Editor's Introduction

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IN HONOR OF DARRYL FRANCIS

Jerry Jordan and Allan Meltzer honor Darryl Francis by chronicling his tenure as president of the Federal Reserve Bank of St. Louis, especially his role as a member of the Federal Open Market Committee (FOMC). Both articles are valuable, not only as assessments of past policy errors and what might have occurred had the FOMC chosen the path that Darryl outlined, but also as a warning for current policy.

Jerry Jordan recalls that Darryl Francis was frank, clear minded, and resolute. He referred to Francis as a maverick—"an independent individual who refuses to conform with his group." Jordan argues that Francis played a key role in the evolution of monetary policymaking within the Federal Reserve. In particular, Jordan notes that Francis was instrumental in the FOMC's decision to (i) adopt a more strategic operating directive and (ii) include monetary aggregates in its operating procedure. He notes, however, that the strategic objectives—reducing inflation and encouraging sustainable economic growth and balance of payments equilibrium—sounded a lot like "motherhood and apple pie," and the FOMC's apparent greater reliance on monetary aggregates seems now to have been just a facade. Despite Francis's courage and leadership, it was not until 1979, three years after Darryl left the Federal Reserve Bank of St. Louis, that the FOMC finally was "persuaded that inflation was a monetary phenomenon," correctable only by slow money growth.

Jordan suggests that there are three parallels between Darryl Francis's FOMC experience and the FOMC experience today. First is the importance of "maintaining an overriding strategic objective for price stability." Jordan notes that, in spite of the fact that the FOMC has made it clear that "the ultimate objective of monetary policy is to maximize long-run growth by preserving a stable purchasing power of money," there is a common perception that there is a trade-off between growth and inflation.

The second parallel is that "raising a nominal overnight interbank rate does not necessarily ensure a restrictive monetary policy." The high nominal rates in the 1960s and 1970s did not indicate a restrictive policy because, as Jordan notes, "the inflation premium in interest rates was rising faster than the Committee was raising the overnight policy rate." Jordan then suggests that, "in the 1999-2000 environment, raising the overnight policy rate did not indicate that the stance of policy had become more restrictive" because the return to capital was rising faster than the policy rate. Real interest rates, Jordan notes, are often the manifestation of economic forces that are independent of Fed policy. If market forces move real interest rates, and consequently nominal interest rates, faster than policymakers move the target, Jordan notes that the result will be faster money growth and quite likely higher inflation.

The third parallel is the vital role that "the maverick, the dissenter, the sometimes lonely voice in the crowd" plays in the continuing evolution of policythinking and policymaking. And so Jordan reminds us of Darryl Francis's exceptional courage and foresight as he filled this role during a very important and challenging period of Federal Reserve history.

Allan Meltzer also praises Darryl Francis's role in the evolution of monetary policymaking in the late 1960s and early 1970s. During the 1960s and 1970s, as Meltzer notes, Keynesian economics was the reigning orthodoxy. Fiscal policy was the principal tool of economic stabilization. Monetary policy and, more importantly, money growth were ineffective in reducing inflation—a policy choice between full employment and price stability along a stable Phillips curve.

Meltzer recalls that a furious debate arose over Friedman and Meiselman's (1963) finding that the money multiplier was more stable that the Keynesian investment multiplier and Andersen and Jordan's (1968) demonstration that changes in the money stock had larger and more predictable effects on nominal gross national product than did changes in government expenditures. He notes that "at the height of the controversy in the late 1960s and early 1970s, Darryl Francis was the principal, and usually only, spokesman who challenged this orthodoxy at meetings of the Federal Open Market Committee."

Noting the tendency to associate high and rising interest rates with restrictive monetary policy, Meltzer notes that Darryl Francis stood alone in recommending slower money growth to reduce

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inflation in the late 1960s. Believing that a more restrictive fiscal policy, not a slower rate of money growth, would reduce inflation, Francis's counterparts on the FOMC favored what Meltzer terms "a fiscal solution to a monetary problem." Specifically, they recommended a policy of reducing interest rates to offset the effect of the then proposed temporary 10 percent surtax on incomes.

Noting that history suggests the neglect of money was a "major mistake," Meltzer uses the FOMC meeting transcripts at the time to offer this prediction: "If Darryl's advice had been taken, we would have avoided the great inflation." According to Meltzer, the failure to heed Darryl's advice had wide-ranging implications. He argues that,

> If the FOMC had acted on Francis's recommendations, the U.S. and much of the rest of the world would have avoided the worst of the Great Inflation. The Bretton Woods System may not have survived, but it would have lasted longer. The relative price of oil would not have fallen precipitately after the U.S. devaluation, so the oil shock might have been avoided, with positive consequences for productivity growth in the developed countries. With greater certainty, inflation, disinflation, and regulation would not have destroyed the thrift industry. Perhaps, also, there would have been fewer petrodollars to recycle, less bank lending to Latin America, and no debt crisis in the 1980s.

The important lesson to be learned from Darryl Francis and the period of monetary history over which he served is that it is a mistake to ignore money growth when conducting monetary policy. Meltzer agues that this period and others serve as reminders that "it is impossible to have high and sustained inflation without monetary accommodation."

He concludes that economic theory and experience have shown that there have been times when interest rates mislead policymakers when money growth did not: "Darryl Francis's tenure was one of those times. Fortunately, he recognized the error. Unfortunately for us, and much of the rest of the world, his colleagues did not."

MONETARY POLICY IN THEORY AND PRACTICE

The conference papers and discussants' comments cover a wide range of issues that are important

for a better understanding of monetary policy and its effectiveness. Questions addressed include the following: How does the Fed move the federal funds rate? Is the effect of monetary policy completely reflected in the behavior of the overnight federal funds rate, or does money have separate effects on economic activity and inflation? Is systematic monetary policy effective, or must the monetary authority surprise the market to have an effect? Could the practice of monetary policy be improved by having the central bank commit to a time path for the interbank rate?

Each paper or discussant comment is interesting in its own right. Nevertheless, I found reading them together a rewarding experience. Despite their very different themes, the papers overlap and interact in a variety of interesting and, occasionally, unexpected ways. I highlight some of the most important discussions and findings here. I encourage the reader to read all of the papers. My short summary does none of them justice.

The Central Bank's Influence on Interest Rates

The first two conference papers deal with the issue of how the Fed influences the federal funds rate. According to convention, the Fed controls the funds rate through open market operations. Specifically, the Trading Desk of the Federal Reserve Bank of New York (Desk) sells securities when the Fed desires to raise the funds rate and purchases securities to reduce the rate. In recent years, however, the Desk acknowledges doing nothing different when the FOMC changes the funds rate target.

John Taylor begins his analysis with a detailed discussion of the federal funds market and two important changes that affect the demand for reserves—the virtual elimination of reserve requirements as a consequence of banks "sweeping" reservable deposits and the 1998 move to lagged reserve accounting. In addition, Taylor notes that, since 1994, target changes have been announced immediately upon making the decision, but after the Fed has carried out that day's open market operations. Consequently, since 1994 it has been impossible for the Fed to implement open market operations to change the funds rate until the day following the announcement.

After discussing these changes, Taylor presents a simple model of the reserve market to illustrate how the funds rate can move when the FOMC

announces a target change without the Desk carrying out open market operations. Given the declining importance of reserve requirements as a reason for holding deposit balances at the Fed, Taylor's specification for the demand for balances at the Fed is motivated by Furfine's (2000) model of a typical optimizing bank. According to Furfine's model, today's demand for deposit balances at the Fed depends, in part, on the bank's expectation for the funds rate on the next day. The arbitrage need not be complete. Consequently, the model is consistent with the finding of Hamilton (1996) and others—namely, incomplete intra-maintenance-period substitutability of reserves.

Taylor hypothesizes that the demand for deposit balances at the Fed is determined by the weighted spread between the actual funds rate at day t and the market's expectation for the fund rate at day $t + 1$. Because of this, the demand for Fed balances can change in advance of the change in the target rate or in advance of Desk actions to change the target, i.e., open market operations.

If expectations are formed rationally, however, they must be linked to market fundamentals. While traditional open market operations are not necessary to move the funds rate following an announced target change, they are the "fundamentals" that underlie Taylor's announcement effect. Specifically, Taylor hypothesizes that the supply of deposit balances at the Fed is an increasing function of the spread between the funds rate and the funds rate target on the previous day. When the funds rate is above the previous day's target, the Desk injects reserves to move the rate close to the target. When the funds rate is below the target, the Desk drains reserves. The implication of this reserve supply mechanism is that the Desk gradually adjusts the level of reserves to the level needed to maintain the funds rate target.

Using data for the period since the return to lagged reserve accounting (July 1998) Taylor parameterizes his model to show how the funds rate might move substantially on the day of an announced change in the funds rate target without the Fed engaging in open market operations. Taylor compares the response from his theoretical model to the nine target changes between July 1998 and May 2000, and notes that movements in the funds rate at the times of target changes appear to be getting closer to the theoretical response. He notes, however, that there are big differences in the exact timing of funds rate changes, with some changes occurring

in advance of the announcement and others occurring with a delay.

Taylor also notes that the theoretical response of the funds rate to a simulated shock to the demand for balances at the Fed exhibits persistence that is very similar to that of actual deviations of the funds rate from the funds rate target.

He argues that his model captures the essential features of *open mouth* operations. Specifically, he suggests that the recent decline in the variability of the funds rate from the target, which he documents, may be a consequence of funds rate traders placing increased credibility on the Fed's reaction function and the greater clarity about the target itself.

Orphanides begins his discussion of Taylor's work with an expanded discussion of several recent institutional changes in the structure of the reserve market that he believes are important for modeling the demand for deposit balances at the Fed. After discussing two motives that link today's demand to expectations of tomorrow's federal funds rate—one explicitly captured in Taylor's analysis and the other, discount window borrowing, explicitly ignored—Orphanides argues that these motives combine to yield Taylor's demand equation under some "trivial" restrictions. Orphanides argues that Taylor's specification captures the essential features of open mouth operations, while abstracting from important but not essential elements of the reserve market.

Orphanides is less enthusiastic about Taylor's specification of reserve supply. While not taking exception to Taylor's primary motivation, i.e., that open mouth operations require a credible commitment by the Fed, Orphanides argues that Taylor's reserve supply equation is a "rather poor description of what the Desk actually does." Specifically, he argues that, rather than simply responding to yesterday's misses, the Desk aims each day to keep the funds rate close to the funds rate target. This implies that the Desk is forward looking and attempts to offset, to the degree possible, known or expected shocks to reserves. Noting evidence that the funds rate tracks the funds rate target very closely during periods when changes in the target were widely and correctly anticipated, Orphanides argues that such success would be unlikely if the Desk followed a mechanical rule of partially offsetting the previous day's misses.

Thornton takes up a similar theme in the second conference paper. Thornton's analysis focuses on the period before 1994, when target changes were not announced, thus making it much less likely

that the Fed was controlling the funds rate with open mouth operations.

Thornton's work is motivated by the fact that there is little evidence of a liquidity effect using relatively low-frequency data, and by Hamilton's (1997) discovery of a liquidity effect using daily data. Hamilton finds an economically important and statistically significant liquidity effect only on the last day of the reserve maintenance period (settlement Wednesday).

Deriving a detailed structural model of the reserve market that reflects the Desk's current operating procedure, Thornton shows that the liquidity effect, as traditionally defined, cannot be estimated on settlement Wednesday because of the two-day lag in the Fed's then "contemporaneous" reserves accounting procedure. A further analysis reveals that, even if this were not the case, it is unlikely that the liquidity effect can be estimated by estimating the shock to one of the components of reserve supply.

Using a larger sample (unavailable to Hamilton) and Hamilton's procedure, Thornton finds that there is no evidence of a settlement-Wednesday liquidity effect for periods before and after Hamilton's. Moreover, Thornton finds that the results for Hamilton's sample period are due to relatively few observations when there were simultaneously (i) relatively large changes in the funds rate and (ii) relatively large estimated shocks to the Treasury's balance. In addition, Thornton cites other work indicating that Hamilton's forecasting technique does not use all of the information available to the Desk at the time.

Thornton then proposes estimating the liquidity effect indirectly by estimating the response of reserves to known changes in the funds rate target. Thornton suggests that, if the Fed moves the funds rate through open market operations, there should be a systematic relationship between changes in the funds rate target and changes in reserves after controlling for other factors that the Desk's operating procedure takes account of.

Thornton includes variables that appear in the reduced form of his structural model of the reserve market: specifically, the Board of Governors' estimates of the demand for required reserves and excess reserves, the discount rate, the funds rate target, and the average error in forecasting the Treasury's balance. Because the Desk does not slavishly follow its own operating procedure, Thornton augments the model with other variables that are

likely to reflect how the Desk conducts its daily operations. Specifically, he includes the previous day's difference between the funds rate and the funds rate target, except on days when the target is changed. This variable may reflect slow adjustment of reserves to the level consistent with the target, as Taylor hypothesizes, or it may arise because the Desk uses the previous day's rate spread to supplement its imperfect estimates of the supply and demand conditions in the reserve market. Following up on some previous analysis (Thornton, 2001) showing that the Desk attempts to offset the effect of borrowing on the funds rate, Thornton also includes the difference between the previous day's borrowing and the borrowing assumption.

Using observations over the period February 1987 through December 1996, Thornton finds a negative relationship between changes in the funds rate target and nonborrowed reserves before and after February 1994, but the relationship is only statistically significant before 1994. This finding is generally consistent with the view that the Fed used open market operations before it announced target changes and open mouth operations after. The response of nonborrowed reserves to changes in the funds rate is very small relative to the day-to-day variation in nonborrowed reserves, suggesting that it is unlikely that the Desk consistently moved the funds rate through open market operations.

Gilchrist begins his analysis of Thornton's work with a brief review of the theoretical and empirical literature on the liquidity effect. Using a simplified version of Thornton's model, Gilchrist generally accepts Thornton's analysis of Hamilton's results, suggesting that "one lesson to be drawn here is that any estimates of 'structural parameters' based on the daily federal funds market must carefully consider the institutional detail of the market and how it has changed over time."

With respect to Thornton's alternative approach to estimating the daily liquidity effect indirectly, Gilchrist is much less sympathetic. He indicates there is reason to suspect that Thornton's estimates of the relationship between nonborrowed reserves and changes in the funds rate target may be biased and econometrically fragile. Specifically, he shows that Thornton's estimates will be unbiased only if the Desk attempts to adjust reserves daily to the desired level. He then notes that, if the Desk follows a partial adjustment process along the lines of the reserve supply function suggested by Taylor, the estimates of the response of nonborrowed reserves

to changes in the funds rate target can be biased from the true value.

Gilchrist also notes that Thornton's instantaneous adjustment assumption implies that there is no persistence in deviations of the funds rate from the funds rate target. He argues that this is clearly at odds with the data, noting that there is considerable evidence of serial correlation in deviations of the funds rate from the funds rate target.

Identifying Exogenous Monetary Policy Actions

In the first paper in this section, Eric Leeper and Tao Zha try to come to a deeper understanding of the effects of monetary policy by comparing New Keynesian (NK) macroeconomic models that are "easy to understand, but whose fit to data is crude" and "structural" vector autoregressions (VARs) "whose fit and forecast performance are good, but with economic behavior that is not very detailed."

There are two motivations for their analysis. Their first argument is that the "linchpin" of the NK analysis stems from the observation that the performance of monetary policy has improved markedly in the 1980s and 1990s compared with the 1960s and 1970s. This observation contrasts with evidence from VARs, suggesting little instability in the policy parameters or in the dynamic impacts of exogenous shifts in policy.

The second is the fact that in NK models money is a "sideshow." Money plays no role in either the formulation or transmission of monetary policy. Hence, at one level, Leeper and Zha can be seen as attempting to restore money to discussions of monetary policy. They note that, even if the Fed ignores money now, this was not always the case. Hence, their desire to reintroduce money in discussions of monetary policy stems, in part, from a desire to "get it right" historically. They argue that historical interpretations of monetary policy that ignore money run the risk of not getting it right.

One suspects that the motive is deeper, however. Indeed, at one point they note that "interest rates need not be the only channel through which monetary policy affects economic activity." At a bare minimum, they suggest that money should be included as an information variable for setting the target level for the federal funds rate.

Arguing that NK models are simply restricted VARs, Leeper and Zha investigate the NK model by analyzing the impulse response functions from the reduced-form equations obtained by imposing various identifying restrictions on a stylized NK model. They argue that similar models have been used in several important analyses of monetary policy. Noting that the reduced-form parameters are combinations of deeper parameters of private agents and policymakers, they argue that their procedure is appropriate, as they do not change policy parameters while holding fixed the reduced form parameters in non-policy equations.

The three equations of the reduced form are the IS curve, the aggregate supply (Phillips curve) equation, and the monetary policy equation (a form of the Taylor rule). Leeper and Zha note that, if the parameters of the IS curve are unrestricted, it is impossible to identify the IS curve from the Taylor rule. Estimating the model over the periods 1959:Q1–2000:Q2 and 1959:Q1–1979:Q3, they find that the estimated reduced-form equations vary considerably over the two samples. They also find that the effects of monetary policy on the real economy are relatively large, while the effects on inflation are "minimal."

Like a number of analysts, they find that the model is unstable after 1979 in that the eigenvalues of the difference equations are larger than unity. They note that the instability is not due to the fact that the coefficient on the inflation term in the "Taylor rule" is less than 1, as some contend. Rather, they find that it is due to the imposed restriction of a unit coefficient on lag inflation in the aggregate supply equation. When this coefficient is estimated to be less than unity, the model is stable in spite of the fact that the response of the funds rate to inflation is less than unity.

Leeper and Zha investigate the role of money by incorporating money (M2) in the NK model in a couple of ways and by including money in their structural VAR. They find that including money substantially alters the conclusions about monetary policy in the NK model. Hence, there is a conflict between the theoretical assumption that money is irrelevant and the empirical result that including it makes a difference. The also find that including money in the VAR eliminates the price puzzle (a monetary contraction raising the inflation rate), helps stabilize the model across time, and helps distinguish aggregate supply shocks from non-monetary policy aggregate demand shocks.

In his discussion of their work, Ken West applauds Leeper and Zha's goal of comparing NK models with structural VARs, noting that a "system-

atic comparison about what each tells us about monetary policy is long overdue." He notes they make a useful contribution to the literature by showing that single-equation and system estimates sometimes differ a great deal (especially true of estimates of the coefficient on inflation in the Taylor rule) and that the stability of the system does not necessarily hinge on whether the estimated coefficient on inflation in the Taylor rule is greater than 1. West also compliments Leeper and Zha (i) for pointing out that defining stability by a single coefficient in the monetary policy reaction function can be misleading and (ii) for reminding us that monetary models without money, by their nature, make assumptions about money.

Nevertheless, West finds the empirical results, particularly the poor and often implausible fit of the NK models and the good and frequently more plausible fit of the VARs, "unpersuasive." West suggests that the NK model analyzed by Leeper and Zha is not representative of many recent NK models found in the literature. He notes that Taylor's (1999) model, which is similar to that used by Leeper and Zha, was intended to be expository. He notes that there are several larger models that allow for richer dynamics (through lagged endogenous variables), serially correlated errors, or both, suggesting that a comparison of VARs with these models would likely yield different results.

Kevin Hoover and Oscar Jordá begin the second paper in this section with a careful and thoughtful discussion of important developments in monetary policy analysis over the last 30 years. While the principal objective of their paper is to refocus the profession on the possibility that there are real effects associated with systematic monetary policy, they comment directly on several related issues. For example, they call the quest to avoid the Lucas critique, by estimating "deep" structural parameters, "quixotic." They argue that this approach frequently employs identifying assumptions that are "just as incredible as any palmed off by 'structural' econometric modelers even before the dawn of new classical macroeconomics."

Their main purpose is to find some middle ground between the policy-ineffectiveness proposition—the notion that only monetary policy surprises matter—and the pre-Lucas critique's "failure" to distinguish between anticipated and unanticipated monetary policy. They do this by making operational Cochrane's (1998) "hybrid" model. In this model, a vector of economic variables is assumed to be represented by a complex linear combination of anticipated and unanticipated monetary policy variables, where λ is the so-called "mixing parameter." If $\lambda = 0$, only unanticipated policy actions matter. On the other hand, if $\lambda = 1$ there is no distinction between anticipated and unanticipated monetary policy actions.

Cochrane gave no interpretation of λ. He was merely suggesting a procedure for investigating the sensitivity of VAR-based analyses of monetary policy to the assumption that only unanticipated monetary policy matters. Noting that there are two ways to rationalize the effectiveness of systematic monetary policy (slow adjustment of prices and nonrational expectations), Hoover and Jordá choose the latter interpretation for λ. Specifically, they assume that $(1-\lambda)$ is the proportion of agents in the economy who form their expectations rationally and λ is the proportion of agents who follow a "rule of thumb."

The coefficient λ cannot be estimated directly because the number of structural coefficients plus λ exceeds the number of estimated moving-average coefficients by 1. Hoover and Jordá note, however, that if there were shifts in the monetary policy regime that did not affect λ, it would be possible to estimate λ and the structural parameters from estimates of the moving-average parameters obtained by estimating the VAR over periods of different monetary policy regimes. Treating estimates of the moving-average parameters over different policy regimes as datum, they propose estimating λ and the structural parameters using conventional methods.

They implement their procedure by estimating a modified version of a VAR used by Christiano, Eichenbaum, and Evans (1996). They identify "regime shifts" by testing for a maximum of eight structural breaks in the funds rate equation of the VAR over the period 1960:01–1999:01. An analysis of their test results suggests five breaks in the monetary policy regime: 1970:06, the elimination of Regulation Q interest rate ceilings on large CDs; 1974:06, the introduction of new reserve requirements and money growth targets; 1978:06, the beginning of nonborrowed reserves targeting; 1982:04, the end of nonborrowed reserves targeting; and 1986:02, the end of Volcker's chairmanship.

Their estimate of λ, 0.57, suggests that more than half of the agents follow the rule of thumb. Like Cochrane, they find that the response of the economy to a monetary policy acts more like an economy with no rational agents. Hoover and Jordá

analyze the response of the economy to a shock to the federal funds rate due to the systematic component of monetary policy and to a real shock due the systematic component of monetary policy over the six regimes that they identified. They find that the responses to both shocks vary considerably over the different regimes, suggesting that identifying alternative monetary policy regimes is very important.

In her discussion, Valerie Ramey praises Hoover and Jordá for their clever approach to tackling "a very difficult task." She begins her analysis by stating four reasons why she believes that Hoover and Jordá and others have taken up the task of analyzing systematic policy: (i) the empirical reality that shocks to monetary policy explain relatively little of the variation in output coupled with a strong prior belief that money matters a lot; (ii) the potential of systematic monetary policy as a propagation mechanism; (iii) the rise in interest in policy rules, such as the Taylor rule; and (iv) the striking similarity of the response of money and output to a monetary shock.

Most of Ramey's comment, however, is directed toward the assumptions made to estimate the model. Her principal concern is with the assumption that agents are either rational or not. She suggests that more rigorous foundations for λ might be obtained by appealing to the concept of bounded rationality. She notes, however, that, if this were the case, one would expect λ to change with a shift in the monetary policy regime. She suggests that this should be particularly true immediately following the regime shift, when it is difficult to form rational expectations of the new regime.

Ramey then discusses a number of econometric issues. In particular, she questions the source of the error terms in the estimated equations and notes that the source of the errors will have implications for their estimation technique. She also notes their failure to test the over-identifying restrictions.

Implementing Monetary Policy

In the fifth conference paper, Bennett McCallum addresses the important issue of whether the now-common practice of analyzing monetary policy and the macroeconomy in models that neither include money nor make reference to it is misguided. McCallum concludes that excluding money is theoretically incorrect, but that the consequences from doing so are probably small in practice. First, McCallum assumes that the demand for real money balances is determined by the level of the nominal

interest rate and the level of output and that the nominal money stock is completely determined by the central bank. He then notes that, if the central bank sets the nominal interest rate in accordance with a Taylor-type rule, monetary policy and the macroeconomy can be analyzed without concern for, reference to, or measurement of money. This does not mean, McCallum cautions, that money has no consequence: the "central bank's control over the one-period nominal interest rate ultimately stems from its ability to control the quantity of base money in existence." With this caveat, the quantity of money is a sideshow. With reference to the price level, the central bank simply sets the nominal interest rate at the level that is consistent with its inflation objective and supplies the nominal money stock that is needed to achieve that interest rate.

McCallum also responds to the criticism that in the NK model inflation is a non-monetary phenomenon, governed by the Phillips curve. Noting that, if the central banker sets the constant in the Taylor rule equal to the long-run equilibrium real interest rate, "as a sensible central banker would," the average rate of inflation will equal the target rate which is independent of the parameters in the Phillips curve. Hence, the Phillips curve parameters play no role in determining the long-run inflation rate. They are essential for the adjustment of inflation to that rate, however.

McCallum notes that his argument that the NK model does not ignore money rests on the assumption that money affects the economy only through its effect on the interest rate. He then reconsiders the theoretical foundations of the NK model, allowing for the possibility that money affects output directly by reducing transactions costs, thereby freeing resources for production. McCallum finds that the usual dynamic IS function results only under the "implausible" assumption that marginal reduction in transaction costs associated with holding money is independent of the level of consumption. Otherwise, the level of output is affected by money independent of the nominal interest rate.

McCallum calibrates his model to evaluate the importance of omitting money from the standard NK model using impulse response functions. He finds that, while omitting money from the model is not justified theoretically, as a practical matter it makes little difference under plausible calibrations of his model. He notes that the findings of his exercise are consistent with recent findings of Ireland (2000), whose econometric estimates suggest that

the effects of omitting money are insignificantly different from zero.

In the remainder of the paper, McCallum takes up the issue of indeterminacy in the presence of Taylor-style interest-rate policy rules. He notes that the "indeterminacy" in the recent literature represents solution multiplicity rather than the nominal indeterminacy studied by Patinkin and others much earlier. McCallum argues that the possibility of multiple equilibria in these situations likely represents a theoretical curiosity rather than a problem of real-world significance. McCallum mentions other reasons, but emphasizes the concept of E-stability, which is usually equivalent to learnability by a real-time, least-squares learning procedure. The main result is that, when there are multiple stable solutions because the policy rule responds to expected future inflation rates, the unique solution is E-stable and learnable, whereas the other (bubble) solutions are not. McCallum's argument draws on results of Bullard and Mitra (2000) and Evans and Honkapohja (2001).

In addition, McCallum takes up the status of the "Taylor principle," i.e., that the policy rule should adjust the nominal interest rate more than point for point with changes in observed or expected inflation. Failure of the Taylor principle to hold implies the existence of multiple stable solutions. McCallum argues, however, that in this case neither fundamental nor bubble solutions are E-stable or learnable. Consequently, he concludes that the observance of the Taylor principle is of real-world importance for policy rule design.

John Leahy reviews the intertemporal optimization problem that gives rise to the dynamic IS equation in McCallum's NK model, noting that there are a number of hypotheses that could give money an effect independent of the interest rate by either changing the utility of consumption or altering the relationship between consumption and output. Leahy's non-exhaustive list includes non-separable utility, transactions costs, liquidity constraints, a cash-in-advance constraint, segmentation of the goods and asset markets, and the credit channel. Noting that McCallum considers one of these (i.e., transactions costs), Leahy argues that the transactions cost effect of money might not be independent of economic circumstances. Specifically, he conjectures that the effect might be greater during periods when "the payments mechanism is under stress, such as during high inflations."

With respect to the issue of indeterminacy,

Leahy suggests that, while mathematically precise, McCallum's analysis "is not very enlightening." Specifically, he notes that McCallum's analysis does not explain why indeterminacy arises or why agents might be led to follow these sunspot or bubble solutions. Rewriting McCallum's model in terms of behavior rules that agents might follow, Leahy considers alternative behaviors that can give rise to such outcomes. He argues that from this perspective "it is hard to imagine the conditions under which these solutions would arise." He further notes that such situations can be avoided easily by being credible or by setting interest rates sufficiently in response to agents' expectations of inflation.

In the final conference paper, Charles Goodhart takes on the issue of strategic considerations in the implementation of an inflation target. Goodhart considers three issues that arise from the way that the Bank of England's Monetary Policy Committee (MPC) (of which Charles was a member) implements monetary policy. The first arises because the letter that is sent from the Chancellor of the Exchequer to the MPC sets no formal horizon for achieving the inflation objective. In practice, though, the MPC has chosen inflation and output forecasts over a two-year horizon, implying that this is the horizon over which the MPC expects to achieve its objective. The natural question that arises is, What is the optimal forecast horizon for monetary policy?

Goodhart does not answer this question, per se, but rather reviews research undertaken at the Bank of England on this issue. He presents evidence suggesting that the optimal forecast horizon depends on the model used, the source of the shocks, and the policymaker's loss function. These results imply that the policymaker cannot select an optimal forecast horizon without first specifying the model that they will use to implement policy.

The second issue arises because policymakers determine the target for the overnight interbank rate and, of necessity, their inflation forecasts are conditional on the assumed path for it. The MPC assumes that the rate will be constant over the next two years. Goodhart asks whether this approach is suboptimal and whether it undermines the MPC's credibility.

Goodhart cites five reasons why the optimal control approach, frequently used by academic economists to analyze policy issues, would be of no practical assistance in settling such issues. Consequently, he considers other ad hoc approaches, such as replacing the current practice with a forward-

looking Taylor rule. He argues that a simple rule would not overcome an important problem associated with the constant rate assumption; namely, the assumed path for the interbank rates would still normally deviate from the market's expectation. Moreover, he suggests that the rule-based time path would be no more credible in the face of shocks, such as the Russian default or the demise of the Long Term Capital Management hedge fund.

He suggests that the constant interest rate assumption is just that, an assumption. Hence, no one really expects it to materialize in practice. In contrast, a rule-based time path would create the expectation that policymakers were committed to it. The level of commitment would likely be interpreted differently by the public and likely even by different members of the monetary authority.

Goodhart is also concerned that the appearance that policymakers are committed to a specific time path for the interbank rate may constrain them from responding promptly to new information—especially if the information is to some degree at odds with the predictions upon which the path of rates is based. From this analysis, he conjectures that the current practice "tilts us in the direction of early, preemptive actions, and my personal viewpoint is that this is desirable."

In the end Goodhart concludes that, despite what some may consider MPC's shortcomings, the current practice "simplifies the decision-making process…provides the greatest flexibility…and encourages the MPC to act in a robust, preemptive fashion."

Goodhart then discusses his final issue: the extent to which policymakers should respond to asymmetric shocks. He begins this discussion by noting the distinction between variability and risk, suggesting that when MPC members consider risk they are "generally concerned with asymmetric possible outcomes." He then notes that asymmetry implies skew, which drives alternative measures of central tendency (the mean and the median) apart.

Goodhart then addresses the question, Should perceived risks affect current policy? He argues that the answer to this question depends on whether the relevant event is non-observable (or observable too long after the event for policymakers to respond effectively). He believes that policymakers need to respond to risks of events that are non-observable "because you will never find a time in the future when you can perceive with any greater clarity or certainty whether or not they have been occurring."

On the other hand, policymakers should not respond to the risk of future observable events that have low probability, such as sharp changes in certain asset prices, even if such a strategy might have a high payoff.

In his discussion of Goodhart's paper, Governor Meyer takes on one of the conclusions of Bank of England research that Goodhart tacitly endorses— namely, the conclusion that "any degree of output stabilization can be synthetically recreated by judicious choice of parameters entering an inflation-targeting formula," so that an explicit output term need not enter the rule. Meyer argues that, while this may be true on average, "it is certainly not true episode by episode, depending on the source of the shocks." He asks, Why not stabilize output explicitly? The answer he usually gets from policymakers at inflation-targeting central banks is "admitting that a central bank carries out its monetary policy actions in part to stabilize output would undermine the public's confidence in its commitment to price stability."

Meyer then argues that the MPC's practice of fixing its nominal interest rate path in its inflation forecast is unnecessary and suboptimal and suggests that it may undermine the credibility of the MPC's inflation forecasts. Meyer suggests that a simple, but "clearly improved, approach would be to set a constant real interest rate." He suggests that a better alternative still would be for the MPC to "set the entire path for the real interest rate as consistent with its forecast and policy objectives." He goes on to point out that, although it never assumes a change in the funds rate target as the outcome at the current meeting, the staff of the Board of Governors sometimes use a constant real rate assumption or alternative paths for the real rate in its forecasting exercises. He suggests that such exercises are often useful in seeing the extent to which a constant rate assumption is out of line with policymakers' projections.

While much progress has been made in conducting monetary policy since Darryl Francis' tenure on the FOMC, much more work needs to be done. Indeed, many basic questions remain: How does the Fed control the federal funds rate? Is money growth essential for inflation; if not, why not? What role, if any, should monetary aggregates play in the conduct of monetary policy? Should monetary policy be implemented with a "policy rule," and, if so, what should the rule look like? What is the best way to implement monetary policy to achieve the objective

of long-run price stability? As you can see from my brief summary, this volume makes a significant contribution to answering several of these questions. In so doing, it expands our understanding of monetary policy and its effectiveness. It is our hope that the analysis and discussion presented here will prompt further research on these and other important monetary policy issues.