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BPEA and Monetary Policy over 50 Years

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In the beginning....

In thinking about the historic role the Brookings Panel has played as an intermediary/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. The 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 NYU in November 1968, the same year that Karl Brunner (1968) coined the term "monetarism." The then-famous Andersen-Jordan (1968) paper, which purported to show empirically that money growth mattered for GDP 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 (1970) seminal paper on money 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 co-equal partner, and perhaps even as the senior partner. In the policy world, both monetary policy and fiscal policy had turned contractionary in the late 1960s to fight inflation—the former joined 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 U.S. economy.[1] The 1969-1970 recession, which followed, was in progress when the first BPEA meeting convened. Then as since, the leaders of the Brookings Panel did not much like recessions.

Art Okun, who had chaired President Johnson's Council of Economic Advisers until January 1969, and George Perry, who had recently arrived from the University of Minnesota, made a fantastic team. Together, they recruited an all-star cast for the inaugural Brookings Panel. Its

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

members included some gray hairs but skewed decidedly young—featuring (in age order), Poole (born in 1937), Bill Branson (b. 1938), Bob Gordon (b. 1940), Barry Bosworth (b. 1942), and Bob Hall (b. 1943). Yes, Okun and Perry had an eye for talent. (In case you're wondering, I'm younger than all those guys.) That first BPEA meeting also included as "senior advisers" to the panel, such luminaries as Larry Klein, Paul Samuelson, and Bob Solow—not to mention a business consultant named Alan Greenspan. Taken *in toto*, this list evokes the Kennedy quip about a 1962 gathering of Nobel Prize winners at the White House being the greatest collection of brainpower to dine there since Jefferson dined alone.

George, of course, is still part of BPEA, 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 prominent topics: the Phillips curve, money growth and monetarism, and ideas for and evaluations of monetary policy.

I. The Phillips curve

It is altogether fitting and proper that the first paper at the first BPEA meeting was on the Phillips curve, and that it was written by Bob Gordon (1970), who was then just 30. 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. Phillips (1958) original paper had used *wage* inflation as the lefthand variable and basically dismissed inflation, not to mention *expected* inflation, as a righthand variable. This was not an oversight. Phillips (1958, p. 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' 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) is a nonlinear function of the unemployment rate, π_t is the inflation rate, and ε_t is a stochastic error term. When Lipsey estimated that same equation with more modern data, rather than Phillips' 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 (1968) and Phelps' (1967, 1968) theoretical arguments for why α should be 1.0 were persuasive, the data showed $\alpha<1$.[2] For example, that first Gordon BPEA paper (1970), estimated α to be just 0.45.[3] It was, as Groucho Marx might have put it if he had a Ph.D. in economics, "who ya gonna believe, Milton or your lyin' eyes."

Soon, however, empirical eyesight improved, largely through Bob Gordon's efforts in BPEA. By the 1972:2 meeting, he already had an estimated Phillips curve with a nonlinear α coefficient that rose as expected inflation rose, reaching 1.0 at an expected inflation rate around 7 percent. Thus, by 1972 or 1973, the empirical debate 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 (December-to-December) in the U.S. rose from 3.4% in 1972 to 8.9% in 1973 and 12.1% in 1974 (during a recession, no less!), monetarists crowed that they had been right all along: Keynesian economics, with its misguided Phillips curve, was inherently inflationary. A few years later, Lucas and Sargent (1978, p. 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 villains at BPEA meetings were not profligate Keynesian spenders, but rather supply shocks. Months before OPEC struck, Barry Bosworth and John Farmer (1973) called attention to crop failures, disappearing anchovies, and the food-price explosion as sources of inflation. A year later, Joel Popkin (1974, p. 259) concluded that "the effect of commodity inflation was substantial in 1973." More fundamentally, in that same issue, James Pierce and Jared Enzler (1974) of the Federal Reserve Board staff modified the Keynesian MPS model

[2] Tom Sargent's (1971) brilliant little paper showing why $\alpha=1$ was beside the point under rational expectations was not yet appreciated.
[3] For this equation, Gordon (1970, pp. 36–37) used an auxiliary equation for nominal bond rates to estimate π^e as a function of past inflation rates.
[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).

to analyze the macroeconomic impacts of what they called "external inflationary shocks." Their simulations showed stagflation, of course: Output fell and inflation rose.[6]

Notice, please, that all this analysis came very quickly—far faster than scholarly journals could react. This has always been an important advantage of BPEA.

By the 1975:1 issue, Bob 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 Bill Nordhaus (1975, p. 663), in discussing that paper, referring 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), Charlie 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 ½ percentage point. That rule worked well for decades. In the mid-1990s, as Vice Chairman of the Fed, 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 curves gave 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 Olivier Blanchard (1987) and one by Larry Ball, Greg Mankiw, and David Romer (1988).[8] A 1996 paper by George Akerlof, Bill Dickens, and Perry (1996)—which the trio followed up by Akerlof, Dickens, and Perry (2000)—shook the intellectual tree a bit by adding what I'll 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

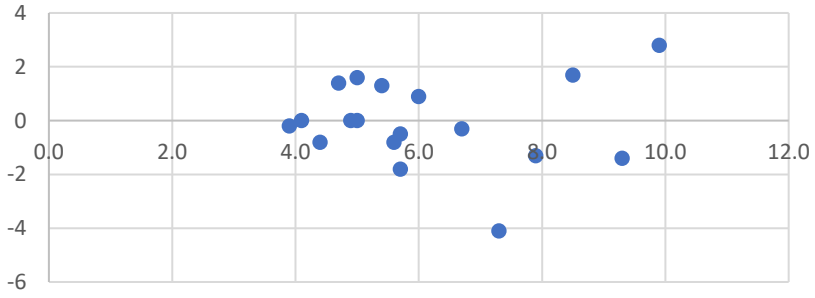
[6] Well, not quite. Their main simulations held *nominal* money supply growth constant, meaning that *real* money growth fell, which eventually extinguished the inflation.
[7] See, for example, my "notorious" (to some) Jackson Hole speech (Blinder 1994, p. 340).
[8] For this count, and in what follows, I interpret the phrase "concentrating on Phillips curves" fairly strictly. It excludes, for example, many related papers on labor market developments. I leave these to Robert Hall's paper in this issue.

the late 1990s raise inflation more? Gordon (1998) partly patched things up by incorporating several new supply shocks,[9] and by adapting the idea of a time-varying NAIRU from Doug Staiger, Jim Stock, and Mark Watson (1997). Larry Katz and Alan 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, Judd Cramer, and David 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 Sandeep 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.

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.

Figure 1
Change in CPI Inflation v. Unemployment, 2000-2018



II. Money growth and monetarism

In the early days of BPEA, Okun and Perry seemed to take stamping out the scourge of monetarism as part of their mission. They succeeded—with much help from both academic papers, some of which appeared in BPEA, and real-world events. I still remember that, while

[9] On these, see also Blinder and Yellen (2001).

Steve Goldfeld and l (1972) were working on what became my first published paper (though not my first *submitted* paper; as noted, BPEA was fast), 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 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 S&L 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 25 such papers in the 1970s alone, and six more in the 1980s. Of these, Goldfeld's two papers on money demand (1973, 1976) stand out. Lest you think the BPEA editors wouldn't brook dissent, six of those 25 papers were authored or co-authored by Bill 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 demise of monetarism. Poole (1970) 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 the LM shocks were gigantic—presumably because of financial innovation.

Although Poole's central finding was obtained 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-Wallace (1975) paper, which held even more dire implications for academic views on monetary policy. Given the importance of Sargent-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-Wallace (1975) led. But this is not the place to review the long, acrimonious debates over New Classical economics because it 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 the demise of monetarism in the 1970s. Because

inflation rose in the late 1960s, Friedman and other monetarists had successfully branded Keynesianism as inherently inflationary. That was effective PR, 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) 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.

In October 1979, of course, Fed Chair Paul Volcker announced the Fed'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, p. 118)) makes it clear that his conversion to monetarism was mainly a mechanism for tying the 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.[10] As then-Governor of the Bank of Canada Gerald Bouey (1982) famously quipped, "we didn't abandon monetary aggregates, they abandoned us." 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 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 Fed's Enzler, Lewis Johnson, and John Paulus (1976) had

[10] At the Fed, it seemed to be mostly pretense.
[11] Full disclosure: I believe I suggested that title.

"speculated that much of the weakness in money demand reflects innovations and regulatory changes." It was sound speculation, although Poole, in discussing their paper, was unconvinced. The next year, with Poole again the discussant, Ben Friedman (1977) wrote provocatively (to monetarists) about "The Inefficiency of Short-Run Monetary Targets for Monetary Policy." Fightin' words.

Attention turned to financial innovation in earnest at the first BPEA meeting of 1979, when another team from the Fed (Richard Porter, Thomas Simpson, and Eileen Mauskopf 1979) presented a paper entitled, "Financial Innovation and the Monetary Aggregates." Their analysis held little good news for using the Ms, although Poole (1979), in an accompanying paper, was still unconvinced. And don't forget that 1979 was the year the Fed turned putatively monetarist.

The final nails in the monetarist coffin, at least where the Brookings Panel was concerned, were hammered in by Donald Hester (1981), David Lindsey (1982), and Simpson (1984) in the early 1980s. Hester (p. 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 (p. 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 "Institutional Change and the Efficacy of Monetary Policy" and Christy and David Romer (1990) wrote their "New Evidence on the Monetary Transmission Mechanism," monetarism was not even mentioned. Art Okun was probably smiling from the grave. George Perry was probably smiling in this room.

III. Advice for monetary policymakers

Analysis and evaluation of monetary policy in BPEA did not, of course, end with the debunking of 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 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) "Upward Mobility in a High-Pressure Economy," Blanchard and Simon's (2001) paper on the Great Moderation, and Chris Sims' (2002), "The Role of Models and

Probabilities in the Monetary Policy Process." My filter nonetheless left a whopping 15 Brookings papers in the 1970s, five in the 1980s, seven in the 1990s, nine in the 2000s, and 15 in the 2010s. That adds up to 51, so 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 John Kareken (1970), 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" (p. 161). I wonder if Art and George put him up to that.

Jump all the way to the Fall 2018 issue, and you'll find two papers offering advice to the Fed. One was written by a team from the Boston Fed that included its president, Eric Rosengren (Fuhrer and others 2018). It set the stage for the Fed'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 Janet Yellen (2018),[12] in which she advocated a "lower for longer strategy" for short rates similar to what the Fed had promulgated during her chairmanship. (More on this later.) So here was a case of Fed chair n−1 using BPEA to give advice to Fed chair n.

But back to history. In the second BPEA issue, Poole (1970) examined, and seemed to laud, gradualism in fighting inflation. In his words, "politicians and the informed public now clearly recognize that excessive zeal in fighting inflation will produce excessive unemployment" (p. 273). Right! But this is not a message I associate with either monetarists or New Classicals. I do, however, associate it with empirical Phillips curves.

Furthermore, Poole (1970) 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 Bill shunning his role as the house monetarist? No. He soon bolstered his monetarist credentials with a long paper on how the Fed 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 Bill Poole was overworked.

In 1972, Okun (1972) provided a thorough and thoughtful examination of what we now call the rules-versus-discretion debate. This paper came years before Kydland and Prescott (1977), but long after Friedman (1948). Friedman and the monetarists, of course, were then arguing for a

[12] And also short papers by Kristin Forbes (2018), Jim Hamilton (2018), and Eric Swanson (2018).

k-percent rule for money growth, basing their case largely on imperfect knowledge of the economy and imperfect behavior by policymakers. Okun 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" (p. 157). 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 cure it. Their argument was further developed by Barro and Gordon (1983) and others, and 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 U.S.) and then *fall* (as it did after 1980).

The third incarnation of the rules-versus discretion debate revolves around the Taylor (1993) rule. It is still with us today and was taken up in Narayana Kocherlakota's fascinating (2016) paper--of which I was a discussant. 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 Jim Tobin (1974), perhaps the leading Keynesian of the day, and Bill Poole (1974) over what the Federal Reserve should do to end the deep recession. You probably can guess what each gentleman said. But you will be amused to learn the almost-poetic choices of discussants: the father-and-son team of Aaron Gordon and Bob Gordon (1974)—both of whom took Tobin's side.

The following year was notable for the paper by Franco Modigliani and Lucas Papademos (1975) that coined the term NIRU (later corrected to NAIRU), and offered estimates thereof ranging from 5.1% to 5.8%. They advised the Fed 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" (p. 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 the Romers (1990) published a sequel to the "narrative

approach" they had pioneered in Romer and Romer (1989). Their focus at BPEA 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 evidence favored the former, but that was 30 years ago.

A year later, Ben Bernanke, who was destined for greater things, teamed up with Cara Lown (1991) of the New York Fed to write a widely-cited paper on the credit crunch of 1990. It would not be Ben's last notable Brookings paper. In 1997, he partnered with Mark Gertler and Mark Watson (1997) to write what some people view as the definitive analysis of oil shocks and monetary policy. In 2004, while a Fed governor, he, Vince Reinhart, and Brian Sack (2004) presented an important assessment of "Monetary Policy Alternatives at the Zero Bound" that is frequently cited on this still-very-much-alive issue. Then, several years after he retired from the Fed, Ben (2018) was in a better position than almost anyone to assess "the real effects of disrupted credit" during the financial crisis." The BPEA audience was all ears that day. Interestingly, but not surprisingly, he put 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, Paul 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 late, Gauti Eggertsson and Mike 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 is often credited, along with Reifschneider and Williams (2000), with being the inspiration for the "lower for longer" idea that the Fed adopted in 2013.

John Williams (2009) was the research director at the San Francisco Fed when he addressed the zero bound question at the Fall 2009 BPEA meeting, suggesting that the 2% inflation target might be too low—a conclusion that, in his current position, he may blame on his identical twin. The lower bound issue was also addressed, in a wide variety of ways, by Rochelle Edge and Refet Gurkaynak (2010); Swanson (2011); Arvind Krishnamurthy and Annette Vissing-Jorgensen (2011); Lars Svensson (2011); Jeffrey Campbell, Charles Evans, Jonas Fisher, and Alejandro Justiniano (2012); Gabriel Chodorow-Reich (2014); Evans, Jonas Fisher, Francois Gourio, and Spencer Krane (2015); Michael Kiley and John 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 and/or quantitative easing.

Notice that many of the authors on this list were either staff members or decisionmakers of the Federal Reserve System. It is no exaggeration to say that BPEA has been one of the main outlets for research and writing on unconventional monetary policy.

IV. After fifty years

So, as we look back today on 50 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 monopoly—role in the development and evolution of empirical Philips curves. Bob Gordon was clearly the leader in this domain, though he had plenty of help; and I look forward to tasting 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 stand out.

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 academics 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 from, first, monetarism, then New Classical economics, then real business cyclists, and even supply-side economics. Today's Keynesianism differs in many ways from Keynes' *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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