it stated that anticipated weak economic conditions “are likely to warrant exceptionally low levels for the federal funds rate at least through mid-2013,” that is, for nearly two years. In January 2012, the “pledge” to keep the funds rate in the 0-25 bp range was extended to “at least through late 2014,” and then in September 2012 to “at least through mid-2015.”

At the last meeting of 2012, the FOMC discarded calendar-based guidance and switched to *data-based* forward guidance, adopting a formulation similar to the one Chicago Fed President Charles Evans had been advocating for years. It was a bit complicated. The Committee stated that it anticipated sticking with the near-zero funds rate “at least as long as the unemployment rate remains above 6-1/2 percent, inflation between one and two years ahead is projected to be no more than a half percentage point above the Committee’s 2 percent longer-run goal, and longer-term inflation expectations continue to be well anchored.” I count three variables, two inequalities, and a vague range there; it was a complex message. But the Fed stuck with this three-part test through the remainder of Bernanke’s term, which ended after the January 2014 FOMC meeting.

A problem was brewing, however. Contrary to the Fed’s clear declaration that $U < 6.5\%$ was a *necessary but not sufficient* condition for an interest rate hike, not to mention that it was just one part of a three-part test, markets viewed $U = 6.5\%$ as a trigger when Bernanke handed over the reins to Janet Yellen. Unfortunately for her, it wasn’t. Even though the unemployment rate

in January 2014 was already down to 6.6%, the Fed was of no mind to raise rates. So what to say? Yellen's first FOMC statement deftly split the Solomonic baby by stating that "it likely will be appropriate to maintain the current target range for the federal funds rate for a considerable time after the asset purchase program ends"—a totally unknown date—and further adding that "the change in the Committee's guidance does not indicate any change in the Committee's policy intentions as set forth in its recent statements." Really? Yellen managed to have it both ways as the markets digested the new guidance smoothly. But the Fed had learned once again that markets will latch on to a number, any number, if you give them one.

That initial Yellen statement turned out to mark the end of data-based guidance. Instead, she introduced in March 2014 what became known as the "lower for longer" sentence: "The Committee currently anticipates that, even after employment and inflation are near mandate-consistent levels, economic conditions may, for some time, warrant keeping the target federal funds rate below levels the Committee views as normal in the longer run." To knowledgeable academics, this was a clear adaptation of the Eggertsson-Woodford (2003) idea. To market participants, it was a signal to stand down. No rate hikes would be coming any time soon.

Since then, the FOMC's guidance has been purely qualitative. The inaugural Yellen statement pledged to keep rates constant "for a considerable time" after asset purchases ended. How long was that? As it turned out, asset purchases ended in October 2014, and the near-zero interest rate policy lingered on until December 2015. But Janet Yellen didn't know that in March 2014.

In sum, changes in Federal Reserve communications practices since Martin's day have been huge. A few, like the "lower for longer" statement, seem rooted in academic research. Most,

however, looked like seat-of-the-pants experiments. That's not a criticism, however; there was no database on central bank communications to learn from. The Fed's attitudes toward transparency have also changed radically since 1969, and academic writings seem to have been influential here. Were he alive today, Bill Martin would be astonished to find the Fed praising transparency as a central part of its mission.²⁷ Paul Volcker, blissfully, still *is* alive today, and he *is* astonished.²⁸

V. The Rules-versus-Discretion Debate

Among the subjects that have been debated for more than 50 years—though, in this case, more by academics than by central bankers—is whether monetary policy decisions should follow a mechanical *rule* or be left to human *discretion*.

During William McChesney Martin's long tenure as Chairman of the Fed (1951-1970), Milton Friedman developed and promoted what came to be called the "k-percent rule": That the central bank should ignore the state of the economy and make the money supply grow at a constant percentage rate (k), from year to year. The number k was presumably the sum of the desired inflation rate (Friedman preferred zero or negative) and the growth rate of potential GDP (which was in the 3% range for the US then), though it could readily be modified for any trend growth or shrinkage in velocity.

Why tie the Fed's hands in this way, eschewing any attempts at monetary stabilization? Apart from the conservative's generic desire to keep the government's hands off wherever

²⁷ In a recent speech (June 25, 2019), Chairman Powell (2019a) observed that, "Along with [central bank] independence comes the obligation to explain clearly what we are doing and why we are doing it, so that the public and their elected representatives in Congress can hold us accountable."

²⁸ I know this from personal conversations.

possible, Friedman made four main (and related) arguments for rules. First, that the job is simply too hard: The economy is a complicated beast, and the models we have are not very good. Second, that macroeconomic forecasts, which are necessary due to the long lags in monetary policy, are inaccurate—implying that forecast-based policies may often go astray. Third, that a market economy has strong self-correcting tendencies, so recessions will fade away on their own—often before policymakers have acted. And fourth, that the people who make monetary policy are subject neither to the profit motive nor to the Darwinian mechanism that weeds out incompetent capitalists, and so are prone to making poor decisions. All of these arguments were known to Martin. He probably found the last of them offensive.

Without going over any of the details of history, evidence, and argumentation, it is notable that *none* of these arguments play much of a role in modern incarnations of the rules-versus-discretion debate. Most modern academic work assumes a particular model—*known* by everyone—which it uses to generate “rational” expectations; assumes zero-mean shocks, so forecasts are at last unbiased; accepts the existence of persistent output gaps; and assumes that policymakers are smart enough to solve complicated optimization problems—sort of like business managers do.

Finn Kydland and Edward Prescott (1977) offered a very different argument for rules in the acclaimed paper that later won them the Nobel Prize: time inconsistency. In their view, the short-run tradeoff between inflation and unemployment for *unanticipated* changes in monetary policy creates a strong temptation for central bankers to try to surprise the private sector by promising low inflation but reneging in order to grab temporary output/employment gains. The problem, of course, is that the central bank can’t keep repeating this trick if agents are rational.

So the outcome is higher inflation without higher employment. Kydland and Prescott's recommended solution to this particular time inconsistency problem was to tie the central bank's hands (to the mast?) by a rule.

Though it didn't hurt them in academia, Kydland and Prescott's timing was exceptionally poor from a real world perspective. As mentioned earlier, the late 1970s and early 1980s witnessed sharp and painful disinflations in the US, the UK, and elsewhere. Neither Paul Volcker nor Margaret Thatcher (the Bank of England was not independent then) succumbed to the temptation posed by time inconsistency; they probably never even heard of it. It turned out that Kydland, Prescott, and other academics were prescribing how to fight the last war just as the next war was getting underway. Furthermore, these victories over inflation did not come from following rules or from any other "commitment devices" discussed in academia. They came from tough-minded *discretionary* applications of tight money—as I pointed out decades ago (Blinder, 1998, p. 41).

All this is generic, applicable to rules in general. But Friedman's specific rule had another serious drawback, which came up earlier in a different context. The basic "satisficing" rationale for constant growth of the money *supply* disappears if money *demand* grows erratically. So advocacy of a k-percent rule has pretty much disappeared. The rules-versus-discretion debate lingers on, however, in the form of the Taylor rule (1993) for manipulating the short-term interest rate.

The Taylor rule can be, and I think should be, thought of as an allegory for activist stabilization policy. It instructs the central bank to cut the interest rate below its neutral value whenever either inflation or output is too low. What would make the Taylor "rule" a true *rule*

would be mechanical application of the equation with specific numerical parameter values. Taylor's original paper eschews this use of the "rule" that would soon bear his name. He begins his paper (1993, p. 195) by declaring that his purpose is "to preserve the concept of... a policy rule in a policy environment where it is practically impossible to follow mechanically any particular algebraic formula." He later (p. 198) states that "a policy rule need not be a mechanical formula... [it] can be implemented and operated more informally by policymakers... who also recognize that operating the rule requires judgment and cannot be done by computer." Thus the Taylor rule was originally intended as a *guideline*, not a rule *à la* Kydland and Prescott. All this makes good sense.

In more recent years, however, Taylor's view has hardened, and he has become less flexible in his interpretation of what applying his eponymous rule means—even suggesting that some version should be backed up by legislation (Taylor, 2015). I do not view this as progress. Neither does Ben Bernanke (2015).

As I stated above, Bill Martin knew all about Friedman's rule—and didn't much like it. Similarly, Jay Powell knows about Taylor's rule, which is sensible as a guideline but not as a literal rule. To mention just two examples, it called for an impossibly negative federal funds rates during the financial crisis, and the Fed staff has been having fits trying to nail down the neutral real funds rate (Holston, Laubach, & Williams, 2017), which Taylor (1993) had pegged at 2 percent. It is also pretty clear that no leader of the Federal Reserve between Martin and Powell ever wanted to be bound by a rigid rule. So I think this is a case in which academic fashion wandered in the direction of mechanical rules several times over 50 years, but the Fed never did.

VI. Last Word

If William McChesney Martin returned to earth today, I think he could have a fruitful and meaningful discussion of monetary policy with Jerome Powell. Other than unconventional monetary policy, including forward guidance, the ground would seem relatively familiar. On the other hand, if Harry Johnson returned to earth today, and listened to modern lectures on *Macroeconomics and Monetary Policy* in graduate courses at the LSE and elsewhere, I think he would find them baffling, perhaps even a bit silly. Draw your own conclusions.

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