What Have We Learned Since October 1979?

Ben S. Bernanke

The question asked of this panel is, "What have we learned since October 1979?" The evidence suggests that we have learned quite a bit. Most notably, monetary policymakers, political leaders, and the public have been persuaded by two decades of experience that low and stable inflation has very substantial economic benefits.

This consensus marks a considerable change from the views held by many economists at the time that Paul Volcker became Fed Chairman. In 1979, most economists would have agreed that, in principle, low inflation promotes economic growth and efficiency in the long run. However, many also believed that, in the range of inflation rates typically experienced by industrial countries, the benefits of low inflation are probably small— particularly when set against the short-run costs of a major disinflation, as the United States faced at that time. Indeed, some economists would have held that low-inflation policies would likely prove counterproductive, even in the long run, if an increased focus on inflation inhibited monetary policymakers from responding adequately to fluctuations in economic activity and employment.

As it turned out, the low-inflation era of the past two decades has seen not only significant improvements in economic growth and productivity but also a marked *reduction* in economic volatility, both in the United States and abroad, a phenomenon that has been dubbed "the Great Moderation." Recessions have become less frequent and milder, and quarter-to-quarter volatility in output and employment has declined significantly as well. The sources of the Great Moderation remain somewhat controversial, but, as I have argued elsewhere, there is evidence for the view that improved control of inflation has contributed in important measure to this welcome change in the economy (Bernanke, 2004). Paul Volcker and his colleagues on the Federal Open Market Committee deserve enormous credit both for recognizing the crucial importance of achieving low and stable inflation and for the courage and perseverance with which they tackled America's critical inflation problem.

I could say much more about Volcker's achievement and its lasting benefits, but I am sure that many other speakers will cover that ground. Instead, in my remaining time, I will focus on some lessons that economists have drawn from the Volcker regime regarding the importance of credibility in central banking and how that credibility can be obtained. As usual, the views I will express are my own and are not necessarily shared by my colleagues in the Federal Reserve System.

Volcker could not have accomplished what he did, of course, had he not been appointed to the chairmanship by President Jimmy Carter. In retrospect, however, Carter's appointment decision seems at least a bit incongruous. Why would the President appoint as head of the central bank an individual whose economic views and policy goals (not to mention personal style) seemed, at least on the surface, quite different from his own? However, not long into Volcker's term, a staff economist at the Board of Governors produced a paper that explained why Carter's decision may in

fact have been quite sensible from the President's, and indeed the society's, point of view. Although the question seems a narrow one, the insights of the paper had far broader application; indeed, this research has substantially advanced our understanding of the links among central bank credibility, central bank structure, and the effectiveness of monetary policy.

Insiders will have already guessed that the Board economist to whom I refer is Kenneth Rogoff, currently a professor of economics at Harvard, and that the paper in question is Ken's 1985 article, "The Optimal Degree of Commitment to an Intermediate Monetary Target" (Rogoff, 1985).[1] The insights of the Rogoff paper are well worth recalling today. Rather than considering the paper in isolation, however, I will place it in the context of two other classic papers on credibility and central bank design, an earlier work by Finn Kydland and Edward Prescott and a later piece by Carl Walsh. As I proceed, I will note what I see to be the important lessons and the practical implications of this line of research.[2]

Central bankers have long recognized at some level that the credibility of their pronouncements matters. I think it is fair to say, however, that in the late 1960s and 1970s, as the U.S. inflation crisis was building, economists and policymakers did not fully understand or appreciate the determinants of credibility and its link to policy outcomes. In 1977, however, Finn Kydland and Edward Prescott published a classic paper, entitled "Rules Rather than Discretion: The Inconsistency of Optimal Plans" (Kydland and Prescott, 1977), that provided the first modern analysis of these issues.[3] Specifically, Kydland and Prescott demonstrated why, in many situations, economic outcomes will be better if policymakers are able to make credible

commitments, or promises, about certain aspects of the policies they will follow in the future. "Credible" in this context means that the public believes that the policymakers will keep their promises, even if they face incentives to renege.

In particular, as one of Kydland and Prescott's examples illustrates, monetary policymakers will generally find it advantageous to commit publicly to following policies that will produce low inflation. If the policymakers' statements are believed (that is, if they are credible), then the public will expect inflation to be low and demands for wage and price increases should accordingly be moderate. In a virtuous circle, this cooperative behavior by the public makes the central bank's commitment to low inflation easier to fulfill. In contrast, if the public is skeptical of the central bank's commitment to low inflation (for example, if it believes that the central bank may give in to the temptation to overstimulate the economy for the sake of short-term employment gains), then the public's inflation expectations will be higher than they otherwise would be. Expectations of high inflation lead to more aggressive wage and price demands, which make achieving and maintaining low inflation more difficult and costly (in terms of lost output and employment) for the central bank.

Providing a clear explanation of why credibility is important for effective policymaking, as Kydland and Prescott did, was an important step. However, these authors largely left open the critical issue of how a central bank is supposed to obtain credibility in the first place. Here is where Rogoff's seminal article took up the thread.[4] Motivated by

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[1] Rogoff's paper was widely circulated in 1982, a sad commentary on publication lags in economics.

[2] In focusing on three landmark papers, I necessarily ignore what has become an enormous literature on credibility and monetary policy. Walsh (2003, Chap. 8) provides an excellent overview. Rogoff (1987) was an important early survey of the "first generation" of models of credibility in the context of central banking.

[3] In another noteworthy paper, Calvo (1978) made a number of points similar to those developed by Kydland and Prescott (1977). The extension of the Kydland-Prescott "inflation bias" by Barro and Gordon (1983a) has proved highly influential.

[4] Rogoff was my graduate school classmate at M.I.T., and I recently asked him for his recollections about the origins of the "conservative" central banker. Here (from a personal e-mail) is part of his response:

[T]he paper was mainly written at the Board in 1982…It came out as an IMF working paper in February 1983 (I was visiting there), and then the same version came out as an International Finance Discussion paper [at the Board of Governors] in September 1983…The original version of the paper…featured inflation targeting. Much like the published paper, I suggested that having an independent central bank can be a solution to the time consistency [that is, credibility] problem if we give the bank an intermediate target and some (unspecified) incentive to hit the target…I had the conservative central banker idea in there as well, as one practical way to ensure the central bank placed a high weight on inflation. Larry Summers, my editor at the [*Quarterly Journal of Economics*], urged me to move that idea up to the front

the example of Carter and Volcker, Rogoff's paper showed analytically why even a President who is not particularly averse to inflation, or at least no more so than the average member of the general public, might find it in his interest to appoint a well-known "inflation hawk" to head the central bank. The benefit of appointing a hawkish central banker is the increased inflation-fighting credibility that such an appointment brings. The public is certainly more likely to believe an inflation hawk when he promises to contain inflation because they understand that, as someone who is intrinsically averse to inflation, he is unlikely to renege on his commitment. As increased credibility allows the central bank to achieve low inflation at a smaller cost than a noncredible central bank can, the President may well find, somewhat paradoxically, that he prefers the economic outcomes achieved under the hawkish central banker to those that could have been obtained under a central banker with views closer to his own and those of the public.

Appointing an inflation hawk to head the central bank may not be enough to ensure credibility for monetary policy, however. As Rogoff noted in his article, for this strategy to confer significant credibility benefits, the central bank must be perceived by the public as being sufficiently independent from the rest of the government to be immune to short-term political pressures. Thus Rogoff's proposed strategy was really two-pronged: The appointment of inflation-averse central bankers must be combined with measures to ensure central bank independence. These ideas, supported by a great deal of empirical work, have proven highly influential.[5] Indeed, the credibility benefits of central bank autonomy have been widely recognized in the past 20 years, not only in the academic literature but, far more consequentially, in the real-world design of central banking institutions. For example, in the United Kingdom, the euro area, Japan, and numerous other places, recent legislation or other government action has palpably strengthened the independence of the central banks.[6]

Rogoff's proposed solution to the credibility problems of central banks does have some limitations, however, as Ken recognized both in his paper and in subsequent work. First, although an inflation-averse central banker enhances credibility and delivers lower inflation on average, he may not respond to shocks to the economy in the socially desirable way. For example, faced with an aggregate supply shock (such as a sharp rise in oil prices), an inflation-averse central banker will tend to react too aggressively (from society's point of view) to contain the inflationary impact of the shock, with insufficient attention to the consequences of his policy for output and employment.[7] Second, contrary to an assumption of Rogoff's paper, in practice, the policy preferences of a newly appointed central banker will not be precisely known by the public but must be inferred from policy actions. (Certainly the public's perceptions of Chairman Volcker's views and objectives evolved over time.) Knowing that the public must make such inferences might tempt a central banker to misrepresent the state of the economy (Canzoneri, 1985) or even to take suboptimal policy decisions; for example, the central banker may feel compelled to tighten policy more aggres-

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section and place inflation targeting second. This, of course, is how the paper ended up.

[Regarding the Fed], Dale Henderson and Matt Canzoneri liked the paper very much…[M]any other researchers gave me feedback on my paper (including Peter Tinsley, Ed Offenbacher, Bob Flood, Jo Anna Gray, and many others)… Last but perhaps most important, there is absolutely no doubt that the paper was inspired by my experience watching the Volcker Fed at close range. I never would have written it had I not…ended up as an economist at the Board.

[5] Walsh (2003, Section 8.5) reviews empirical research on the correlations of central bank independence and economic outcomes. A consistent finding is that more-independent central banks produce lower inflation without any increase in output volatility.

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[6] The benefits of central bank independence should not lead us to ignore its downside, which is that the very distance from the political process that increases the central bank's policy credibility by necessity also risks isolating the central bank and making it less democratically accountable. For this reason, central bankers should make communication with the public and their elected representatives a high priority. Moreover, central bank independence does not imply that central banks should never coordinate with other parts of the government, under the appropriate circumstances.

[7] Lohmann (1992) shows that this problem can be ameliorated if the government limits the central bank's independence, stepping in to override the central bank's decisions when the supply shock becomes too large. However, to preserve the central bank's independence in normal situations, this approach would involve stating clearly in advance the conditions under which the government would intercede, which may not be practicable.

sively than is warranted in order to convince the public of his determination to fight inflation. The public's need to infer the central banker's policy preferences may even generate increased economic instability, as has been shown in a lively recent literature on the macroeconomic consequences of learning.[8]

The third pathbreaking paper I will mention today, a 1995 article by Carl Walsh entitled "Optimal Contracts for Central Bankers," was an attempt to address both of these issues.[9] To do so, Walsh conducted a thought experiment. He asked his readers to imagine that the government or society could offer the head of the central bank a performance contract, one that includes explicit monetary rewards or penalties that depend on the economic outcomes that occur under his watch. Remarkably, Walsh showed that, in principle, a relatively simple contract between the government and the central bank would lead to the implementation of monetary policies that would be both credible and fully optimal. Under this contract, the government provides the central banker with a base level of compensation but then applies a penalty that depends on the realized rate of inflation—the higher the observed inflation rate, the greater the penalty.

If the public understands the nature of the contract and if the penalty assessed for permitting inflation is large enough to affect central bank behavior, the existence of the contract would give credence to central bank promises to keep the inflation rate low (that is, the contract would provide credibility).[10] Walsh's contract has in common with Rogoff's approach the idea that, in a world of imperfect credibility, giving the central banker an objective function that differs from the true objectives of society may be useful. However, Walsh also shows that the contracting approach ameliorates the two problems associated with Rogoff's approach. First, under the Walsh contract, the central banker has incentives not only to achieve the target rate of inflation but also to respond in the socially optimal manner to supply shocks.[11] Second, as the inflation objective and the central banker's incentive scheme are made explicit by the contract, the public's problem of inferring the central banker's policy preferences is significantly reduced.

There have been a few attempts in the real world to implement an incentive contract for central bankers—most famously a plan proposed to the New Zealand legislature, though never adopted, which provided for firing the governor of the central bank if the inflation rate deviated too far from the government's inflation objective.[12] But Walsh's contracts are best treated as a metaphor rather than as a literal proposal for central bank reform. Although the pay of central bankers is unlikely ever to depend directly on the realized rate of inflation, central bankers, like most people, care about many other aspects of their jobs, including their professional reputations, the prestige of the institutions in which they serve, and the probability that they will be reappointed.

Walsh's analysis and many subsequent refinements by other authors suggest that central bank performance might be improved if the government set explicit performance standards for the central bank (perhaps as part of the institution's charter or enabling legislation) and regularly compared objectives and outcomes. Alternatively, because central banks may possess the greater expertise in determining what economic outcomes are both feasible and most desirable, macroeconomic goals might be set through a joint exercise of the govern-

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[8] Evans and Honkopohja (2001) is the standard reference on learning in macroeconomics. Recent papers that apply models of learning to the analysis of U.S. monetary policy include Erceg and Levin (2001) and Orphanides and Williams (forthcoming).

[9] Persson and Tabellini (1993) provided an influential analysis of the contracting approach that extended and developed many of the points made by Walsh (1995).

[10] An objection to this conclusion is that, although the central bank's incentives are made clear by the contract, the public might worry that the government might renege on its commitment to low inflation by changing the contract. Those who discount this concern argue that changing the contract in midstream would be costly for the government, because laws once enacted are difficult to modify and because changing an established framework for policy in an opportunistic way would be politically embarrassing.

[11] A key assumption underlying this result is that the central banker cares about the state of the economy as well as about the income provided by his incentive contract.

[12] In personal communications, Walsh reports to me that he was visiting a research institute in New Zealand at the time of these discussions. Walsh's reflection on the New Zealand proposals helped to inspire his paper.

ment and the central bank. Many countries have established targets for inflation, for example, and central bankers in those countries evidently make strong efforts to attain those targets. The Federal Reserve Act does not set quantitative goals for the U.S. central bank, but it does specify the objectives of price stability and maximum sustainable employment and requires the central bank to present semiannual reports to the Congress on monetary policy and the state of the economy. Accountability to the public as well as to the legislature is also important; for this reason, the central bank should explain regularly what it is trying to achieve and why. In sum, Walsh's paper can be read as providing theoretical support for an explicit, well-designed, and transparent framework for monetary policy, one which sets forth the objectives of policy and holds central bankers accountable for reaching those objectives (or at least for providing a detailed and plausible explanation of why the objectives were missed).

In the simple model that Walsh analyzes, the optimal contract provides all the incentives needed to induce the best possible monetary policy, so that appointing a hawkish central banker is no longer beneficial. However, in practice—because Walsh's optimal contracts can be roughly approximated at best, because both the incentives and the policy decisions faced by central bankers are far more complex than can be captured by simple models, and because the appointment of an inflation-averse central banker may provide additional assurance to the public that the government and the central bank will keep their promises—the Walsh approach and the Rogoff approach are almost certainly complementary.[13] That is, a clear, well-articulated monetary policy framework, inflation-averse central bankers, and autonomy for central banks in the execution of policy are all likely to contribute to increased central bank credibility and hence better policy outcomes. Of course, other factors that I could not cover in this short review, such as the central bank's reputation for veracity as established

over time, may also strengthen its credibility (Barro and Gordon, 1983b; Backus and Driffill, 1985).[14]

Let me end where I began, with reference to Paul Volcker and his contributions. I have discussed today how Volcker's personality and performance inspired one seminal piece of research about the determinants of central bank credibility. In focusing on a few pieces of academic research, however, I have greatly understated the impact of the Volcker era on views about central banking. The Volcker disinflation (and analogous episodes in the United Kingdom, Canada, and elsewhere) was undoubtedly a major catalyst for an explosion of fresh thinking by economists and policymakers about central bank credibility, how it is obtained, and its benefits for monetary policymaking. Over the past two decades, this new thinking has contributed to a wave of changes in central banking, particularly with respect to the institutional design of central banks and the establishment of new frameworks for the making of monetary policy.

Ironically, the applicability of the ideas stimulated by the Volcker chairmanship to the experience of the U.S. economy under his stewardship remains unclear. Though the appointment of Volcker undoubtedly increased the credibility of the Federal Reserve, the Volcker disinflation was far from a costless affair, being associated with a minor recession in 1980 and a deep recession in 1981-82.[15] Evidently, Volcker's personal credibility notwithstanding, Americans' memories of the inflationary 1970s were too fresh for their inflation expectations to change quickly. It is difficult to know whether alternative tactics would have helped; for example, the announcement of explicit inflation objectives (which would certainly have been a radical idea at the time) might have helped guide inflation expectations downward more quickly, but they might also have created a political backlash that would have doomed the entire effort. Perhaps no policy approach or set of institutional

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[13] Several authors have shown this point in models in which the inflation bias arising from noncredible policies differs across states of nature; see, for example, Herrendorf and Lockwood (1997) and Svensson (1997).

[14] But see Rogoff (1987) for a critique of models of central bank reputation.

[15] Evidence on the behavior of inflation expectations after 1979 supports the view that the public came to appreciate only very gradually that Volcker's policies represented a break from the immediate past (Erceg and Levin, 2001).

arrangements could have eliminated the 1970s inflation at a lower cost than was actually incurred. If so, then the significance of Paul Volcker's appointment was not its immediate effect on expectations or credibility but rather that he was one of the rare individuals tough enough and with sufficient foresight to do what had to be done. By doing what was necessary to achieve price stability, the Volcker Fed laid the groundwork for two decades, so far, of strong economic performance.

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What Have We Learned Since October 1979?

Alan S. Blinder

My good friend Ben Bernanke is always a hard act to follow. When I drafted these remarks, I was concerned that Ben would take all the best points and cover them extremely well, leaving only some crumbs for Ben McCallum and me to pick up. But his decision to concentrate on one issue—central bank credibility—leaves me plenty to talk about.

Because Ben was so young in 1979, I'd like to begin by emphasizing that Paul Volcker retaught the world something it seemed to have forgotten at the time: that *tight monetary policy can bring inflation down at substantial, but not devastating, cost.* It seems strange to harbor contrary thoughts today, but back then many people believed that 10 percent inflation was so deeply ingrained in the U.S. economy that we might be doomed to, say, 6 to 10 percent inflation for a very long time. For example, Otto Eckstein (1981, pp. 3-4) wrote in a well-known 1981 book that "To bring the core inflation rate down significantly through fiscal and monetary policies alone would require a prolonged deep recession bordering on depression, with the average unemployment rate held above 10%." More concretely, he estimated that it would require 10 point-years of unemployment to bring the core inflation rate down a single percentage point,[1] which is about five times more than called for by the "Brookings rule of thumb."[2] As it turned out, the Volcker disinflation followed the Brookings rule of thumb rather well. About 14 cumulative percentage point–years of unemploy-

ment above the nonaccelerating inflation rate of unemployment (NAIRU) drove core inflation down by 6.2 percentage points over the six years spanning 1980 to 1985.[3] Yes, disinflation hurt, but much less than what the pessimists envisioned. Volcker may have enhanced the Fed's credibility; I certainly think so. But that did not improve the inflation-unemployment trade-off.

The forced march of core inflation down from 10 percent to 4 percent in the early 1980s taught us a second lesson that, I believe, is the essence of Paul Volcker's legacy: that *sometimes the central bank has to be single-minded about fighting inflation*, and that the strong will of a determined leader like Volcker is one key ingredient. When Volcker took the helm, the nation's problem was clear—too much inflation—and so was the solution—sustained tight money. It only required someone with iron will to apply the solution to the problem. Lindsey, Orphanides, and Rasche (2005) ask at this conference whether Volcker was a monetarist, a Keynesian, an inflation targeter, and so on. They seem to answer no in each case. To me, the right short characterization of Paul Volcker as Chairman of the Fed is simple: He was a highly principled and determined inflation hawk.

I would like to contrast these two Volcker lessons, which are the foci of this conference, with two quite different lessons that we can take away from the Greenspan era. The first is that, in apparent contradiction to what I just said, *flexibility in monetary policy is very important.* The contradiction is only apparent, not actual, because the worlds faced by Paul Volcker and Alan Greenspan were starkly different. During the Greenspan years, inflation has flared up only once, in 1990-91, and then only briefly. Instead, Greenspan has faced, among other things, two severe stock market crashes, a period of fragile bank balance sheets in the early 1990s, the rolling international financial

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[1] Eckstein (1981, p. 46).

[2] This rule of thumb was due to a number of members of the Brookings Panel on Economic Activity in the 1970s, including Arthur Okun, George Perry, and William Nordhaus, but especially arose from a series of papers by Robert Gordon.

[3] The calculation assumes a NAIRU of 5.8 percent, which was the actual unemployment rate of 1979.

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crises of 1997 and 1998,[4] the surprising productivity acceleration after 1995, a brief flirtation with deflation, and the need to pull off several "soft landings." Excruciatingly tight money was not the right solution to any of these problems. I daresay that history will not remember Alan Greenspan as the man who took 17 years to bring inflation down from 4 percent to 2 percent. Rather, it will remember him as the Fed Chairman who dealt so well with a remarkable variety of difficult challenges over a prolonged period of time.

Here's a test. Try a little mental free-association with the phrase "accomplishments of Paul Volcker as Chairman of the Fed."[5] I think all of you will immediately think of "conquering inflation," or something synonymous with that. Now try "accomplishments of Alan Greenspan as Chairman of the Fed." Here there are so many choices that I doubt that even this well-informed group could ever agree on a single answer. My own choice would be how spectacularly well he recognized and dealt with the productivity acceleration after 1995. But others will have their own favorite on the long and impressive Greenspan hit parade.

That hit parade brings me naturally to the fourth lesson, which is that *fine tuning is actually possible* if you combine enough skill with a modicum of good luck. I began my economic education in the halcyon days of Walter Heller, when a number of economists really believed in fine-tuning. By the time I started teaching at Princeton in 1971, however, this belief had been shattered. But Alan Greenspan's remarkable performance should bring it roaring back. Greenspan probably shuns the label "fine-tuner." But his record is replete with delicate decisions over moves of 0 versus 25 basis points or 25 versus 50 basis points, with careful management of the exact monthly timing of this rate increase or that rate decrease, with several actual and attempted soft landings, with influencing markets with minor variations in wording, and so on. If that is not fine-tuning, I don't know what is. And you know what? It

worked. We've had only two mild recessions during Greenspan's long watch. As a result, the bar for the next Chairman of the Fed has been set extraordinarily high.

My fifth lesson goes back to the Volcker years. Curiously, it seems not to have been mentioned at this conference yet. So let me say it: *Money-supply targeting can be hazardous to a nation's health.* Lindsey, Orphanides, and Rasche (2005) have discussed whether or not we should view the money-growth rule as a "political heat shield" that Volcker selected opportunistically to fend off criticisms of excruciatingly tight money. Frankly, after reading their paper I'm not sure whether their answer is yes or no. (My own view is yes.) But regardless, two things seem clear—and I state them here, at the Federal Reserve Bank of St. Louis, of all places. First, the Fed overdid monetary stringency in 1980-81 partly because of the misbehavior of velocity.[6] And second, rescuing the economy in 1982 required abandoning the experiment with monetarism. I shudder to think what might have happened to the U.S. economy in 1982 and thereafter if the Federal Open Market Committee (FOMC) had stubbornly stuck to its money growth targets. But Volcker and his colleagues were too smart—and insufficiently doctrinaire—to do that. (By the way, that's a good combination of attributes for a central banker.)

If a central bank abandons monetary aggregates, what should it put in their place? Many experts now answer: inflation targets. But that just pushes the question back one stage to this: What instrument should the central bank use to pursue its inflation target? After all, no matter how much theoretical models try to pretend that it is, the inflation rate is *not* a control variable. Milton Friedman taught us years ago that the nominal interest rate is a bad choice; fixing it can even lead to dynamic instability. The *real* interest rate, we have learned in the Volcker and Greenspan years, is a far better choice. And that is my sixth lesson.

Greenspan, in particular, has focused attention on an update of Wicksell's "natural interest rate" concept that we now call the *neutral real federal funds rate*. And, more by his actions than

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[4] Analogously to these last two, Volcker had to devote a great deal of time and energy to debt crisis in the developing countries that erupted in 1982 and the consequent concentration of risks on the balance sheets of many money center banks.

[5] The last five words are important. I am in awe of Volcker's many accomplishments since leaving the Federal Reserve System.

[6] Specifically, I do not believe the Fed ever intended to cause a recession as deep as the one we had.

by his rhetoric, he has called attention to the Taylor rule as a useful benchmark. For current purposes, I write the Taylor rule as a guide for setting the *nominal* funds rate in a way that stabilizes both inflation and output:

$$i = r^* + \pi + \alpha(\pi - \pi^*) + \beta(y - y^*),$$

where *i* is the nominal funds rate, r^* is the neutral level of the real funds rate, π is the inflation rate, *y* is the (log) of output, and π^* and y^* are the targets for inflation and output, respectively. We think of monetary policy as "easy" when $i < r^* + \pi$ and as "tight" when $i > r^* + \pi$.

I view the Taylor rule as a useful way of thinking about monetary policy, although it is not, and John Taylor did not intend it to be, a literal rule in the Friedmanite sense. Several aspects of the Taylor rule are worth mentioning. The first is that *both α and β are positive*. This means, for example, that there may be times when it is appropriate for the central bank to hold its interest rate *below* neutral even though the inflation rate is *above* target.[7]

The second aspect constitutes my seventh lesson. The requirement that α be positive means that *the central bank should react more than point for point to changes in the inflation rate*. For example, under Taylor's choice of $\alpha = 1/2$, each 1-point move in the inflation rate would induce the central bank to adjust its policy rate by 150 basis points in the same direction, meaning that the real funds rate moves by 50 basis points in that direction. If α is not positive, the central bank would be allowing rising inflation to *reduce* the real federal funds rate—a potentially destabilizing policy.

My last few lessons were learned in the Greenspan era. The eighth lesson is hardly ever mentioned, but I think it should be. Three times during the Greenspan era, the Fed demonstrated that *doing nothing can constitute a remarkably effective, even bold, monetary policy*.

The first such episode started in July or September 1992 and lasted until February 1994.[8] To stimulate an economy that seemed to be fight-

ing substantial financial "headwinds," the Fed held the nominal funds rate at 3 percent, which at the time meant that the *real* funds rate was kept at around zero, for about 18 months. This sizable and long-lasting monetary stimulus helped get the economy rolling in 1994 and thereafter. The third such episode was a similar effort to stimulate a sluggish economy. The Fed lowered the nominal funds rate to 1.25 percent in November 2002 and then to 1 percent in June 2003—and then held it there until June 30, 2004, a period of 12 to 19 months, depending on when you want to start counting. In both of these cases, the degree of monetary stimulus was quite large and the length of time for which it was applied was very long, by the standards of central banking. In that sense, each of these periods of "doing nothing" constituted a boldly expansionary policy.[9]

The middle episode of "doing nothing" was a bit different from the other two but, if anything, was an even bolder departure from standard central banking practice. From January 1996 until June 1999, the Fed did not raise interest rates to restrain the booming economy even though the unemployment rate kept falling through any reasonable estimate of the NAIRU.[10] Janet Yellen and I (2001) have called this episode the years of "forbearance," and it constituted a real gamble that Greenspan took over the objections of a number of FOMC members.[11] Other than his oft-expressed skepticism about the NAIRU concept, the stated basis for Greenspan's refusal to raise rates was his belief—which was subsequently ratified by the data—that productivity had accelerated and would continue on a high trajectory, thereby justifying a faster trend growth rate.[12] The gamble paid off handsomely.

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[7] Conversely, if *y* is high enough, the central bank will want "tight money," even if inflation is already below target.

[8] The Fed cut the funds rate to 3.25 percent in July 1992 and to 3 percent in September 1992.

[9] During much of the more recent episode, the inflation rate was drifting down, so the real funds rate was actually rising slightly. In the 1992-94 episode, inflation was quite constant.

[10] There was actually one 25-basis-point rate hike in March 1997. But the FOMC also reduced the funds rate by 75 basis points following the financial crisis in the fall of 1998.

[11] For more details on this episode from an insider's perspective, see Meyer (2004).

[12] Higher productivity growth, by itself, does not lead to an ever-decreasing NAIRU. But favorable supply shocks and the related hypothesis that *actual* productivity was running ahead faster than productivity as *perceived* by workers will lead to a transitory decline in NAIRU. On the latter, see Blinder and Yellen (2001, Chap. 6).

All three of these episodes, but especially the last, lead naturally to my ninth lesson. Another significant part of the Greenspan legacy is the demonstration that *a central bank can be strongly pro-growth without being irresponsible*. This, I think, is a genuine benefit of the Federal Reserve's much-maligned dual mandate to support *both* low inflation *and* high employment, coupled with a Chairman willing to make use of it. It would, I believe, have been much more difficult for an inflation-targeting central bank, or for a bank like the European Central Bank with a mono-goal, to forbear in 1996-99 the way the Fed did.

During these three periods of FOMC "inaction," intermediate and long rates were not marking time. Similarly, during the most recent Federal Reserve tightening (June 1999–May 2000) and easing (January 2001–June 2004) cycles, bond rates moved around quite a bit—generally in the direction the Fed wanted. This leads to the tenth lesson learned since 1979: If the central bank lets the markets in on its thinking, *the markets can do part of the work of monetary policy*. Specifically, if the markets believe the central bank will soon be raising (lowering) rates, intermediate and long rates will rise (fall) in anticipation, thereby tightening (easing) "monetary policy" before the policymakers lift a finger.

Outsourcing part of the work to the bond market in this way has two interesting, and probably salutary, implications for monetary policy. First, and less important, the central bank should not have to move its policy rate around as much, in either direction, as would be necessary without the anticipatory behavior of the bond market. Second, and more important, the lags in monetary policy should be reduced by the bond market's reactions. Not so many years ago, central bankers and economists viewed long rates as *following* short rates with a substantial lag—which slowed down the transmission of monetary policy impulses into the real economy. Nowadays, many central bankers and economists see long rates as *leading* short rates.

This anticipatory process can work, however, only if the central bank communicates its intentions to the markets effectively. Thus, and this is my final lesson from post-1979 experience, *greater transparency can enhance the effective-ness of monetary policy*. The old tradition at central banks was, of course, to say little and to say it cryptically. That's how the temple kept secrets. There is still far too much secrecy for my taste. But the unmistakable trend, both at the Fed and around the world, is toward greater transparency.

I could go on and on about why I think this is a salutary trend, both for democracy and for monetary policy—and I have.[13] But I think it is now time to relinquish the platform to Ben McCallum. Suffice it to say that while the Federal Reserve has often hesitated over specific incremental increases in disclosure, and while it has sometimes warned of adverse consequences from greater transparency, virtually none of these adverse consequences have ever come to pass, and the Fed has never regretted its step-by-step movements toward greater openness.[14] At least that's my reading of the history since 1994. If they disagree, there are plenty of current and former Federal Reserve officials present here today to dispute what I have just said.

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[13] On this trend, see Blinder (2004, Chapter 1).

[14] After this October 2004 St. Louis conference, the FOMC took yet another step in the direction of greater transparency by deciding to release its minutes earlier.

What Have We Learned Since October 1979?

Bennett T. McCallum

MODEL COMPARISON

The question posed for this panel cannot be answered entirely straightforwardly, for different analysts knew (i.e., believed) different things about monetary policy in October 1979, and the same is true now. But I will try to speak to the spirit of the question in an operational way by briefly contrasting mainstream models that are being used now, for policy analysis, with ones that were being used then. For the "now" portion of this comparison it is easy to write down a prototypical model, which is basically the one labeled as the "consensus" model by Goodfriend (2005) in his contribution to this conference. One might quibble with the term consensus, since some economists do not approve of this model, but it is in fact a very standard starting point, among policy analysts, for elaboration or in some cases disagreement. So, the agenda now is to compare it to its counterpart of 1979. How might one select a 1979-vintage model for that purpose? Well, in October of 1979, I was in the midst of writing a paper (McCallum, 1980) that was designed to demonstrate the effects of incorporating rational expectations (RE) into an otherwise mainstream macro model. Using that paper's model to represent those typical in 1979 might not be a perfect solution, but it is probably as good as anyone could reasonably expect.

Consider, then, the following basic model, circa 1979:

$$(1)\ y_t = b_0 + b_1(R_t - E_t \Delta p_{t+1}) + v_t \qquad b_1 < 0$$

$$(2)\ \Delta p_t = E_{t-1}\Delta p_t + \alpha_1(y_t - \bar{y}_t) + \alpha_2(y_{t-1} - \bar{y}_{t-1}) + u_t \qquad \alpha_1 > 0,\ \alpha_2 < 0$$

$$(3)\ m_t + \mu_0 + \mu_1 m_{t-1} + \mu_2(y_t - \bar{y}_{t-1}) = e_t \qquad \mu_1 > 0,\ \mu_2 < 0$$

$$(4)\ m_t - p_t = c_0 + c_1 y_t + c_2 R_t + \eta_t \qquad c_1 > 0,\ c_2 < 0$$

$$(5)\ \bar{y}_t = \gamma_0 + \gamma_1 \bar{y}_{t-1} + a_t \qquad \gamma_1 > 0$$

Here the symbols are as follows: y_t = log of output, $\bar{y}_t$ = log of natural-rate output, p_t = log of price level, m_t = log of money stock, R_t = one-period interest rate, and $v_t, u_t, e_t, \eta_t, a_t$ = stochastic shocks. Equation (1) represents an IS function in which the rate of spending on goods and services is taken to depend (negatively) on the real rate of interest. Equation (2) is a "natural rate" type of Phillips curve or price adjustment relationship, with the unit coefficient on $E_{t-1}\Delta p_t$ implying the absence of any long-run trade-off, as in Fischer (1977) or Lucas (1973). In addition, (4) is a money demand (or "LM") function of a standard type, while (3) represents monetary policy behavior with the central bank adjusting the money supply[1] each period in a way that responds to the current (or possibly a recent past) output gap. The latter concept refers to the fractional difference between output and its natural rate value, with the latter being generated (exogenously, for simplicity) in equation (5).

Using models of basically the foregoing specification, researchers such as Lucas (1973), Fischer (1977), Sargent (1973), Taylor (1979), and McCallum (1980) conducted RE analysis to determine the dynamic properties of various systems and alternative policy rules. One of the main objects of analysis was to determine whether the systematic components of monetary policy rules, or only the purely random components, have effects on the cyclical properties of real variables—including employment and especially the output gap—when expectations are formed rationally.

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[1] Researchers who were concerned with operationality, such as Andersen and Jordan (1968) and Brunner and Meltzer (1976), tended to use the monetary base as the instrument variable in policy specifications that would be represented by (3) in the model.

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Lucas (1972, 1973), Sargent (1973), and most notably Sargent and Wallace (1975) argued that the behavior of the gap would be unaffected by alternative monetary policy rules, while Fischer (1977) and Taylor (1979) took the opposing position. My review (McCallum, 1980) concluded that there were plausible specifications that would support each position. It should be emphasized, however, that most policy analysis being conducted at the time was not of this type, focusing on properties of dynamic systems, but instead featured point-in-time exercises of the type that RE analysis showed to be (in many cases) fundamentally misleading.

For comparison, today's prototype model can be written, in its simplest form, as follows:

(6) $y_t = b_0 + b_1(R_t - E_t \Delta p_{t+1}) + E_t y_{t+1} + v_t$ $b_1 < 0$

(7) $\Delta p_t = \beta E_t \Delta p_{t+1} + \alpha_1(y_t - \bar{y}_t) + u_t$ $\alpha_1 > 0$

(8) $R_t = \mu_0 + \Delta p_t + \mu_1(\Delta p_t - \pi^*)$
     $+ \mu_2(y_t - \bar{y}_{t-1}) + e_t$ $\mu_1 > 0, \mu_2 > 0$

(9) $m_t - p_t = c_0 + c_1 y_t + c_2 R_t + \eta_t$ $c_1 > 0, c_2 < 0$

(10) $\bar{y}_t = \gamma_0 + \gamma_1 \bar{y}_{t-1} + a_t$ $\gamma_1 > 0$

Here, there are three major changes from the model of 1979. First, the term $E_t y_{t+1}$ enters the counterpart of the IS function (1), reflecting that equation's origin as a consumption Euler equation, with consumption substituted out in favor of output, to represent optimizing behavior by rational optimizing households.[2] Second, the usual Phillips or price adjustment relation (7) differs from (2) by having $\beta E_t \Delta p_{t+1}$ instead of $E_{t-1} \Delta p_t$ as the reference expected inflation rate. Again, this specification is more readily justified by optimizing analysis, due in this version to Calvo (1983) and a host of follow-up papers, including King and Wolman (1996). Finally, the most striking change is in the monetary policy rule (8), which is here expressed in terms of the one-period nomi-

nal interest rate, used as an instrument variable, instead of the growth rate of the (base) money supply. This change in the usual modeling practice, which was given an important impetus by Taylor (1993), undoubtedly represents a move in the direction of realism since actual central banks of industrial countries almost invariably use some short-term nominal interest rate as their "operating target." Whether that mode of policy behavior is socially desirable is not an entirely settled matter, although the preponderance of opinion has certainly moved in that direction, partly under the forceful influence of Woodford (1999, 2003).

Of course, today's models often do not include any money demand relation such as (9). Given the absence of monetary aggregate variables from (6) and (7), this omission becomes formally innocuous when policy is conducted as in (8), as has been explained numerous times (e.g., McCallum, 1999). Today's models do not imply that no such money demand relation obtains, of course, but merely that their specification does not influence the dynamic behavior of the main macro variables given the remainder of the (recursive) system.

There are two other ways, besides this change in the monetary policy instrument, in which today's policy analysis usually differs from that of 1979. The first has already been mentioned; it is that today the standard mode of policy analysis involves a comparison of the behavior of target variables (e.g., inflation and the output gap) under different maintained policy rules, rather than point-in-time exercises.[3] The other is that today's models are constructed in a manner that attempts to respect both theory and evidence, by using optimization-based general equilibrium analysis in an attempt to develop systems that are potentially structural—and thus immune to the Lucas critique (Lucas, 1976)—and by specifying the models quantitatively, either as a result of econometric estimation or by selection of their parameter values on the basis of a careful calibration (of the type emphasized in the real business cycle literature).

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[2] This simplest version of the model does not include endogenous investment spending, as distinct from consumption. Endogenous investment can be included fairly readily, but some users instead calibrate the sensitivity of spending to the real interest rate so as to match the consumption-plus-investment value, rather than the one appropriate to consumption alone.

[3] I would definitely include the design of optimal policy rules under the former heading, despite various reservations mentioned in McCallum and Nelson (2004).

PROMINENT TOPICS

A second way to approach the question "What have we learned?" would be to consider specific topics that have been prominent—of major professional interest—among monetary economists since October 1979. A list of such topics that I have put together fairly quickly includes those given below. The ordering is roughly, but not strictly, chronological.

    i. Operating procedures
   ii. Sacrifice ratios
  iii. Credibility
  iv. Commitment versus discretionary policy optimization
   v. Central bank independence
  vi. Vector autoregression (VAR) models
 vii. Real business cycle models
viii. New Keynesian models
  ix. Structural VAR models
   x. New neoclassical synthesis models
  xi. Transparency and communication
 xii. Interest rate smoothing
xiii. Taylor rules
xiv. Inflation targeting
 xv. Analysis with real-time data
xvi. The zero lower bound on nominal interest rates
xvii. Optimality from a "timeless perspective"
xviii. Targeting versus instrument rules
xix. Indeterminacy, learnability, and E-stability

Most of these topics are of considerable intellectual content and interest; indeed, I have been interested in a majority of them myself. But, in trying to answer "What have we learned?" it would seem best to strive for a shorter and more practically oriented list, in part because merely to specify the meaning of each of the terms and provide a citation of the key references would require several pages. In the next and final section, accordingly, I will try to produce one.

WHAT HAVE WE LEARNED? A SHORT LIST

First, we have learned to conduct monetary policy analysis in a manner that seems reasonable to both academic and central bank economists. This is important because it facilitates communication between these two groups of analysts. I have argued (McCallum, 1999) that this convergence of viewpoints has proceeded to the point where one usually cannot tell from examination of a particular research paper whether it was written by an academic or a central bank economist. For this healthy development I would give much credit to the simple but insightful exposition of Taylor (1993). It is, of course, possible to worry about how much of today's highly technical research actually influences policymakers, such as members of the FOMC. But there are positive indications, both at the Board of Governors and at regional Federal Reserve Banks. Not only in the Fed, but also in the central banks of other countries (e.g., the Bank of England, the European Central Bank, the Bank of Japan), it has become fairly common for the top monetary policymaking committee to include research economists among its voting members. (Indeed, several are present at this conference!)

Second, we have learned that the crucial requirement for a central bank is to give top priority to the task of keeping inflation low. At least this is the message that I perceive from all the attention that has been paid to "inflation targeting." Terminologically, there is a bit of a problem with respect to the formal literature on that subject, for it is unclear why an optimizing central bank with an objective function of the form

(11) maximize

$$E_0 \sum_{t=0}^{\infty} \beta^t \left[(\pi_t - \pi^\star)^2 + \lambda(y_t - \bar{y}_t)^2 \right] \qquad \lambda \geq 0$$

should be called an inflation targeter rather than an "output gap targeter," especially if λ is relatively large. But in practice, each recognized inflation-targeting central bank has emphasized achievement of a low inflation rate as its top priority. So I think that it can be said that there is much agreement on what I regard as the crucial requirement.

With respect to the objective function (11), several researchers (e.g., Orphanides, 2001, 2003; McCallum, 2001) have argued that it would be dangerous for the central bank to respond strongly to an operational measure of the output gap, in part because of the great difficulty that prevails in practice in obtaining satisfactory estimates of the natural-rate value, $\overline{y}_t$, or even in agreeing on the proper concept to utilize for the latter. A strong response to the level of the gap is not necessarily the same as adopting a large value of λ in (11), it should be noted. It is the same under discretionary optimization, but with the "timeless perspective" approach the implied optimality condition involves the *change* in the output gap, in which case the undesirable effects of natural-rate mismeasurement tend to cancel out to a substantial extent (Orphanides, 2003).[4]

Finally, I think that we have seen that it is possible for central banks to avoid the inflation bias that results from period-by-period discretionary re-optimization when the target level of output exceeds the natural-rate value. I hope that this is because central banks are now avoiding discretionary period-by-period re-optimization, choosing instead to make policy in a committed, rule-like fashion. Some form of timeless perspective behavior, that does not try to exploit conditions that happen to prevail currently, is necessary to avoid several types of suboptimality, including the one mentioned above. But it remains somewhat unclear what the actual current situation is, in terms of central bank behavior.

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[4] For a discussion of the timeless-perspective approach, see Woodford (2003, Chap. 7).

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