

Editors' Note

Brookings Papers on Economic Activity (BPEA) marked its fiftieth anniversary in 2020. Papers by three longtime contributors highlighted *BPEA*'s seminal research over the years in areas at the heart of macroeconomic policymaking: labor markets, productivity and growth, and monetary policy. Robert E. Hall and Robert J. Gordon participated in the first *BPEA* conference in April 1970, and Alan S. Blinder was a participant in the Fall 1972 conference. All three had fresh PhDs from the Massachusetts Institute of Technology when their service on the panel began. Recordings of their retrospective presentations can be found on the Brookings website at <https://www.brookings.edu/events/bpea-spring-2021-conference/>.

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BPEA and Monetary Policy over Fifty Years

ABSTRACT Ever since its first issue in 1970, *BPEA* has played a leading role in the analysis of monetary policy. This paper surveys *BPEA*'s many contributions to three specific areas: (1) the Phillips curve, which provides the empirical bridge between real economic activity and inflation; (2) the analysis and demise of monetarism, the doctrine that emphasized the money supply over interest rates; and (3) evaluations of and recommendations for actual monetary policy in the United States, which began in the first *BPEA* issue and continues to this day. *BPEA* has played a dominant (though not monopoly) role in each of these areas.

In thinking about the historic role the Brookings Panel has played as an intermediary and incubator of ideas between the academic world and the world of actual monetary policy, it is critical to remember both the intellectual and policy settings when *BPEA* began in 1970.

On the intellectual front, academic macroeconomics was far less theoretical and far more grounded in reality than it has been in recent decades. Giant econometric models, built rather loosely on a Keynesian theory that was itself loose, roamed the earth. In fact, one such dinosaur inhabited the Brookings Institution. Theoretical looseness was tolerated in those days.

While the Keynesian paradigm dominated the policy world, the monetarist-Keynesian wars were raging—both in academia and in some central banking circles. A lively debate on the subject between Milton Friedman and Walter Heller (1969) had taken place at New York University in November 1968, the same year that Karl Brunner (1968) coined the term “monetarism.” The then famous paper by Andersen and Jordan (1968), which purported to show empirically that money growth mattered for GDP

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but fiscal variables did not, had sparked controversy and consternation both in the academy and outside it. Although the intellectual world didn't know it yet, it was awaiting William Poole's (1970b) seminal paper on money supply targeting versus interest rate targeting, which was sitting in the publication queue at the *Quarterly Journal of Economics*.

Perhaps most important, the subsequent view that macro stabilization policy *is* monetary policy, period, had not yet taken hold. Fiscal policy was thought of as at least a coequal partner, and perhaps even as the senior partner. In the policy world, both monetary policy and fiscal policy had turned contractionary to fight inflation in the late 1960s—the former joining the fight eagerly, the latter reluctantly. I believe the income tax surcharge of 1968—studied by Arthur Okun (1971) in one of the earliest Brookings papers—was the first and last time contractionary fiscal policy was deliberately used to slow the US economy.¹ The 1969–1970 recession which followed was in progress when the first Brookings Panel convened. Then, as since, the conveners of the panel did not much like recessions.

Okun, who had chaired President Johnson's Council of Economic Advisers until January 1969, and George Perry, recently arrived from the University of Minnesota, made a fantastic team. Together, they recruited an all-star cast for the inaugural Brookings Panel. Its members included some who were older but skewed decidedly young—featuring Poole (b. 1937), William Branson (b. 1938), Robert J. Gordon (b. 1940), Barry Bosworth (b. 1942), and Robert Hall (b. 1943). Okun and Perry had an eye for talent (and in case you're wondering, I'm younger than all those guys). That first Brookings Panel also included, as senior advisers, such luminaries as Lawrence Klein, Paul Samuelson, and Robert Solow—not to mention a business consultant named Alan Greenspan. Taken in toto, this list evokes John F. Kennedy's quip about a 1962 gathering of Nobel Prize winners at the White House being the greatest collection of brainpower to dine there since Thomas Jefferson dined alone.

Perry, of course, is still part of the Brookings Panel, and we tip our hats to him today.

Early meetings of the panel basically covered the Keynesian waterfront as mapped out in the macro textbooks of the day. There were papers on consumption, investment, the government budget, money demand, and net exports—and, of course, on the Phillips curve. Almost all of that was relevant to monetary policy, but I will confine myself here to three

1. There were subsequent fiscal contractions, but they were motivated by bringing down the budget deficit, not by slowing down the economy.

prominent topics: the Phillips curve, money growth and monetarism, and ideas for and evaluations of monetary policy.

I. The Phillips Curve

Monetary policy is in large measure about the control of inflation, including the linkages between the real side of the economy (e.g., output and employment) and the nominal side (e.g., money and inflation). So it was altogether fitting and proper that the first paper at the first meeting of the Brookings Panel was on the Phillips curve, which links the two. It was written by Gordon (1970), who was just thirty at the time, and turned out to be the first in a long series of papers by Gordon on the Phillips curve in *BPEA*. Indeed, the names Gordon, Brookings, and Phillips will be linked forever in the history of macroeconomic thought.

Once again, it is important to remember the intellectual setting in 1970. A. W. Phillips's (1958) original paper had used wage inflation as the left-hand variable and basically dismissed inflation, not to mention expected inflation, as a right-hand variable. This omission was not an oversight. Phillips (1958, 283) argued that "cost of living adjustments will have little or no effect on the rate of change of money wage rates." Really? Two years later, Phillips's colleague Richard Lipsey (1960) remedied that deficiency by estimating an inflation coefficient of 0.37 in a wage Phillips curve of the form:

$$w_t = \alpha \pi_t + f(U_t) + \varepsilon_t,$$

where w_t is the rate of change of nominal wages, $f(U_t)$ is a nonlinear function of the unemployment rate, π_t is the inflation rate, and ε_t is a stochastic error term. When Lipsey (1960) estimated that same equation with more modern data, rather than Phillips's 1861–1913 sample, his estimate of α rose to 0.76 (with standard error 0.08). Much higher, but still significantly below 1.

The view in 1970 was that, while Friedman's (1968) and Phelps's (1967, 1968) theoretical arguments for why α should be 1 were persuasive, the data showed $\alpha < 1$.² For example, that first *BPEA* paper by Gordon (1970) estimated α to be just 0.45.³ It was as Groucho Marx might have put

2. Thomas Sargent's (1971) brilliant little paper, which showed why $\alpha = 1$ was beside the point under rational expectations, was not yet appreciated.

3. For this equation, Gordon (1970, 36–37) used an auxiliary equation for nominal bond rates to estimate expected inflation as a function of past inflation rates.

it if he had a PhD in economics: “Who ya gonna believe, Milton or your lyin’ eyes?”

Soon, however, empirical eyesight improved, largely through Gordon’s efforts in *BPEA*. By the second 1972 meeting, he already had an estimated Phillips curve with a nonlinear α coefficient that rose as expected inflation rose, reaching 1 at an expected inflation rate around 7 percent (Gordon 1972). Thus, by 1972 or 1973, the empirical/theoretical conflict over the verticality of the long-run Phillips curve was all but over.⁴ It was vertical both in theory and in practice.

But the Gordon-*BPEA*-Phillips curve saga was far from over. The first big postwar supply shocks hit in 1972–1973, driving inflation far above what Phillips curves without supply shocks predicted.⁵ As CPI inflation in the United States rose from 3.4 percent in 1972 (December to December) to 8.9 percent in 1973 and 12.1 percent in 1974—during a recession, no less!—monetarists crowed that Keynesian economics, with its misguided Phillips curve, was inherently inflationary. A few years later, Lucas and Sargent (1978, 49) chimed in that the “predictions” of Keynesian economics “were wildly incorrect, and that the doctrine on which they were based is fundamentally flawed” so “the task which faces contemporary students of the business cycle is that of sorting through the wreckage.” Wow! And that was just on the first page.

The Brookings Panel was not persuaded, however; it kept the Keynesian embers glowing. The main inflationary villain at the meetings was not profigate Keynesian government spending, but rather supply shocks. Months before the Organization of the Petroleum Exporting Countries (OPEC) struck, Bosworth and Farmer (1973) called attention to crop failures, disappearing anchovies, and the resulting food price explosion as proximate sources of inflation. A year later, Popkin (1974, 259) concluded that “the effect of commodity inflation was substantial in 1973.” More fundamentally, in that same issue, Pierce and Enzler (1974) of the Federal Reserve Board staff modified the Keynesian MIT-Penn-SSRC (MPS) model to analyze the macroeconomic impacts of what they called “external inflationary shocks.” Their simulations showed stagflation, of course: output fell and inflation rose.⁶

4. The debate over whether the short-run Phillips curve was vertical was still several years away.

5. For a full discussion of those early supply shocks, see Blinder (1982) or Blinder and Rudd (2013).

6. Well, not quite. Their main simulations held *nominal* money supply growth constant, meaning that *real* money growth fell, which eventually extinguished the inflation.

Notice, please, that all this analysis came very quickly—far faster than the scholarly journals could react. Speedy publication has always been an important advantage of *BPEA*; the *Journal of Political Economy* never specialized in current events.

By the first issue of 1975, Gordon (1975a) was back with a clear conceptual analysis of supply shocks that was quite similar to what Phelps (1978) would publish three years later. And two issues after that, Gordon (1975b) presented his first Phillips curve that fully incorporated supply shocks. I remember well that Nordhaus (1975, 663), in discussing that paper, referred to it as “Chateau Gordon 1975.” It was a good vintage, though not Gordon’s last.

Out of this early work—and including also contributions by Nordhaus (1972), Perry (1970), Schultze (1971), and others—came what I have long called the Brookings Rule of Thumb—that each point-year of unemployment above the natural rate reduced inflation by half a percentage point. It’s a rule that worked well in the United States for decades. In the mid-1990s, as vice chairman of the Federal Reserve, I routinely referred to the fine performance of the Phillips curve as “the clean little secret of macro econometrics.”⁷ As a matter of fact, I still use the Brookings Rule of Thumb to show my Economics 101 students that contemporary estimates of the Phillips curve give an almost perfect explanation of the Volcker disinflation. You don’t need any magical credibility effects or M2 growth rates.

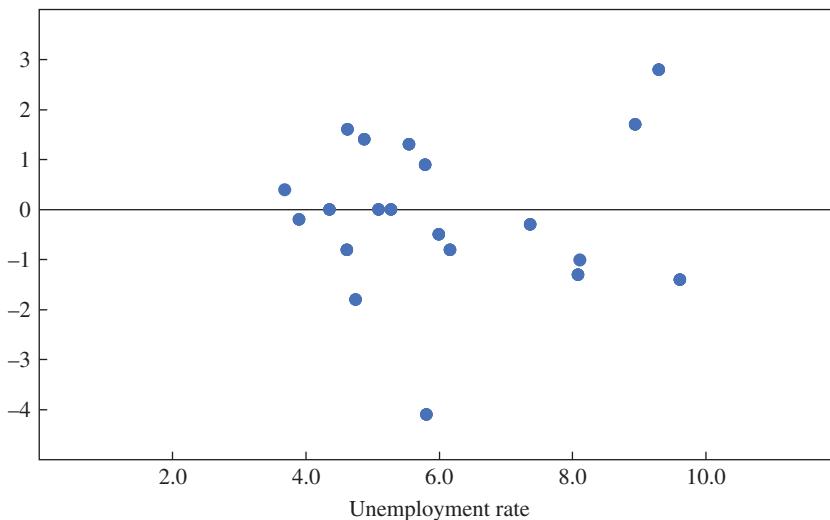
Phillips curve research went relatively quiet in *BPEA* after the early 1980s, with just two papers that concentrated on Phillips curves in the late 1980s, one by Blanchard (1987) and one by Ball, Mankiw, and Romer (1988).⁸ A fascinating paper by Akerlof, Dickens, and Perry (1996)—which the trio followed up with in Akerlof, Dickens, and Perry (2000)—shook the intellectual tree a bit by adding what I’d call “non-Gordonesque” aspects, such as extreme downward wage rigidity and money illusion.

Starting with Chateau Gordon 1998, *BPEA* papers began grappling with the empirical failure of the Phillips curve. The first question was: Why didn’t the low unemployment rates of the late 1990s raise inflation more? Gordon (1998) partly patched things up by incorporating several new supply shocks and by adapting the idea of a time-varying non-accelerating inflation rate of unemployment (NAIRU) from Staiger, Stock, and Watson

7. See, for example, my “notorious” (to some) Jackson Hole speech (Blinder 1994, 340).

8. For this count, and in what follows, I interpret the phrase “concentrating on Phillips curves” fairly strictly. It excludes, for example, many related papers on labor market developments, which I leave to Hall’s paper in this issue.

Figure 1. Change in CPI Inflation versus Unemployment Rate, 2001–2020

 Change in CPI inflation


Source: US Bureau of Labor Statistics.

(1997).⁹ Katz and Krueger (1999) subsequently estimated the effects on NAIRU of several labor market developments—such as demographic change and mass incarceration.

The second question arose after the Great Recession: Why didn't such a deep recession reduce inflation more? Krueger, Cramer, and Cho (2014), echoing Gordon (2013), argued that part of the explanation was that the long-term unemployed exert much less downward pressure on wage inflation than the short-term unemployed. Ball and Mazumder (2011) suggested that the slope of the Phillips curve varied over time. But by the time you've allowed both the intercept (the NAIRU) and the slope to change over time, you haven't got much of a Phillips curve left. And we didn't.

Notice that both of these questions suggest a flatter Phillips curve—as does the scatter plot in figure 1. Suffice it to say that the Brookings Rule of Thumb no longer works, and the stable Phillips curve is no longer a “clean little secret.” Its failure is well known.

9. On new supply shocks, see Blinder and Yellen (2001).

II. Money Growth and Monetarism

Monetarism, which became popular in the 1960s and early 1970s, combined the positive doctrine that changes in the growth rate of money dominated changes in the growth rate of nominal (and perhaps even real) GDP with the normative doctrine that central banks would do their monetary policy duties better if they kept the money supply growing at a constant (and modest) rate. In the early days of *BPEA*, Okun and Perry seemed to take stamping out the scourge of monetarism as part of their mission. And they succeeded—with much help from both academic papers, some of which appeared in *BPEA* (see below), and real-world events. I still remember that when Steve Goldfeld and I were working on what became my first published paper (Goldfeld and Blinder 1972), either Okun or Perry insisted that we include what became a one-page “Digression on the Behavior of the Money Supply.” Yes, it was off topic. But to those two missionaries, it was right on point.

I examined the historical record to see how many *BPEA* papers focused on money growth and monetarism. In doing so, I applied a strict filter, excluding papers that were mainly about interest rates, exchange rates, bank regulation, or the savings and loan debacle, even though all of these bear on monetary policy. To get into my count, a paper had to focus on the relationship between money growth and GDP growth, the instability of money demand, or the role of financial innovation therein. There were a whopping twenty-five such papers in the 1970s alone, and six more in the 1980s. Of these, Goldfeld’s (1973, 1976) two papers on money demand stand out. Lest you think the *BPEA* editors didn’t brook dissent, six of those twenty-five papers were authored or coauthored by Poole, the house monetarist.

The mention of Poole leads straight to two historical ironies.

First, although Poole was a monetarist himself, it was his seminal 1970 paper that laid the intellectual groundwork for the eventual demise of monetarism. Poole (1970b) used an extremely simple—and therefore intuitively transparent—model to show that money supply targeting is preferred when IS shocks dominate macro fluctuations, but interest rate targeting is preferred when LM shocks dominate. As time went by in the 1970s and 1980s, it became abundantly clear that LM shocks were gigantic, in the United States and elsewhere, presumably because financial innovations kept funds sloshing around from one definition of M to another.

Although Poole obtained his central finding in an extremely simple fixed-price model, it proved to be remarkably robust. In fact, although the

connection seems to have been all but forgotten, Poole's paper led directly to the famous Sargent and Wallace (1975) paper, which held even more dire implications for academic views on monetary policy. Given the importance of Sargent and Wallace (1975) in the history of macroeconomic thought in the academy, it is worth remembering that the central point of their paper was that adding rational inflationary expectations to Poole's model carried stunning implications. We all know where Sargent and Wallace (1975) led. But this is not the place to review the long, acrimonious debates over new classical economics because they took place mostly outside of *BPEA*.

The second big irony is that high inflation, the root of monetarism's ascendancy during the 1960s and 1970s, wound up accounting for its demise in the 1970s. As inflation rose in the late 1960s, Friedman and other monetarists successfully branded Keynesianism as inherently inflationary. That was effective public relations, but the charge wasn't true. In fact, both Heller and Okun, as CEA chairs, had urged President Johnson to raise taxes as a way to first head off, and later to reduce, demand-pull inflation from Vietnam spending. But Johnson didn't want anything to interfere with his grand plan to prosecute the war in Vietnam and the war on poverty at the same time. As always, politics triumphed over economics in the policy arena. But in the intellectual market, Keynesian stock sunk and monetarist stock rose.

Later, Lucas and Sargent (1978, 51) upped the ante, declaring Keynesian models to be guilty of "econometric failure on a grand scale" for much the same reason: inflation rose. This time, while there was a small dose of demand-pull inflation in, say, 1977–1978, the main culprits were a series of food and energy shocks that the rational expectations school somehow ignored. (Was doing that really rational?)

In October 1979, Federal Reserve Chair Paul Volcker announced the Federal Reserve's putative conversion to monetarism. Was it genuine? I'm pretty sure Volcker was not an avid reader of either Lucas and Sargent (1978) or *BPEA*. His wonderful memoir (Volcker 2018, 118) makes it clear that his conversion to monetarism was mainly a mechanism for tying the Federal Reserve Open Markets Committee (FOMC) to the inflation-fighting mast—and also a better way to explain the fight to the general public.

Years before Volcker's chairmanship, the high inflation of the late 1960s and 1970s had interacted badly with nominal interest rate ceilings and other corsets on banks, thereby incentivizing wave after wave of financial innovation designed to elide dysfunctional regulations. Seeing such LM shocks happening on a grand scale, one central bank after another abandoned either the pretense or practice of monetarism. (At the Federal

Reserve, it seemed mostly to be pretense.) As Gerald Bouey, the governor of the Bank of Canada at the time, famously quipped, “We did not abandon M1, M1 abandoned us.”¹⁰ So where monetarism was concerned, it was: inflation giveth, and inflation taketh away.

Appropriately, the Brookings Panel turned its attention to financial innovation and the instability of money demand early and often. Goldfeld’s two papers in the 1970s were already mentioned; the second (Goldfeld 1976) was provocatively titled, “The Case of the Missing Money.”¹¹ Among other things, that paper discussed financial innovations as causes of the decline in money demand. Two *BPEA* issues earlier, the Federal Reserve’s Enzler, Johnson, and Paulus (1976, 279) had “speculated that much of the weakness in money demand reflects innovations and regulatory changes.” It was sound speculation even though Poole, in discussing their paper, was unconvinced. The next year, with Poole again the discussant, Friedman (1977) wrote provocatively (to monetarists) about “The Inefficiency of Short-Run Monetary Targets for Monetary Policy.” Them’s fightin’ words.

Attention turned to financial innovation in earnest at the first Brookings Panel meeting of 1979, when another team from the Federal Reserve (Porter, Simpson, and Mauskopf 1979) presented a paper titled, “Financial Innovation and the Monetary Aggregates.” Their analysis held little good news for using the monetary aggregates, although Poole (1979), in an accompanying paper, was still unconvinced. And don’t forget that 1979 was the momentous year the Federal Reserve turned putatively to monetarism.

The final *BPEA* nails in the monetarist coffin were hammered in by Hester (1981), Lindsey (1982), and Simpson (1984) in the early 1980s. Hester (1981, 142) emphasized that “monetary policy is poorly designed if it fails to take into account the possibility that conditions which result from policy changes may lead to innovations.” I was the discussant of Simpson’s paper, and my opening words (Blinder 1984, 266) summarized it as “an intelligent brief about why the Federal Reserve should not have done what it did between October 1979 and October 1982.” By the time Bosworth (1989) penned his paper titled “Institutional Change and the Efficacy of Monetary Policy” and Romer and Romer (1990) wrote “New Evidence on the Monetary Transmission Mechanism,” monetarism was not even mentioned. Okun was probably smiling from the grave. Perry was probably smiling in his seat.

10. Canada, House of Commons Standing Committee on Finance, Trade and Economic Affairs, Minutes of Proceedings and Evidence, no. 134, March 28, 1983, 12.

11. Full disclosure: I believe I suggested that title.

III. Advice for Monetary Policymakers

Analysis and evaluation of monetary policy in *BPEA* did not, of course, end with debunking monetarism. It has been a focus of the Brookings Panel from its earliest days to today. In examining this voluminous literature, I applied another strict filter, restricting myself to papers that clearly either evaluated monetary policy decisions (generally, the Federal Reserve's) or dispensed advice to monetary policymakers. This filter excludes, for example, many interesting and important papers on financial crises (not just the big one), bank regulation, credit controls, and the like. It also excludes a number of notable “big think” papers that are highly relevant to monetary policy, such as Okun's (1973) famous “Upward Mobility in a High-Pressure Economy,” Blanchard and Simon's (2001) early paper on the Great Moderation, and Sims's (2002) insightful “The Role of Models and Probabilities in the Monetary Policy Process.” My filter nonetheless left a whopping fifteen Brookings papers in the 1970s, five in the 1980s, seven in the 1990s, nine in the 2000s, and fifteen in the 2010s. Since that adds up to fifty-one, I'll just hit some highlights.

The Brookings Panel has never shied away from giving advice to monetary policymakers. That tradition started in the first issue of *BPEA* with a short paper by Kareken (1970, 161), who concluded by observing that “the implication would seem to be that the economy may take one course if the FOMC uses the [Treasury] bill rate and money market variables in specifying policy, as it did in 1969, and another if it uses one or more of the monetary aggregates.” I wonder if Okun and Perry put him up to that.

Jump all the way to the fall 2018 issue, and you'll find two papers offering advice to the Federal Reserve. One was written by a team from the Boston Federal Reserve that included its president, Eric Rosengren (Fuhrer and others 2018), and set the stage for the Federal Reserve's subsequent review of its strategy, tools, and communications. The other was a symposium on policy at the effective lower bound, which featured a contribution from Yellen (2018), in which she advocated a lower-for-longer strategy for short rates similar to what the Federal Reserve had promulgated during her chairmanship.¹² So here was a case of a former Federal Reserve chair using *BPEA* to give advice to a current chair.

But back to history. In the second *BPEA* issue, Poole (1970a, 273) examined, and seemed to laud, gradualism in fighting inflation. In his words,

12. The issue also included short papers by Forbes (2018), Hamilton (2018), and Swanson (2018).

“politicians and the informed public now clearly recognize that excessive zeal in fighting inflation will produce excessive unemployment.” Right! But this is not a message I associate with either monetarists or new classical economists. I do, however, associate it with empirical Phillips curves featuring sticky prices.

Furthermore, Poole (1970c) was back in the following issue with a paper titled “Whither Money Demand?” which examined the econometric difficulties of estimating a demand-for-money function. Was Poole shunning his role as the house monetarist? No. He soon bolstered his monetarist credentials with a long paper on how the Federal Reserve could and should improve its control of the money stock (Poole and Lieberman 1972). Perusing those early *BPEA* volumes, it is hard to escape the conclusion that Poole was overworked.

In 1972, Okun (1972) provided a thorough and thoughtful examination of what we now call the rules versus discretion debate. That paper came years before Kydland and Prescott (1977), but long after Friedman (1948). Friedman and the monetarists, of course, were arguing for a k-percent rule for money growth, basing their case largely on imperfect knowledge of the economy and imperfect behavior by policymakers. Okun (1972, 157) concluded at the time that “rules for fixed instrument settings would not achieve our objectives. . . . The proponents of rules . . . have provided good questions and bad answers.” Much the same could be said today, except that today’s rules don’t have “fixed instrument settings.”

The k-percent rule fell of its own weight when monetarism collapsed. It was replaced by Kydland and Prescott’s (1977) argument that central bankers have an inflation bias—itself a dubious proposition—and that tying their hands with rules is the way to correct it. Their argument was further developed by Barro and Gordon (1983) and others, and it had enormous influence within academia—but not, I believe, in central banks. The popularity of these time inconsistency models in academia was somewhat amazing, given what was happening in the real world at the time. The models basically predicted that inflation would always be too high, not that it would rise (as it had from 1965 to 1980 in the United States) and then fall (as it did after 1980). Did time inconsistency somehow get worse and then get better?

The third incarnation of the rules versus discretion debate, which is still with us today, revolves around the Taylor (1993) rule. It was taken up in Kocherlakota’s (2016) fascinating paper—of which I was a discussant (Blinder 2016). Kocherlakota’s conclusions were (a) that it seems unlikely on basic theoretical grounds that an inevitably imperfect rule would be

superior to inevitably imperfect discretion, and (b) that the Taylor rule, in particular, may have led the FOMC to be too timid in pushing the economy out of the Great Recession.

Returning to the 1970s, the first *BPEA* issue of 1974 featured a debate between Tobin (1974), perhaps the leading Keynesian of the day, and Poole (1974) over what the Federal Reserve should do to end the deep recession. You probably can guess what each gentleman said. But you probably can't guess the names of the two discussants: the father-and-son team of Robert Aaron Gordon and Robert J. Gordon (1974). Almost poetic.

The following year was notable for the paper by Modigliani and Papademos (1975) that coined the term non-inflationary rate of unemployment (NIRU, later corrected to NAIRU) and offered estimates thereof ranging from 5.1 percent to 5.8 percent. They advised the Federal Reserve that, as the economy struggled its way out of the deep recession, "monetary policy should be aimed at explicitly stated targets for real output and employment" (Modigliani and Papademos 1975, 141). Nominal anchors were not yet in vogue.

The previously discussed preoccupation with monetarism dominated the 1970s and 1980s. So I'll skip ahead to 1990, when Romer and Romer (1990) published a sequel to the "narrative approach" they had pioneered in Romer and Romer (1989). Their focus at the Brookings Panel meeting that day was comparing the conventional IS-LM view of how monetary policy works (via bank reserves and money) with the so-called lending or credit view, which emphasizes the unique importance of bank loans. Their reading of the evidence favored the former, but that was thirty years ago.

A year later, Bernanke, who was destined for greater things, teamed up with Lown of the New York Federal Reserve to write a widely cited paper on the credit crunch of 1990 (Bernanke and Lown 1991). It would not be Bernanke's last notable Brookings paper. In 1997, he partnered with Gertler and Watson to write what some people view as the definitive analysis of oil shocks and monetary policy (Bernanke, Gertler, and Watson 1997). In 2004, while a governor of the Federal Reserve, Bernanke, Reinhart, and Sack (2004) presented an important assessment, "Monetary Policy Alternatives at the Zero Bound," that is frequently cited on this issue which is still very much alive. Several years after he retired from the Federal Reserve, Bernanke (2018) was in a better position than almost anyone else to assess the real effects of disrupted credit during the financial crisis. The panel audience was all ears that day. Interestingly, but not surprisingly, he placed great emphasis on the credit view that the Romers had debunked in 1990. *BPEA* is not monolithic.

But back to history—way baaack. In 1998, Krugman (1998) created a stir, and subsequently a boatload of citations, with his famous paper, “It’s Baaack: Japan’s Slump and the Return of the Liquidity Trap.” That paper was the first of what would become a series of Brookings papers dealing in one way or another with the “zero” lower bound on nominal interest rates. Five years later, Eggertsson and Woodford (2003) published their famous paper on optimal monetary policy at the zero lower bound, which made the case for price-level targeting. That paper, along with Reifschneider and Williams (2000), is often credited with being the inspiration for the “lower for longer” idea that the Federal Reserve adopted in 2013.

Williams (2009) was the research director at the San Francisco Federal Reserve when he addressed the zero lower bound question at the fall 2009 Brookings Panel meeting. He suggested that day that the 2 percent inflation target might be too low—a conclusion that, given his current position, he may want to blame on his identical twin. The zero lower bound issue was also addressed, in a wide variety of ways, by Edge and Gürkaynak (2010), Swanson (2011), Krishnamurthy and Vissing-Jorgensen (2011), Svensson (2011), Campbell and others (2012), Chodorow-Reich (2014), Evans and others (2015), Kiley and Roberts (2017), and, as mentioned earlier, a fall 2018 symposium featuring Yellen, Forbes, Hamilton, and Swanson. Whew! Some of these papers focused on forward guidance or quantitative easing. Notice that many of the authors on this list were either staff members or decision makers of the Federal Reserve.

It is no exaggeration to say that *BPEA* has been one of the main outlets for research and writing on unconventional monetary policy. Brookings was also exploiting its comparative advantage on speed here; the more academic journals were much slower.

IV. After Fifty Years

So, as we look back today on fifty years of writing about and debate over monetary policy in *BPEA*, what are the major contributions of the panel?

Most clearly, I think, the Brookings Panel has played a dominant—though not a monopoly—role in the development and evolution of empirical Phillips curves. Gordon was clearly the leader in this domain, though he had plenty of help; and I look forward to sampling Chateau Gordon 2022 once he’s figured it all out.

On the demise of monetarism, which was one of the presumed original goals of Okun and Perry, you might say the job was easy: monetarism fell of its own weight. But it didn’t always look that way in real time, and

Goldfeld's (1976) missing money plus a host of *BPEA* papers on financial innovations and money demand played significant roles.

When it came to thinking sensibly about supply shocks, inflation, and monetary policy, I'd say that *BPEA* was there early and often while too many academic economists were not—and indeed are still not. And on monetary policy more generally, I'd emphasize, as *BPEA* standouts, the defense of discretion against rules, the great attention given to estimates of the NAIRU, and the spate of ideas on how to cope with (or to avoid) the zero lower bound.

More fundamentally, I'd argue, the Brookings Panel kept Keynesian ideas alive and kicking through onslaughts first from monetarism, then from new classical economics, real business cyclists, and even supply-side economics. Over the decades, *BPEA* has been consistently less faddish, and more closely tied to the earth, than the major academic journals. Today's Keynesianism differs in many ways from Keynes's *General Theory* (1936), and also from what you can read in the early issues of *BPEA*—as it should. But it remains the best game in town.

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