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Monetary Policy in Theory and Practice

Proceedings of the Twenty-Fifth Annual Economic Policy Conference of the Federal Reserve Bank of St. Louis

Darryl Francis: Maverick in the Formulation of Monetary Policy
Jerry L. Jordan

Money and Monetary Policy: An Essay in Honor of Darryl Francis
Allan H. Meltzer

Expectations, Open Market Operations, and Changes in the Federal Funds Rate
John B. Taylor

Identifying the Liquidity Effect at the Daily Frequency
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Assessing Simple Policy Rules: A View from a Complete Macroeconomic Model
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Measuring Systematic Monetary Policy
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Monetary Policy Analysis in Models Without Money
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Monetary Transmission Lags and the Formulation of the Policy Decision on Interest Rates
Charles A.E. Goodhart

Commentaries

Athanasios Orphanides *Kenneth D. West* *John V. Leahy*
Simon Gilchrist *Valerie A. Ramey* *Laurence H. Meyer*

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Darryl R. Francis

Former President and CEO, Federal Reserve Bank of St. Louis

Darryl R. Francis took office as president and chief executive officer of the Federal Reserve Bank of St. Louis on January 17, 1966, a position he held until his retirement, March 1, 1976. His career in banking and economics spanned almost four decades.

Mr. Francis began his career in the Federal Reserve System at the St. Louis office in 1944 as an agricultural economist, a position he kept for five years. From 1949 until 1953, he was employed as a commercial banker in Memphis. He later served as a vice president at Boatmen's National Bank in St. Louis. In 1953, he rejoined the St. Louis Bank as a vice president, later serving as branch manager at the Memphis Branch for five years. In 1959, he was named first vice president in St. Louis. Then, in 1966, he became president of the Bank, a position he held for 10 years until his retirement.

Mr. Francis's major contribution to the field of economics was his strong advocacy of monetarist theory. During his career, he was one of the Fed's sharpest critics, and he often was at odds with the Fed's stance on monetary policy. He also spoke openly at Federal Open Market Committee meetings and frequently dissented from the majority view.

Because of Mr. Francis's leadership, the Eighth District earned a reputation as a "maverick" within the System. He expanded the Research Department's work on monetary economic policy and made sure all of their research was readily available to the public.

According to economist Murray Weidenbaum, during Mr. Francis's tenure at the Bank, the Eighth District's publications were seen as "the best material available on the nation's money supply." When testifying before Congressional committees, Weidenbaum frequently used District publications, as did many of his contemporaries.

Mr. Francis is a native of Ridgeway, Missouri. He attended the University of Missouri and received a bachelor's degree in agriculture in 1936. In 1975, he received the university's Distinguished Alumni award. Before his retirement from the Bank, he also was a member of the board of directors of Deaconess Hospital and the board of trustees for Washington University. After his retirement, he served as chairman of Merchants Bank in Fort Smith, Arkansas.

President's Message

It is a special privilege to introduce the Twenty-Fifth Annual Economic Policy Conference of the Federal Reserve Bank of St. Louis, held October 19-20, 2000. The privilege is special because we dedicated the conference to Darryl Francis, who is one of my personal heroes.

Darryl Francis was at the helm of the St. Louis Fed when policy went awry in the 1960s and 1970s. We owe him a debt of gratitude for having the vision to see that research and open debate would succeed in strengthening monetary policy. Francis had the courage to carry his ideas for improving policy—ideas emphasizing the importance of controlling inflation and of limiting money growth to achieve that objective—to the nation when they were not popular. There is no more fitting way to open this conference than for me to read a tribute to Darryl Francis written a few months before the conference by Milton Friedman.[1]

> When Darryl Francis became president of the Federal Reserve Bank of St. Louis in 1966, the conventional wisdom in the academy and in the Federal Reserve System was overwhelmingly Keynesian—fiscal policy, not monetary policy, was the major recourse for economic stabilization, and could be used to fine-tune the economy. The only role for monetary policy was to keep interest rates low to stimulate investment; inflation was largely a cost-push phenomenon. The 1966 *Annual Report of the Council of Economic Advisers* is an amusing and distressing example. A 31-page chapter on "Prospects for Cost-Price Stability" has only two passing references to monetary policy and does not even contain the word "money." Of the seven pages devoted to the "Outlook for Cost-Price Stability in 1966," six deal with guideposts! The careful reader of the 186-page report will have to wait until page 176, in an historical chapter on experience under the employment act, to find the first explicit recognition that there is any relation between monetary policy and inflation.
>
> Within the Federal Reserve System, monetary policy was being driven by multiple objectives, of which inflation was not necessarily the most important, and was being conducted largely in terms of the esoteric "tone and feel of the market," with minimal attention paid to the quantity of money.
>
> By 1976, when Darryl completed his decade as president of the bank, stagflation of the seventies was in full flood. It was being increasingly recognized, inside and outside the academy, that inflation was a monetary phenomenon, and that monetary policy, not fiscal policy, would have to be the first line of defense. In 1975, Congress passed Concurrent Resolution 133 which required the Fed to specify targets in terms of monetary aggregates.
>
> Darryl Francis played an important role in this revolutionary change in views. Already in November 1967, a year after Darryl took over as president, *Business Week* noted that "the Federal Reserve Bank of St. Louis has become a maverick, questioning both the current monetary policy of the System, and the basic premises on which that policy is based." It went on to say that "as early as last May, Darryl R. Francis, president of the St. Louis Bank, parted company with his fellows on the Fed's policy-setting Open Market Committee and urged a turn to tight money—a step the committee has yet to take."
>
> For the next decade, at meetings of the Open Market Committee and in more than 100 public speeches, Darryl pounded away at the defects of fine-tuning, the treacherousness of using money market conditions as the primary guide to monetary policy, and the importance of taking a long view, particularly with respect to inflation. These were not popular views and it took real integrity, conviction, and independence of character to press them as forcefully as he did. His words were backed up with data provided by his research staff. Their influence was spread by the flood of publications coming from the Bank. As I wrote in 1986 in an obituary tribute to Homer Jones, "Between them, Homer and Darryl Francis converted the St. Louis Bank into by far the most important unit in the System. For the

[1] Personal communication, 2000.

first time since the days of Walter Stewart, Winfield Riefler, and Randolph Burgess in the twenties, monetary research from within the System began to influence academic research and thinking. For the first time, a bank publication, the *Review* of the Federal Reserve Bank of St. Louis, began to be cited regularly in the academic journals." The pattern that was then established has continued. The Federal Reserve Bank of St. Louis continues to be a major contributor to the literature on monetary theory and policy.

Emphasis on monetary aggregates has not lasted. The Taylor rule, not monetary aggregates, is the talk of the central banker today. Yet the battle against fine-tuning and for steady growth in monetary aggregates has left its mark. Fine-tuning is today more a term of opprobrium than of praise. No one doubts that monetary policy, not fiscal policy, must be relied on to prevent inflation or that the prevention of inflation is the main task of central bankers. Moreover, however shielded monetary aggregates are from popular view, they are a major source of concern for central bankers, and I venture to predict that they are destined to resume in the not too distant future a more prominent role as a guide to monetary policy.

During Darryl's decade, I and my colleagues at Chicago were fighting the same battle. Darryl and St. Louis were strong, effective, and dependable allies. That is why it is a great pleasure and honor for me to pay tribute to the important role Darryl Francis played in the long-term fight against inflation. As his elder by 21 days, may I say, well done, young man.

William Poole
President and Chief Executive Officer
Federal Reserve Bank of St. Louis

Editor's Introduction

Daniel L. Thornton

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

Daniel Thornton is a vice president and economist at the Federal Reserve Bank of St. Louis.

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.

Darryl Francis: Maverick in the Formulation of Monetary Policy

Jerry L. Jordan

Darryl Francis retired from this Reserve Bank almost a quarter century ago, but his imprint on the culture and reputation of the Bank endures to the present time. Before I left the Bank, and for some time after, people would ask about him and I found that all immediately understood when I answered, "Darryl is the Harry Truman of the Federal Reserve."

I don't know what it is about someone with a rural background, and a disarmingly friendly nature, from the "Show Me State" that causes him to be so clear minded and resolute in his convictions. Both for himself and his staff, Darryl lived true to the expression, "First be sure you are right, then go ahead." This fostered the ideal atmosphere for a balanced emphasis on economic theory and empirical research.

Darryl Francis was already president of this Bank when I joined the research staff at the invitation of Homer Jones, its venerable research director. For any Reserve Bank economist captivated by monetary policy issues, the nature of the Bank's president is bound to be a crucial matter. In my case, Darryl Francis nurtured the ideal environment for someone in transition from academic studies to hands-on policy advising. In addition to monetary policy, Darryl was interested in the effectiveness and efficiency of all aspects of our central bank mission.

A couple of years after I came here, he sent me off into exile to run the data processing and other departments. Exile was not punishment, rest assured, but it was in the nature of missionary work on behalf of economists, among others. At that time computers scarcely had surfaced in System research departments. The Banks' computers were used for processing checks and accounting records, and their design would have to be rethought in the light of growing demands to run regressions, maintain data banks, and, potentially, operate the increasingly popular large models of the economy. These models, containing hundreds of equations, were making inroads with the Board of Governors' research staff, who were hoping to assist in formulating and implementing monetary policy by opening many of the black boxes that connect monetary and fiscal policy tools to the ultimate objectives.

As everyone knows, St. Louis did not follow the Board down the path of gargantuan models of the economy. Instead, we focused on direct empirical tests of rival conjectures about monetary and fiscal impulses, as exemplified by what became known as the St. Louis model, which is associated with an article that Leonall Andersen and I published in the Bank's *Review* in 1968.

Darryl Francis was indeed a monetary policy maverick. But let me first explain the sense in which I use the term "maverick," for the word has several meanings. The old Webster's unabridged dictionary in the Cleveland Reserve Bank library explains that the word can be traced to one Sam Maverick, a Texas rancher of the mid-nineteenth century who refused to brand his cattle. On that account, unbranded, stray cattle came to be called "mavericks" and were considered the property of the first person who branded them. Clearly, this is not my meaning. Darryl Francis never allowed himself to be appropriated and branded by any person or group.

The second dictionary definition of maverick is simply a person who has "escaped from the herd." This, too, is not my meaning. Darryl Francis did, in fact, escape from the Federal Reserve herd in 1949 to spend five years in private sector banking. But then he rejoined the Fed in 1953 and remained until he had spent 10 years leading the St. Louis herd as president of this Bank.

A third definition comes closer to my intended usage: "an independent," but a fourth hits the nail on the head. This defines a maverick as "an independent individual who refuses to conform with his group."

Now, some might say that Darryl Francis was a conformist, not a maverick. That is, he conformed to the image of St. Louis Reserve Bank presidents as mavericks, for he was not the first. Delos C. Johns, president of this Bank from 1951 to 1962, is said to have been chosen for the job in part because he could be counted on to nettle William McChesney Martin, Jr., Chairman of the Board of Governors and of the Federal Open Market Committee (FOMC)

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from 1951 until 1970. McChesney Martin himself, of course, started his career as an examiner here at the St. Louis Fed. Even earlier than that, according to Milton Friedman, Chairman Martin's father, William McChesney Martin, Sr., displayed a maverick's instinct in open market committee meetings while he was governor of this Bank from 1929 to 1941. But I digress.

Foremost among the groups with which Darryl Francis refused to conform, of course, was the FOMC. His record is clear. He participated in well over one hundred FOMC meetings and was a voting member of the Committee for three years in this Bank's rotation with the Atlanta and Dallas Reserve Banks. During those voting periods of 1967-68, 1970-71, and 1973-74, he dissented from the Committee's decision more than one-third of the time, at 13 meetings. More than that, he was alone is his dissents on 8 of those 13 occasions. This is a brave man.

Darryl became president of the Bank in January of 1966, but he didn't become a voting member of the Committee until March of 1967. By the time of his third vote, at the May meeting, he struck out on the path he was to follow for the remainder of his term as president. He dissented—alone among the 12 members of the Committee—in favor of what history surely must judge to have been a better direction than the other members chose. He dissented three more times in that first voting year, each time arguing that the Committee's anticipated paths of money and bank credit were more expansionary than would be consistent with an already expansionary fiscal policy and with the renewed economic expansion that could be expected that year and thereafter.

It would have been difficult to engage the Committee in a meaningful discussion of this alternative point of view because the members displayed no visible commitment to a common strategic target such as maintaining a stable purchasing power of the dollar. This "incoherence," as Bill Poole recently called it, shows through in the directives to the trading desk for the conduct of monetary policy during inter-meeting periods. They simply reflected the news of the day, evolving from meeting to meeting.

The policy of the first meeting of 1967 was to be "conducive to non-inflationary economic expansion while recognizing the need for progress toward reasonable equilibrium in the country's balance of payments." The next three meetings sought to be "conducive to combating the effects of weakening tendencies in the economy." At the May meeting the Committee wanted to be "conducive to renewed economic expansion." In July, "renewed" became "continuing" and the balance of payments concern for the remainder of the calendar year became "recognizing the need for reasonable price stability for both domestic and balance of payments purposes." In August, the Committee switched from "continuing" economic expansion to "sustainable" economic expansion.

By the time of the November 1967 meeting, inflation was taking its toll on the viability of the dollar exchange standard and the Bretton Woods international monetary system. The British had devalued the pound sterling by 50 percent, the President of the United States was compelled to publicly reiterate that the United States intended to maintain the $35-per-ounce peg to gold, and the Fed had raised the discount rate half a percent. The Committee described this as a period of market "turbulence" and gave no pretence of strategic objective. It merely stated a desire to facilitate orderly adjustments to the increased discount rate. A month later, the Committee changed course again, stating its policy was to "foster financial conditions conducive to resistance of inflationary pressures and progress toward reasonable equilibrium in the country's balance of payments." In January 1968, resisting inflationary pressures remained, but the policy went back to "progress toward reasonable equilibrium in the country's balance of payments."

I have a sense of déjà vu when I compare FOMC post-meeting press releases of 1997 through 1999 with the directives of 1967 and 1968, for they both simply describe the evolving reaction of the Committee to incoming information. In that earlier period, the Committee apparently had become so accustomed to the post-Accord experience of price stability that they simply assumed it would take care of itself while they tried to fine-tune the economy. Meanwhile, the rest of the world began to pile up unwanted dollar balances.

Such policy incoherence must be avoided today, and—as of the beginning of 2000—the FOMC has tried to avoid it by changing the content of its press releases. Each release now reminds both the Committee and the public of its continuing strategic policy objective, stating, "Against the background of its long-run goals of price stability and sustainable economic growth and of the information

currently available, the Committee believes" whatever it decides to say about the period until its next meeting.

When Darryl started his second tour of duty as a voting member of the Committee, Arthur Burns had become Chairman just the month before. The directive began to show signs that Darryl was no longer alone in his concern for a more strategic approach to policy decisions. The objectives of the Committee, as stated in the directive, remained unchanged from meeting to meeting: to "foster orderly reduction in the rate of inflation, while encouraging the resumption of sustainable economic growth and attainment of reasonable equilibrium in the country's balance of payments."

Who knows? Perhaps these three objectives might have been jointly attainable in the long run, but for the short run they sound more like motherhood and apple pie. In the monetarist tradition of the St. Louis Bank, it might have been preferable to choose a money growth rate as an instrument for achieving the strategic objectives. Nonetheless, at least the Committee said it was guided by a constant constellation of major stars in 1970 and 1971, rather than focussing on every starlet going down the street.

More evidence of the Committee's movement toward the maverick from St. Louis comes from the second operating instruction paragraph of the directive. Initially, it merely stated an objective for money market conditions thought to be consistent with expected growth paths of money and credit. Frequently, this was conditioned on smoothing the path for Treasury financing. Sometimes it even was conditioned on evidence that money and credit actually were following the Committee's expected paths. Darryl dissented in May 1970 because the Committee's expected paths for M1 growth seemed excessive.

Then the Committee took time out to deal with a commercial paper crisis caused by the bankruptcy of the Penn Central railroad. Concrete evidence of movement came in December when the Committee identified M1 as what it meant when it mentioned money. This step toward the maverick from St. Louis did not prevent Darryl from dissenting. Having specified M1 as money, he wanted less emphasis on bank credit. In addition, he preferred a lower rate of M1 growth. He dissented again at the January 1971 meeting and also at the February meeting, which was the last of his second voting term. He wanted less emphasis on money market conditions in the operational instruction to the Trading Desk in New York.

A lot happened in the next two years. The United States imposed wage and price controls in a futile attempt to reduce inflation. It abandoned official convertibility of the dollar into anything, even for other central banks. And it devalued the dollar twice in 14 months, from a nominal gold content of $^{1}/_{35}$ of an ounce to $^{1}/_{42}$ of an ounce.

As Darryl began his third term as a voting member, the Committee appeared to have moved further in his direction, at least superficially. The directive's operational paragraph now instructed the Desk to maintain bank reserves and money market conditions consistent with expected paths, or ranges, of M1 and M2. By the end of the year, this operational instruction had become more sharply defined, stating a range within which the Desk might move the funds rate in response to movements of the growth rates of M1 and M2 within allowable ranges.

In retrospect, this seems to have been closer to a shell game than to an effective operating procedure. That assessment might have been clear to Darryl Francis at the July and August 1973 FOMC meetings. In dissenting from the Committee's instruction to the Desk at both of those meetings, he pointed out that the growth ranges for M1 and M2 were inconsistent with the constraint placed on movements in the funds rate.

Nonetheless, the procedure might have worked had the Committee relaxed its constraint on the funds rate when inconsistencies became apparent, but that was almost never the case. The December 1973 meeting was a classic example. The oil crisis was on; price pressures were apparent. Seeking the best of both worlds, the Committee announced a new strategic objective: policy was to resist inflationary pressures and, at the same time, to cushion the effects on production and employment growing out of the oil shortage. The directive called for some easing in bank reserves and money market conditions, provided that the monetary aggregates did not appear to be growing excessively. By early January 1974, the Committee was asked to increase the inter-meeting limit on additions to the System Open Market Account (SOMA) portfolio that became necessary to accommodate excessive growth in the aggregates. The majority agreed; Darryl Francis dissented, arguing that banks should be forced to borrow the needed extra reserves.

A week later, with the funds rate at the top of

its allowable range and the aggregates above the tops of their allowable ranges, the Committee members had to choose which constraint to honor. The majority agreed to obey the funds rate constraint, rather than the monetary aggregate constraint. Darryl Francis dissented. The rest is history. Not until 1979 did the Committee finally become persuaded that inflation was a monetary phenomenon—that unless the growth of money was brought down, the rate of inflation could not be brought down. What might have been a modest economic adjustment to lower money growth after the petroleum crisis in 1973-74 in fact became the very painful adjustment of 1979-82.

On reflection, Darryl Francis's three-year maverick voting record reveals a distinct pattern. In 1967-68, the Committee had no apparent anchor to its decisions—not even maintaining the viability of the international dollar standard. Darryl pointed to an anchor—a stable trend growth rate of M1 that was consistent with price stability. By 1970-71, the Committee was moving in his direction, with an unchanging set of strategic objectives and operating expectations for M1 and M2 growth. By 1973-74, the Committee faced the inconsistency between its operating objectives for M1 and M2 and the operating procedure it used to attain those objectives. Despite Darryl Francis's dissents, which pointed out that the procedural emperor had no clothes, the Committee went naked into most of the remainder of the decade, setting an interest rate without reckoning with the inflationary consequences. To switch metaphors, push had come to shove, and, until 1979, the Committee shoved along the path Darryl Francis had tried to push them off.

I find several parallels between Darryl Francis's FOMC voting experience and more recent FOMC experience. One is the importance to the Committee and to each potential maverick on the Committee of maintaining an overriding strategic objective for price stability. Unfortunately, as time passed, Darryl's lessons about monetary targets seem to have become impossible to apply. At present, the simple and appealing concept of "money" has no generally accepted empirical surrogate that can serve as a strategic objective or an operating guide for policy.

It is true, however, that we have been making progress over the past decade in rebuilding a regime in which people believe that any increase in inflation and interest rates is temporary, and that the longer-term trend is toward price stability. Nevertheless, an unfortunate tendency persists in the financial press to assert that the Fed seeks to slow growth in order to prevent inflation—in spite of every current FOMC member having declared that growth does not cause inflation and that the ultimate objective of monetary policy is to maximize long-run growth by preserving a stable purchasing power of money.

Declaring that growth does not cause inflation did not prevent some Fed-watchers from viewing each increase in the Committee's funds rate target in 1999 and 2000 as a potential assault on the longevity of the longest economic recovery on record. In my view, however, the environment of rapid technological innovation and increased productivity had a crucial impact on monetary policy that must be recognized if we are to understand a second parallel to Darryl Francis's maverick tenure on the FOMC: Raising a nominal overnight interbank rate does not necessarily ensure a restrictive policy. In the 60s and 70s, policy was not restrictive because the inflation premium in interest rates was rising faster than the Committee was raising the overnight policy rate. In the 1999-2000 environment, raising the overnight policy rate did not indicate that the stance of policy had become more restrictive if the real return to capital was rising faster than the policy rate. Let me explain.

All of us probably are familiar with the idea that household consumption behavior tends to reflect expectations about longer-term ability to consume. This phenomenon has been called the life-cycle hypothesis, standard or standardized income, and, of course, permanent income by Milton Friedman in the *Theory of the Consumption Function*.

The basic idea is familiar. Transitory changes in measured income or cash flow fluctuate around some longer-term average; household consumption behavior does not fully reflect these transitory changes in the short run. Sharp increases in measured income are not fully reflected in corresponding increases in current consumption—nor are sudden rapid declines in measured income reflected in corresponding declines in consumption spending.

Both the theoretical framework and empirical observations traditionally suggest that permanent income is relatively steady, while transitory changes in measured income are more variable. However, it can also be the case that periods of rising produc-

tivity and significant technological innovation produce a generalized perception that permanent income is rising relative to measured income.

People may come to form this expectation in a variety of ways. Sustained periods of steady employment and growing paychecks may lead people to expect that not only has their real standard of living risen, but it will continue to rise in the future—possibly at a faster rate than previously expected. Or, they may come to expect fewer or shorter periods of unemployment. Or, they may observe that their 401K savings plans or defined-contribution retirement programs now promise a higher future stream of income than previously thought. In a variety of ways, people come to expect that they will be able to consume more in the present, as well as in the future, than they previously thought.

As a result of any (or some combination) of these various forces in the "new economy" environment, households perceive that their long-term ability to consume is higher. In economists' language, they have moved to a higher indifference curve.

In the business or entrepreneurial sector, rising productivity and an enhanced pace of technological innovation mean that the marginal efficiency of capital is higher. Again in economists' jargon, the production possibility boundary has both shifted out and changed its shape, offering more tomorrow in return for giving up consumption today. Real interest rates rise as new opportunities bring a higher rate of return on new business investment.

These higher real interest rates are not a matter of policy choice or of anyone's discretion. Rather they are a manifestation of economic forces that result in better uses for available productive resources. With households and businesses both increasing their claims on current productive resources, real interest rates must rise in competitive markets.

Higher real interest rates need not imply higher nominal interest rates. Just to exclude complications for the moment, consider the case under a gold standard. Increased productivity and technological innovation would exert downward pressure on the prices of some goods. Institutionalized monetary stability implied by a gold standard means that the price level falls. Thus, the purchasing power of money rises in the face of greater productivity.

The falling price level means that greater permanent real income can be distributed to society with the same level of nominal income. The falling price level also implies that unchanged nominal interest rates, or possibly even somewhat lower nominal interest rates, correspond to higher real interest rates. These higher real rates are the essential market mechanism by which competition between consumers and investors rations present consumption against augmented future consumption.

But we're not on a gold standard. What happens in a discretionary monetary policy regime using an interest-rate-pegging procedure? The upward pressure on real interest rates that is a necessary consequence of greater productivity and the faster pace of technological innovation initially puts upward pressure on nominal interest rates. Greater and greater injections of central bank money then are necessary to keep the pegged level of the nominal overnight interbank rate unchanged. Rising market interest rates mean that the opportunity cost of holding money balances is rising. That, in turn, means the quantity of money demanded declines and the income velocity of money rises. This combination of a higher trend growth of velocity and the faster growth of central bank money means that a higher rate of nominal final-demand growth is accommodated by a more expansionary rate of money growth.

In such an environment the increase in nominal interest rates—while initially reflecting upward pressure on real interest rates—will be augmented by a rising inflation premium. The overnight interbank rate is under persistent upward pressure so long as it continues to lag behind market-determined interest rates.

This dynamic process describes an environment in which acceleration in the pace of technological innovation and productivity can inadvertently become an inflationary process. The central bank's passive maintenance of an unchanged overnight rate accommodates nominal price increases by failing to accommodate real interest rate increases. As a result, credit markets are unable to play their role in rationing available real productive resources amongst heightened competing demands that reflect the increased return to real capital.

If that description of policymaking in a period of accelerating productivity growth makes me seem like a maverick, I'm happy to wear the label.

This brings me to the third and final parallel I see between Darryl Francis's experience with policy implementation and today's experience.

Each member of the Committee is, by design, an independent free agent. While the will of the majority always prevails, each member must be prepared respectfully to disagree. As Darryl demonstrated, the maverick, the dissenter, the sometimes-lonely voice in the crowd, plays a vital role in the continuing evolution of policy thinking and policy making. We all salute him for his courage during an exceptionally challenging period of our central bank's history.

Money and Monetary Policy: An Essay in Honor of Darryl Francis

Allan H. Meltzer

It is a pleasure to take part in a conference honoring Darryl Francis. Recognition of his courage and leadership is long overdue. Darryl took a leadership role by encouraging the St. Louis monetarist tradition, when it was scorned and derided, by pointing to the need for more restrictive monetary policy in the late 1960s, and in continuing to make the case for lower inflation during his service on the open market committee and as President of the St. Louis Bank.

The 1960s and 1970s were the Keynesian decades in U.S. economic policy. Keynesian policy views were mainstream views in the academic profession. The Council of Economic Advisers then had a more prominent role in economic policy than it has now. The chairmen of the Council in the 1960s were Walter Heller, Gardner Ackley, and Arthur Okun, all leading Keynesian economists. In the 1970s, the roster includes Paul McCracken, Herbert Stein, and Alan Greenspan, three conservative Keynesians at the time, but Keynesians nonetheless. They were followed by Charles Schultze in the late 1970s.

The primary role of fiscal policy in economic stabilization was a distinguishing characteristic of Keynesian policy. I recall the fervor with which senior professors, young faculty, and bright graduate students sought to overturn Friedman and Meiselman's (1963) demonstration that, except for the Great Depression, monetary velocity was more stable than the Keynesian investment multiplier. Similar fervor greeted Andersen and Jordan's (1968) paper showing that changes in money had stronger and more reliable effects than budget expenditures on changes in nominal gross national product (GNP). As late as Jerome Stein's (1976) conference volume on monetarism, these issues remained highly contentious and hotly debated.

At the height of the controversy in the late 1960s and early 1970s, Darryl Francis was the principal, and usually the only, spokesman who challenged this orthodoxy at meetings of the Federal Open Market Committee (FOMC). At the time, I was aware of Darryl's role, but I appreciate it even more now that I have access to the FOMC's minutes for that period. I will soon give some examples suggesting that, if Darryl's advice had been taken, we would have avoided the great inflation.

The role of money in monetary policy remains an issue. The Federal Reserve recently gave up the publication of money growth estimates or targets when requirements to publish their estimates expired. The mistake occurred much earlier, when the FOMC stopped using money growth as a measure of the thrust of monetary policy.

Federal Reserve history suggests that neglect of money growth is a major mistake. The Federal Reserve would have avoided mistakes such as the Great Depression and the Great Inflation if it had used money growth as an indicator of the thrust of monetary policy. Below I present some other examples and contrast the role assigned to money growth by the European Central Bank with its neglect by the Federal Reserve.

BEGINNING THE GREAT INFLATION, 1966-69

From the trough of the 1960-61 recession in February 1961 to the end of 1965, the monetary base accelerated steadily. Annual base growth rose from 2.9 percent to 6 percent. Annual rates of consumer price index (CPI) inflation followed slowly, rising from 0.67 percent in 1961 to 1.9 percent in 1965. The quarterly average rate, however, reached 3.8 percent (annualized) in fourth quarter 1965, and the GNP deflator rose more than 4 percent in the first half of 1966.

The Federal Reserve responded to the 4 percent inflation by raising interest rates. Money growth fell. Data available at the time show average M1 growth falling from a maintained rate of 4.4 percent to zero for the second and third quarters of 1966.[1] Short-term interest rates rose above 5 percent, the highest levels in the postwar period to that time, indeed the highest levels since the early 1930s.

The economy slowed. Real GNP growth fell from 9 percent in fourth quarter 1965 to less than 2 percent a year later. The inflation rate fell also. Within a few months, the annual rate of inflation was back

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[1] Contemporary data are from the Federal Reserve Bank of St. Louis (1972).

to 2.5 percent. Federal Reserve action reduced the rate of inflation quickly. At the time, the Federal Reserve had considerable credibility. In the 15 years since it had regained independence from the administration, inflation averaged 2 percent and real growth averaged 3.6 percent despite recessions in 1953-54, 1957-58, and 1960-61. The United States had not experienced such good times since the much shorter period of growth and low inflation in the 1920s. William McChesney Martin, Chairman of the Federal Reserve, was not quite the popular figure that Alan Greenspan has become, but he was highly regarded and respected as a defender of price stability.

The good news was about to end in 1966. As we know now, productivity growth reached a peak at about this time and, except for brief periods, inflation rates have not maintained an average rate of 2 percent or less. Indeed, the goal of price stability has been replaced by a current goal of 2 to 3 percent inflation.

Darryl Francis became President at St. Louis in 1966, just as inflation began to rise. Contemporary data show that the CPI inflation rate reached 4.7 percent by late 1967 and more than 6 percent by early 1969. Current data lower these inflation rates to 3.25 percent and 5 percent. The Federal Reserve based decisions and actions on contemporary data, however.

As usual, the Federal Reserve ignored money growth. Facing a recession brought on by the "high" interest rate policy, it reversed its course decisively in 1967. Contemporary data show average M1 growth at 7 percent for the years 1967-68 with inflation at almost 5 percent.

President Johnson, under pressure because his budget deficit financed both an increasingly unpopular war and substantial redistribution, proposed a temporary 10 percent surtax on incomes. This was a fiscal solution to a monetary problem. It could work only if, with a small deficit, the Federal Reserve maintained interest rates and allowed money growth to slow. Keynesian commentary urged the opposite—lower interest rates to offset the tax surcharge. In fact, the monetary base accelerated and, as noted, M1 growth remained about 7 percent.

Excerpts from the FOMC minutes for 1967 and 1968 show that President Francis, usually alone, opposed Federal Reserve policy. His remarks suggest awareness that reducing inflation had a temporary, but real, social cost. He favored fiscal action to reduce the budget deficit but, unlike many of his contemporaries, he did not support the Keynesian approach of easing monetary policy by lowering interest rates and increasing money growth to moderate the contractive effects of higher tax rates. He wanted to reduce money growth, then maintain it at a rate consistent with price stability.

Here are some samples drawn from the FOMC minutes.[2] In May 1967, with GNP growth down to 1.5 percent, Francis told his FOMC colleagues:

> It appeared that the deceleration of economic activity had been halted, and that growth would soon be resumed. The increasing budget deficit of the Federal Government was providing a growing stimulus… Monetary actions had been very expansionary since early this year, and the response in the real sectors of the economy appeared to have been relatively quick. Since monetary and fiscal developments usually had their chief impact after some lag, the basic present problem was not how to achieve adequate total demand but how to avoid excessive demand…[E]xcessive inflation was likely to appear later in the year unless monetary actions were moderated now. (FOMC, 1967, pp. 567-69)

Francis concluded by urging a money growth rate of 2 to 4 percent instead of the staff's projected rate of 8 to 11 percent.

Chairman Martin "noted that the Committee was not unanimous…with Mr. Francis favoring some firming of money market conditions and the remaining eleven members favoring the maintenance of prevailing conditions" (ibid., p. 573). The Federal Reserve kept the interest rate unchanged. Reported M1 growth reached a 9.7 percent annual rate in that quarter.

In September 1967, a majority of the FOMC favored a modest and cautious increase in interest rates. Francis, joined by two others, dissented. He favored a more aggressive change in the federal funds rate and free reserves. Citing the 9 percent reported growth of M1 and personal income for the most recent six months, he warned the FOMC that current policy "would feed and extend the imminent excessive demand and price inflation" (ibid., p. 973).

Three weeks later, Francis argued that any in-

[2] The quotations are from the FOMC minutes, which are edited; thus, the statements quoted here may not be exactly the same as the statement made at the time.

crease in interest rates would be temporary, a necessary adjustment to slow inflation and aggregate demand. Congress had not enacted the President's tax increase. He said that the need for monetary restraint was urgent.

Contemporary reports show inflation slowing in the fourth quarter. Several members of the FOMC agreed with Francis that interest rates should be increased to curb the effects of fiscal and monetary stimulus on aggregate demand. They believed, however, the time was not right; the Treasury would soon have to sell bonds.

Francis argued against delay. "[T]his was a pernicious doctrine…[W]hen the Federal budget was excessively stimulative…monetary actions were also excessively stimulative" (ibid., p. 1165).

So it went. Francis encountered one excuse after another. In December 1967, the FOMC discussed selective credit controls as an alternative to higher interest rates and slower money growth. Late in 1967, the Board increased reserve requirement ratios. With interest rates unchanged, however, it supplied the reserves in the open market that it had removed by regulation. Francis made this point at the February 1968 meeting. He urged "an immediate and substantial move toward monetary restraint" (FOMC, 1968, p. 153). Monetary base growth increased to 6 percent or more. In April 1968, Francis argued against those who opposed restraint because of its selective effects on housing. This argument against restraint had political force because higher market interest rates would require an increase in ceiling rates on time deposits under Regulation Q. The thrift industry opposed the increase.

At the March 1968 meeting, Francis lost his vote on the FOMC as part of the annual rotation. He continued to argue forcefully for monetary restraint and to urge the administration to reduce the budget deficit. Throughout 1968, joined by Governor Sherman Maisel, he urged more attention to growth of the monetary aggregates. He complained frequently that the Manager of the System Open Market Account did not respond adequately to the proviso clause in the directive that called for restraint when growth of credit exceeded a specified rate.

The FOMC did not respond to these arguments and did not act. Money growth remained excessive, and inflation continued to rise. Contemporaneous reports show quarterly average CPI inflation above 5 percent in the last half of 1968.

When, at last, Congress agreed in June 1968 to a tax surcharge, some members of the FOMC wanted to reduce discount rates and ease policy. St. Louis estimated that the full employment budget would shift from a $14 billion deficit in the first half of 1968 to a $10 billion surplus in the first half of 1969. In August 1968, Francis told the FOMC that he did not agree with analysts "preoccupied with fear of a coming recession…[and that] in his judgment, the view that the recently adopted fiscal package would, by itself, adequately restrict total demand and cure the inflationary problems was overly optimistic. Monetary actions had continued to be excessively stimulative, negating the desirable anti-inflationary impact of the fiscal package" (ibid., p. 991).

The St. Louis view was that increased money growth would dominate the effect of the tax surcharge. The St. Louis staff forecast increased nominal GNP growth. Contemporary data suggest that growth rates of nominal and real GNP and industrial production slowed in the second half of 1968. The inflation rate did not fall, however.

In 1969, the St. Louis Federal Reserve Bank proposed increases in the discount rate on six occasions between January and July. The Board initially opposed any increase. At first, St. Louis asked for a 0.5 percent increase to 6 percent. Opponents of the increase gave different reasons. Some Governors wanted to wait until the Treasury completed its financing. Governor Mitchell did not care for 0.5 point increases. He preferred to limit the availability of credit, although he did not say how that could be done without raising interest rates. Governor Brimmer thought that credit expansion resulted from the actions of a few large banks that borrowed Euro-dollars.

By late March, Richmond and Kansas City joined St. Louis in asking for a 0.5 percent increase to 6 percent. Chicago asked for 6.5 percent. Dallas favored an increase also. On April 3, the Board approved the 6 percent rate. Eight weeks later, St. Louis requested an additional 1 percent increase. It renewed the request several times, through late June. By that time, M1 growth had fallen to a 3 percent annual rate. St. Louis, guided by money growth, stopped requesting rate increases. Throughout the summer, Chicago continued to ask for a 7 percent rate. By November, the economy was in recession.

The rest of the story is familiar. By 1971, monetary base growth reached an 8 percent annual rate. The Bretton Woods System ended. Controls reduced the measured rate of inflation, but base money growth increased to 9 percent by 1973. With controls removed, measured CPI inflation reached 7

percent before the first oil shock and more than 11 percent shortly after.

Darryl Francis had the good sense to rely on the strong research group at St. Louis led by Homer Jones and including Jerry Jordan and Leonall Andersen. Darryl encouraged and supported the group, used their analyses, and articulated a strong position of which he can be proud. His views were unpopular, but with hindsight he knows that he was right.

If the FOMC had acted on his recommendations, the United States 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.

START OF THE GREAT DEPRESSION, ANOTHER EXAMPLE

Everyone is familiar with the fact that mistaken Federal Reserve policies prolonged and deepened the Great Depression. Attention to money growth would have alerted the Open Market Policy Committee (OMPC) to the severity of deflationary policy. I believe it is now generally accepted that a recession would likely have occurred in 1929, but the Great Depression could have been avoided.

Not only did the Federal Reserve ignore money growth, but its leading official did not know that demand deposits were the major component of the money stock. Darryl was not there to correct the errors, but Irving Fisher was. Fisher told of his visit to Eugene Meyer, Governor of the Federal Reserve Board, in 1931. Fisher told Meyer that demand deposits adjusted had fallen sharply. Meyer asked what they were. According to Fisher, a clerk brought in the ledger. Meyer observed the figures on the page, but he showed little interest and took no expansive action.

The two years before October 1929 are another example of policy error caused by failure to consider money growth as a measure of the thrust of monetary policy. The United States was on the gold standard, but it did not follow gold standard rules. Gold flows were frequently sterilized. Growth of credit and member bank borrowing were the main guides for policy. The Federal Reserve's principal concerns were the rise in stock prices and the use of credit in the stock market.

The rise in stock prices that ended in 1929 is extraordinary by almost any standard except the most recent. From the end of 1924 to September 1929, Standard and Poor's index rose at a 21 percent compound annual rate. The Dow Jones industrial average, at its peak of 381 in September 1929, had doubled in less than two years. The rise was propelled, in part, by rising profits and economic activity. Real GNP and corporate profits rose at annual rates of 4 percent and 12 percent, respectively, with only one mild recession during the nearly five-year period. The increase in market capitalization, relative to nominal GNP, brought the ratio of the two to a level that was not surpassed until 1996.

The rate of rise in corporate profits was much greater than the rate of increase in GNP, but only half the rate of increase in the value of traded stocks (market capitalization). Between 1925 and 1929 the ratio of market capitalization to corporate profits doubled. In absolute value, the ratio rose from 25 in 1925 to 50 at its peak in 1929.

For those brought up on the belief that the 1929 stock market was a wild speculative orgy, the data are surprising. The capitalization ratio rose most rapidly in 1926, with rising profit anticipations. The ratio then remained between 40 and 50 until the market break in 1929. These data suggest that the so-called speculative boom of 1927-29 was driven by rising profits and, most likely, by anticipations of further increases to come. The 17 percent decline in corporate profits in fourth quarter 1929 and the 30 percent decline in first quarter 1930, or anticipation of the decline, must have reversed some of the beliefs built up during the expansion. At the time, many could recall vividly the volatility of the late nineteenth century and the frequent banking panics that Congress intended the Federal Reserve to prevent. Call money rates briefly reached 20 percent in 1929. Rates of 100 percent or more had not occurred in the 15 years of the Federal Reserve's existence. There had been recessions, but the only major deflation, in 1921, was attributed universally to the end of wartime excesses. The belief spread that the Federal Reserve had learned how to maintain prosperity, dampen recessions, and prevent inflation. The return of many countries to the gold standard by 1927 reinforced the view that the world

economy was on a stable foundation and that inflation and deflation were unlikely to occur.

Irving Fisher commented that the stock market "went up principally because of sound, justified expectations of earnings, and only partly because of unreasoning and unintelligent mania for buying" (Fisher, 1930, p. 53). He credited increased profits to the application of science, technology, and new management methods.

Annual rates of CPI inflation remained negative from July 1926 to May 1929. Restoration of the international gold standard and Federal Reserve actions were a main reason for the sustained, mild deflation of the period. The 12-month moving average of monetary base growth fell below 1 percent in November 1926, turned negative in May 1928, and remained negative through June 1929. In this period of rapid economic growth, monetary policy was deflationary.

The Federal Reserve gave no attention to the monetary base. Its records show, however, that the 1929 increase in output and fall in prices was known at the time. The U.S. economy had a spectacular performance in the first half of the year. Corporate earnings increased about 30 percent in the first nine months. "Large corporate earnings, together with the ability of corporations to float stocks at high [stock] prices…put them in possession of funds with which to complete contemplated expansion programs" ("Review of Business in 1929," Federal Reserve Board; January 15, 1930, preliminary, Box 2461, p. 4). The only negative influence reported at the time was a decline in residential structures. Industrial and commercial building was at a record level. Exports of manufactured goods increased 50 percent for the year, despite the recession in the last four months (ibid., p. 2).

These data suggest that, as Fisher said, the optimistic projections underlying the rise in stock prices had a factual base. Even after the severe decline at the end of 1929, the Board's staff described the first six months of 1929 as "the continuation of the steady expansion throughout the year 1928" (ibid., p. 5). Industrial production was recorded as 26 percent above the trough of the 1927 recession.

Some questioned or dissented from these optimistic beliefs. Allyn Young, a respected economist of the period, warned about deflation early in 1929. Unlike many of his contemporaries, who blamed deflation on either a decline in the gold stock or a maldistribution of gold holdings, Young blamed central bank gold hoarding. He saw that central bank policies forced deflation.

Restoration of the gold standard increased demand for monetary gold stocks. The effect was deflationary for the world economy. The principal creditor countries, France and the United States, increased deflationary pressure by sterilizing gold inflows. France had returned to the gold standard at an exchange rate that undervalued the franc, and Britain at an exchange rate that overvalued the pound. After the French stabilization in 1927, capital returned to France, followed by gold. Much of the flow came from Britain.

The French government disliked the gold exchange standard. It chose to hold gold, not foreign exchange—a policy it repeated in the 1960s. It sold foreign exchange and bought gold to shift the balance in its reserves. These operations reduced French money growth and prevented the rise in prices needed to adjust for the franc's devaluation. The United States had a gold outflow in 1928, an inflow in the first three quarters of 1929. It, too, sterilized most of the gold flows; as noted earlier, the monetary base declined.

U.S. and French policy, therefore, shifted the adjustment burden to the deficit countries. These countries had to deflate or devalue to adjust their real exchange rates. By 1931, Britain, the Scandinavian countries, and many others chose devaluation over continued deflation.

The Federal Reserve ignored the monetary base and money. It focused its attention on the stock market, credit, interest rates, and member bank borrowing. Although aware that the price level had fallen and continued to fall, its concern was inflation, not deflation.

The basis of this concern was the real bills doctrine. Under that doctrine, credit to hold real estate, government debt, and common stocks was "speculative" credit. Such credit permitted wealth owners to hold assets but did not increase the production of goods. According to this doctrine, because credit increased more than output, prices had to rise. Real bills, unlike speculative credit, finance additions to output. Because stock market prices, member bank borrowing, and credit had increased, credit expansion had financed speculation; inflation must result.

The Board's solution was to restrict credit to the stock market by direct pressure and exhortation. The Board did not want to increase the discount rate because that would penalize legitimate borrowers, the suppliers of real bills. Speculative borrowing

would be encouraged; speculators would continue to borrow because the rate of increase in share prices made them insensitive to small changes in interest rates. That was the reasoning.

Benjamin Strong, Governor of the New York Reserve Bank, was ill in 1928 and died in the autumn. Officials at the New York Bank had been trained by Strong. Strong rejected the notion that the Reserve Banks could control the quality or types of credit that financial institutions granted. He and his followers believed that banks borrowed from the Federal Reserve only in case of need and with reluctance. Increased borrowing at a higher rate would induce banks to contract credit, so they could reduce borrowed reserves. Although increased aggregate borrowing supplied reserves, and this supported more lending, the New York Reserve Bank believed that it would have the opposite effect. In their view, discount rate increases reinforced the effect of reluctance to borrow.

The hot dispute between Washington and New York in 1929 was not about whether monetary contraction was desirable. Although the price level was falling, they agreed about the threat of inflation. They disagreed about how to achieve monetary contraction. If they had paid attention to the monetary base, or followed gold standard rules, they would have allowed money and credit to increase, reducing interest rates and deflation in other countries.

For this policy to succeed fully, France would have had to agree. At the time, they received and sterilized more gold than the United States.

By the spring of 1929, it was probably too late to stop a worldwide recession, although not too late to stop the severe deflation that followed. The National Bureau of Economic Research marked a cycle peak in April for Germany and July for Britain. March was the peak month for production in Belgium; Canada's peak came in the spring. By fall, financial and business failures had increased in Britain, Germany, and elsewhere (Kindleberger, 1986, pp. 102-04). The Federal Reserve's production index, available at the time, peaked in June. By October, it was 8 percent below the peak. Monthly peaks in the stock markets in the United States, Canada, and France came in September 1929; but markets in Germany, Sweden, and Switzerland reached peaks in 1928; and in Britain the peak came in January 1929 (Kindleberger, 1986, pp. 110-11, based on League of Nations data).

The Great Depression had begun.

THE ECB AND THE FED

Money has a central role in the framework that the European Central Bank (ECB) uses to link policy actions to their ultimate goal, low inflation. In a recent monograph describing the ECB's strategy and processes, the senior economics staff of the ECB wrote:

> The central message from the [monetary] theory, confirmed by the evidence, of a strong link between money and prices, cannot be ignored by a central bank that has price stability as its primary goal. This relationship is a reminder that it is impossible to have high and sustained inflation without monetary accommodation. Since the relationship holds in the long term, there are obvious difficulties to interpret the impact of current monetary developments on inflation in the medium term. Concluding, from the acknowledgement of these difficulties, that money should be neglected altogether is, however, a logical *non sequitur*. Ignoring money amounts to disregarding an important piece of information. This information is particularly useful to avoid large mistakes in policy, which could arise when the other available indicators—whose exact relationship with inflation is unknown *both* in the short and in the long run—fail to signal inflationary risks. (Angeloni et al., 2000, p. 59)

This statement properly emphasizes two central propositions. First, our knowledge of the short- or medium-term linkage between money and prices is imperfect. U.S. experience in the early 1980s, when money growth gave a series of misleading signals, supports the ECB's interpretation. Second, the long-run relation between money and prices is one of the best-established propositions in economics. Prices may change without money changing, but sustained inflation cannot occur without sustained money growth.

The ECB monograph recognizes that money growth is not a very reliable predictor of short-term changes in prices or nominal gross domestic product. This should not come as a surprise to Darryl Francis or the St. Louis Federal Reserve Bank. The Andersen-Jordan (1968) equation showed that the response of nominal GNP to money builds up slowly. Evidence suggests that the initial effects of a change

in the monetary base are absorbed almost entirely by an opposite change in base velocity. There is little or no immediate effect.

One reason for the delayed response is that central banks do not pay attention to short-term changes in money. The most useful hypothesis for the market to use under these circumstances is that short-term changes in money growth are mainly random changes. Base money growth is a noisy series, so it is difficult to extract any useful information from its short-term movements.

The ECB senior staff reaches a similar conclusion. It finds little useful information in short-term changes in money. It adopts a "reference value" of money that the staff believes is "consistent with—and will serve to achieve—price stability" (ibid., p. 62). The reference value is the best available forecast of the future growth of money that is consistent with price stability.

The ECB and Federal Reserve procedures differ in several ways. Most striking is the determined attempt by the ECB staff to set out the medium- to long-term framework that guides its actions. It has a long-term objective, price stability, and the outline of a strategy to achieve that objective. The objective is given by law; it is not free to change objectives. Although it takes account of the social cost of achieving price stability, the law does not require it to do so. In principle, it could force the member countries to accept high unemployment to reduce inflation. If it acted that way, however, the law setting its objective would likely change.

The Federal Reserve has a much less clear objective. It claims that price stability is its objective, but there are few times in its almost 90-year history that the nation experienced sustained high employment and low or moderate inflation. Although the last decade is one of the most successful decades in its history, there is as yet no reason to believe that the mixture of wise decisions and good luck will be sustained.

The reason for skepticism is a strong prior belief that is reinforced by my study of Federal Reserve history. The main policy errors of the past were not made by evil men who wanted to create depression or inflation. They were made by officials who believed that money growth was not a useful indicator of future inflation or deflation. Some of the theory or theories on which these officials relied had no place for money. They did not accept, and in many cases rejected explicitly, the proposition that money growth is relevant for inflation or deflation. They paid no attention to money growth.

In the 1920s and early 1930s the most popular theory of money was the real bills doctrine. In the 1950s and 1960s, a simple Keynesian model held the dominant position. The main policy implications of these models highlighted the role of interest rates or borrowing, not money. In the earlier period, the Board dismissed as curious the work and recommendations of Irving Fisher, and in the later period, of Milton Friedman, the St. Louis bank, and other monetarists. Currently, the staff's econometric model has no separate role for money. The quantity of money is determined endogenously. Money growth has no independent role in determining inflation.

This error permeates the staff and, apparently many of the policymakers as well. I believe a main reason for neglecting money now, and for many years, is the Federal Reserve's intense concentration on near-term events. The Federal Reserve does not have a meaningful long-term strategy. At least, it has not made its strategy explicit.

Of course, one can find many words written and spoken about the medium-term, the need to control inflation a year or two ahead, or the small effect of current actions on near-term outcomes. I find it hard to connect these statements to the actions that the Federal Reserve takes, the way it decides to take action, and the messages it sends to the financial markets and others.

The latter message concentrates on short-term changes in principal variables. Many of these data have large reporting errors and are subject to major changes between original announcement and final revision. In an interesting paper, Athanasios Orphanides (2000) showed great differences between the fit of John Taylor's well-known rule when based on initially available data and the later data that appear in the record books. This is one of many examples showing that much of the data used for policy decision is subject to major revisions that can change interpretations.

One cannot rationally object to using new information, if it has content. At issue is the relative weight placed on such data. The Federal Reserve gives lip service to longer-term goals but focuses most attention on short-term changes about which it can do little or nothing. Although it recognizes this point verbally, it has not changed its behavior.

A recent paper by Edward Nelson (2000) revisits the role of money in the United States and the United Kingdom. He finds that, for the period from 1961 to 1999 in the United States, lagged

growth of real base money balances has a strong, significant effect on a measure of the deviation of output from its long-term trend. The effect of money growth remains powerful when the sample includes only the period from 1982 to 1999. Nelson includes the real value of the federal funds rate. As he notes, the significant effects of money, given the interest rate, imply that there are effects of money beyond those given by the interest rate. Nelson (2000) estimates a similar equation for the United Kingdom. Again, there are strong, persistent effects of real money growth on the output gap, holding real interest rates constant.

The transmission of monetary policy from initial impulse to final effect involves changes in many relative prices of assets and output. That last statement may seem obvious to many of you, but it is inconsistent with most, if not all, recent work on quarterly dynamic models of monetary policy.

Nelson (2000) introduces base money growth into a small, dynamic optimizing model. He assumes that there are costs of adjusting portfolios. His empirical work shows strong effects of money growth, holding the interest rate constant. These findings supplement and reinforce the conclusion drawn from historical episodes (Meltzer, 2000).

The Federal Reserve's recent decision to ignore money is a mistake. The primary reason for this mistake, and for the difference between the ECB on this issue is, I believe, the excessive emphasis that the Federal Reserve gives to short-term changes.

I have raised the last point several times. Let me develop it further. Every week the government announces new statistics on the economy. Many of these announcements are poor approximations of the data that are later corrected. The daily and weekly commentary speculates on whether the last announcement changes the probability that the Federal Reserve will change interest rates. Markets react. Asset prices move. The next announcement may augment or reverse today's changes.

The weight that each announcement receives depends much less on the quality of the data than on market commentators and participants beliefs about the weight that the Federal Reserve puts on the announcement. The Chairman and other officials give hints and clues about what they watch. These weights shift frequently, and they are not uniform across the members of the FOMC. If there is a hypothesis relating these noisy indicators to long-term objectives, it has never been stated or evaluated.

These criticisms may seem odd, coming as they do in the nineteenth year of a remarkable expansion punctuated by only one mild recession a decade ago. The Federal Reserve deserves and gets much credit for this unique period in U.S. economic history. Its actions reduced inflation in the early 1980s. It responded to the 1990-91 recession without renewing inflation. In 1998-99, it responded to the Asian crisis by financing a domestic boom that permitted the world economy to export to us and, thus, recover. The cost was a rise in U.S. inflation.

These triumphs of judgment over rules and models—and that is what they appear to be—go some way to balance the ledger against the mistakes of the past. A student of monetary history who knows something about probability theory has to recognize that recent successes are, in their way, remarkable. Judgment has been excellent and has produced an excellent record. This general feeling of success is aided by the current popular belief that 2 to 3 percent inflation is acceptable, even desirable.

I will close with three observations. First, the FOMC watches many different indicators that guide changes in the federal funds rate. By chance these may give signals that do not differ very much from the signals given by money growth. Let me offer some examples to suggest that a rule-based strategy relying on money growth would have produced similar results in recent years.

The Shadow Open Market Committee (SOMC) supported the FOMC's gradual policy in 1991-92, when many critics wanted more expansion. The SOMC argued that steady gradual monetary expansion would avoid a surge in aggregate demand and rising inflation and, thus, prolong the expansion. It did not favor more restrictive policy until the fall of 1993, when the monetary base growth rose. The Federal Reserve acted in the winter of 1994, less than six months later.

The SOMC did not join the Phillips curve watchers who warned about the threat of rising inflation in 1996-97. Base money growth remained moderate. The SOMC omitted its September 1997 meeting because it approved of policy at the time. The SOMC did not begin to complain about excessive money growth until March and September 1998. The Federal Reserve was slower to move this time, perhaps because there was no sign of inflation. When the Federal Reserve moved in mid-1999, it moved in a series of decisive steps.

At present, base money growth does not seem excessive. The Federal Reserve has stopped raising

interest rates, at least for the time being. This is again consistent with growth of the monetary base and other aggregates.

Second, the FOMC in the 1990s differs in an important respect from earlier FOMCs. It gives the markets and the public much more information about its changing policy guides. Markets use this information and, at times, move in advance of Federal Reserve action. This has been true especially since the FOMC surprised the markets in 1994.

Third, and last, a warning from history. The Federal Reserve had a good record in the 1920s. Benjamin Strong, the dominant member of the committee that decided on open market purchases and sales, was highly praised, then and later, for his astute judgments and decisive actions. Irving Fisher wanted Congress to enact a rule mandating price stability. Congress held hearings in 1922, 1926, and 1928. The Federal Reserve opposed the rule, and it was never adopted. Had the rule been followed, it would have avoided the deflation and the worst of the depression. It would have worked when nominal interest rates gave misleading signals.

Fisher talked to Strong about Strong's opposition to his rule. According to Fisher, in a private conversation, Strong replied: "If you will let me alone, I will try to do the best I can, but if you make me do by law what I am trying to do without legislative control, I will be so afraid that I cannot fill the bill that I will not accept the responsibility" (Fisher, 1946, p. 3).

Fisher responded: "I will trust you as long as you live but you will not live forever and when you die I fear your policies will die with you." Strong replied: "I have trained my assistants so that they know these policies and they will be continued" (ibid., p. 3; see also Hetzel, 1985, p. 8).

Strong died soon after. The rest of the story is history. We should not repeat that history even partially.

Both theory and evidence suggest that the interest rate, at times, has been a misleading indicator when money growth gave a correct signal. 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.

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Expectations, Open Market Operations, and Changes in the Federal Funds Rate

John B. Taylor

The process through which Federal Reserve decisions about monetary policy are transmitted to the federal funds market has changed significantly in recent years. In 1994 the Federal Open Market Committee (FOMC) began to issue a public statement whenever it increased or decreased its *target* for the federal funds rate. This target is now the focus of activities at the Trading Desk at the Federal Reserve Bank of New York. In particular, the FOMC directs the Trading Desk to buy and sell securities so that conditions in the federal funds market are consistent with an average federal funds rate near this target. While FOMC decisions in earlier years were made in terms of a federal funds rate target, these decisions were communicated to the market in a less explicit way than they are now, and the communications were misinterpreted on a number of occasions.[1] Now, when the FOMC publicly announces changes in its funds rate target, the market reacts very quickly and sometimes without any immediate open market purchases or sales by the Trading Desk to alter the supply of Fed balances.[2] As stated by Meulendyke (1998, p. 142), "The rate has tended to move to the new, preferred level as soon as the banks knew the intended rate."

There have also been regulatory and technological changes affecting the federal funds market. Starting in July 1998 the Federal Reserve changed from a contemporaneous system of reserve accounting to a lagged system similar to a policy in place years ago. Under the new lagged reserve requirements, the two-week reserve maintenance period begins 17 days after the end of the two-week reserve computation period. This has probably led to better estimates of reserve requirements by the Trading Desk and the banks, but it has also eliminated the possibility of any contemporaneous response of required reserves to the interest rate.

Another significant change made possible by computer technology is the ability of financial institutions to efficiently "sweep" their consumers' accounts from those with reserve requirements into those without reserve requirements. These sweeps have allowed required reserve balances to decline sharply from about $30 billion in 1990, to $15 billion in 1996, to only about $5 to 6 billion today. As a result, holding Fed balances to facilitate interbank payments is of greater importance for many banks than holding reserves for legal requirements. At the same time, technology has improved the speed and accuracy with which banks can keep track of their payment inflows and outflows and thereby may have reduced the demand for Fed balances.[3]

In December 1999 the FOMC further expanded and clarified its public announcement policy. It began to issue a public statement after every meeting indicating not only its current decision about the federal funds rate target, but also which factor—"the risks of heightened inflation pressures or economic weakness in the foreseeable future"—is likely to affect upcoming decisions about the target. Although the statement about possible future actions is not part of the FOMC's directive to the Trading Desk, it could have indirect effects on the Desk's transactions if it affected the demand for Fed balances. See Federal Reserve Board (2000) for a summary of the announcement policies from 1994 to 2000.

The purpose of this paper is to develop a simple model of the federal funds market that reflects some of these changes and that can be used to investigate other changes. The model makes use of a "Trading Desk reaction function" to describe the changes in the supply of Fed balances over time. The model also makes use of important recent microeconomic research by Furfine (1998, 2000a) and by Guthrie

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[1] Messages about federal funds rate target changes were sent through specific types of purchases or sales of securities under certain circumstances. Rudebusch (1995) shows that the market did not always get the message about the timing or the magnitude of the change.

[2] Thornton (2000) refers to this phenomenon as "open mouth" operations, following the terminology of Guthrie and Wright (2000) who examined it in the case of the Reserve Bank of New Zealand.

[3] This may be why neither excess reserves nor funds rate volatility has increased with the decline in required reserve balances.

and Wright (2000) to explain the demand for Fed balances.[4] Expectations are very important in the model because the Trading Desk is assumed to follow the reaction function day after day and because the demand for Fed balances depends in part on expectations of the future federal funds rate. I use the model to show how changes in the target for the federal funds rate can sometimes affect the actual funds rate even without any current open market operations. However, this effect requires credibility that the Trading Desk will follow a reaction function on future days. That way the Trading Desk is expected to do open market operations in the future if necessary. In effect, the expectation of future open market operations moves the federal funds rate today. I also examine whether the model can explain the serial correlation structure of the deviations of the actual rate from the target rate.

I begin with a description of the structure of the daily federal funds market—looking at the price discovery process and the timing of trading activity during the day. I then describe the time series behavior of the deviations of the actual federal funds rate from the target rate, placing special emphasis on the nine target rate changes from mid-1998 through 2000 and the unusually large deviations of the federal funds rate from the target rate at the end of 1999. It appears that the volatility of these deviations has been diminishing over this time period, with a big reduction in volatility around the time of the target rate changes. This may indicate that the federal funds market has been incorporating the target rate information into the price discovery process more quickly. I then describe the daily model of the federal funds market, briefly compare it with classic models of the supply and demand for reserves (e.g., Gilbert, 1985) and examine some representative simulations of the model.

TRADING IN THE FEDERAL FUNDS MARKET

A model of the federal funds market can only be a rough approximation of the complex trading that actually takes place in the federal funds market. To understand the nature of the approximation it is necessary to consider the actions and the incentives of the federal funds traders who buy and sell in the market each day. Federal funds traders are usually employed by financial institutions—frequently banks. Banks hold deposits at the Federal Reserve, which are referred to as *Fed balances*.[5] The traders do most of the buying and selling of federal funds over the telephone, using modern telecommunications networks just like bond and foreign exchange traders. At large financial institutions federal funds traders might be on the same floor with the traders in other markets so they can easily get information about what is happening in government securities and other short-term markets.

The Traders' Bids and Asks

The federal funds market is essentially a "double auction" market in which buyers bid and sellers ask different prices on overnight loans of federal funds.[6] Buying federal funds means borrowing Fed balances overnight. Selling federal funds means loaning Fed balances overnight. The price of federal funds is thus the overnight interest rate. Financial institutions hold Fed balances (again, deposits at the Fed) for two main reasons: (i) to satisfy reserve requirements and clearing balance requirements and (ii) to facilitate large payment flows. Some financial institutions have more Fed balances than they desire to hold on a given day; they will seek to sell (loan) the balances overnight to some other financial institution; their traders will try to get the highest price (overnight interest rate) that they can. Other financial institutions may find that they have fewer Fed balances than they desire; their traders will try to buy (borrow) Fed balances at the lowest price (interest rate) they can. When a buyer's bid is matched by a seller's ask, a trade takes place at the agreed upon interest rate. Buyers are rewarded for getting a low price on the market, and sellers are rewarded for getting a high price on the market.

At the start of the day federal funds traders usually have some expectation of whether their institution will be a net buyer or net seller that day. Large banks are frequently net buyers, though there are exceptions. A trader who anticipates having Fed

[4] See Furfine (2000a) for references to earlier models of the federal funds market.

[5] Note that Fed balances, which will be the quantity focused on in this paper, are quite a bit different from reserves. Fed balances do not include vault cash and do include required clearing balances. More specifically, Fed balances are equal to the sum of required balances plus excess reserves. Note that required balances include both required *reserve* balances and required *clearing* balances. Required reserve balances are the part of reserve requirements that are not satisfied with applied vault cash. Other terms sometimes used to describe Fed balances are "operating balances" or "aggregate supply of balances" or simply "balances."

[6] There is also a small market in longer-maturity federal funds, but here I focus on the one-day (overnight) market.

balances to sell during the day may ask a high interest rate early in the day and then wait until the market starts to move before lowering the ask. That trader is hoping that another institution will suddenly be short of Fed balances and be willing to pay that price. A trader who anticipates having to buy funds may bid a low price and wait in the same way. Of course the unexpected can happen. A bank may get an unexpected payment outflow or inflow and suddenly change the strategy. Or a trader may hear a rumor and decide to enter the market early. Banks want to avoid being in an overnight overdraft position; the Fed charges a large penalty (400 basis points over the effective federal funds rate for that day) for overnight overdrafts.

The pace of federal funds trading varies a lot throughout the day, but it has a regular daily rhythm. The market is frequently very slow early in the day, and it is not unusual for the traders at even large financial institutions to have no trades for several hours in the morning; but trading frequently becomes quite chaotic toward the end of the day. Large financial institutions therefore need to have several traders available to handle large changes in volume.

The Federal Funds Brokers

A large portion of federal funds trades goes through federal funds brokers.[7] The brokers are linked electronically to their clients—the federal funds traders—though they are generally at different physical locations. Of course, the main role of the brokers is to match up the buyers and the sellers, but the brokers also provide information to the buyers and sellers about trends they see in the market during the day. Brokers record (sometimes using white boards on the walls of the large rooms where they work) all the outstanding bids and asks from the traders who are their clients. Furfine (1998) estimates that there are about 1,000 participants in the federal funds market with about $144 billion in trades each day (these numbers refer to 1998). Any one brokerage firm therefore needs to have many individuals handling the bids and asks of different clients.

At some times during the day the highest outstanding bid recorded on the white board is lower than the lowest outstanding ask recorded on the white board; at those times no trades take place (though if there is a small difference a broker may get on the phone and encourage either the buyer or seller to move a little bit). When a buyer calls in a bid that is equal to the existing ask of a seller (or vice versa), the broker notes the match, informs the buyer and the seller, and receives a commission for making the match. The buyer and the seller then arrange for the funds to be transferred from the selling institution to the buying institution, frequently through FedWire, the transfer system operated by the Federal Reserve for large value payments. (Some loans are made between small banks and their correspondents and are simply noted by bookkeeping entries). When trading volume increases—frequently toward the end of the day—there is no time to actually record all the bids and asks because they are changing so quickly; then there is only a record of the bilateral trades.

The Trading Desk

One thing the traders look for during the day is the size of the open market purchases or sales by the Trading Desk of the New York Fed. Temporary injections and temporary withdrawals of Fed balances through open market operations generally occur at one time during the day. Currently (since April 1999) the Trading Desk enters the market during a ten-minute interval around 9:30 a.m. with the exact minute determined randomly by rolling a die. The Trading Desk uses repurchase agreements (RPs) to increase Fed balances and matched sale-purchase agreements (MSPs) or reverse RPs to reduce Fed balances. To be sure, neither the Trading Desk nor any other part of the Fed participates directly in the federal funds market. Rather, the Fed participates in the RP or the Treasury security market, increasing or decreasing the level of Fed balances, not the flow of federal funds transactions among banks.

Many of the temporary injections and withdrawals of Fed balances by the Trading Desk (via RPs and MSPs) are intended to *offset* predicted short-run changes in the supply of Fed balances (due, for example, to changes in currency or U.S. Treasury balances) or in the demand for Fed balances. But some injections or withdrawals are aimed at moving the supply of Fed balances in order to increase or decrease pressure on the federal funds rate and thereby bring the federal funds rate closer to the target. It is very difficult to distinguish empirically between (i) Trading Desk purchases and sales that are designed to decrease or increase pressure

[7] Furfine (1998) provides a very rough estimate that about four-fifths of funds trades go through brokers; this is based on an extrapolation (using FedWire transactions growth) from the late 1980s when the total daily volume of brokered trades in the federal funds market was estimated to be about $70 billion.

on the federal funds market (to guide the interest rate toward its target) and (ii) open market operations designed to offset factors that shift the supply or demand for Fed balances. The latter appear to be much greater in volume than the former especially if permanent increases are included. Over longer periods of time, by far the greatest reason for permanent injections of Fed balances is to offset increases in currency.[8]

The flow of trades in the federal funds market (about $150 billion per day) is more than ten times greater than the stock of Fed balances. This big stock-flow difference is true for many individual banks; for example, a bank might hold less than $100 million in balances at the Fed and buy (borrow) $4 billion in the federal funds market on a typical day. Such a bank buys funds in the federal funds market to make loans in other markets; the banks that sell federal funds to the large bank must have more deposits than the sum of their loan opportunities and Fed balance needs. That federal funds purchases are a source of funds for other loans creates a connection between the federal funds rate and the other loan rates.

The Daily Effective Federal Funds Rate

It is clear from the above discussion that each bilateral trade in the federal funds market can take place at a different price. In fact, Furfine (1999), using data on individual federal funds trades collected from FedWire records, has documented that there are significant price differences, even at the same time during the day. Some of these price differences are correlated with the size of the banks, the volume of their trading, and the size of the transaction—factors that may be related to risks or transaction costs. But there are also differences within the day for the same banks. The commonly reported *daily effective federal funds rate* is a weighted average of the rates on trades reported by brokers. It is this daily effective rate that the Trading Desk is instructed by the FOMC to keep on average around the target rate.

The time series analysis and the model in this paper focus on the behavior of this effective rate. The effective rate incorporates a lot of cross section and interday volatility. For example, on December 30, 1999, there was more than a 100 basis point difference between the rate on early morning trades and the daily effective rate (Fisher, 2000, Chart 30). Some single trades at the end of the day can be several percentage points away from the effective rate on that day when a bank finds itself to have a shortage or surplus of funds at the last minute. In fact, there is much more price information in the market than the effective rate itself. An important task for future research is to use microeconomic data of the kind Furfine (1999) has assembled to study price formation and price dynamics in the federal funds market in more detail.

THE RECENT TIME SERIES PROPERTIES OF THE DAILY EFFECTIVE FEDERAL FUNDS RATE

Figure 1 shows the daily effective federal funds rate along with the target rate for the period from July 1998 to September 2000. I focus on this period because it is after the big changes mentioned in the opening paragraphs of the paper, including the public announcements of changes in the target rate and lagged reserve accounting. It also incorporates a relatively large number of changes in the target rate—three down and six up. In contrast there were only two target rate changes in the two years before this—one in 1996 and one in 1997. With daily data there are 550 observations on the effective federal funds rate during this period. Weekends are excluded because the federal funds market is not open on weekends. (Holidays not falling on a Saturday or Sunday were not excluded so as to keep a repeating 10-day interval—the maintenance period—during the sample.)

Volatility

It is clear from Figure 1 that the Trading Desk has been very successful in following the FOMC's directive to keep the daily effective federal funds rate at an average around the target. Figure 2 shows the deviations in percentage points between the effective rate and the target. The average deviation for this period was 0.1 basis points, so there is clearly no bias between the target and the actual rate that persists over time. The maximum deviation was –150 basis points and occurred on December 31, 1999, the day of the century date change. The standard deviation of the actual rate-target rate deviation over this period was 18 basis points.

Note that the volatility of the federal funds rate has been declining during this period; the standard

[8] More lasting changes in the factors affecting the supply of Fed balances are offset by outright purchases of Treasury securities; these can occur at any time during the day. In 1999 the total System open market portfolio increased by $44 billion.

Figure 1

Federal Funds Rate and Target Rate
July 1998–September 2000

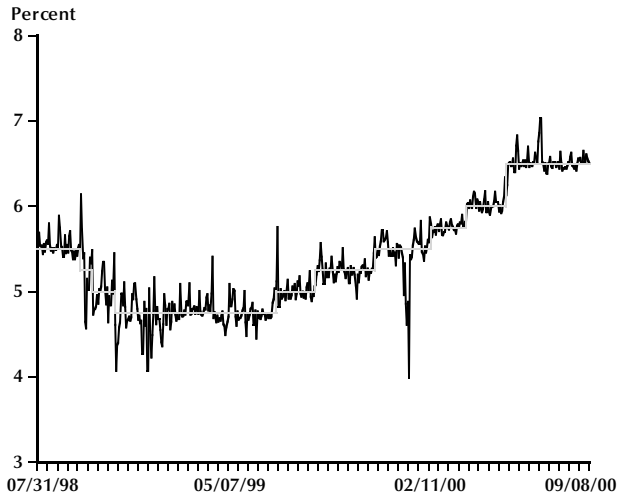
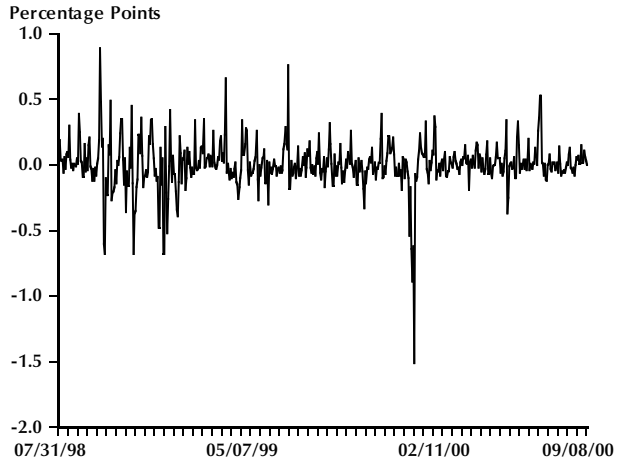


Figure 2

Deviations of the Federal Funds Rate from Target
July 1998–September 2000



deviation was 20 basis points from August 1998 to July 1999 and 16 basis points since July 1999 (even including the large deviations at the end of 1999). During this two-year period sweep accounts continued to lead to lower levels of required reserve balances, which some had thought would increase volatility; the decline in volatility indicates either that the lower level of required reserves has not made reserve management more difficult for banks or that there have been countervailing forces. One such force has been greater certainty about the federal funds target; other possibilities are better information about payment flows, perhaps made possible with lagged reserve requirements.

Serial Correlation and Persistence

The deviations of the federal funds rate from the FOMC's target rate are not serially independent, but they show rapid mean reversion for daily data. Let d_t be the deviation of the daily federal funds rate from the target federal funds rate; that is, $d_t = (r_t - \rho_t)$ where r_t is the daily effective federal funds rate and ρ_t is the target rate. Equation (1) below is a first-order autoregression in the deviation (d_t) estimated over the period from August 3, 1998, to September 8, 2000.

(1)

$$d_t = \underset{(0.038)}{0.4221} d_{t-1} + \underset{(0.007)}{0.0008} + e_t \qquad \sigma_e = 0.166, \qquad R^2 = 0.18.$$

The coefficient on the lagged dependent variable has a very low estimated standard error and is very significantly different from zero and one.[9] On average only about 6 percent of a deviation persists beyond two days.

Serial correlation of the deviations has increased during the past two years, as measured by the coefficient on the lagged dependent variable. For the days from August 1998 to July 1999, the coefficient on the lagged dependent variable was 0.3690; from August 1999 to September 2000 it was 0.4932. This increase in persistence parallels the reduced volatility and could reflect greater smoothing of shocks, either through the actions of market participants or by the Trading Desk.

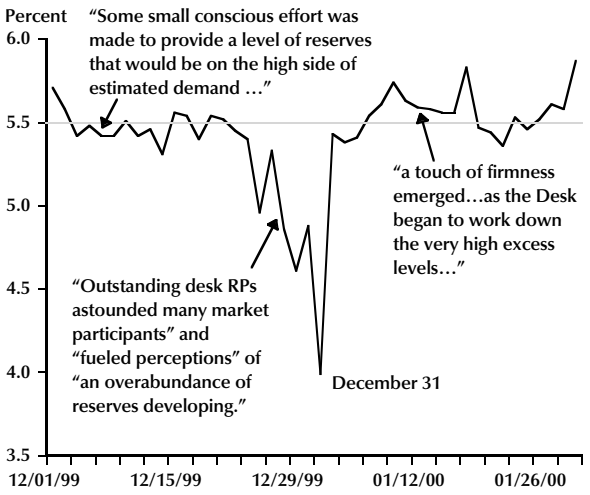
Settlement Wednesday and Other Maintenance Period Effects

During this period there was only a small difference between the average funds rate on the last day of the maintenance period—settlement Wednesday—and other days. The settlement Wednesday average of the funds rate deviation (d_t) was 6 basis points *below* the average of zero for the whole period. This is in contrast to the finding of Hamilton (1996) in which the funds rate traded an

[9] The simple first-order assumption seems sufficient to capture the time series behavior during this period. See Rudebusch (1995) for estimates during earlier periods and for a discussion of outlier issues.

Figure 3

Federal Funds Rate at Century End
(quotes from Fisher, 2000)



average of 14 basis points *above* average on settlement Wednesdays in earlier years. The report by Fisher (2000) also notes this change, with the settlement Wednesday deviations being positive in 1996 and 1997 and negative in 1998 and 1999. The average negative deviations seem to be persisting into the year 2000. The tendency of federal funds to trade relatively low on Fridays and relatively high on Mondays is continuing.[10]

The Century Date Change Deviation

It is instructive to examine what happens during unusually large and persistent deviations of the daily effective rate and the target rate. The largest and most persistent deviation during the last several years was the one that occurred at the end of 1999. Figure 3 shows this deviation on December 31, 1999, as well as on the days before and after; Figure 3 also superimposes some relevant passages from the annual report from the System Open Market Account (Fisher, 2000) that explains what the Trading Desk was doing during this period.

Clearly the deviation was related to the century date change and to the activities of the Trading Desk to make sure that there was adequate liquidity during this period. For example, during the whole month of December the Trading Desk was putting a little more reserves into the market than it otherwise would have done; this led to a federal funds rate slightly below the 5 percent target rate for much of December, but still within the 20 basis point standard deviation of the whole sample period. However, the rate fell sharply in the days after the Christmas holiday and especially on New Year's Eve. During the last days of the year the Trading Desk was doing a large amount of temporary open market purchases (through RPs) to offset unusually large drains on Fed balances due to both increased demands for currency (by the public and by the banks) as well as increases in U.S. Treasury balances. The large amount of RPs by the Trading Desk was noted by federal funds traders; many thought it would create a surplus of Fed balances in the market. Apparently they did not know that the drain on reserves was as strong as it was. This perception of "an overabundance of reserves" led to funds rate trades at prices well below the FOMC's target; the daily effective rate fell below 4 percent.

The situation quickly reversed when trading began in January 2000. In fact, the effective rate traded above the target for most of January. According to the annual report in Fisher (2000), this pressure in the market occurred as the Trading Desk was rapidly offsetting the effects of declines in currency demand. Evidently it was this increased pressure on the reserve market that caused the federal funds rate to trade high for all of January.

Target Rate Changes

How has the federal funds rate moved at times of changes in the target rate during the 1998-2000 period? Figure 4 shows the changes of the federal funds rate for the nine changes in the target rate during this period. The exact dates of the target rate change are shown in Table 1. The actual federal funds rate moves very quickly around the times of the changes in the target rate.

Table 2 shows that there has been a sharp reduction in the volatility of the funds rate around the times of the target rate changes. The standard deviation of the difference between the funds rate and the target rate was about 28 basis points for the first four target range changes in Figure 4 and only 12 basis points for the latter five. In three of the latter five cases the funds rate appeared to move significantly before the change in the target, indicating

[10] Dan Thornton has pointed out to me that, because the distribution of funds rates on settlement Wednesday is skewed to the right, negative deviations have usually been more common than positive deviations, even though the average deviation (over the period from 1974 to 1997) has been positive.

Figure 4

Nine Changes in the Target for the Federal Funds Rate: 1998-2000

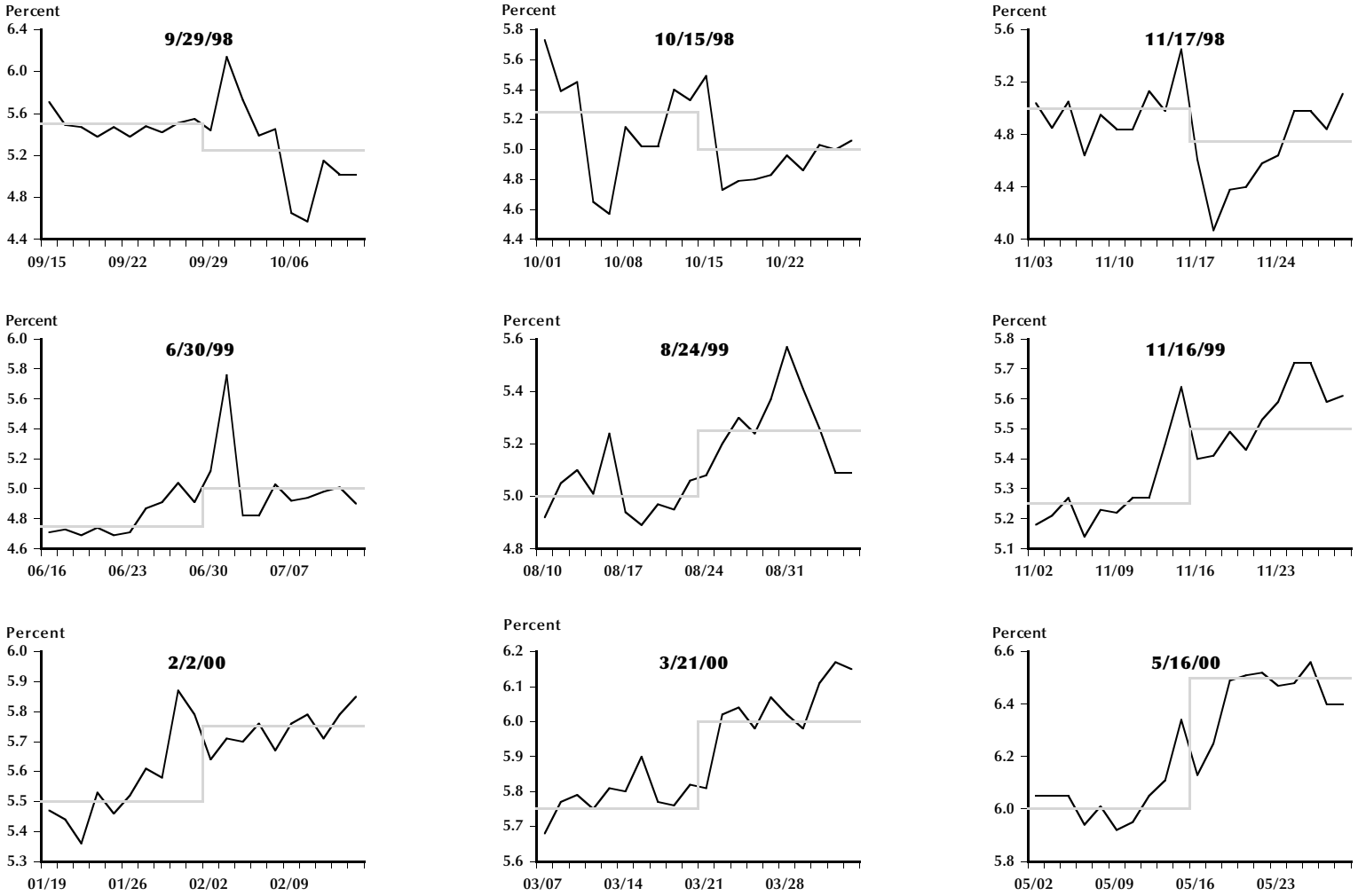


Table 1

Changes in Target Federal Funds Rate: 1998-2000

Date	Day of week	Day of maintenance period	New target for funds rate	Change in target rate
September 29, 1999	Tuesday	4	5 1/4	–1/4
October 15, 1998	Thursday	6	5	–1/4
November 17, 1998	Tuesday	9	4 3/4	–1/4
June 30, 1999	Wednesday	10	5	+1/4
August 24, 1999	Tuesday	9	5 1/4	+1/4
November 16, 1999	Tuesday	9	5 1/2	+1/4
February 2, 2000	Wednesday	4	5 3/4	+1/4
March 21, 2000	Tuesday	9	6	+1/4
May 16, 2000	Tuesday	9	6 1/2	+1/2

Table 2

Behavior of Federal Funds Rate (FFR) Near Target Federal Funds Rate (TFFR) Changes

Date	Not Granger-causing F-test, probability value TFFR not →FFR	FFR not →TFFR	Volatility: standard deviation (basis points) during 20 day interval
September 29, 1998	.0005	.4360	34
October 15, 1998	.0881	.9434	29
November 17, 1998	.0123	.1560	27
June 30, 1999	.0001	.4077	21
August 24, 1999	.0090	.7587	13
November 16, 1999	.0142	.0107	13
February 2, 2000	.2442	.0171	12
March 21, 2000	.0090	.7587	8
May 16, 2000	.0142	.0108	1

perhaps that the target change itself was anticipated. Table 2 provides some rough statistical support for this visual impression about the timing. It reports the probability value of a test of the hypothesis that (i) the target rate does not Granger-cause the federal funds rate and that (ii) the federal funds rate does not Granger-cause the target rate. The first hypothesis is rejected uniformly, but the second is rejected only in the three cases mentioned above.

A Model of the Supply of Fed Balances

The Trading Desk determines the supply of Fed balances and tries to keep an appropriate supply relative to demand with the federal funds target in mind. Most of the transactions of the Trading Desk aim to neutralize other factors that shift the supply or the demand around. Each day the staff at the New York Fed and the Board compare notes on the factors that are affecting the supply of and the demand for Fed balances. As mentioned above, changes in currency in circulation and changes in Treasury balances are two of the factors that must be offset. The Fed does a good job at estimating and thus neutralizing these flows. A good case study is how the remarkable increase in currency and in U.S. Treasury cash balances in December 1999 was offset. These increases, along with a similarly motivated increase in demand for cash by foreign central banks, led to a huge drain on Fed balances: $70 billion in only two weeks from mid-December until

the end of the year. The Desk neutralized this drain by doing about the same amount of RPs. This $70 billion of purchases is many times greater than the total amount of Fed balances, so efforts to alter the pressure on Fed balances would have to involve a much smaller amount of purchases and thus would be very hard to observe simply by looking at open market transactions. Moreover, pressure up or down can be exerted by doing slightly fewer or more RPs (or MSPs) than would otherwise be required to offset the other factors. For example, an increase in Fed balances of 2 percent would be only about $200 million—that is, only a fraction of a percent of the offsetting purchases during this period.

A Trading Desk Reaction Function

In any case, the directive to the Trading Desk is to foster conditions in the federal funds market that will keep the federal funds rate close to the target. That means that the Trading Desk must adjust the supply of Fed balances up and down to keep the federal funds rate close to the target even as it is making much larger offsets to other factors that shift the supply and demand for Fed balances. If the Trading Desk follows such a procedure systematically, then one should be able to model the procedure with some kind of a reaction function. One possibility, which seems to capture the words of the directive and the way market participants think about the Desk's actions, is the reaction function shown in equation (2):

$$b_t = b_{t-1} + \beta\left(r_{t-1} - \rho_{t-1}\right), \tag{2}$$

where b_t is the supply of Fed balances on day t, r_t is the daily effective federal funds rate on day t, and ρ_t is the target federal funds rate. It is important to recognize that equation (2) is designed to focus on Trading Desk activities over and above those that are taken to offset *predicted* shifts in supply and demand. For example, an increase in currency will require an open market purchase simply to keep b_t from declining. That kind of Trading Desk action is implicit in equation (2). To make such actions explicit one would have to add shift terms to equation (2) and then have the Trading Desk offset those terms; but that would only clutter the notation with no gain in realism. Similarly, as mentioned above, some trading desk activity is designed to offset shifts in the demand for Fed balances, perhaps due to predicted seasonal factors or to known regulatory changes. According to the notation here, the symbol b_t is assumed to be adjusted for these seasonal and other predicted changes in demand, in the same way that a seasonally adjusted series for the money supply adjusts for holiday demand for money.[11]

Now let me discuss the form of the reaction function in equation (1) and its rationale. The parameter β is assumed to be positive. The time subscript on the target rate emphasizes that this rate does change, but of course it changes much less frequently than the daily effective rate. The target rate is determined by the FOMC's assessment of developments in the broader economy—such as inflation or real GDP—which usually change over a period of weeks or months rather than days. In this paper I will consider one-time changes in the target rate and see how the daily rate moves in response. I will assume that the Trading Desk follows this reaction function or policy rule in the analysis that follows. Since this analysis assumes that traders are forward looking, it is important to also stress that we assume that traders know that the Trading Desk follows such a reaction function and that they expect the Trading Desk to continue to follow it in the future. That equation (2) is expected to be followed in future periods has a big impact on how equation (2) actually affects the federal funds rate.

The reaction function in equation (2) captures several features of the Trading Desk's policy. First, according to equation (2) the FOMC increases the supply of reserves if the daily rate is above the target rate; presumably this increase in supply will reduce pressures on the federal funds market and tend to bring the actual rate back down to the target rate. Similarly, the Desk decreases the supply of reserves if the daily rate is below the target rate; the decreased supply then puts upward pressure on the federal funds rate. The coefficient of 1 on the lagged level of Fed balances implies that it is only the *change* in reserves that responds to the deviation between the actual rate and the target rate. The Desk therefore does not try to achieve a *level* of reserves (even in the long run), but simply changes reserves to bring the daily rate into line with the target rate. It is possible that the coefficient on the lagged reserve balances is less than 1, but that would imply a unique level of reserves corresponding to the gap between the daily rate and the target rate.

[11] By abstracting from the actions taken to offset known or predicted changes in supply and demand, I do not mean to imply that they are not important or interesting in their own right under current policy in the United States. On the contrary, as mentioned in the discussion above, such actions represent a major fraction of the actions of the Trading Desk's time and analysis.

Another important feature of equation (2) is that the Trading Desk is assumed to make these adjustments in Fed balances with a lag; that is, $(r_{t-1}-\rho_{t-1})$ appears in equation (2) rather than $(r_t-\rho_t)$. As discussed above, the Desk intervenes in the market in the morning, before most of the day's trading takes place and before the effective daily rate is even calculated. The first time the Desk can intervene under its current procedures is therefore the next day. Even if the Desk wanted to react on the same day, the timing of trading in the federal funds market described above—in particular with so much trading and price movements late in the day—would make it difficult to do so. To be sure, the lag could be longer or distributed over time, but equation (2) seems to capture an important feature of the necessarily lagged response in a simple way.

Equation (2) could be made more complex. The reaction could be nonlinear or the parameters could vary over time, or at least within days of the maintenance period. Another possibility would be to react to other variables, perhaps some other market interest rate, the maximum price in the federal funds market during the day, or the price of the last few trades, along with the effective rate.[12] Such additions could provide a better description of actual behavior or work better from a normative perspective. But equation (2) without these modifications seems like a good place to begin.

Comparison with the Classic Supply Curve for Reserves

Equation (2) implies that the supply of Fed balances is a vertical line in a diagram with the current federal funds rate on the vertical axis and Fed balances on the horizontal axis. The vertical line moves back and forth over time as the daily funds rate fluctuates.

How does equation (2) differ from earlier graphical models of Trading Desk operations, such as the traditional supply and demand model by Gilbert (1985)?[13] One difference is the explicit description of the dynamics; rather than simply considering a one-time shift in the supply of balances, equation (2) is a contingency plan for many such shifts. Another difference is that the supply of Fed balances in equation (2) is vertical throughout its whole range. In Gilbert (1985), the supply of Fed balances becomes sensitive to the contemporaneous federal funds rate when that rate goes above the discount rate. Thus the supply curve has a positive slope over part of its range. The higher the rate the more incentive banks have to borrow from the discount window; since more borrowing from the Fed increases the supply of Fed balances, this creates a positively sloped section of the supply curve. Given the reluctance of banks to borrow from the discount window and the apparent low interest rate elasticity of borrowing, it is a good approximation to assume that the supply curve is vertical throughout the range.[14]

Although many textbook uses of the classic supply of reserves model are vague about the time periods, Gilbert (1985) was careful to point out that the model applied to the (then one-week) maintenance period rather than to individual days. Also, "reserves" in the classic model (Gilbert, 1985) includes vault cash; in equation (2) the quantity variable refers only to the balances that financial institutions hold at the Fed. These balances include required reserve balances, required settlement balances, and excess reserves. There is a big difference between these measures. For example, in November 1999, required reserves were about $40 billion of which about $34 billion was applied vault cash, making required reserve balances about $6 billion. Required clearing balances were about another $6 billion, and excess reserves were about $1 billion. Thus, Fed balances were about $13 billion.

A MODEL OF THE DEMAND FOR FED BALANCES

Recent microeconomic research on the demand for Fed balances by financial institutions has emphasized the intertemporal nature of the decision to buy and sell Fed balances in the federal funds market. For example, Hamilton (1996) stressed that banks face an average reserve requirement over the 10 days of the reserve maintenance period (Fridays count three times as much in the average because the market is closed on weekends). This averaging

[12] Modern research on policy rules has emphasized that it may be optimal to react to variables that are not in the objective function. For example, it is optimal for the FOMC to react to real variables when adjusting the federal funds rate target, even if real variables are not in the objective function.

[13] Woodford (2000) uses such a model to discuss interest rate setting by the Fed. Demand side differences between the model in this paper and the traditional model are discussed below.

[14] Recent evidence of this well-known reluctance is provided by Furfine (2000b), who argues further that low utilization of the special borrowing facilities around the time of the century date change indicated that banks are very reluctant to borrow from the Fed even when they are encouraged to do so.

would seem to allow federal funds traders to eliminate, through arbitrage, any predictable increases or decreases within each reserve maintenance period. For example, if a bank expects the federal funds rate to rise tomorrow, the bank should be willing buy Fed balances today and sell Fed balances tomorrow to take advantage of the differential; but such action, if taken by enough banks, will tend to eliminate the difference between today's rate and tomorrow's expected rate. Thus, the federal funds rate should not have any predictable movements, at least during the maintenance period. The reason that the rate does have predictable movements, according to Hamilton's (1996) model, is that transactions costs and limits on borrowing prevent full arbitrage. His model shows, for example, why the federal funds rate was regularly 14 basis points on average higher (during the period he studied) on settlement Wednesdays than on other days.

Furfine (2000a) presents a model that lays out the full intertemporal profit maximization problem for the typical bank in the federal funds market. His time horizon is many maintenance periods. He derives a first-order condition in which today's federal funds rate depends on expectations of the next day's federal funds rate. However, there are predictable movements in the rate because of the great reluctance of banks to be left with an overnight overdraft, as discussed above.

Furfine's (2000a) model also incorporates payment flows as a motivation for holding Fed balances, even for banks without binding required reserve balance or required clearing balance constraints. An examination of FedWire transactions shows that banks make about $2 trillion in large-value payments each day, which is many times their stock of Fed balances. These FedWire payments create instantaneous debits or credits to a bank's Fed balances, and the payments can exceed Fed balances by a factor of several hundred. A bank's demand for federal funds is affected by this payments activity. Banks can have daylight overdrafts at relatively low cost; but, to avoid overnight overdrafts, banks buy Fed balances in the funds market whenever payment outflows would bring their accounts into overnight overdraft.

Guthrie and Wright (2000) also derive the demand for balances at the central bank from a detailed optimization problem. Their analysis is designed to describe operations of the New Zealand money markets where the Bank of New Zealand provides both a lending and a borrowing facility with a spread between the lending and borrowing rates. (The Bank of Canada recently adopted a similar method for controlling the overnight interest rate.) The demand for balances in the Guthrie-Wright analysis is thus based on the opportunity cost of borrowing from or lending to the Reserve Bank rather than to the market.

An important feature of Furfine's intertemporal model is that the expectation of the next day's federal funds rate affects the demand for Fed balances today. The effect does not have to be full arbitrage, where today's funds rate moves one-to-one with expectations of tomorrow's rate or where arbitrage completely eliminates the difference between the expectation of tomorrow's rate and today's rate. A simple equation that reflects this effect on the demand for Fed balances is:

$$b_t = -\alpha\left(r_t - \gamma E_t r_{t+1}\right) + \varepsilon_t, \tag{3}$$

where ε_t is a demand shock (which might be serially correlated) and E_t is the conditional expectation operator based on information through day t. The parameters α and γ are constant with $\alpha > 0$ and $1 \geq \gamma \geq 0$. As defined above, b_t is the stock of Fed balances and r_t is the daily effective federal funds rate.

Equation (3) incorporates the impact of the expectation of the next day's interest rate using the rational expectations assumption. Traders are assumed to forecast the next day's rate using the information available during the current day, including the Trading Desk reaction function and the demand for Fed balances. But equation (3) falls short of a full arbitrage assumption in two ways.

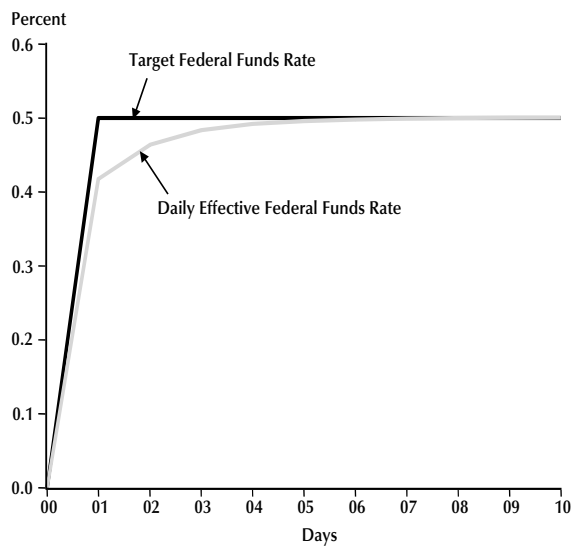
First, the expectation of the next day's rate is multiplied by a parameter γ that may be less than 1. Thus there may be a down-weighting of the next day's rate. Furfine (2000) allowed for such a down-weighting in estimating his model and found that the value was less than 1, but not significantly so.

Second, even if γ were equal to 1, equation (3) has a finite response of the stock of Fed balances to the difference between the current funds rate and the next day's funds rate. Only if α were infinite would r_t have to equal $\gamma E_t r_{t+1}$ (or $E_t r_{t+1}$ if γ was 1). In fact, transactions costs and high penalties for overnight overdrafts suggest that α should be less than infinity and possibly quite small.

As with the supply of Fed balances (equation (2)), the assumption about the demand for Fed balances (equation (3)) is an approximation. The size of both α and γ is likely to vary across days in the maintenance period. Fridays count higher in the

Figure 5

Simulated Impact on the Daily Effective Federal Funds Rate of an Increase in the Target Funds Rate of 0.5 Percentage Points on Day 1



average, and on settlement Wednesdays the relevance of the next day's rate is less because the arbitrage connection between maintenance periods is limited by restrictions on carry-over provisions.

In fact, in discussing this paper with others I have found that there is a great deal of difference of opinion among economists and traders about the size of γ and the theory that underlies this intertemporal effect. Most agree that γ is greater than zero, which is the important property needed for the anticipatory movements in the funds rate as shown below. But it would be very useful to obtain estimates of γ using daily Fed balance data and measures of all the factors that cause equation (2) to shift.

How does this demand equation differ from that of the demand equation in Gilbert (1985)? An important difference is that the interest rate elasticity of the demand for Fed balances is not derived from the interest rate elasticity of money demand through reserve requirements. Even when legal reserve requirements are binding, the fact that they are lagged means that there is a zero interest rate elasticity of required reserves. Another difference is that the expectation of the next period's interest rate enters the demand equation. It is this difference that makes the model interesting (in my view) and enables it to explain a number of the features in the data described above.

CHANGES IN THE DAILY EFFECTIVE RATE IN RESPONSE TO SHOCKS

Equations (2) and (3) together are a complete dynamic stochastic rational expectations model of the deviations of the daily effective federal funds rate from the target federal funds rate. Using these equations we can evaluate the ability of the model to explain some of the empirical regularities in the data, including the phenomenon that the federal funds rate changes immediately after a target change before any open market operations by the FOMC designed to move the rate. An advantage of the simple linear model is that it is very easy to solve.

Simulations of a Change in the Target Federal Funds Rate

Suppose that the FOMC votes to increase the federal funds target by 50 basis points. What does the model predict about the impact on the daily effective federal funds rate on the day of the announcement? Recall that the Trading Desk would not conduct an open market operation designed to change the supply of Fed balances on the day of the FOMC press release. Similarly, the Trading Desk reaction function in equation (2) would imply no change in the supply of Fed balances. Any such response will be lagged by one day, as is clear from equation (2). However, since the Trading Desk is assumed to follow equation (2) day after day, we know that, unless the funds rate immediately jumps by 50 basis points to the new target, the Trading Desk is expected by federal funds traders to decrease Fed balances on the next day. That is, $E_t b_{t+1}$ falls as the target rate goes above the daily rate on day t.

However, this expectation will cause the funds rate to rise on the day of announcement. Here is the intuitive reasoning: If Fed balances are expected to fall on day $t+1$, then the expectation of the federal funds rate on day $t+1$ must rise according to the demand equation (3); that is, $E_t r_{t+1}$ rises. But if the federal funds rate is expected to rise on day $t+1$, then the federal funds rate will rise today because demand shifts out today with the expectation of the future funds rate in the demand function.

We can simulate equations (2) and (3) to see the size and the patterns of the change in the federal funds rate. Figure 5 shows the results for a particular set of parameter values. In particular, I assume that $\gamma = 0.9$, $\alpha = 0.3$, and $\beta = 0.1$.

The federal funds rate increases by 41.7 basis points on the day of the 50 basis point announcement, even though there is no open market operation on that day designed to bring about this change. It is the expectation of a change in the next period that brings about this change. Note that most of the funds rate increase takes place immediately for these parameter values. On the next day the federal funds rate moves up by 4.7 more basis points for a cumulative increase of 46.4 basis points; on day 2 it increases further to 48.3 and continues to converge geometrically to the total 50 basis point change in the target.

Note that the increase in the funds rate from day 1 to day 2 is consistent with the Fed balance demand equation and with the supply of Fed balances not increasing at all on day 1: With r_1 up by 0.417 and r_2 up by 0.464, we have that $r_1 - 0.9E_1r_2 = 0.417 - 0.9(0.464) = 0$, which is consistent with b_1 remaining at zero. However, from day 2 to day 3 the increase in the daily rate increases by a smaller proportion so that $r_2 - 0.9E_2r_3 = 46.4 - 0.9(48.3) > 0$; this reduces the quantity of Fed balances demanded. This reduction is consistent with the fact that the Trading Desk must, if it follows its rule, reduce the supply of Fed balances on day 2.

In sum we have shown that the model can explain why the daily effective funds rate might move on the day of announcement of the target change even though there has been no change in balances. There is the appearance of what Guthrie and Wright (2000) refer to as an *open mouth operation*: the announcement of the target rate change brings about an actual rate change of almost the same magnitude without any immediate change in Fed balances. However, as the model shows, if expectations are rational the Trading Desk must actually respond to any realized differences between the funds rate and the target rate; in other words, its actual policy must be consistent with the announced policy. Otherwise, funds traders will soon begin to expect that there is some other policy reaction function at work and expectations will form in a different manner.

To be sure, while the movements of the federal funds rate at the times of the federal funds rate target changes (shown in Figure 4) seem to be getting closer to the movements in the targets, the timing is not as precise as the model. There are big differences in the exact time that the funds rate changes: Sometimes the rate changes before the day of the announcement and other times with more of a delay.

Moreover, the model may be too successful. There is evidence from federal funds futures markets that changes in the federal funds rate target are sometimes anticipated by the market many days or even many weeks before they actually take.[15] In such cases the model in equations (2) and (3) suggests that the actual federal funds rate should move somewhat with these anticipations, yet such movements are hard to detect. One explanation consistent with the model is that the Trading Desk offsets the pressure for the current rate to change, but further research is needed to determine whether these effects can be modeled accurately.

Simulation of the Effect of a Shock to Fed Balances

How rapidly does the federal funds rate return to the target funds rate after a shock to the demand for Fed balances? Figure 6 shows the simulated response to a permanent shock to equation (3) on day 1. The daily rate immediately increases on day 1, but the Trading Desk does not respond immediately. The rate begins to move back to target on day 2, and within three or four days the shock's effect has largely disappeared. The persistence of the shocks is very similar to the persistence of the deviations from the federal funds rate from target observed in the data and reported in equation (1). This similarity lends further support to the model.

Graphical Illustration

Figure 7 provides a graphical illustration of the supply and demand model and illustrates the simulations of an increase in the target federal funds rate discussed above. The announcement causes the demand curve to shift because the fund rate is expected to rise. Thus the initial impact is an increase in the funds rate with no change in supply. Eventually supply does move as the Trading Desk follows the reaction function.

CONCLUSION

The simple model of the supply and demand for Fed balances presented in this paper is capable of explaining the "open mouth operation" phenomenon: Changes in the target federal funds rate cause changes in the actual federal funds rate with little or no immediate action by the Trading Desk. However, traditional "open market operations" are

[15] See Poole and Rasche (2000).

Figure 6

Response of the Federal Funds Rate to a Shock to the Demand for Reserve Balances on Day 1

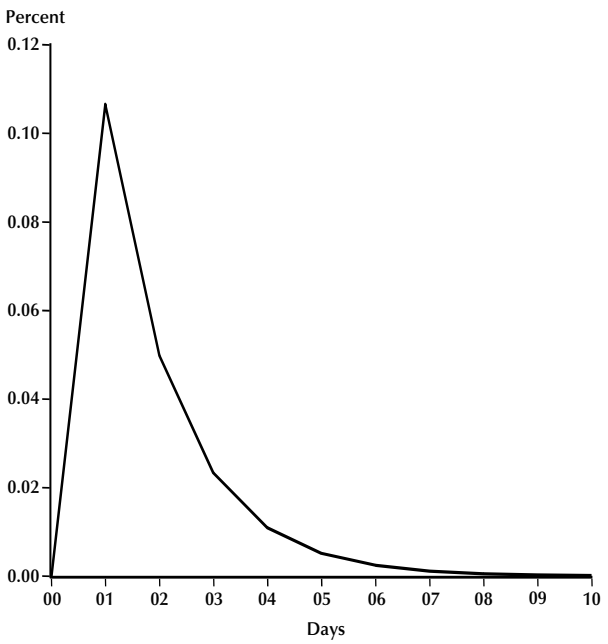
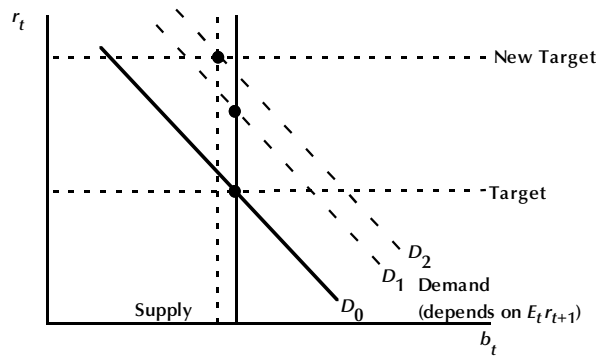


Figure 7

Graphical Illustration of an "Open Mouth" Effect



NOTE: This Figure corresponds with Figure 5. When the target rate increase is announced the demand curve shifts from D_0 to D_1 and without any changes in supply the funds rate r_t rises. In later periods the supply is reduced and the demand curve shifts up a little more.

the fundamentals that underlie these announcement or expectations effects. By specifying a reaction function of the Trading Desk and showing how the demand for Fed balances depends on expectations of the future federal funds rate, the model demonstrates how it is the "threat" of future open market operations that actually moves the rate. One reason why, at the times of target rate changes, the deviations of the federal funds rate from the target rate have diminished may be that traders are placing increased credibility on the reaction function of the Trading Desk as the FOMC has provided greater clarity about the target itself.[16]

The same model also seems capable of explaining the time series properties of the deviations of the federal funds rate from target. Future research should be aimed at getting more precise functional forms and parameter values so that the model can be used to investigate other policy issues, including the reduced volatility and increased persistence of funds rate deviations from target in recent years demonstrated in the paper. Time series data on prices on individual federal funds transactions will aid this estimation.

It is also important to examine how this type of model works in overnight markets in other countries. Blenck (2000) provides a useful comparison of how the Bank of Japan, the European Central Bank, and the Federal Reserve System intervene in the overnight markets. And the use of regular lending and borrowing facilities is becoming more common, with the Reserve Bank of New Zealand and the Bank of Canada being two prominent examples. The similarities and differences between these financial markets and the central bank operations will provide a useful perspective on the kind of model presented here.

[16] Three of the four cases where volatility of the deviations was high in this sample were associated with the easing at the time of the Russian and Long Term Capital Management crises. Thus, an alternative explanation for the decline in volatility is that traders found those changes to be more of a surprise.

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Commentary

Athanasios Orphanides

John Taylor is well known as a teacher of macroeconomics, and time and again he has challenged academics and practitioners alike to focus attention on how monetary policy should be formulated and analyzed in terms that are useful and practical.

Until perhaps a decade ago, most academic discussions of monetary policy were formulated in terms of the money stock, despite the fact that central banks invariably implemented monetary policy decisions through control of money market interest rates—the federal funds rate in the United States. Taylor's work, especially since his widely cited 1993 Carnegie-Rochester conference article, has been instrumental in shifting academic discussions closer to what is practically relevant for central banks. In large part, as a result, a substantial volume of useful research has been generated on how the Federal Reserve does or should set the federal funds rate to achieve and maintain good macroeconomic performance.

In this paper, Taylor goes a step further and examines the mechanics of open market operations and the day-to-day determination of the federal funds rate. The primary objective, once again, is to bring theory and academic discussion closer to practice. The standard textbook treatment of the mechanics of monetary policy implementation suggests that the Federal Reserve may need to perform some open market operations in order to change the federal funds rate. It also abstracts from open market operations when no change in the federal funds rate is desired. In practice, the Federal Reserve performs open market operations almost every day. But contrary to the standard textbook treatment, an open market operation may not be necessary to bring about a change in the intended federal funds rate. Once the Federal Open Market Committee (FOMC) announces a change in its target, market reaction moves the federal funds rate from trading around the old target to trading around the new one, most often without any action beyond the announcement, at least not right away.[1]

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I read the paper as an attempt to move towards a new textbook treatment of open market operations. It presents a simple model of the supply and demand for reserve balances. The model emphasizes expectations and is capable of explaining how the Federal Reserve appears to be able to change the federal funds rate simply by announcing its target for the rate, that is with "open mouth" operations instead of open market operations. Of course, detailed models of the supply and demand for reserve balances and the determination of the federal funds rate already exist, and Taylor points to some of them. The objective of this paper, in my view, is not to break new ground in developing detailed models. Instead, Taylor tries to distill key features of existing models in a stylized fashion that can serve as a new analytical framework for describing the reserves market. This effort deserves a lot of praise.

The paper begins with a clear summary of institutional features of the federal funds market and examines in some detail the daily differences between the Federal Reserve's target rate and the effective rate at which federal funds are actually traded. There have been a number of changes in these institutional details over the past few years and it is of great value to have a summary such as the one presented by Taylor in one place.

To better understand the need for a new textbook treatment of the reserves market, it is useful to review a couple of pertinent institutional developments that are only briefly mentioned in the paper. Reserves serve two important functions for depository institutions. The traditional use of reserve holdings by depository institutions is to meet reserve requirements. Reserve requirements have historically served a very useful role for implementing policy and have provided the primary motivation for earlier textbook treatments of the reserves market. Reserve requirements must be met on an average basis over a two-week maintenance period that ends every other Wednesday. As a result, they provide some scope for intertemporal substitution and adjustment in the daily demand for reserves that

[1] I follow Taylor's usage of the word "target" for the intended or expected level of the federal funds rate, but note that this usage provides an accurate description of the Committee's intent only in the past few years. The FOMC has made announcements immediately following a change in its policy stance since February 1994, but has explicitly announced changes in the federal funds rate expected to prevail following a policy action only since July 6, 1995. Prior to the 1990s, in particular, identification of the Committee's policy intent with a "target'' for the federal funds rate can be misleading.

Figure 1

Required Reserves and Applied Vault Cash

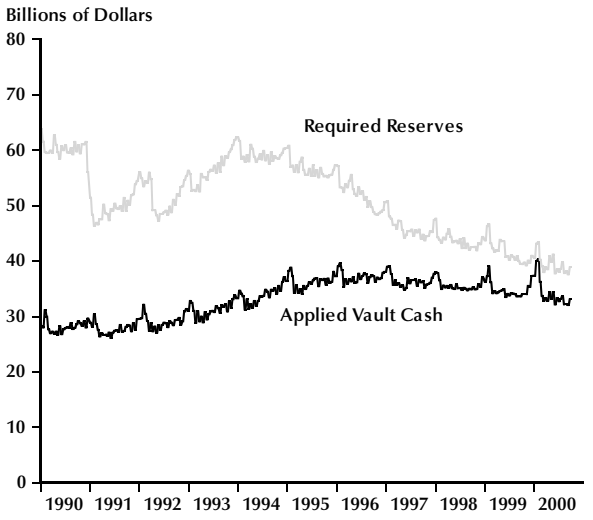
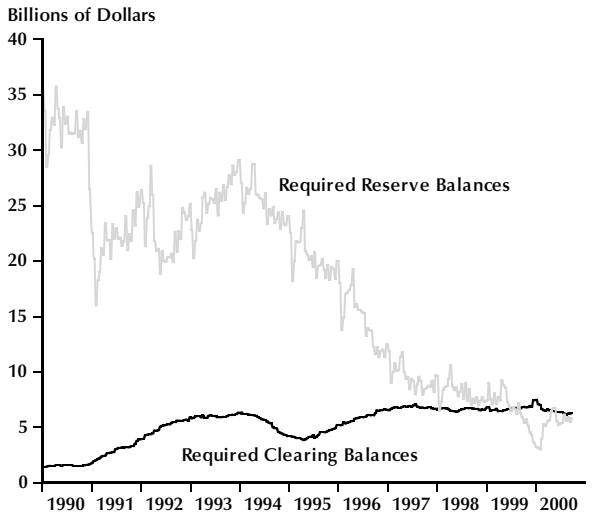


Figure 2

Required Reserve and Clearing Balances



help stabilize the federal funds rate.[2] The potential for this stabilizing role has diminished in the 1990s, however. During this decade, reductions in reserve requirements and improvements in technology that allowed depository institutions to sweep consumer transactions deposits into nonreservable accounts have resulted in significant reductions in required reserves. Depository institutions can meet reserve requirements by holding vault cash and by maintaining reserve balances at the Federal Reserve. As a result of the reductions in reserve requirements, many institutions can now satisfy their reserve requirements with vault cash alone with no need to maintain additional balances at the Federal Reserve. As shown in Figure 1, in the aggregate, the wedge between required reserves and vault cash that can be applied against reserve requirements, has diminished from over $30 billion in 1990 to about $5 billion in 2000.[3] As this wedge represents the amount of required reserve balances that depository institutions must hold at the Federal Reserve against reserve requirements, its significant reduction was one of factors that raised concerns about the potential for volatility in the reserves market over the past several years.[4]

The second, and increasingly more important function of reserve balances held by depository institutions in their accounts at Federal Reserve Banks, is to serve as a buffer against clearing needs. The Federal Reserve strongly discourages overnight overdrafts, which can result if an institution faces a payment order without sufficient funds in its account to cover it when the order is received. As a precaution against such overdrafts, depository institutions choose to maintain some overnight balances with the Federal Reserve even if the balances are not needed for satisfying reserve requirements. This consideration was of much less importance in the past when depository institutions needed to maintain greater required reserve balances as those balances could also serve this precautionary role, when needed. With required reserve balances diminishing, however, the role of such precautionary balances has been amplified.

Maintaining excess reserve balances is costly to depository institutions since these balances earn no interest. One way in which depository institutions can reduce this burden, to some degree, is by establishing so-called required clearing balances with the Federal Reserve. These provide some compensation in the form of credits that can be used to

[2] Carryover provisions that allow some excess reserve holdings in a maintenance period to be counted against requirements in the following (or previous) period ensure that some intertemporal substitution is possible across maintenance periods as well.

[3] Vault cash held in excess of reserve requirements is not counted as part of aggregate reserves as it cannot be used to meet reserve requirements. The applied vault cash series shown in Figure 1 does not include this surplus. Maintenance period averages are shown.

[4] See Clouse and Elmendorf (1997) and Bennett and Hilton (1997).

pay for Federal Reserve priced services. During the 1990s, depository institutions have negotiated higher levels of clearing balances and these balances have become much more important than they had been in providing a predictable demand for the total reserves that the Federal Reserve needs to supply through open market operations to achieve its federal funds target. As can be seen in Figure 2, as of 2000, required clearing balances have in fact surpassed required reserve balances. Despite the increase in required clearing balances, however, the total of required reserve balances and required clearing balances has been falling during the 1990s. With required balances falling, the Federal Reserve has been concerned that it may become more difficult to determine each morning the appropriate daily quantity of reserves to supply to the market, especially on days with increased clearing activity when the precautionary demand for reserve balances may rise significantly relative to other days. In light of these concerns, the Federal Reserve has taken some measures to foster stability and help reduce volatility in the federal funds market and this is the reason for some of the recent institutional changes surveyed in the paper. As confirmed by the detailed discussion in the paper, the Open Market Desk (the Desk) has not experienced difficulties implementing monetary policy in recent years, suggesting that these steps have been successful.

Taylor presents a disarmingly simple model for the supply and demand of reserve balances. The demand for reserve balances is

$$b_t = -\alpha\left(r_t - \gamma E_t r_{t+1}\right) + \varepsilon_t ,$$

and the supply for reserve balances (Desk reaction function) is

$$b_t = b_{t-1} + \beta\left(r_{t-1} - \rho_{t-1}\right).$$

Here b_t is the supply of reserve balances, r_t is the daily effective federal funds rate, and ρ_t is the target federal funds rate, all on day t.

Concentrating first on demand, the theoretical background is provided by work on the management of reserves in a stochastic environment.[5] To motivate Taylor's specification of the demand function, I prefer to consider separately two implications for the daily management of reserves that arise from the two key functions of reserves mentioned earlier.

The first is the desire and ability to arbitrage at least some of the demand for reserve balances across days. To meet an average reserve requirement over a two-week maintenance period, for example, it does not matter much whether a depository institution holds the reserves on day t or day $t + 1$. If the rate on federal funds is expected to fall tomorrow, demand for reserves will fall today. Thus, the demand for reserves will be decreasing in the spread, $r_t - E_t r_{t+1}$. The ability to intertemporally substitute reserve holdings across time is limited, of course, and consequently the elasticity of demand with respect to this spread between the rate at t and the expected rate at $t + 1$ cannot be infinite. This reasoning implies that the parameter α in the demand equation is finite, as Taylor posits. The same motivation suggests that this elasticity should also be present in another margin available to depository institutions for reserve management, namely discount window borrowing. When reserve holdings are expensive relative to their expected cost in subsequent days, depository institutions will be more willing to resort to the discount window when they face a shortfall in their reserve positions. Indeed, Hanes (2000) presents convincing empirical evidence for this mechanism, which lends support on this aspect of Taylor's simple demand specification.

The second motive relevant for the daily demand equation is the precautionary motive to hold reserves for clearing needs—particularly towards the end of the day after the closing of the FedWire. Since excess reserve holdings reduce the probability of costly overnight overdrafts, their demand is akin to the demand for insurance, with the usual implication that more insurance will be purchased when its price is relatively lower. This suggests that, other things equal, the precautionary demand for reserves is somewhat inversely related to the cost of holding reserves, that is, today's rate, r_t.

Combined, the two motives suggest a demand equation:

$$b_t = -\theta\left(r_t - E_t r_{t-1}\right) - \delta r_t + \varepsilon_t ,$$

with $\theta, \delta > 0$. Trivially, this collapses to Taylor's equation and also satisfies his imposed restriction that the partial elasticity with respect to today's rate, $\alpha = \theta + \delta$, is somewhat greater that the partial elasticity with respect to the expectation of tomorrow's rate, $\alpha\gamma = \theta$.

My reaction to the demand function as a text-

[5] This is based on the intuition from the work Taylor reviews, though similar features are also present in other recent work, such as Bartolini, Bertola, and Prati (2000), Clouse and Dow (2000), and Hanes (2000). The grandparent of research on reserve management in a stochastic environment, of course, is Bill Poole's 1968 classic.

book treatment of the demand for reserves is very positive. It is elegant. It nicely captures the element that appears important for understanding "open mouth" operations—namely, the expectation of tomorrow's rate. And it abstracts from a number of elements that, although they may be important, are not essential for understanding the market at an elementary level.

I am afraid I cannot be as positive for the second key equation in Taylor's model, regarding the supply of reserves. Taylor models the Desk's operations as a mechanical reaction function that relies exclusively on the previous day's miss of the effective federal funds rate from the Desk's target rate to guide open market operations in the morning of the current day. As motivation, Taylor suggests that effective implementation of monetary policy in this model requires credibility that the Desk will do its best to achieve the FOMC's target for the federal funds rate. This credibility, Taylor argues, can be achieved if the Desk follows a simple mechanical reaction function such as the one he posits.

To be clear, I agree with Taylor's motivation. Effective implementation of monetary policy, when the daily demand for reserves is sensitive to expectations regarding the federal funds rate on subsequent days, requires a credible commitment that the Desk will do its best to achieve the FOMC's target rate for federal funds, thereby stabilizing expectations for the federal funds rate on subsequent days. However, I do not find the reaction function Taylor posits consistent with this motivation. Taylor's mechanical reaction function is far from the best way to achieve the Desk's objective. It is also a rather poor description of what the Desk actually does.

To evaluate Taylor's Trading Desk reaction function as a description of Desk operations and contrast it with possible alternatives, it is useful to examine how the members of the Markets Group at the Federal Reserve Bank of New York (the people behind the Desk) have described the Desk's operations in the recent past. A useful source for this is the annual report of open market operations prepared by the Markets Group. For example, the second paragraph on the first page of the report for 1997 states[6]:

> The Committee's directives instruct the Trading Desk to maintain the federal funds rate on average around a specified level. Open market operations are used to provide a level of nonborrowed reserves that will allow the federal funds market to clear at the indicated level. Each day, the Desk aims to keep the rate as close to the targeted level as possible with a minimum of volatility. But in deciding each day's operations, the Desk also considers how its flexibility for arranging operations in upcoming days might be affected by that day's course of action as well as how the behavior of the funds rate that day might influence rates in subsequent days.

In stark contrast to the emphasis on outcomes during the previous day in Taylor's Trading Desk reaction function, this description points towards a forward looking approach to Desk operations and indicates that in selecting its early morning open market operation the Desk "*each day*…aims to keep the rate as close to the targeted level as possible with a minimum of volatility" (emphasis added). The description also suggests alternatives to Taylor's Trading Desk reaction function that could arguably offer better characterizations of the Desk's operations. For instance, one could simply posit that the Desk sets the supply of reserves each morning so that the effective rate during the day is expected to be as close as possible to the target rate. Letting E_{t_m} reflect the expectation operator with information available in early morning of day t, t_m, this suggests a supply function implicitly determined by the expectation

$$E_{t_m} r_t = \rho_{t_m}$$

and market clearing conditions. This characterization offers considerable and realistic flexibility. For instance, it recognizes that each morning the Desk takes account of anticipated variations in the daily demand and supply of reserves from normal patterns and attempts to offset known shocks that may be expected to influence the market during the day.[7]

To appreciate the Desk's task and its approach in performing daily operations, it is instructive to

[6] Similar paragraphs appear in the reports for 1998 and 1999 as well.

[7] On the other hand, this is also somewhat simplistic as it ignores the influence of the choice of operations during one day on the degree of success with which the Desk could achieve its target on subsequent days—a consideration suggested in the last sentence of the quoted paragraph. One implication alluded to in this sentence is that the Desk might opt for an operation consistent with an expected deviation of the federal funds rate from its target during a day if doing so could be expected to significantly reduce the likely volatility of the federal funds rate on subsequent days. (Footnote 11 offers one example where such considerations may have been relevant in early 2000.)

revisit Taylor's detailed analysis of recent policy changes. The analysis suggests that the Desk has been quite successful in implementing changes in the federal funds rate in the recent past. Federal funds have traded close to the new rate virtually immediately following a change in policy. And trading has tended to closely reflect the old rate before a change in policy.

One feature *not* emphasized in the paper, which I believe is important for the analysis, is that this has been the case even though a policy move may have been expected with high probability by market participants. For example, *it is not the case* if market participants expect a tightening on, say, Tuesday, that this tightening is reflected in trading a week earlier. This would be inconsistent with the Desk's objectives and Desk operations would actively seek to counteract it.[8]

To see this, in Figure 3 I reproduced the effective and target rates shown in Taylor's paper for the last three policy moves: February 2, March 21, and May 16, 2000.[9] On these series I superimposed the rate on the federal funds futures contract for February, April, and June, respectively. These months correspond, as closely as possible but obviously not perfectly, to the month following the FOMC meeting at which each tightening was decided but preceding the subsequent FOMC meeting. For each move, the figure shows four weeks of business-daily data centered around the two-week reserve maintenance period during which the policy move occurred. (The two vertical lines in each panel indicate maintenance-period ending Wednesdays.)

Consider first the March 21, 2000, policy move shown in the middle panel. The federal funds future contract for April 2000 can be viewed as a bet on the average level of the federal funds rate that the market expected to prevail during April 2000. But since no FOMC meeting was scheduled during April, it would be reasonable to expect that the FOMC's target rate during April would be the one decided during the March meeting. Thus, the rate implied in the April contract on any given day prior to the March meeting reflected market expectations of the FOMC decision at that meeting.

As can be seen, the April contract was consistently trading around 6 percent for weeks before the March meeting and on this occasion correctly anticipated the move to a 6 percent target rate that was decided at the meeting. At the same time, the effective rate on federal funds remained around 5.75 percent and one could not possibly detect the market's near certain expectation of the policy change in that rate prior to the policy move.

Likewise, with the May policy move (lower panel), trading of the June 2000 futures contract suggested that the 50 basis points increase in the target rate was widely anticipated before it materialized, though a small probability that the increase could be only 25 basis points appears to have been priced in the contract until just prior to the meeting.[10]

Similarly, with the February policy move (top panel), the futures contract for February anticipated that a tightening would likely take place on February 2. The contract traded consistently somewhat above 5.75 percent prior to the meeting, suggesting that although the 25 basis points tightening that took place was considered the most likely decision, a small probability of a 50 basis points tightening was also priced in the contract.[11]

Daily tracking of the FOMC's target rate, during a maintenance period when a change in the target is anticipated, poses special challenges for Desk operations as efforts by depository institutions to

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[8] In general, the behavior of the federal funds rate during days preceding an FOMC meeting primarily reflects the Desk's constant efforts to track its target and is a rather poor indicator of market beliefs regarding the likelihood of a policy change at the meeting. One consequence of this is that Granger causality tests, such as those reported in Table 2 of the paper, are not particularly informative.

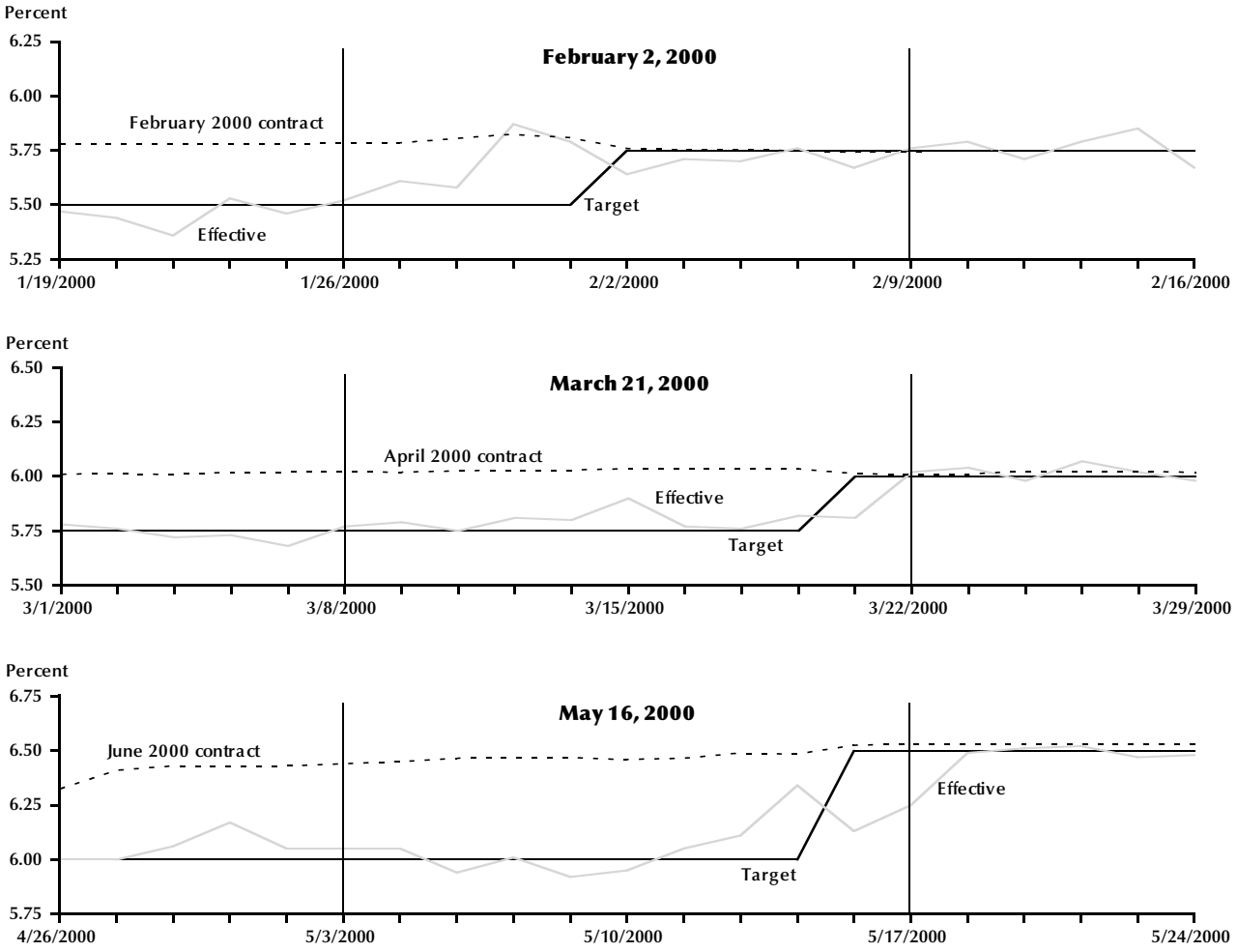
[9] These were the first three meetings of 2000. The federal funds rate target was left unchanged following the fourth meeting of the year, which took place June 27-28.

[10] The next policy decision was scheduled for June 28 (the second day of the June 27-28 FOMC meeting). As a result, the June contract could be roughly interpreted as a weighted average of the May decision (28/30 of the contract) and the June decision (2/30 of the contract). The fact that the contract for June traded a bit above 6.5 percent following the May meeting reflected the influence of expectations of another possible tightening move at the end of June.

[11] Unlike in March and May, the effective federal funds rate prior to the February 2 tightening, on Monday, January 31 and Tuesday, February 1, was significantly higher than the target and closer to the expected rate following the policy action. As is clear from the data, however, this did not reflect a major change in expectations regarding the tightening from the previous Friday, January 28. One explanation of the higher rates on January 31 and February 1 is larger than expected increases in the demand for reserves associated with the greater clearing needs often seen on the first and last day of a month. Another (and complementary) explanation is that, although the Desk may have correctly perceived the increase in demand during those days, it may have been reluctant to fully counteract it because doing so might have raised the volatility of the market on subsequent days. (This is the possibility raised in footnote 7). Such a concern may arise when the Desk perceives that adding the level of reserves required to keep the rate close to its target early in a maintenance period may significantly reduce the need for reserve balances later during the same period. The combination of end-of-month and expected tightening pressures just prior to the February 2 move could have justified such a concern on this occasion.

Figure 3

Federal Funds Rate and Futures Contracts

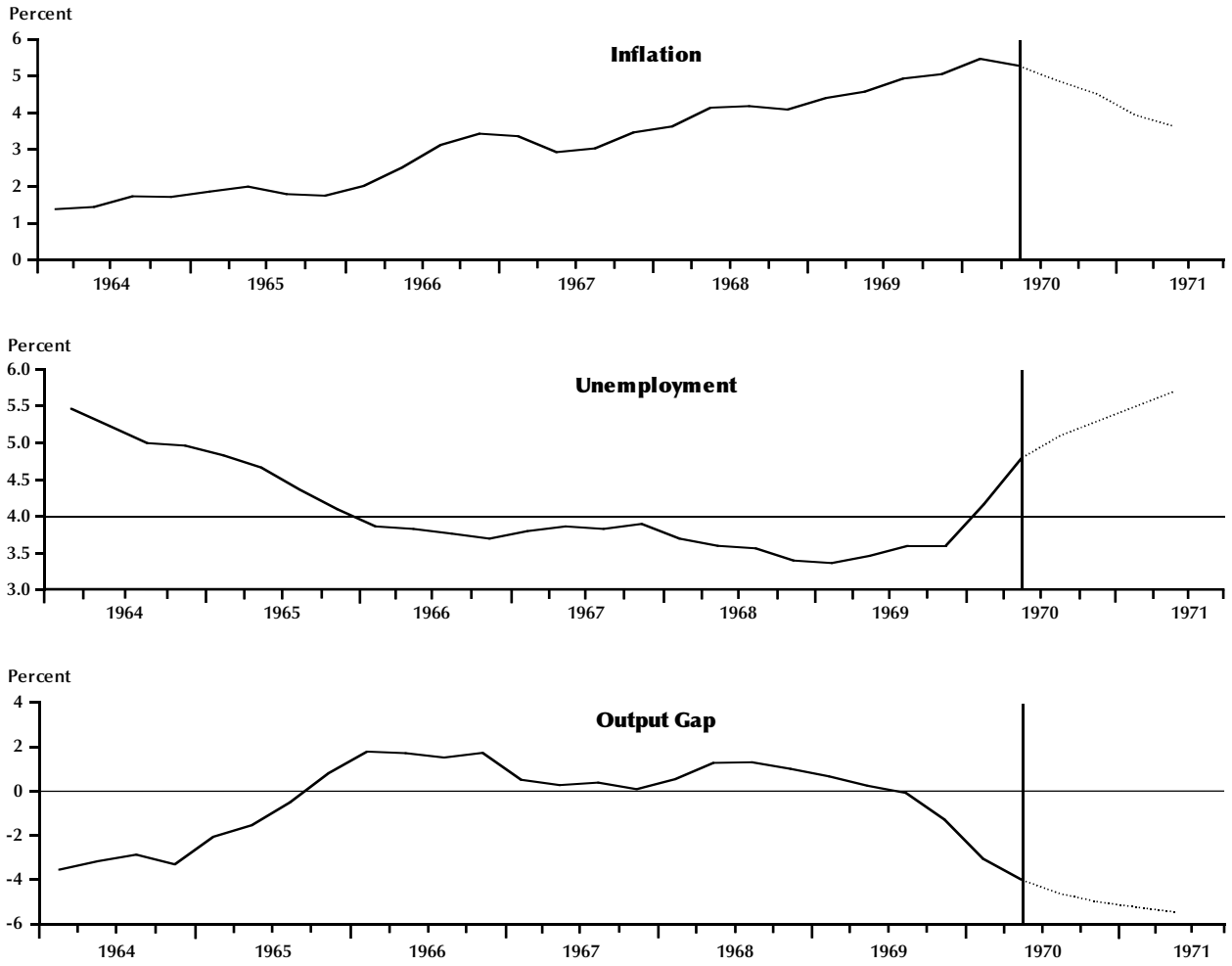


economize their cost of maintaining average reserve balances during the period tend to distort the normal daily intra-period pattern of reserve demand. Obviously, assessment of such relative changes in intra-period demand can be quite difficult and likely not very accurate. Viewed in this light, the apparent success of the Desk in tracking the FOMC's target relatively closely around the three anticipated tightenings, shown in Figure 3, offers a striking illustration of the Desk's credibility and its ability to adjust reserves on a daily basis to achieve its objective.

Similar success would have been unlikely had the Desk implemented policy by mechanically reacting to the previous day's miss of the effective federal funds rate from the Desk's target. Of course, to the extent the previous day's miss, $(r_{t-1}-\rho_{t-1})$, is an important summary statistic of stochastic supply and demand conditions that might influence the market on day t, it might be useful for determining the appropriate open market operation during day t. The previous day's miss could sometimes summarize information regarding demand and supply shocks that influenced the market during day t–1, and some of these shocks might persist during day t. In this sense, the previous day's miss is part of the information set the Desk employs in determining its operation during day t. As with any other variable that might provide useful information about market conditions, a response to this miss could be included in a mechanical reaction function for the Desk. The

Figure 4

Data and Forecasts at the August 1970 FOMC Meeting



difficulty is that not all variables are always equally useful as indicators. Statistical relationships change over time and so do our perceptions about such relationships and their usefulness for the future. Consequently it is practically impossible for a simple mechanical reaction function to reasonably capture the appropriate reaction at all times. And suggesting that the Desk follow a specific simple backward-looking rule at all times could quickly prove inadequate as a guide.

There are parallels to this and the analysis of how the FOMC should set its target for the federal funds rate to achieve its objectives of price stability and maximum sustainable growth over time (although the Committee's problem is arguably much more complex). The FOMC could follow a simple rule and adjust the interest rate mechanically to past inflation, for example. When inflation picks up, the rate could be raised and vice versa. This strategy could achieve price stability and reasonably stable growth on average, but it is unnecessarily crude and would leave scope for improvement. Allowing for judgment and flexibility in policy decisions is one approach. Alternatively, one could try to suggest an "improved" policy rule, specify that the FOMC follow a rule that mechanically reacts to the output gap or its close cousin, the unemployment gap, in addition to inflation. Indeed, as Taylor points out in footnote 12 of his paper, some modern research has emphasized that such a response may

be optimal. Perhaps the best-known example of such a rule is the one Taylor proposed in his 1993 Carnegie-Rochester conference article. As Taylor showed at the time, this rule described policy in the late 1980s and early 1990s rather well. This period was one in which monetary policy is generally believed to have been successful, so one could conclude that this rule would have provided reasonable guidance to monetary policy during that time. But there is no guarantee that the same rule would have provided good advice in other settings. Indeed, as shown in Orphanides (2000), the very same rule also appears to describe monetary policy during the early 1970s, a period during which monetary policy is generally considered to have been much less successful—if not outright disastrous.

Since this conference is in honor of Darryl Francis, I think it is most appropriate to illustrate what can go wrong with an example from a fascinating FOMC meeting in which he participated as a voting member, on August 18, 1970. To set the stage, recall that a recession earlier that year had shifted policy towards ease, while inflation, which had risen in the previous two years, remained a serious concern. This was the first of the "stagflation" years in the United States. By the August FOMC meeting, available data indicated a small increase in second quarter gross national product (GNP), suggesting the economy was turning around. However, the unemployment rate had risen to nearly 5 percent and Federal Reserve staff projected a further increase, well above the widely held at the time 4 percent estimate of the natural rate of unemployment. Similarly, available estimates of the output gap showed a continued deterioration. And it appeared quite reasonable to expect an improvement to the inflationary problem.[12] Based on this information, application of the Taylor rule would have suggested that a further policy easing was appropriate. Indeed a policy easing was adopted at the meeting and policy remained easy (and broadly consistent with the Taylor rule) for several quarters.

Darryl Francis dissented at this meeting. In his view, easing policy further would have been a mistake. His words, as reflected in the Memorandum of Discussion (1970) for the meeting, would prove prophetic:

> Mr Francis said that much of the current unemployment was structural and could not be obviated except temporarily and with adverse price effects by stimulation of total spending. In view of the strong inflationary actions, quick results in obtaining relatively stable prices and a reduction of transitional unemployment should not be expected. Attempts directed at rapid cures or fine tuning had usually caused more serious problems later on.

Mechanical reliance and response to past data always runs the risk of providing misleading advice at times, however well intentioned the design of a simple rule may be. For Desk operations, it is unnecessarily crude to adopt such a mechanical rule for determining daily open market operations each morning. So in my view, there is scope for improvement in the half of Taylor's model that deals with the daily supply of reserves. But the other half of the model could hardly be more elegant as a description of the daily demand for reserves. Overall, this is a very valuable paper to read.

[12] Quarterly historical data and staff projections for inflation (measured as the percent change in the GNP deflator over four quarters), unemployment, and the output gap as available at the meeting are shown in Figure 4. The vertical lines in the figure identify the last quarter of available historical data, 1970:Q2.

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Identifying the Liquidity Effect at the Daily Frequency

Daniel L. Thornton

It is widely believed that the Fed controls the overnight federal funds rate, and thereby other short-term interest rates, through open market operations.[1] The Fed sells government securities, reducing the supply of reserves relative to demand, when it wants to raise the funds rate. Similarly, it purchases securities when it wants the funds rate to fall. The change in the short-term interest rates brought about by the exogenous policy actions of the monetary authority is referred to as the *liquidity effect*. By many accounts—public and professional—the Fed's use of this procedure is responsible for most, if not all, lower frequency variation in the federal funds rate and, thereby, other short-term interest rates at least since the late 1980s.

Despite the widespread belief in the Fed's ability to control short-term interest rates through open market operations, attempts to estimate the liquidity effect—using a variety of approaches—generally have been unsuccessful.[2] Many analysts attribute this lack of success to the inability of economists to isolate the exogenous policy actions of the Fed. Hamilton (1997) notes that, most often, the Fed adjusts its policy instrument in response to new information about current or future values of output, inflation, the exchange rate, or other variables. Because of this, Hamilton argues that "the correlation between such 'policy innovations' and the future level of output of necessity mixes together the effect of policy on output with the effect of output on policy" (p. 80). This problem tends to be more severe when the monetary aggregates used to estimate the liquidity effect are only loosely associated with the policy actions of the Fed or when the data are averaged over a period of a month or more.

Hamilton suggests that "a more convincing measure of the liquidity effect" can be obtained by estimating the "instantaneous consequences of an open market purchase. For daily data, Federal Reserve accounting and Trading Desk procedures suggest some quite plausible identifying assumptions that can be used to measure the instantaneous liquidity effect" (pp. 80-81). Specifically, Hamilton estimates the response of the funds rate to reserve supply shocks that are analogous to those that the Fed could create through open market operations. Hamilton finds that the funds rate responds significantly to reserve supply shocks, but only on the last two days of the maintenance period, and that the response is economically significant only on settlement Wednesdays.

I model the reserve market based on the Fed's operating procedure and show why the liquidity effect cannot be identified using Hamilton's methodology. Consistent with this analysis, I show that Hamilton's settlement-day result is fragile and due to a few settlement Wednesdays when there were unusually large changes in the funds rate. In addition, I show that there is no liquidity effect using his methodology for sample periods before and after his.

I then suggest an alternative approach to estimating the liquidity effect at the daily frequency. Specifically, I argue that the liquidity effect can be identified by estimating the change in nonborrowed reserves associated with changes in the Fed's target for the federal funds rate. If the Fed controls the federal funds rate by changing the funds rate target, reserves should change when the target is changed. Estimates of this model using data prior to 1994 suggest that, while the Fed undertakes open market operations consistent with changing the funds rate when the funds rate target is changed, the size of the actions is relatively small compared with stochastic fluctuations in reserves. One possible interpretation

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[1] It is arguably the case that the Federal Reserve has implemented monetary policy by targeting the short-term interest rates, particularly the federal funds rate, during much of its history (Goodfriend,1991).

[2] Some analysts like Christiano, Eichenbaum, and Evans (1997, 1996, 1994), Pagan and Robertson (1995), Bernanke and Mihov (1997a, b), and Strongin (1995) have suggested that policy actions can be identified using innovations from nonborrowed reserves in the context of a structural VAR; however, Thornton (1999b) has shown innovations to nonborrowed reserves from these models are not measures of Fed policy actions. In a similar vein, Bernanke and Blinder (1992) and Sims (1992) have suggested that policy actions of the Fed could be measured by innovations to the federal funds rate in the context of a VAR. However, the deviations from the Fed's funds rate target, which these measures capture, are better interpreted as random variation or control errors rather than policy innovations. Changes in the Fed's funds rate target occur less frequently and are more erratic than such innovations. Moreover, evidence suggests that the funds rate adapts quickly to the new target level.

of the results is that the Fed undertakes such actions to signal target changes. Additional evidence is presented that is inconsistent with this explanation, however.

THE LIQUIDITY EFFECT

The idea that an exogenous central bank–engineered increase in the nominal money stock can temporarily reduce interest rates is fundamental to most analyses of monetary policy. The liquidity effect has classical roots (e.g., Patinkin, 1965, and Humphrey, 1983b). Classical economists believed that an exogenous increase in the supply of money would increase the supply of loanable funds, causing the interest rate to fall.[3] Ultimately, however, prices would rise and the interest rate would return to the previous level.[4] While the real/nominal interest rate taxonomy is usually attributed to Fisher (1907), Humphrey (1983a) argues that classical economists from Thornton ([1802] 1939) on understood the distinction between real and nominal interest rates. Indeed, Thornton (p. 255) argues that the interest rate can be kept below its natural level only by continually increasing the money supply and ever rising prices.

Despite its long history in monetary economics, it appears that the phrase *liquidity effect* was first used publicly by Milton Friedman in a speech at the 1968 Conference on Savings and Residential Financing sponsored by the United States Savings and Loan League (Friedman, 1969).[5] For Friedman, the liquidity effect was a natural extension of Keynes's theory of *liquidity preference*. Specifically, Friedman (1969, p. 365) notes that the "liquidity effect in simplest form is the usual textbook relationship between the quantity of money and the interest rate which says that the larger the quantity of money, the lower the interest rate will have to be to induce people to hold it."

Like his classical ancestors, Friedman saw the liquidity effect as the first of three possible effects associated with an exogenous change in the money supply.[6] Friedman considered two cases. The first was a once-and-for-all central bank–engineered increase in the supply of money. An increase in the money stock causes the supply of real money balances to increase relative to demand at the current price level. Because of an implicit assumption that prices and output would be slow to adjust to the central bank's action, Friedman argued that initially interest rates would fall—there would be a liquidity effect. The increase in the real money stock coupled with the decline in interest rates would cause prices and/or output to rise until the supply and demand for real money balances were once again equal. While the path to equilibrium depended on the relative response of output and prices, Friedman assumed that money was neutral. Eventually the price level would rise to restore real money balances to their previous level.

Friedman's second experiment was dynamic. Specifically, he assumed that there was "an unanticipated jump in the rate of monetary increase" (1976, p. 367). In this case there is a third effect, which Friedman called the *price anticipation effect*. Faster money growth would give rise to an increase in inflation and a corresponding rise in expectations for inflation. Eventually, equilibrium would be achieved at a high nominal interest rate and an unchanged real rate. Like Thornton ([1802] 1939), Friedman (1968, p. 6) argued that the rate could be kept below its natural level only with ever-accelerating money growth.[7]

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[3] Some classical economists, e.g., Ricardo, believed that the effect of money on the interest rate was inconsequential, see Humphrey (1983b) for more details.

[4] In the case of Thornton ([1802] 1939) and Ricardo, this effect worked through the demand side. In the case of Wicksell, the effect was through the supply side, reducing the equilibrium stock of real money as the price level rose (see Patinkin, 1965, pp. 369-70).

[5] The phrase *liquidity effect* appears to have first appeared in print in William Gibson's (1967) dissertation. Gibson was a student of Milton Friedman, and Bob Hetzel tells me that Friedman used the phrase in his monetary economics class in the early to mid 1960s. Hence, it seems likely that Gibson was merely using a phrase that Friedman had coined. Gibson's dissertation appeared as a Board of Governors Staff Economic Study and was subsequently published in the *Journal of Political Economy* (Gibson, 1970). Neither Friedman nor Gibson presented the phrase as their own and, in personal correspondence, Friedman noted that "liquidity played such an important role in Keynes's General Theory and liquidity preference became a common term after The General Theory came out [so that] if you asked me, it would have never occurred to me that I was the first person to introduce the term." Nelson (1996) also traces the use of the phrase to Friedman.

[6] Friedman (1969) acknowledged the liquidity effect's classical origins by pointing out that John Stuart Mill had noted this "first-round" effect of a change in the quantity of money, indicating that Mill argued that the effect would be different if it occurred through the production of gold or through the credit market. In the former case, the initial effect would be on the wages of gold miners and the prices of commodities they consumed; in the latter case, it would be on the interest rate.

[7] Despite its long tradition of being associated with the demand for money and credit, some economists (e.g., Ohanian and Stockman, 1995, and Christiano, 1991) have suggested there can be a liquidity effect in models that completely abstract from money and the financial markets. Hoover (1995, p. 30) has noted, however, that such models do not seem to "capture our intuitive understanding of the liquidity effect," suggesting that "in all of these models—including even the basic IS-LM model with a vertical LM curve—the liquidity effect seems to operate through the wrong mechanism."

A MODEL OF THE RESERVE MARKET AND THE FED'S OPERATING PROCEDURE

The idea that the liquidity effect is inexorably linked to the interest elasticity of money demand is extended to the market for reserves. Until recently, the demand for total reserves, TR, was determined in large part by reserve requirements imposed on banks by the Fed.[8] Specifically, banks are required to maintain reserves (vault cash and deposits at the Fed) equal to some proportion, τ, $0 < \tau < 1$, of their reservable deposit liabilities, which make up a large portion of the supply of money. Hence, the demand for reserves is given by

$$TR_t^d = \tau[f(i_t, x_t) + v_t] + ER_t^d . \tag{1}$$

The demand for reservable transaction deposits is denoted by $f(i,x)$, which is a function of the public's opportunity cost of holding transaction deposits, i_t, and a vector of other variables that determine the demand for transaction deposits, x_t, and a random demand shock, v_t.

The demand for excess reserves is not well defined within the maintenance period because banks are free to alter their reserve holdings within the maintenance period so long as they hold enough reserves on a daily average basis to meet their requirement. Excess reserves become increasingly meaningful as the maintenance periods draws to a close.

Because reserves are non-interest bearing, these banks must derive an implicit benefit from holding excess reserves, ER_t, that is greater than or equal to the opportunity cost of holding them. If banks are holding excess reserves up to the point where the marginal benefit from the last dollar of excess reserves held is equal to the opportunity cost of holding it, one would expect the demand for excess reserves to be sensitive to changes in the interest rate. Indeed, some (e.g., Hamilton, 1998) model the demand for excess reserves in this manner. Nevertheless, banks' holdings of excess reserves are rather insensitive to changes in interest rates at a maintenance period frequency. One possible reason is that excess reserves are primarily held by relatively small banks that do not monitor their reserve positions closely. Hence, even if the concept of the daily demand for excess reserves were meaningful, day-to-day fluctuations in excess reserves would likely be unpredictable.

Consequently, the demand for excess reserves is assumed to have the form

$$ER_t^d = \vartheta + \chi_t , \tag{2}$$

where ϑ is constant and χ_t is an i.i.d. random disturbance. One could think of ϑ as the maintenance-period average demand for excess reserves. While this assumption is simple, it has no important consequences for identifying the liquidity effect because, as was noted earlier, the liquidity effect depends on the demand for money and not banks' demand for excess reserves.

The quantity of total reserves that the Fed supplies is determined by the Fed's holding of government securities, B_t, borrowed reserves, BR_t, and all other factors that affect reserve supply, F_t—primarily Treasury balances at the Fed and the float. That is,

$$TR_t^s = B_t + BR_t + F_t . \tag{3}$$

The quantity of reserves that the Fed supplies each day is determined by its operating procedure. According to Feinman (1993, p. 234), the Fed's current federal funds rate targeting procedure is as follows:

> Each day the staff estimates the period-average demand for reserves by projecting required reserves against deposits and the desired excess reserves of the banking system. Subtracting the FOMC-specified level of discount window borrowing from this forecast of reserve demand yields the nonborrowed reserve path, the Desk's prime objective. Each morning the staff's forecast of nonborrowed reserves owing to market factors beyond the Fed's control (for example, Treasury balances at the Fed, float, etc.) is subtracted from the path to produce an estimate of the quantity of reserves that must be added or subtracted, on a period-average basis, to reach the objective.

The Fed's projected level of government securi-

[8] Reserve requirements once applied to all but the currency component of M1. Over time, however, reserve requirements have encompassed a smaller proportion of M1 balances. Indeed, today a large portion of banks maintain deposits with the Fed, not to meet reserve requirements, but to process transactions on FedWire. Since the advent of "sweep" programs in 1994 (designed to reduce the effect of the reserve tax), the demand for deposits at the Fed increasingly has been determined by banks' demand for Fed services (see Anderson and Rasche, 2001).

ties that will be needed to meet the Fed's objective, B^*, is:

$$B_t^* = \tau E_{t-1}[f(i_t, x_t) \mid FF_t^*] + E_{t-1}ER_t^d - BRAS_t - E_{t-1}F_t\,, \tag{4}$$

where E_{t-1} denotes the expectation operator conditional on information up to the start of the day, FF_t^* denotes the Fed's target for the federal funds rate on day t, and $BRAS_t$ denotes the Federal Open Market Committee's (FOMC's) borrowed reserves assumption.[9]

When B_t is larger than B_t^*, the operating procedure indicates a need to drain reserves. When B_t is smaller than B_t^*, there is a need to add reserves. The quantity of reserves available each maintenance period is equal to the reserves supplied in accordance with the operating procedure, B_t^*, plus the actual level of borrowing and the level of all other factors that affect reserves, i.e.,

$$TR_t^s = B_t^* + BR_t + F_t\,. \tag{5}$$

Substituting for B_t^* yields

$$TR_t^s = \tau E_{t-1}[f(i_t, x_t) \mid FF_t^*] + E_{t-1}ER_t^d + (BR_t - BRAS_t) + (F_t - E_{t-1}F_t)\,. \tag{6}$$

If the Desk follows this rule precisely, the amount of reserves available each day is equal to the Fed's forecast of reserve demand plus the unexpected components of the other factors that affect reserve supply—BR_t and F_t.

To close the model and derive the reduced form, several additional assumptions are necessary. Following Hamilton (1997), I assume that the demand for reservable deposits is

$$f(i_t, x_t) = -\lambda i_t + \gamma' x_t \tag{7}$$

and that the opportunity cost rate is the federal funds rate, i.e., $i_t \equiv FF_t$.[10] I also assume that bank borrowing is a function of the spread between the federal funds rate and the discount rate and other factors. Specifically, I assume that

$$BR_t = \alpha(FF_t - DR_t) + \eta_t\,, \tag{8}$$

where DR denotes the Federal Reserve's discount rate and η represents all other factors that determine bank borrowing. For simplicity, I assume the process generating F_t is mean stationary, i.e., $F_t = \mu + \omega_t$, and that the Fed makes unbiased forecasts of F_t, i.e., $E_{t-1}F_t = \mu$.

The official $BRAS_t$ changed infrequently; however, I have shown elsewhere (Thornton, 2001a) that the Desk has frequently offset changes in borrowing at the maintenance-period frequency. Given its practice of entering the market once per day in the morning, the Fed cannot offset borrowing at the daily frequency. Hence, I assume that the Desk operates to offset a proportion, ξ, $0 \le \xi \le 1$, of expected borrowing, i.e.,

$$BRAS_t = \xi E_{t-1}[BR_t]\,. \tag{9}$$

The value of ξ is a choice variable for the Desk. In the steady state the funds rate equals the funds rate target on average only if $\xi = 1$. Hence, if the objective is to keep the funds rate close to the funds rate target, the Desk has an incentive to completely offset expected borrowing.[11] Because the Desk has a strong incentive to offset borrowing, and there is evidence that it has done so at the maintenance period frequency, I impose the restriction $\xi = 1$.

The reduced-form for the federal funds rate, total reserves, and nonborrowed reserves is

(10)

$$FF_t = FF_t^* + \kappa[\tau\gamma'(x_t - E_{t-1}x_t) - \eta_t - \omega_t + \tau v_t + \chi_t]$$

$$TR_t = -\tau\lambda FF_t^* + \vartheta + \kappa[\tau^2\lambda\gamma' E_{t-1}x_t + \alpha\tau\gamma' x_t + \alpha\chi_t + \tau\lambda\eta_t + \tau\lambda\omega_t + \alpha\tau v_t]$$

$$NBR_t = -(\tau\lambda + \alpha)FF_t^* + \vartheta + \tau\gamma' E_{t-1}x_t + \alpha DR_t + \omega_t\,,$$

where $\kappa = (\tau\lambda + \alpha)^{-1}$.

Note that the equilibrium funds rate equals the Fed's funds rate target on average so long as the Desk makes unbiased forecasts of the demand for reserv-

[9] Before June 1995, the borrowed reserves assumption was implicitly set by the FOMC in that it was embedded in the policy alternatives that the FOMC voted on at each meeting. The borrowed reserves assumption was changed frequently during the intermeeting period, however. Beginning with the June 30, 1995, meeting, the FOMC dropped the explicit reference to the level of seasonal plus adjustment borrowing that it believed was consistent with various policy alternatives being considered.

[10] Strictly speaking, this cannot be the case since individuals cannot participate in the federal funds market. In most monetary policy discussions, the opportunity cost rate is assumed to be linked to the funds rate by the expectations theory of the term structure (e.g., Goodfriend, 1991; Balduzzi, Bertola, and Foresi, 1997; and Cook and Hahn, 1989).

[11] See Thornton (2001a) for an analysis of why the Fed has an incentive to offset borrowing completely under other operating procedures, such as nonborrowed reserves targeting to maintain the stock of money at the target level.

able deposits. Positive shocks to the demand for reservable deposits or excess reserves will cause the funds rate to rise relative to the target, while positive shocks to borrowing or other supply factors will cause the equilibrium funds rate to fall. The amount by which the Desk must change nonborrowed reserves to change the funds rate target by one percentage point is $-(\tau\lambda + \alpha)$, which is larger than the amount of the change in total reserves because of the restriction that the Desk completely offsets borrowing on average. If, on the other hand, ξ were zero, the change in total reserves would be larger than the change in nonborrowed reserves because the change in the equilibrium funds rate would result in a change in borrowing and, hence, total reserves.

THE RESERVE MARKET MODEL AND THE LIQUIDITY EFFECT

Hamilton (1997) has suggested that the liquidity effect can be estimated at the daily frequency by estimating the response of the funds rate to a shock to the supply of reserves analogous to the shocks the Fed could create through exogenous open market operations. It is important to emphasize that, in private correspondence, Hamilton indicates that he thinks of the daily liquidity effect "as arising from (a) banks' unwillingness to borrow from the discount window unless the cost of fed funds is sufficiently high, and (b) banks' reluctance to reduce excess reserves unless the cost of fed funds is sufficiently high, not (c) the response of required reserves to interest-sensitive transactions deposits."[12] He noted that the effect could not come through (c) because the value of transaction deposits would not respond within the day to changes in the interest rate. I am sympathetic to the third point, but this is tantamount to arguing that the liquidity effect—as traditionally defined here—cannot be estimated directly using daily data.

With these caveats, I used Hamilton's model to discuss the problems with identifying the liquidity effect at the daily frequency using his approach. Specifically, Hamilton assumes that the demand for nonborrowed reserves is given by

$$NBR_t^d = -\tilde{\lambda} FF_t + \tilde{\gamma}' X_t + \upsilon_t , \tag{11}$$

where X_t denotes a vector of predetermined variables. Hamilton notes that $\tilde{\lambda}$ cannot be estimated from the conditional expectation of equation (11) because the Fed "actively chooses the supply of reserves each day in response to the value of the federal funds rate it anticipates for that day…" (1997, p. 82). Rather than deriving a structural model of the supply of reserves based on the Fed's operating procedure, Hamilton assumes that the supply of nonborrowed reserves is given by

$$NBR_t^s = \tilde{\delta}\, FF_t + \tilde{\psi}' X_t + \tilde{\chi}_t . \tag{12}$$

Noting that the classic solution to this problem of simultaneous equation bias is to find an instrument that is correlated with NBR_t but not with υ_t, Hamilton argues that it is not necessary to identify policy innovations, per se, as is usually done, in order to estimate the liquidity effect. Specifically, he suggests that the liquidity effect can be estimated by estimating the error the Fed makes in forecasting the Treasury balance each day. That these forecast errors are uncorrelated with shocks to money demand is evident from equation (10).

Hamilton exploits the instrumental variable approach by estimating the reduced form, derived from equations (11) and (12), i.e.,

$$\begin{aligned} FF_t &= \pi\varepsilon_t^U + \xi' X_t + \varepsilon_t^i \\ NBR_t &= \rho\varepsilon_t^U + \zeta' X_t + \varepsilon_t^R , \end{aligned} \tag{13}$$

where $\pi = -(1 + \tilde{\delta}\tilde{\lambda})^{-1}$, $\rho = \tilde{\lambda}/(1 + \tilde{\delta}\tilde{\lambda})$ and ε_t^U denotes the "error that the Fed makes in forecasting the Treasury balance" (1997, p. 82). Hamilton observes estimates of $\tilde{\lambda}$ can be inferred from estimates of π and ρ, i.e., $\tilde{\lambda} = \rho/\pi$. If ε_t^U is contemporaneously uncorrelated with ε_t^i and ε_t^R, then consistent estimates of π and ρ, and, hence, $\tilde{\lambda}$ can be obtained.[13]

Shocks to the Treasury's Balance and the Liquidity Effect

Note that this approach does not eliminate the identification problem, but replaces one identification problem with another.[14] Specifically, this

[12] Hamilton (1997) is not specific about the source of the liquidity effect, stating only that it depends on the demand for reserves. Hamilton (1998) is much more specific in modeling the demand for borrowed and excess reserves.

[13] Note that this condition need not hold for several reasons. For example, an interruption to the transportation system could cause both Treasury balances and the float to change from the Fed's expectation. See Thornton (2001b) for other reasons.

[14] In addition, equation (10) shows that the coefficient implied by the ratio of the estimated coefficients on ω_t from the reduced-form of nonborrowed reserves and the federal funds rate is $(\tau\lambda + \alpha)$ and not $\tau\lambda$. This difference should be relatively unimportant over Hamilton's sample period because borrowing was relatively small and interest insensitive, i.e., α is small (Thornton, 2001a).

approach requires one to identify the "error that the Fed makes in forecasting the Treasury balance." Failure to correctly identify the Treasury balance component of ω_t will bias the estimate of the response to the funds rate.[15]

Evidence that Hamilton failed to correctly identify the Fed's forecast errors comes from noting in his nonborrowed reserves equation that his estimate of the coefficient on $\hat{\varepsilon}_t^U$, –0.42, is considerably different from the theoretically correct value of –1. It turns out that Hamilton's forecasts differ significantly from those used each day to implement monetary policy. Elsewhere (Thornton, 2001b), I evaluate the forecasts of the Treasury's balance made each day by the New York Fed, the Treasury, and the Board of Governors.[16] A comparison of these forecasts with those from Hamilton's model shows that model-based forecast errors are significantly larger than those made by these agencies each day.

To see why this approach cannot identify the liquidity effect, note the Fed's system of reserve requirements is not strictly contemporaneous.[17] Instead, banks are required to maintain reserves over a two-week period ending Wednesday based on the average deposit liabilities held over a two-week period ending Monday, two days previous.[18] Specifically, banks must satisfy the condition

$$\sum_{t=1}^{14} \tau[f(i_{t-2}, x_{t-2}) + v_{t-2}]/14 + \sum_{t=1}^{14} [\vartheta + \chi_t]/14 =$$

$$(14) \quad \sum_{t=1}^{14} [\tau E_{t-1}[f(i_{t-2}, x_{t-2}) \mid FF_t^*] + \vartheta$$

$$+ (BR_t - E_{t-1}[BR_t]) + \omega_t]/14$$

on the last day of the maintenance period.

Because the demand for reserves to satisfy reserve requirements is completely determined by Monday, the quantity of transaction deposits cannot respond to the federal funds rate on the last two days of the maintenance period. Since estimating the slope of this demand curve is critical to estimating the liquidity effect, it is impossible to estimate the liquidity effect on the last two days of the maintenance period.

If Hamilton's settlement-day response is not due to the liquidity effect, what accounts for it? There are a couple of possibilities. First, equation (14) shows that a reserve-supply shock that occurs on any day during the maintenance period—including settlement Wednesdays—contributes to only one-fourteenth of the aggregate average imbalance at the end of the maintenance period. Consequently, it would take a $14 billion error in forecasting the Treasury's balance to produce a $1 billion aggregate reserve imbalance at the end of the maintenance period. Hence, it could be that there are several very large forecast errors that caused the funds rate to move significantly on a few settlement days.

Second, unusually large changes in the funds rate tend to occur more frequently on settlement Wednesdays.[19] Consequently, a few unusually large settlement-Wednesday changes in the funds rate could occur on days when Hamilton estimates that there were relatively large shocks to the Treasury's balance.[20] If either of these explanations is correct, Hamilton's settlement-Wednesday results should be fragile, resulting from a relatively few settlement Wednesdays when there are either very large reserve-supply shocks or relatively large changes in the funds rate.

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[15] There are two reasons for this. First, and most important, the Desk's decisions are based on its forecast of the Treasury's balance. If estimates used are not equal to the true estimates, the forecast errors must be correlated with other factors that affect the supply of reserves. Second, there could be a scale problem. For example, assume that estimates of $\hat{\varepsilon}_t^U$ are twice as large on average as the true forecast errors made by the Desk. In this case, the estimated response would be half as large as the true response.

[16] That Hamilton's forecast errors do not coincide with those made by the Desk is not surprising. Each day the staffs of the Board of Governors, the New York Fed, and the Treasury all make separate estimates of Treasury balances based on knowledge of inflows and outflows from the Treasury's tax and loan accounts and knowledge of specific Treasury financing operations being undertaken. Even with this extensive knowledge, the three estimates of Treasury balances frequently differ by large amounts. When this happens, a judgement is made in conducting that day's open market operations. It seems very unlikely that Hamilton's econometric model adequately accounts for the nuances of the actual procedure used.

[17] For a complete description of contemporaneous reserve accounting post-1982, see Gilbert and Trebing (1982). The lag in the structure of reserve requirements was increased in July 1998.

[18] The Federal Reserve reverted to a system of lagged reserve accounting in July 1998.

[19] The reason is that the Fed cannot offset large unanticipated surpluses or deficits in the aggregate provision because with rare exceptions, since the early 1980s, the Desk has entered the market only once per day. In January 1997 the Desk moved up the timing of its interventions from 11:30 a.m. EST to 10:30 a.m.; in April 1999 the time was moved to 9:30 a.m. to allow the Desk to intervene closer to the time that the market for repurchase agreements is most liquid. It is interesting to note that Furfine (2000) finds, with the exception of settlement Wednesday, much of the intra-maintenance period variation in the funds rate is the result of predictable patterns in the level and volatility of interbank payments.

[20] Given the sensitivity of least squares to outliers, it is very possible that Hamilton's results are due to large funds rate changes on a few days. Hamilton reports no test of the sensitivity of his results to outliers that occur on settlement days.

Hamilton's Empirical Methodology

To investigate the source of Hamilton's settlement-day results, I estimate his model over an extended sample period and I examine the results for his sample period in more detail. Following Hamilton, let y_t denote the variable being studied and x_t and z_t denote vectors of variables that govern the mean and variance of y_t, respectively. In general,

$$y_t = x_t'\beta + \varepsilon_t, \tag{15}$$

where $\varepsilon_t = \sigma_t v_t$ and σ_t is assumed to depend on current and lagged values of other variables, z_t, and of lagged values of v_t. Hamilton chose the variables to include in x_t based on an analysis of institutional factors and checked his specification using a variety of specification tests.

In addition, v_t is assumed to be drawn from a mixture of normal distributions. Specifically, $v_t \sim N(0,1)$ with probability p and $v_t \sim N(0,\tau^2)$ with probability $(1-p)$. Hamilton argues that this distribution is useful for capturing very frequent small changes and the occasional very large changes observed in the funds rate and that it produces estimates of β that are robust to the influence of outliers. The variance of this distribution is

$$E(v_t^2) = p + (1-p)\tau^2. \tag{16}$$

The conditional variance of y_t is $\sigma_t^2 E(v_t^2)$. Hamilton assumes that the magnitude of σ_t^2 obeys an EGARCH model proposed by Nelson (1996). Specifically,

$$\ln(\sigma_t^2) - z_t'\kappa = \delta[\ln(\sigma_{t-1}^2) - z_{t-1}'\kappa] + \alpha[q(v_{t-1}) - Eq(v_{t-1}) + \aleph v_{t-1}]. \tag{17}$$

After experiencing difficulty maximizing the likelihood function with $q(\cdot) = |v|$, Hamilton assumed that

$$q(v_t) = \begin{cases} (1+v_t^2)/2 & \text{for } |v_t| < 1 \\ |v_t| & \text{for } |v_t| \geq 1. \end{cases} \tag{18}$$

The function given by equation (18) is smooth around zero, so that it is everywhere differentiable. The conditional log likelihood function,

$$L = \sum_{t=1}^{T} \ln[f(y_t \mid I_t)], \tag{19}$$

is maximized numerically subject to the constraint that $0 \leq p \leq 1$. The sequence $\{\sigma_t\}_{t=1}^T$ is generated recursively using equation (19), starting with $\ln(\sigma_0^2) = z_0'\kappa$ and $v_0 = 0$.

Results from Hamilton's Model

Hamilton investigated the behavior of Treasury balances at the Fed, nonborrowed reserves, and the federal funds rate using daily data over the period April 6, 1989, to November 27, 1991. Hamilton's sample begins with the first Thursday in a new maintenance period and ends on a settlement Wednesday. The data used in this study also begin on the first Thursday of a new maintenance period and end on a settlement Wednesday. The sample period, January 16, 1986, to January 29, 1997, includes Hamilton's sample period.

Hamilton estimated various forms of this general model for three variables: Treasury balances at the Fed, U_t, nonborrowed reserves, NBR_t, and changes in the federal funds rate, ΔFF_t. Hamilton's measure of nonborrowed reserves—deposit balances at the Fed less total bank borrowing from the Fed—is unconventional. The measure used here—total reserves less seasonal and adjustment borrowing—is used by the Trading Desk of the Federal Reserve Bank of New York to implement the FOMC's policy directive. The equations are estimated over three nonoverlapping periods: January 16, 1986, to April 5, 1989; April 6, 1989, to November 27, 1991; and November 29, 1991, to January 29, 1997.

Estimates of Hamilton's model for Treasury balances are presented in Table 1 for the three sample periods. In addition to lagged values of the dependent variable, x_t includes a variety of dummy variables that capture day-of-the-week, day-of-the-year, and other special day effects (a list of these variables is provided in the Appendix).

The estimates are identical to Hamilton's for his sample period. Over other sample periods, however, the results are somewhat different. There is considerable variation for many of the parameters across samples. Nevertheless, the estimates are broadly consistent with two features of Hamilton's estimates: namely, (i) that the Treasury balances converge fairly rapidly to the control level of $5 billion in normal circumstances and (ii) that they follow a random walk when Treasury balances exceed $8 billion. Moreover, summary statistics for the forecast errors, reported in Table 2, suggest that overall the model performs as well, or better, over the periods before and after his. The in-sample root-mean-squared error before and after Hamilton's period is smaller than for his sample period. The same is true of the median forecast error.

The EGARCH model does not force the forecast

Figure 1

Treasury Balance Equation Residuals
(1/16/86–4/5/89)

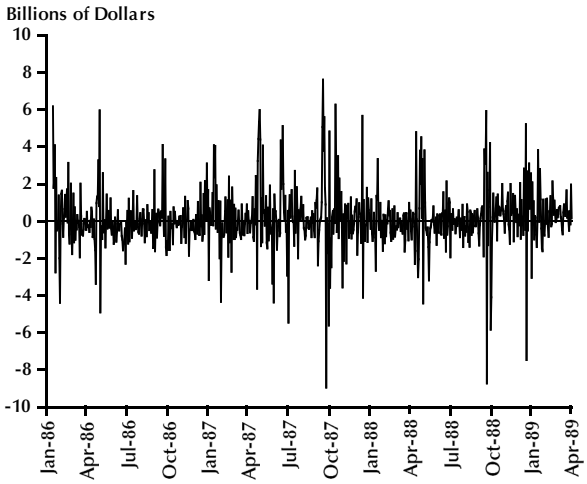
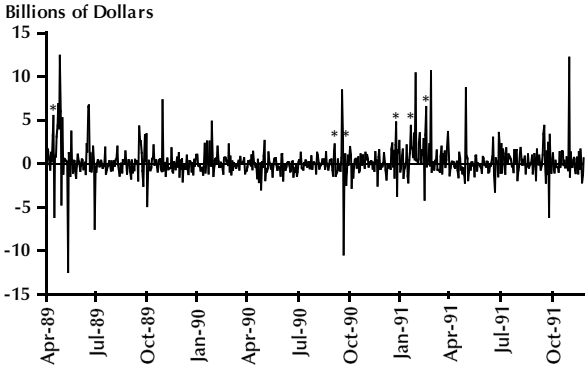


Figure 2

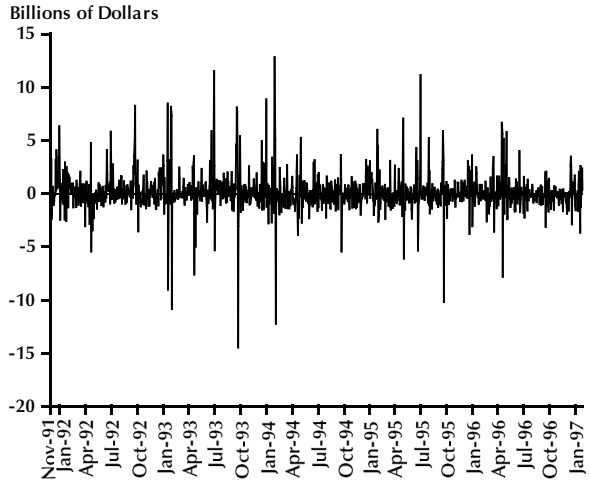
Treasury Balance Equation Residuals
(4/6/89–11/27/91)



NOTE: *Indicates settlement Wednesdays when the absolute change in federal funds rate was greater than 0.80 and the absolute value of the estimated treasurey residual was greater than 1.85.

Figure 3

Treasury Balance Equation Residuals
(11/29/91–1/29/97)



errors to be unbiased, and the average forecast error for all three periods is significantly greater than zero. The average forecast error is smaller for periods before and after Hamilton's, however. The forecast errors, plotted in Figures 1 through 3, suggest that there is no appreciable difference in the model's performance over the three periods. Furthermore, Q-statistics at various horizons show no significant serial correlation in the series during any of the three sample periods.[21]

In Hamilton's model the liquidity effect is given by the estimated response of changes in the funds rate to $\hat{\varepsilon}_t^U$. Estimates of Hamilton's funds rate equation are presented in Table 3.[22] My estimates of the funds rate equation for Hamilton's sample period are identical to his estimates. The liquidity effect is large and statistically significant only on settlement Wednesdays. On all other days, the response of the funds rate to $\hat{\varepsilon}_t^U$ is small and, with the exception of settlement Tuesday, not statistically significant. The estimated liquidity effect is small and not statistically significant on settlement Wednesday during the other sample periods. Indeed, the response of the funds rate to unanticipated Treasury balances on settlement Wednesday is perverse during the third sample period. Hence, there is no evidence of a liquidity effect using Hamilton's model except

[21] Little is gained from modeling the variance. Residuals obtained from applying OLS estimates to the mean are very similar to those presented in Figures 1 through 3. More important, none of the qualitative conclusions presented here are altered.

[22] Hamilton's nonborrowed reserve equation was also estimated. (For brevity, the results are not reported here.) The results are very similar to those reported by Hamilton. Indeed, the estimate of the coefficient on $\hat{U}_t$ differs little from that reported by Hamilton despite the significant difference in the measures used. Moreover, the estimate of the coefficient on $\hat{U}_t$ was negative and statistically significant in all samples regardless of whether nonborrowed reserves or total reserves were used. This is not surprising since unanticipated increases in Treasury balances reduce both nonborrowed and total reserves.

Table 1

Maximum Likelihood Estimates of the Model of U.S. Treasury Deposits (Dependent Variable $y_t = U_t$)

	1/16/86–4/5/89		4/6/89–11/27/91		11/29/91–1/29/97	
Parameter	Estimate	SE	Estimate	SE	Estimate	SE
Constant	1.097	0.125	2.348	0.221	3.507	0.166
U_{t-1}	0.779	0.042	0.537	0.041	0.309	0.029
U_{t-2}	–0.135	0.045	–0.048	0.027	0.007	0.019
U_{t-3}	0.073	0.030	0.080	0.021	0.029	0.018
d_{1t}	–0.872	0.833	–3.640	0.706	–4.424	0.804
$d_{1t}U_{t-1}$	0.223	0.064	0.460	0.068	0.622	0.080
$d_{1t}\zeta_{1t}$	–2.947	2.446	8.919	1.316	4.646	1.416
$d_{1t}\zeta_{1t}U_{t-1}$	–0.045	0.177	–0.989	0.085	–0.735	0.111
d_{2t}	0.801	0.252	0.674	0.235	1.193	0.127
d_{3t}	–0.286	0.082	–0.365	0.088	–0.068	0.057
γ_{4t}	0.043	0.108	–0.458	0.128	–0.227	0.103
ζ_{3t}	–0.115	0.158	–0.321	0.160	0.099	0.141
ζ_{4t}	0.370	0.150	0.614	0.174	0.147	0.121
ζ_t^n	0.030	0.142	0.942	0.369	0.607	0.195
Conditional variance parameters						
Constant	–0.372	0.128	–1.135	0.358	–0.937	0.117
d_{1t}	0.629	0.317	0.894	0.312	1.253	0.253
d_{2t}	0.972	0.292	1.327	0.263	0.552	0.238
d_{3t}	–0.030	0.137	–0.748	0.186	–0.298	0.131
ζ_t^n	0.122	0.361	2.031	0.322	1.193	0.226
Other parameters						
δ	0.864	0.039	0.948	0.023	0.710	0.083
α	0.239	0.058	0.087	0.036	0.096	0.049
$\aleph$	0.415	0.253	0.471	0.453	1.344	0.758
p	0.898	0.552	0.584	0.331	0.711	0.161
τ	3.128	0.392	2.565	0.288	2.851	0.157

NOTE: SE, standard error.

during his sample period and then only on settlement Wednesdays.

Hamilton's Results Reconsidered

The analysis presented above suggests Hamilton's results could be due to a few days when there were either unusually large forecast errors or unusually large changes in the funds rate. For the former explanation to be correct, it would take a very large forecast error. Hamilton's maximum absolute forecast error is about $12 billion. All other things the same, shocks of this magnitude would produce about a $0.86 billion aggregate reserve imbalance. Moreover, there were only eight shocks that were more than $8 billion in magnitude. Given the relative small size of these shocks, it seems unlikely that Hamilton's settlement day result is due to unusually large settlement-Wednesday shocks.

To see whether Hamilton's settlement-Wednesday results could be due to days when there

Table 2

Summary Statistics for Treasury Balance Shocks

Statistic	1/16/86–4/5/89	4/6/8–11/27/91	11/29/91–1/29/97
Mean	0.128	0.220	0.155
Median	0.009	0.037	0.006
Minimum	–8.94	–12.41	–14.45
Maximum	7.60	12.39	12.83
SD	1.58	1.85	1.77
RMSE	1.59	1.87	1.78
No. of observations	809	666	1301

NOTE: SD, standard deviation; RMSE, root-mean-squared error.

were unusually large changes in the federal funds rate, I identified settlement Wednesdays when the funds rate changed by more than 80 basis points—about two standard deviations of the daily change in the funds rate over the entire sample period. There were 10 settlement Wednesdays when the funds rate changed by more than 80 basis points. These dates are reported in Table 4, along with the associated shock to the Treasury's balance. On four of these days, the shock to the Treasury's balance was smaller than one standard deviation. There were just six settlement Wednesdays when the funds rate changed by more than 80 basis points and the shock to the Treasury's balance was relatively large (these observations are denoted with an asterisk in Figure 2).

To test the sensitivity of the results to these observations, settlement Wednesdays were partitioned into these six settlement Wednesdays and the remaining 63. Specifically, a dummy variable l was created, where $l = 1$ on these six days and $l = 0$ elsewhere. The funds rate equation was reestimated allowing for different level and slope effects. The estimates, presented in Table 5, indicate that there is a statistically significant level effect associated with these six days. When this effect is accounted for, the response of the funds rate to the shock to Treasury balances is no different on these six days than on the remaining 63 settlement Wednesdays and is consistent with the results for the other sample periods. Moreover, in both cases the response is statistically insignificant.

Three of these days (including two of the seven largest one-day changes in the funds rate in the sample period) came in the wake of the Fed's December 26, 1990, announcement that it was reducing to zero the reserve requirement on time and savings deposits. This action took banks by surprise. As a consequence of this action, the federal funds rate became more volatile and the federal funds market did not settle down completely until late March 1991.[23]

These results confirm what the analysis of the reserve market suggested. The significant response to a particular reserve supply shock on the last day of the maintenance period is not due to the liquidity effect. Rather, the significant settlement-day response is due to a few settlement Wednesdays when there were unusually large and transient changes in the funds rate.[24]

AN ALTERNATIVE METHOD OF INDENTIFYING THE LIQUIDITY EFFECT

The analysis of the preceding section showed that it was impossible to estimate the liquidity effect directly using daily data. It is possible, however, to estimate the liquidity effect indirectly. If the Fed moves the funds rate through open market operations, as is commonly assumed, there should be a systematic relationship between changes in the Fed's funds rate target and nonborrowed reserves. Specifically, the Fed should increase the supply of reserves (relative to its expectation of demand) when it lowers the funds rate target and reduce the supply of reserves when the target is raised.

Hence, it is possible to estimate the liquidity effect by estimating the change in nonborrowed reserves associated with a change in the funds rate target. This is done by estimating the reduced form

[23] The standard deviation for the daily change in the funds rate from December 26, 1990, to March 29, 1991, was 0.749 basis points, compared with 0.350 for the rest of the period from January 2, 1986, to January 31, 1997.

[24] John Partlan suggested another reason why Hamilton's results should be fragile: namely, that shocks to Treasury balances are small relative to other shocks. The total shock to nonborrowed reserves, $\hat{U}_{NBR}$, was obtained by subtracting from nonborrowed reserves the BOG's estimate of reserve demand and open market operations of the Desk that day. Regressing $\hat{U}_{NBR}$ on the average forecast error of the Treasury's balance, FE_t^{avg}, over the period February 11, 1987, to December 31, 1996, yields $\hat{U}_{NBR} = -0.654(0.06) - 0.994(0.07)FE_t^{avg}$. The estimated coefficient on FE_t^{avg} is very close to and not significantly different from its theoretical value of –1. Moreover, just as Partlan suggested, $\bar{R}^2 = 0.0776$, indicating that the error in the forecast of Treasury balances accounts for a relatively small proportion of the total shocks to nonborrowed reserves.

Table 3

Maximum Likelihood Estimates of the Model of the Federal Funds Rate (Dependent Variable $y_t = \Delta FF_t$)

	1/16/86–4/5/89		4/6/89–11/27/91		11/29/91–1/29/97	
Parameter	Estimate	SE	Estimate	SE	Estimate	SE
ϕ_{1t}	0.025	0.012	0.004	0.008	–0.014	0.010
ϕ_{2t}	–0.029	0.009	–0.042	0.007	–0.081	0.007
ϕ_{3t}	0.090	0.009	0.012	0.006	0.072	0.007
ϕ_{4t}	–0.061	0.009	–0.022	0.006	–0.067	0.006
ϕ_{5t}	–0.059	0.009	–0.022	0.006	–0.022	0.006
ϕ_{6t}	0.007	0.009	0.011	0.006	0.026	0.006
ϕ_{7t}	–0.030	0.009	–0.035	0.006	–0.043	0.006
ϕ_{8t}	0.098	0.011	0.030	0.008	0.115	0.009
ϕ_{9t}	–0.074	0.012	–0.004	0.008	–0.091	0.012
ϕ_{10t}	0.097	0.029	0.125	0.036	0.172	0.021
$\phi_{1t}\hat{\varepsilon}_t^U$	–0.009	0.005	0.009	0.005	–0.001	0.007
$\phi_{2t}\hat{\varepsilon}_t^U$	–0.004	0.006	–0.007	0.006	0.006	0.006
$\phi_{3t}\hat{\varepsilon}_t^U$	0.011	0.008	0.007	0.007	0.004	0.006
$\phi_{4t}\hat{\varepsilon}_t^U$	0.013	0.007	–0.003	0.004	–0.001	0.004
$\phi_{5t}\hat{\varepsilon}_t^U$	0.008	0.006	0.002	0.004	0.005	0.004
$\phi_{6t}\hat{\varepsilon}_t^U$	0.004	0.005	0.005	0.003	0.005	0.002
$\phi_{7t}\hat{\varepsilon}_t^U$	–0.005	0.006	–0.005	0.006	0.001	0.006
$\phi_{8t}\hat{\varepsilon}_t^U$	–0.002	0.009	–0.004	0.003	0.013	0.006
$\phi_{9t}\hat{\varepsilon}_t^U$	0.012	0.012	0.013	0.003	0.015	0.007
$\phi_{10t}\hat{\varepsilon}_t^U$	0.026	0.027	0.088	0.037	–0.016	0.013
d_{11t}	–0.021	0.035	–0.043	0.024	0.074	0.073
d_{12t}	–0.032	0.018	–0.005	0.015	–0.014	0.013
d_{13t}	0.089	0.036	–0.005	0.023	0.129	0.036
d_{14t}	0.198	0.021	0.116	0.028	0.238	0.019
$d_{15t}sp$	–0.770	0.031	–0.831	0.029	–0.648	0.033
Conditional variance parameters						
ϕ_{1t}	–4.535	0.203	–5.831	0.245	–4.905	0.173
$\sum_{j=2}^{7}\phi_{jt}$	–5.204	0.140	–6.211	0.167	–5.601	0.010
ϕ_{8t}	–4.734	0.207	–5.744	0.250	–4.599	0.169
ϕ_{9t}	–4.428	0.202	–5.644	0.229	–4.326	0.198
ϕ_{10t}	–2.702	0.204	–2.683	0.271	–3.120	0.166
$d_{7t}+d_{9t}$	1.533	0.635	0.944	0.469	1.877	0.436
$d_{8t}+d_{10t}$	1.577	0.320	2.024	0.414	2.161	0.315
d_{9t}	0.975†	—	0.975†	—	0.975†	—
d_{6t}	NA	NA	3.050	0.701	NA	NA
Other parameters						
δ	0.847	0.031	0.688	0.068	0.433	0.053
α	0.351	0.049	0.637	0.071	0.504	0.047
$\aleph$	0.516	0.109	–0.050	0.063	0.335	0.068
p	0.882	0.422	0.773	0.221	0.821	0.198
τ	3.000	0.334	3.144	0.270	3.489	0.225

NOTE: †Parameter constrained to this value; SE, standard error.

Table 4

Potential Influential Observations

Date	$\hat{\varepsilon}_t^U$	ΔFF_t
4/19/89†	5.50	1.00
5/16/90	–1.47	–0.81
8/22/90	–0.26	0.96
9/5/90†	2.24	1.14
9/19/90†	2.89	1.66
10/31/90	0.51	1.68
11/14/90	0.13	0.92
12/26/90†	4.77	2.41
1/23/91†	4.36	2.83
2/20/91†	6.53	0.87

NOTE: †Indicates settlement Wednesdays when $|\Delta FF_t| > 0.80$ and $|\hat{\varepsilon}_t^U| > 1.85$.

for nonborrowed reserves, i.e., estimating the nonborrowed reserves equation in equation (10). The variables include the estimate of reserve demand made by the staff of the Board of Governors, RD^e; the estimate of excess reserve demand made by the staff of the Board of Governors, ER^e; the average forecast error made by the staffs of the Federal Reserve Bank of New York, the Board of Governors, and the Treasury in forecasting the Treasury's balance at the Fed, FE^{avg}; the discount rate, DR; and the federal funds rate target, FF^*.

There were two significant changes in reserve requirements during the sample period. On December 27, 1990, the reserve requirement on non-personal time and savings deposits was reduced from 3 percent to zero in two stages. On April 2, 1992, the reserve requirement on transactions deposits was reduced from 12 percent to 10 percent. Theoretically, the coefficient on RD^e is equal to $\tau\lambda$, so it should change with changes in the type of deposits that are reservable and with changes in reserve requirements, assuming that demand for a given type of reservable deposits is stable. Consequently, RD^e is partitioned into the three periods of different reserve requirements using dummy variables RR_i, $i = 1,2,3$, that equal one when the ith reserve structure is in place and zero otherwise.

It is important to note that the Fed does not strictly adhere to operating procedure described by Feinman (1993). Considerable judgement is involved. Consequently, the estimated equation includes variables that do not enter the reduced form obtained from a strict interpretation of the operating procedure. One variable is the spread between the funds rate, and the funds rate target is lagged one day, $(FF - FF^*)_{t-1}$. There are several reasons why this variable might be important. First, if the Fed wishes to keep the funds rate close to the funds rate target, it is reasonable to assume that the Desk attempts to offset deviations of the funds rate from the target on the previous day (Taylor, 2001). An alternative explanation comes from noting that the Fed did not explicitly state the level of its funds rate target until August 1997. Consequently, the Desk could inject reserves when the funds rate is above the target and drain reserves when the funds rate is below the target to signal the level of the funds rate target to the market. Yet a third possibility comes from noting that under contemporaneous reserve accounting there is considerable uncertainty about the demand for reserves. In such instances the Desk may rely on a signal from the federal funds rate (Meulendyke, 1998, p. 180).

For any or all of these reasons, reserves could systematically increase when the funds rate is above the funds rate target the previous day and decline when the previous day's funds rate is below the target. Of course, this should not happen on days when the Fed changes the funds rate target, so this variable is included only on days when the funds rate target is not changed.

In addition, I have elsewhere shown (Thornton, 2001a) that the Desk tends to offset changes in borrowing. Hence, the spread between actual borrowing and the borrowing assumption on the previous day, $(BR - BRAS)_{t-1}$, is also included. If the Desk offsets borrowing, the coefficient on this spread should be negative and statistically significant. Finally, to estimate the extent to which the Fed systematically changes reserves when it changes the federal funds rate target, the change in the funds rate target, ΔFF^*, is included.

The sample period is from February 11, 1987, to December 31, 1996. Beginning with its February 1994 meeting, the FOMC began announcing target changes. Prior to that, most target changes occurred during the intermeeting period.[25] Target changes have also been much less frequent since 1994 and changes have always been multiples of 25 basis

[25] Of the 63 target changes made prior to February 1994, only 10 were made the day after regularly scheduled FOMC meetings. In contrast, all but two target changes after 1994 were initiated at regularly scheduled FOMC meetings. See Thornton (1996) for more details.

Table 5

Maximum Likelihood Estimates of the Model of the Federal Funds Rate (Dependent Variable $y_t = \Delta FF_t$)

	4/6/89–11/27/91	
Variable	Estimate	SE
ϕ_{1t}	0.007	0.008
ϕ_{2t}	–0.043	0.006
ϕ_{3t}	0.012	0.006
ϕ_{4t}	–0.022	0.006
ϕ_{5t}	–0.022	0.006
ϕ_{6t}	0.011	0.006
ϕ_{7t}	–0.034	0.006
ϕ_{8t}	0.031	0.008
ϕ_{9t}	–0.004	0.008
$(1-l)\phi_{10t}$	0.123	0.034
$l\phi_{10t}$	1.251	0.319
$\phi_{1t}\hat{\varepsilon}_t^U$	0.009	0.005
$\phi_{2t}\hat{\varepsilon}_t^U$	–0.001	0.006
$\phi_{3t}\hat{\varepsilon}_t^U$	0.007	0.006
$\phi_{4t}\hat{\varepsilon}_t^U$	–0.003	0.004
$\phi_{5t}\hat{\varepsilon}_t^U$	0.002	0.004
$\phi_{6t}\hat{\varepsilon}_t^U$	0.004	0.003
$\phi_{7t}\hat{\varepsilon}_t^U$	–0.006	0.005
$\phi_{8t}\hat{\varepsilon}_t^U$	–0.004	0.002
$\phi_{9t}\hat{\varepsilon}_t^U$	0.012	0.003
$(1-l)\phi_{10t}\hat{\varepsilon}_t^U$	0.044	0.034
$l\phi_{10t}\hat{\varepsilon}_t^U$	–0.041	0.113
d_{11t}	–0.042	0.023
d_{12t}	–0.001	0.015
d_{13t}	–0.006	0.023
d_{14t}	0.102	0.026
$d_{15t}sp$	–0.861	0.021
Conditional variance parameters		
ϕ_{1t}	–5.835	0.248
$\sum_{j=2}^{7}\phi_{jt}$	–6.196	0.169
ϕ_{8t}	–5.735	0.256
ϕ_{9t}	–5.597	0.230
ϕ_{10t}	–2.822	0.257
$d_{7t}+d_{9t}$	0.813	0.412
$d_{8t}+d_{10t}$	1.975	0.464
d_{9t}	0.975†	—
d_{6t}	2.913	0.699
Other parameters		
δ	0.728	0.058
α	0.617	0.077
$\aleph$	–0.026	0.055
p	0.780	0.225
τ	3.143	0.272

NOTE: †Parameter constrained to this value; SE, standard error.

points. It is possible that the Desk has controlled the funds rate since 1994 through what Guthrie and Wright (2000) call *open mouth operations*.[26] Meulendyke (1998) and Hanes (1998) have also suggested this possibility. In this case, the Fed need only announce target changes and the market adjusts to the new target level. Consequently, the sample is partitioned into the periods February 11, 1987, to January 31, 1994, and February 1, 1994, to December 31, 1996.

The Pre-1994 Results

Estimates of the nonborrowed reserves equation for the first period are reported in the first two columns of Table 6. Not all of the estimates conform to the structural model of the reserve market. For example, neither the coefficient on the funds rate target nor the discount rate is correctly signed, and neither is statistically significant. Likewise, the coefficient on ER^e is incorrectly signed, but is marginally significant at the 5 percent significance level.

The estimated coefficients on RD^e over the three partitions are positive and highly significant, and these variables alone account for over 70 percent of the variation in nonborrowed reserves over the period. In contrast to what the theory would suggest, however, the estimates do not vary with changes in reserve requirements. Indeed, all three estimates are close to unity and the hypotheses that each is equal to unity, and that they are jointly

[26] Guthrie and Wright (2000) develop this idea in response to an observation made by McCallum (1995).

Table 6

Estimates of Nonborrowed Reserves Equations: February 1, 1987, to January 31, 1994

	FF^*		FF^{**}	
Variable	Estimate	SE	Estimate	SE
Constant	2.144	1.841	2.352	1.836
FF_t^*	0.059	0.162	0.028	0.162
DR_t	–0.064	0.241	–0.011	0.241
$RR_1RD_t^e$	0.975*	0.026	0.972*	0.026
$RR_2RD_t^e$	0.991*	0.031	0.988*	0.031
$RR_3RD_t^e$	0.990*	0.030	0.987*	0.030
ER_t^e	–0.647*	0.310	–0.667*	0.310
FE_t^{avg}	–0.847*	0.066	–0.840*	0.067
$(BR-BRAS)_{t-1}$	–0.643*	0.084	–0.653*	0.084
ΔFF_t^*	–3.768*	1.217	–3.262*	1.096
$(FF-FF^*)_{t-1}$	2.851*	0.242	2.743*	0.244
L10	1.527*	0.186	1.529*	0.187
D10	1.729*	0.464	1.662*	0.465
AR(1)	0.177*	0.024	0.172*	0.024
AR(19)	0.141*	0.024	0.141*	0.024
$AdjR^2$	0.785		0.782	
SE	2.280		2.289	

NOTE: SE, standard error.

equal, cannot be rejected. One reason that these estimates differ from the theoretical values suggested in equation (10) is the demand for reservable deposits is not interest sensitive at the daily frequency. Hence, the Desk is not necessarily estimating the demand for reserves consistent with the funds rate target each day, but rather is trying to accommodate reserve demand.[27]

As theory predicts, the estimated coefficient on FE^{avg} is negative and highly significant. Nevertheless, the estimate differs significantly from its theoretical value of –1.[28] One reason this coefficient differs from its theoretical value is the weight that the Desk gives to the three agency's forecast changes over time (for a discussion of this and other reasons, see Thornton, 2001b).

Also, consistent with my analysis elsewhere (Thornton, 2001a), the coefficient on $(BR-BRAS)_{t-1}$ is negative and statistically significant. The Desk appears to offset a significant proportion of deviations of the previous day's borrowing from the assumed level.

The coefficient on $(FF-FF^*)_{t-1}$ is positive and statistically significant. Hence, the Desk systematically adds reserves when the funds rate is above the target the previous day and drains when the previous day's funds rate is below the target. Unfortunately, it is impossible to know whether the Desk is (i) trying to keep the funds rate close to the funds rate target (e.g., Taylor, 2001), (ii) trying to signal the level of the funds rate target, or (iii) uncertain about reserve demand.

The coefficient on ΔFF_t^* is negative and statistically significant, indicating that the Desk systematically drains reserves when the funds rate target is raised and adds reserves when the target is reduced. The estimates indicate that the actions that the Desk takes when the funds rate target is changed are mod-

[27] The forecasts are updated as information on deposits comes in during the maintenance period. For a description of the procedure see Meulendyke (1998, Chap. 6).

[28] If the residuals from Hamilton's model are used, the estimated coefficient is about –0.4.

est, however. For example, the estimate suggests that on average the Desk drains less than $1 billion from reserves when the funds rate target is increased by 25 basis points. The relative magnitude of these actions can be seen from noting that the standard error for the nonborrowed reserves equation is about $2.3 billion. Hence, random shocks to nonborrowed reserves are nearly 2.5 times larger than the estimate of the open market operation required to move the equilibrium federal funds rate 25 basis points.

Moreover, the average absolute value of the residual to the reserve equations is about $1.6 billion—nearly twice as large as the estimate of the actions the Fed takes to change the funds rate target by 25 basis points. The average absolute deviation of the funds rate from the funds rate target during this period was about 17 basis points. Hence, shocks to reserves, which cause the funds rate to deviate from the target by an average of about 17 basis points, are nearly twice as large as the open market operations that are required to change the equilibrium funds rate by 25 basis points.

Alternative Series of Funds Rate Target Change

The estimated size of open market operations might be small because ΔFF^* includes target changes that were made to accommodate changes in reserve demand. Such changes would not represent exogenous changes in monetary policy. Recently, an unofficial Board staff series of target changes that reflect changes in monetary policy has been made available (Thornton and Wheelock, 2000). This federal funds rate target series, FF^{**}, differs from the series used here prior to 1990 in that there are fewer target changes and, in some cases, the target changes differ in timing and/or magnitude.

In order to test whether the results are sensitive to the funds rate target series used, the nonborrowed reserve equation was reestimated using FF^{**}. The results are presented in the last two columns of Table 6. The response of nonborrowed reserves to changes in the funds rate target is insensitive to the funds rate target series used. In particular, the coefficients on ΔFF_t^{**} and $(FF-FF^{**})_{t-1}$ are nearly identical to those on ΔFF_t^* and $(FF-FF^*)_{t-1}$. Hence, the relatively modest size of the estimates of Fed actions cannot be attributed to a mixing of endogenous and exogenous target changes.

The Post-1994 Results

It could be that open market operations are not undertaken to move the equilibrium federal funds rate, per se. Rather they are undertaken to signal a change in the target and to signal the target level. Meulendyke (1998, pp. 141-42) has suggested this possibility, stating:

> [A]chieving the degree of reserve pressures specified in the directive has been interpreted since the late 1980s to mean creating conditions consistent with the FOMC's desired Federal funds rate. That rate has generally been apparent to the banks; since 1994 it has been announced formally and in prior years it was clearly indicated through an open market operation. The rate has tended to move to the new, preferred level as soon as banks knew the intended rate, with little or no change in the amount of borrowing allowed for when constructing the path for nonborrowed reserves.

For example, the Fed could signal the level of the funds rate by injecting reserves when the funds rate is above the target and by draining reserves when the rate is below the target. On days when the funds rate target is changed, the Desk injects or drains a larger quantity of reserves to signal a change in the funds rate target. Whether intended or not, there is evidence that market analysts used such information to make judgements that the funds rate target had changed in the 1970s (Cook and Hahn, 1989, and Thornton, 1999).

It would be unnecessary to signal target changes after the Fed began announcing them in 1994. Hence, to investigate the signaling hypothesis, the nonborrowed reserve equation was estimated over the period February 1, 1994, to December 31, 1996. The estimates are presented in Table 7. During this period, all but one target change occurred at regularly scheduled FOMC meetings. The target changes were announced the day the FOMC made the decision; but, because of the Desk's practice of entering the market once per day, these changes could not be implemented in terms of open market operations until the next day. Hence, changes in the target are aligned to the first day that the Desk could have implemented the FOMC's decision.

The results over this period are broadly similar to those over the previous period, even though nonborrowed reserves trended down as banks instituted sweep programs to avoid reserve requirements

[29] A trend was included in the equation for this period. The coefficient was negative but not statistically significant, so the results without the trend are reported here.

Table 7

Estimate of the Nonborrowed Reserves Equation: February 1, 1994, to December 31, 1996

Variable	Estimate	SE
Constant	0.059	3.019
FF_t^*	–0.916	0.695
DR_t	1.087	0.748
RD_t^e	1.006*	0.050
$(BR-BRAS)_{t-1}$	0.557*	0.266
FE_t^{avg}	–0.889*	0.083
ΔFF_t^*	–2.205*	1.451
$(FF-FF^*)_{t-1}$	0.740*	0.354
L10	2.834*	0.246
D10	0.844	0.616
AR(1)	0.165*	0.037
AR(19)	0.169*	0.037
$AdjR^2$	0.770	
SE	1.912	

NOTE: SE, standard error.

Table 8

Estimate of the Nonborrowed Reserves Equation: February 11, 1987, to January 31, 1994

Variable	Estimate	SE
Constant	2.178	1.185
FF_t^*	0.064	0.161
DR_t	–0.064	0.240
$RR_1RD_t^e$	0.975*	0.026
$RR_2RD_t^e$	0.991*	0.031
$RR_3RD_t^e$	0.989*	0.030
ER_t^e	–0.619	0.310
FE_t^{avg}	–0.839*	0.066
$(BR-BRAS)_{t-1}$	–0.641*	0.084
$D_{B90}\Delta FF_t^*$	–5.496*	1.632
$D_{A90}\Delta FF_t^*$	–1.812	1.836
$D_{B90}(FF-FF^*)_{t-1}$	2.806*	0.363
$D_{A90}(FF-FF^*)_{t-1}$	2.890*	0.305
L10	1.517*	0.186
D10	1.695*	0.464
AR(1)	0.175*	0.024
AR(19)	0.140*	0.024
$AdjR^2$	0.783	
SE	2.280	

NOTE: SE, standard error.

(e.g., Anderson and Rasche, 2000).[29] The coefficients on the funds rate target and the discount rate are correctly signed but not statistically significant at the 5 percent level. The coefficient on RD^e is again very close to and not significantly different from unity, suggesting that the Fed accommodated the impact of sweep accounts on required reserves. The coefficient on $(BR-BRAS)_{t-1}$ is again positive and significant at the 5 percent level. The coefficient on FE^{avg} is negative and significant and not significantly different from its theoretically correct value of –1.

The coefficient on ΔFF^* is smaller than for the previous period and is insignificant. This result is consistent with Meulendyke's (1998) argument that the Fed has controlled the funds rate though open mouth operations since 1994. Once the Fed began announcing target changes there was no reason to signal them through open market operations.

After 1994 all funds rate target changes were multiples of 25 basis points. Consequently, the market should have had less difficulty determining the level of the funds rate target. If the Fed is systematically responding to the difference between the funds rate and the funds rate target on the previous day in order to signal the level of the funds rate target, one might expect the Desk to take smaller, and perhaps less frequent, actions after 1994. Consistent with that interpretation, the coefficient on $(FF-FF^*)_{t-1}$ is much smaller after 1994.

It is unlikely that the Fed controlled the funds rate through open mouth operations prior to 1994 and even less likely prior to 1990. In order for open mouth operations to work the market must know the level of the funds rate target. Elsewhere (Thornton, 1999) I argue that the market was unaware that the Fed was targeting the federal funds rate because the Fed told the public that it was targeting borrowed reserves. I also present statistical and documentary evidence consistent with the market being unaware that the Fed was targeting the funds rate until late 1990. If the market was

unaware that the Fed was targeting the funds rate, the Fed could not have controlled the funds rate through open mouth operations. While the market was aware that the Fed was targeting the funds rate after 1990, changes in the target were not announced. Consequently, if the Fed was using open market operations to signal target changes, it would have no need to do so until after 1990.

To further investigate this possibility, the nonborrowed reserves equation was estimated partitioning ΔFF^* and $(FF-FF^*)_{t-1}$ into periods before and after November 30, 1990, using a dummy variable, D_{B90} (that equals one before December 1, 1990, and zero otherwise) and a dummy variable, D_{A90} (that is equal to one after November 30, 1990, and zero otherwise). These estimates are presented in Table 8.[30]

Estimates of the coefficients on ΔFF^* differ significantly over the two samples. The coefficient is statistically significant before December 1990 but not after, which is inconsistent with the signaling hypothesis. It is more consistent with the hypothesis that the Fed was trying to move the funds rate through open market operations. While the coefficient before 1990 is about 40 percent larger than that over the entire sample period, the estimate is still small relative to the standard error of nonborrowed reserves. Moreover, the relatively small and insignificant coefficient after 1990 is inconsistent with either the open market or open mouth alternatives.

In contrast to the marked difference in the estimates of the coefficients on ΔFF^*, the coefficients on $(FF-FF^*)_{t-1}$ are statistically significant and nearly identical. Indeed, the null hypothesis that they are equal cannot be rejected. Reconciling these estimates is difficult. It could be that the significant coefficient on $(FF-FF^*)_{t-1}$ arises because of uncertainty with the Desk's estimates of reserve demand, as Meulendyke (1998) and others have suggested. If this explanation accounts for the significant relationship between nonborrowed reserves and $(FF-FF^*)_{t-1}$, why did the relationship weaken after 1994?

Is There a Liquidity Effect at the Daily Frequency?

The evidence presented here suggests that there may not be a liquidity effect at the daily frequency. While the evidence is that the Fed systematically adjusted nonborrowed reserves in a manner that is consistent with moving the equilibrium funds rate, it did so only before December 1990. After 1990 there is no statistically significant systematic relationship between changes in nonborrowed reserves and changes in the Fed's target for the federal funds rate. Moreover, and equally important, the magnitude of the Fed actions are relatively small even before 1990. It is difficult to see how such relatively small actions could account for relatively large changes in the equilibrium federal funds rate. These results are consistent with evidence (Thornton, 1999, and Poole and Rasche, 2000) that indicates no statistically significant response of the funds rate to changes in the funds rate target. It is also broadly consistent with evidence on the dynamic relationship between the federal funds rate and the 3-month T-bill rate (Sarno and Thornton, 2000).

The evidence is broadly consistent with the suggestion by some that the Fed controls the funds rate through open mouth operations. According to this view, all the Fed need do is announce or signal the target change and the market does the rest. It is easy to see how open mouth operations could account for the lack of policy actions after 1994, when the Fed began announcing target changes. It is much more difficult to see how open mouth operations could work very effectively before 1994. While the Fed was open about the fact that it was targeting the funds rate after 1990, target changes were not announced. Consequently, the Fed would have had an incentive to signal target changes with open market operations during the 1990-94 period.

The evidence indicates that there is no significant relationship between these variables after 1990. Hence, the lack of a statistically significant systematic relationship between nonborrowed reserves and changes in the funds rate target after 1994 cannot be taken as strong support for the open mouth hypothesis. Hence it is impossible to conclude that the Fed has controlled the funds rate through open mouth alternatives on the basis of the evidence presented here.

Nevertheless, the lack of systematic relationship between nonborrowed reserves and changes in the Fed's funds rate target after 1990 and the relatively small estimated size of the systematic response before 1990 suggest that the Fed has not implemented target changes through open market operations. Hence, the evidence presented here is inconsistent with the conventional story of how the

[30] These results are also insensitive to the target series used.

Fed controls the funds rate through open market operations.

Finally, there is strong evidence that the Fed responds significantly to the previous day's deviation of the funds rate from the funds rate target. What is less clear is why the Fed responds significantly. The explanation that the Fed does this to keep the funds rate close to the target level would appear to be inconsistent with the evidence that the Fed does not implement target changes through open market operations. The explanation that the Fed does this to signal the target level would appear to be at odds with the fact that before 1990 the Fed was officially implementing monetary policy with a borrowed reserves operating procedure and with evidence presented elsewhere (Thornton, 1999) that the market was unaware that the Fed was directly targeting the funds rate until after late 1990. The evidence is consistent with the Desk using information from the funds rate when it is uncertain of its estimates of reserve demand (Meulendyke, 1998). The remaining issue is why the response became smaller after 1994.

CONCLUSIONS

Following up on work by Hamilton (1997), this paper investigates whether the liquidity effect can be identified using daily data. In so doing I derive a structural model of the reserve market based on the Fed's operating procedure. An analysis of this model shows why the liquidity effect cannot be estimated using Hamilton's methodology. Consistent with this conclusion, I show that there is no evidence of a statistically significant liquidity effect using Hamilton's methodology for periods before and after his and that the liquidity effect which he finds on settlement Wednesdays is due to a few days when there were unusually large changes in the funds rate. When these days are accounted for, there is no evidence of the liquidity effect during Hamilton's sample period.

Estimates of the reduced form of the structural model of the reserve market provide little support for the conventional view that the Fed controls the funds rate using open market operations, i.e., the liquidity effect. At times the Fed has conducted open market operations in a manner consistent with its operating objective. The size of these operations appears to be so small that they cannot account for changes in the equilibrium funds rate. The relationship between Fed actions, the funds rate, and the funds rate target appears to be much more complicated than is usually thought and more analysis will be required to understand it.

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Appendix

Discrete Variables Used in Estimating Hamilton's Model

Variable	Description
γ_{jt}	1 if t falls in month j for j=1,2,…,12 and 0 otherwise
ζ_{jt}	1 if t falls on business day j of the month for j=1,2,…,23 and otherwise
ζ_t^n	1 if t falls on the last business day of the month and 0 otherwise
ϕ_{jt}	1 if t falls on day j of a reserve maintenance period for j=1,2,…,10 and 0 otherwise
sp	Spread of volume weighted average of federal funds rate from (t−1) to (t−3)
l	1 if day t is one of six days when $\lvert\Delta FF_t\rvert > 0.80$ and $\lvert\hat{\varepsilon}_t^U\rvert > 1.85$ and 0 otherwise
$\hat{\varepsilon}_t^U$	Estimated residual from model of U.S. Treasury deposits
d_{1t}	1 if $U_{t-1} > 8$ and 0 otherwise
d_{2t}	1 if t falls on a major tax-collection day (month is January, April, June or September and t comes after the first Monday following the 15th) and 0 otherwise
d_{3t}	1 if t falls on a Friday and 0 otherwise
d_{6t}	1 if 10 January 1991 $\leq t \leq$ 6 February 1991 and 0 otherwise
d_{7t}	1 if t is last day of quarter 1, 2, or 3 and 0 otherwise
d_{8t}	1 if t is the day before, the day of, or the day after the last day of quarter 1, 2, or 3 and 0 otherwise
d_{9t}	1 if t is the last day of the year and 0 otherwise
d_{10t}	1 if t is two days before, the day before, the day of, the day after, or two days after the last day of the year and 0 otherwise
d_{11t}	1 if t precedes a one-day holiday and 0 otherwise
d_{12t}	1 if t precedes a three-day holiday and 0 otherwise
d_{13t}	1 if t follows a one-day holiday and 0 otherwise
d_{14t}	1 if t follows a three-day holiday and 0 otherwise
d_{15t}	1 if t is the first day of a new maintenance period or the first day of a new quarter and 0 otherwise

Commentary

Simon Gilchrist

Dan Thornton has written a very interesting paper on estimating the liquidity effect at the daily frequency. The paper starts with a nice discussion of the historical origins of the term "liquidity effect" (tracing it back to Milton Friedman, but somewhat surprisingly, no farther). The paper then discusses ways to identify the liquidity effect in daily data, and applies this discussion to critique the recent methodology of Hamilton (1997). The paper finds that the Hamilton results are not robust across sample periods and appear to be sensitive to outliers. Finally, the paper proposes an alternative way to estimate the liquidity effect via the relationship between nonborrowed reserves and the target funds rate. Here the paper has mixed success. Some variables seem highly correlated in the appropriate way, but the basic relationship between nonborrowed reserves and the target federal funds rate appears fragile—often it is insignificant, and sometimes it is the wrong sign.

In my discussion, I intend to provide a brief overview of the literature on liquidity effects, followed by a very simple exposition of a model along the lines of the one presented in the paper. I use this model to illustrate the basic arguments in the paper, and then by changing the assumption regarding the relationship between the Open Market Desk's (OMD) operating procedure and the desired level of nonborrowed reserves, I argue that we may have reason to expect biased and econometrically fragile parameter estimates from the nonborrowed reserves equation that is estimated in the paper.

The modern macroeconomic literature on liquidity effects can be divided into two separate strands: the theoretical literature that examines whether the current generation of dynamic general equilibrium models can generate a liquidity effect in response to innovations in money growth rates, and the identified vector autoregression (VAR) literature which examines whether there exists a contemporaneous negative relationship between money and nominal interest rates in response to monetary policy innovations. In the theoretical literature there are two types of models that are capable of generating a liquidity effect: limited participation, cash-in-advance models with heterogeneous agents; and sticky-price models that generate a demand for real balances through a transactions demand for money or, equivalently, money in the utility function. In both types of models, the theoretical difficulty with generating a liquidity effect stems from the fact that, with positively autocorrelated money growth, a rise in money growth today leads to anticipated inflation. Such anticipated inflation tends to push the nominal interest rate up rather than down, even if the real interest rate is falling in response to the innovation in money growth. Under certain parameterizations, either model may deliver a liquidity effect. Such parameterizations often involve imposing restrictions on quantity responses, either through inflexible production—as in the recent example of Christiano and Eichenbaum (1995)—or through the use of substantial curvature of the transactions benefits associated with real balances, combined with a limited response of interest-sensitive components of spending such as investment. In these frameworks, the response of money-demand to interest rates is a key model parameter that helps determine the liquidity effect.

The second strand of this literature using identified VARs to estimate the liquidity effect in time series data also has mixed success, with results depending on specification, sample period, and methods of identification. The problem here is that the assumptions required to identify monetary policy innovations and, hence, the liquidity effect in this literature are not necessarily valid; for example, the specifications considered by Bernanke and Blinder (1992) and Christiano and Eichenbaum (1992) both deliver liquidity effects by imposing the assumption that monetary policy does not have contemporaneous effects on output or prices. Avoiding these types of restrictions motivates Hamilton's use of daily data and his search for a valid instrument. Let me discuss this methodology by considering a simple version of Thornton's model for the daily federal funds market.

A SIMPLE MODEL OF THE FEDERAL FUNDS MARKET

I assume that demand for total reserves depends negatively on r_t, the current overnight federal funds

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rate, and positively on v_t^d, an i.i.d. shock to reserve demand

$$TR_t^d = -\lambda r_t + v_t^d. \tag{1}$$

For simplicity, I abstract from discount-window borrowing and assume that total reserves supply depends on B_t, the current level of nonborrowed reserves, and v_t^s, an i.i.d. shock to reserve supply:

$$TR_t^s = B_t + v_t^s. \tag{2}$$

Monetary policy is specified through the relationship between B_t^*, the desired level of nonborrowed reserves, and $\bar{r}_t$, the target level for the federal funds rate. In particular, B_t^* is chosen as in Thornton's model so that expected reserve supply equals expected demand at the target rate of interest:

$$B_t^* = -\lambda \bar{r}_t. \tag{3}$$

Lastly, following Thornton, I specify the relationship between the actual and desired level of nonborrowed reserves, B_t^*:

$$B_t = B_t^* + \omega_t, \tag{4}$$

where ω_t represents control error which is assumed to be i.i.d. Equation (4) may be thought of as the OMD's operating instructions given its desired level of reserves B_t^*. Here, the Trading Desk chooses desired reserves to minimize the error between actual and desired reserves on a daily basis. Equating supply and demand and applying equations (3) and (4), we obtain the reduced form equation for the federal funds rate as a function of the target rate and the i.i.d. shocks to demand and supply

$$r_t = \bar{r}_t - \frac{1}{\lambda}\left(B_t - B_t^*\right) + \frac{1}{\lambda}\left(v_t^d - v_t^s\right) \tag{5}$$

$$= \bar{r}_t + \frac{1}{\lambda}\left(v_t^d - v_t^s - \omega_t\right). \tag{6}$$

In this simple model, Hamilton's procedure reduces to a regression of the federal funds rate on the target rate and v_t^s an observable component of the shock to supply (the forecast error in Treasury balances). The coefficient on v_t^s allows one to identify the slope of the demand curve λ. Given inelastic supply, we only need one reduced form equation to identify λ. In Thornton's more general model, the supply curve is an upward-sloping function of the federal funds rate through the supply for borrowed reserves. In this case, as in Hamilton, we would need to estimate the reduced form for both the price and quantity equation to identify λ.

Thornton's critique of Hamilton is two pronged. The first prong of criticism is directed at the Treasury balance forecast error constructed by Hamilton. This variable was constructed using a VAR forecasting system. By obtaining the actual forecast errors made by the Board of Governors and the Treasury, Thornton notes that the VAR-based error has substantially higher variance, suggesting, unsurprisingly, that the VAR-based forecast error does not use all information available to the Federal Reserve and is thus subject to measurement error. Pure measurement error would lead to a downward bias in the coefficient estimate of $(1/\lambda)$ and hence upward bias in the estimate of λ itself. To the extent that the difference between the two forecast errors reflects information that is used to set monetary policy, the VAR-based error is no longer a valid instrument. These arguments strike me as reasonable, and the paper makes a valuable contribution to the literature by documenting and discussing such distinctions.

The second prong of criticism is directed at the robustness of Hamilton's findings. By collecting a larger data sample unavailable to Hamilton at the time, Thornton examines the robustness of Hamilton's findings across sample periods. Unfortunately, the liquidity effect only appears in the middle period that reflects Hamilton's data. Furthermore, the results appear to be due to a limited number of data points that involve, simultaneously, large movements in the Treasury balance and the federal funds rate. In addition, owing to the timing of the reserve maintenance period, the parameter estimates obtained by this procedure cannot reflect the coefficient λ in the transactions demand equation specified by Thornton. More likely they reflect some component of the demand for excess reserves. Again, Thornton does a persuasive job documenting the fragility of such results. 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 changes over time. By and large, both Hamilton and Thornton exhibit such care, though what one obtains from one sample period may still not match what one obtains from another.

As an alternative methodology, Thornton suggests estimating λ by regressing nonborrowed reserves on the target level of the funds rate. In the context of this simple model, such a procedure

implies estimating the relationship between B_t and $\bar{r}_t$ embedded in equations (3) and (4):

$$B_t = -\lambda \bar{r}_t + \omega_t. \tag{7}$$

In the data, we should add suitable controls for other variables such as the discount-window borrowing that modify the relationship between nonborrowed reserves and the target funds rate. Thornton does more than that, however, adding both levels and changes in the target along with a host of right-hand-side variables, only some of which are justified by his model. While comprehensive, such an all-inclusive approach makes it difficult to get a solid fix on λ. At best, we can hope to identify the negative relationship implied by the theory. In the rest of my discussion, I wish to consider an alternative source of bias that could make estimation of equation (7) difficult.

A MODEL WITH PARTIAL ADJUSTMENT

Consider a slight variant on the OMD's operating procedure that allows for partial adjustment along the lines considered by Taylor (2001) also found in this volume:

$$B_t - B_{t-1} = \alpha\left(B_t^* - B_{t-1}\right) + \left[\alpha\omega_t + (1-\alpha)\Delta\omega_{t-1}\right]. \tag{8}$$

The first term on the right-hand side of equation (8) implies that the OMD does not immediately close the gap between desired and actual nonborrowed reserves, but does so with some adjustment process. Although not motivated through an explicit theory based on loss-functions, Taylor argues that such a model provides a reasonable approximation to the actual operating procedures of the OMD. The second term on the right-hand side implies that the OMD fully responds to control errors, ω_t. This can be seen by rearranging equation (8),

$$B_t - B_t^* - \omega_t = -(1-\alpha)\Delta B_t^* + (1-\alpha)\left(B_{t-1} - B_t^* - \omega_{t-1}\right),$$

and solving backwards to obtain

$$B_t = B_t^* - (1-\alpha)\sum_{i=0}^{\infty}(1-\alpha)^i \Delta B_{t-i}^* + \omega_t. \tag{9}$$

Equation (9) implies a slow adjustment of B_t to changes in the desired level of nonborrowed reserves—when B_t^* rises by one percent, B_t increases by the fraction α. Over time, B_t will rise by an additional $(1-\alpha)$ percent so that B_t and B_t^* track each other at a longer horizon. Given monetary policy, $B_t^* = -\lambda \bar{r}_t$, we obtain reduced-form expressions for both B_t and r_t:

$$B_t = -\lambda \bar{r}_t + \lambda(1-\alpha)\sum_{i=0}^{\infty}(1-\alpha)^i \Delta \bar{r}_{t-i} + \omega_t \tag{10}$$

$$r_t = \bar{r} - (1-\alpha)\sum_{i=0}^{\infty}(1-\alpha)^i \Delta \bar{r}_{t-i} + \frac{1}{\lambda}\left(v_t^d - v_t^s - \omega_t\right). \tag{11}$$

There are two important implications that can be obtained from these expressions. First, the interest rate equation implies that deviations of the overnight rate from its target, $r_t - \bar{r}_t$, will be positively correlated if the target level displays persistence. This is a realistic feature of the data, documented by Balduzzi, Bertola, and Silvero (1997) and more recently by Taylor (2001).[1] In the extreme case of full adjustment, $\alpha = 1$ and deviations of the funds rate from its target are i.i.d. as in Thornton's model. This restriction is clearly at odds with the data, however.

The second important implication is that a regression of nonborrowed reserves on the federal funds target will yield a biased estimate of λ. To see this, suppose that the target rate follows an AR(1) stochastic process

$$\bar{r}_t = \rho \bar{r}_t + \varepsilon_t \tag{12}$$

and consider estimating the following regression:

$$B_t = \theta \bar{r}_t + u_t. \tag{13}$$

In this case, one can show that

$$p\lim\left(\hat{\theta}\right) - \lambda = \lambda(1-\rho)\frac{(1-\alpha)}{\left(1-(1-\alpha)\rho\right)}. \tag{14}$$

In the case that $\bar{r}_t$ is i.i.d., the bias is $\lambda(1-\alpha)$, which depends on the degree of partial adjustment of B_t to B_t^*. In the case that $\bar{r}_t$ is a random walk, the bias is zero since the parameter estimate is superconsistent. More generally, the fact that r_t is stationary, but changes in discrete intervals at discrete points in time, suggests that the bias could vary substantially across sample periods. Finally, assigning the bias (and interpreting coefficient values) will be even more complicated when both the change and the level of the target funds rate are included on the right-hand side of the regression.

In summary, developing a complete structural model of the daily reserves market seems like a

[1] Both papers obtain a daily autocorrelation coefficient on the order of 0.4, implying a reasonable degree of persistence in daily data.

worthy goal. Understanding daily movements in the federal funds market and the link between open market operations and monetary policy actions requires the correct specification of all the structural equations that describe this market. The results in Thornton's paper suggest to me that one cannot identify structural parameters associated with the liquidity effect without taking into account all the equations in the system. In particular, the fact that the interest rate equation in Thornton's model is clearly misspecified implies that other equations in the reduced-form system are also likely misspecified. These insights can be used to modify and refine the model, however. In addition to providing a very useful analysis of past work using daily data to identify the liquidity effect, Thornton's paper sets us on a path toward such refinements.

Finally, I wish to end with a note of caution. The elasticity of money demand to interest rates that enters the dynamic general equilibrium models discussed above is not directly linked to the demand for reserves by banks on a daily or weekly basis. In particular, the elasticity of demand for reservable deposits by banks represents a short-run elasticity that may not say very much about the costs and benefits to firms and households of switching between money and alternative assets in response to changes in nominal interest rates. Although clearly interrelated, it is presumably the firms' and households' demand for real balances rather than the banking sector's demand for reserves that Milton Friedman referred to when coining the term "liquidity effect."

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Assessing Simple Policy Rules: A View from a Complete Macroeconomic Model

Eric M. Leeper and Tao Zha

I. INTRODUCTION

Policy analysts must make tough choices: should they use a model where the economic behavior is stripped down and easy to understand, but whose fit to data is crude, or should they use a model whose fit and forecast performance are good, but with economic behavior that is not very detailed? The need to tell tidy stories frequently dominates the desire to fit data. This is *not* a choice between "simple" and "complex," though it is sometimes couched as such. A model must be simple if it's to be understood. It must be understood if it's to inform policy debates. Unfortunately, we understand models on a qualitative level, while we use them for policy analysis on a quantitative level. Tensions arise in moving from qualitative discussion to quantitative prediction.

The tensions are well illustrated by two popular approaches to empirical analysis of monetary policy: the New Keynesian (NK) and the identified vector autoregression (VAR) approaches. Stylized models of private behavior coupled with simple rules describing policy behavior characterize NK work. VARs consist of minimally identified dynamic descriptions of private behavior coupled with a detailed rule for policy behavior.[1]

The choice between the two approaches wouldn't matter if they offered the same interpretations of policy behavior and the same predictions for the impacts of changes in policy. But they do not.

Much of the appeal of NK models derives from their simplicity.[2] Implications of the model are easy to communicate and have rapidly become a standard framework for discussing monetary policy. Simple models often produce stark conclusions. NK models deliver the stark conclusion that good monetary policy calls for the central bank to adjust the nominal interest rate more than one-for-one with inflation. Some authors argue that Federal Reserve behavior under Alan Greenspan is superior—nearly optimal, by some calculations—to Fed behavior before Paul Volcker became chair in 1979.[3] NK researchers base their case that policy has improved on estimates of the parameter that determines how much the Fed adjusts the federal funds rate when inflation changes. Estimates of a stronger response to inflation after 1979 than before 1979 underlie the NK case. An unstable policy rule is the linchpin in the NK case that monetary policy has improved. VARs, in contrast, tend to find little evidence of either important instability in policy parameters or instability in the dynamic impacts of exogenous shifts in policy.[4]

Simplicity also makes NK models vulnerable. In simple models, behavioral relationships are tightly circumscribed and sparsely parameterized. As a consequence, each parameter carries a hefty share of the model's implications: the value of a single parameter can mean the difference between inferring that policy was stabilizing or destabilizing. Because dynamics are carefully pruned, there is a great deal of simultaneous behavior. It is no surprise that this environment breeds identification problems.[5] Taken together, simplicity and simultaneity make it very difficult to nail down estimates of critical parameters. Despite this difficulty, single-equation estimation techniques constitute the bulk of the empirical work in the NK literature.

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[1] NK work is associated with Rotemberg and Woodford (1997, 1999), Clarida, Gali, and Gertler (1999, 2000), and McCallum and Nelson (1999); and identified VARs are associated with Leeper, Sims, and Zha (1996), Christiano, Eichenbaum, and Evans (1999), and Bernanke and Mihov (1998). A third approach combines simple Taylor-type rules with large econometric models of the economy as in Bryant, Hooper, and Mann (1993), Taylor (1993b), and Levin, Wieland, and Williams (1999).

[2] At the conference, a semantic debate took place concerning whether the class of models we have in mind are "New Keynesian." Nothing substantive rests on the terminology. We adopted the term from Clarida, Gali, and Gertler (1999) who labeled the literature "New Keynesian."

[3] See, for example, Rotemberg and Woodford (1999) or Gali, Lopez-Salido, and Valles (2000).

[4] For example, Bernanke and Mihov (1998), Sims (1999), Leeper and Zha (2001), or Hanson (2000b).

[5] NK models are not unique in this regard. Virtually all dynamic stochastic general equilibrium models suffer from the kind of identification problems that concern us (see Canova and Pina, 2000).

Ironically, Taylor's (1993b) econometric modeling is the genesis for much of the recent NK work with simple policy rules. Identified VARs share with Taylor's analyses an emphasis on the economic *system*, rather than on single behavioral relationships. In VARs, behavioral relationships are loosely consistent with theory. Dynamics are intricate, typically unrestricted, and difficult to interpret. This makes the output of VARs hard to communicate, and the models often get treated as black boxes. Simultaneity is kept to a minimum: some of the most widely cited VAR models contain none at all. These blunt identifying assumptions, though controversial, can produce robust results. Rarely does instability of a single VAR parameter carry important qualitative implications.

The two approaches share the objective of explaining post-World War II U.S. data. Identifying assumptions, which are what link economic behavior to economic data, sharply distinguish NK and VAR approaches. We pursue that distinction to explore the identification problems that plague any attempt to tease policy behavior out of the tangle of dynamic correlations in macro time series. We take the view that NK models are restricted VARs. Dynamic optimizing behavior generates both linear and cross-equation restrictions. The latter group typically arises to ensure that expectations are rational and consistent with the model's predictions.

In Section II we use an off-the-shelf NK model to obtain identifying restrictions in a three-variable model. We argue that identification problems pervade the model. Calibration offers one solution to these problems. For example, calibrating key private parameters or policy parameters can deliver economically sensible system estimates.

In those models it can be misleading to base inferences about the effects of policy solely on estimated policy parameters. Section III displays models that are stable despite the fact that policy parameters do not satisfy the NK criterion for "stabilizing" policy.

Some of the NK models' simplicity stems from their position on money: it's irrelevant. Money plays no role in the transmission of monetary policy, in the setting of monetary policy, or in the formation of expectations about policy. The monetary sector is a sideshow. Section IV introduces money. Although this creates some new identification challenges, we argue that interpretations of historical policy behavior can change dramatically once money is reintroduced into the analysis. Estimates in that section rely on identifying assumptions that separate the behavior of money demanders from the behavior of the monetary authority. Our results underscore, however, that understandings of behavior can change drastically when one moves away from relying on reduced-form correlations.

Section V puts a sharp point on the tradeoff between simplicity and robustness of inferences. The identified VARs that we report in that section display remarkable stability across subperiods in the postwar data. The stability implies there may not have been important changes in the dynamic responses of the economy to exogenous shifts in policy, raising doubts about the premise of the NK conclusion of superior policy performance in the past 20 years.

Policy may, in fact, have improved over time. But New Keynesians do not make the case.

Some authors argue that because the behavioral equations in NK models emerge from optimization, it is reasonable to treat them as invariant to policy (Rotemberg and Woodford, 1997, and Woodford, 1999b). Similar claims cannot be made for equations in identified VARs. Instead, building on Sims (1987), Leeper and Zha (2001) contend that VARs are linear approximations to an underlying nonlinear model and that, for many practical policy questions, the linear approximations may be quite accurate. We shall not pursue this topic further here; rather, we accept that, for the class of policy interventions we think best characterizes routine Federal Open Market Committee (FOMC) analysis, both approaches estimate private behavioral equations that are virtually invariant.

II. A CANONICAL NEW KEYNESIAN MODEL

In this section we lay out a slight variant of the stylized NK model that forms the basis of the monetary policy analyses in Clarida, Gali, and Gertler (1999, 2000), Rotemberg and Woodford (1997, 1999), Woodford (1999a, 1999b), and elsewhere. Under certain parameterizations, the model specializes to Taylor's (1999b) reduced-form model. The empirical results in this section relate to Taylor's version of the model estimated with U.S. data over the period from 1959:Q1 to 2000:Q2.

The Theoretical Model

Because the micro foundations of the model are well known, we shall simply write down the relevant log-linearized equations.

The generalized IS equation is[6]

$$\text{(1) IS:} \quad x_t = -\frac{1}{\sigma}[i_t - E_t(p_{t+1} - p_t) - r] + \kappa\theta x_{t-1} + \kappa(1-\theta)E_t x_{t+1} + \varepsilon_t^{IS};$$

and the aggregate supply (AS) or price setting equation is

$$\text{(2) AS:} \quad p_t - p_{t-1} = \lambda_0 x_t + \lambda_1 x_{t-1} + \psi(p_{t-1} - p_{t-2}) + (1-\psi)\beta E_t(p_{t+1} - p_t) + \varepsilon_t^{AS},$$

where x is the "output gap," defined as actual output minus potential output:

$$x_t = y_t - y_t^p.$$

Here, i is the nominal interest rate, which is set by the monetary authority; p is the aggregate price level and $p_t - p_{t-1}$ is the inflation rate at t[7]; ε^{IS} is an exogenous process reflecting non-monetary policy sources of aggregate demand; $1/\sigma$ is the intertemporal elasticity of substitution; r is the steady state real interest rate; κ is an indicator function equal to 0 or 1; θ and ψ lie on the unit interval; ε^{AS} is an exogenous process reflecting deviations from the condition that real marginal cost and the output gap are proportional; β is the discount factor. The expectation E_t is taken with respect to an information set that contains all variables dated t and earlier.

As written in equations (1) and (2), we allow for the possibility of both forward- and backward-looking behavior in the IS and AS relationships. The parameters θ and ψ determine the extent to which behavior looks forward and backward. We are less concerned with whether backward-looking behavior can be sensibly rationalized in an optimizing framework than we are with extracting the model's implications for empirical work. To that end, it is desirable to work with a flexibly parameterized model.

Complete the model with the monetary policy (MP) rule

$$\text{(3) MP:} \quad i_t = \gamma_0 + \gamma_{\pi 1}[(p_t - p_{t-1}) - \bar{\pi}] + \gamma_{\pi 2}[(p_{t-1} - p_{t-2}) - \bar{\pi}] + \gamma_{x1} x_t + \gamma_{x2} x_{t-1} + \gamma_{m1}[(M_t - M_{t-1}) - \bar{\mu}] + \omega i_{t-1} + \varepsilon_t^{MP},$$

where ω is a parameter that allows for partial adjustment to the target interest rate and determines the degree of interest rate smoothing and ε^{MP} is a policy disturbance; $\gamma_{\pi 1}$ describes how policy responds to inflation and is the parameter that receives the most attention in the NK literature. $\bar{\pi}$ and $\bar{\mu}$ are target levels for inflation and money growth. This rule represents a substantial generalization of the class of rules typically considered in NK research, as it allows policy choice to depend on the lagged inflation rate and output gap, as well as on current and past money growth.[8] The rule that Taylor (1993a, 1999a, 1999b) employs, and is now nearly standard equipment in an NK model, sets i solely as a function of the current inflation rate and output gap:

$$\text{(4) MP(Taylor):} \quad i_t = \gamma_0 + \gamma_{\pi 1}[(p_t - p_{t-1}) - \bar{\pi}] + \gamma_{x1} x_t + \varepsilon_t^{MP}.$$

The two exogenous processes associated with private behavior are ε_t^{IS} and ε_t^{AS}, and ε_t^{MP} is the exogenous part of policy behavior.

Potential Identification Problems

Nearly all the NK papers assume certain values for the private parameters in equations (1) and (2). They then estimate the policy parameters using ordinary least squares or instrumental variables methods or they impose particular policy parameters. Suppose instead that the reduced form—or solved-out version—of equations (1), (2), and (3) were to be estimated simultaneously. Although the reduced form confounds private parameters and policy parameters, we may work with it as long as we do not intend to change policy parameters while holding fixed the reduced-form parameters in the non-policy equations. We can even solve the model numerically, noting, where possible, the linear restrictions the model implies and then imposing those restrictions on our estimation. In this procedure we concentrate on restrictions on contemporaneous interactions among variables, which are the most common identifying restrictions used in empirical work. Because cross-equation restrictions are often at odds with data, we limit ourselves to the linear restrictions that theory implies.

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6 Although, strictly speaking, IS involves output rather than the output gap, in equation (1) we follow the convention in the NK literature.

7 In the empirical work below, we convert this to the annual rate $4(p_t - p_{t-1})$. To avoid notational clutter, we leave the conversion out of the theoretical expressions.

8 Papers by Clarida, Gali, and Gertler (2000) and Bernanke and Woodford (1997) also include policy responses to expected inflation and output. This makes little difference for our purposes.

Inspection of the three equations of the model suggests the potential for several identification problems to arise. First, both IS and policy link the current nominal rate to inflation and the output gap. If inflation is close to a random walk, then both equations involve (x_t, π_t, i_t) and without additional restrictions they cannot be distinguished. This is a critical problem, as it potentially confounds the impact of monetary policy with other sources of disturbance to aggregate demand, causing misleading interpretations of the role of monetary policy.

Taylor (1999b) resolves the identification problem by considering the model that emerges when $\kappa = 0$, $\lambda_0 = 0$, and $\psi = 1$. In that case, the reduced-form expression for IS makes $x_t = a(i_t - \pi_t - r) + \xi_t$, for some coefficient a, producing an additional restriction that separates IS from monetary policy.[9] If, in contrast to Taylor's specification, the IS curve is dynamic ($\kappa = 1, \theta \in [0,1)$), then Taylor's additional restriction does not hold generally, and nothing distinguishes the reduced forms for IS and policy.

One way to separate IS and MP is to adopt the approach taken in some of the identified VAR literature and advocated by McCallum (1999) in NK models: an operational rule cannot make policy choice depend on variables the Fed does not observe contemporaneously. Because the Fed does not observe inflation and output contemporaneously, we might posit the rule

$$\text{(5) MP (Taylor lagged):} \quad i_t = \gamma_0 + \gamma_{\pi 2}[(p_{t-1} - p_{t-2}) - \bar{\pi}] + \gamma_{x2} x_{t-1} + \varepsilon_t^{MP}.$$

This rule equates the surprise in the federal funds rate, given past inflation and output, to the exogenous disturbance in monetary policy. Unfortunately, it is well documented that this identification can generate empirical anomalies; a prominent anomaly is that an exogenous monetary contraction raises the funds rate, lowers output, and *raises* the inflation rate (see, for example, Gordon and Leeper, 1994).

Although it is no longer fashionable to include money in models of monetary policy, the Fed does observe growth rates of various monetary aggregates contemporaneously. And for much of the postwar period the Fed established target growth rates for aggregates. These targets have been pursued with varying degrees of vigilance over the years because, when velocity is fairly predictable, money growth can be informative about future inflation. Adding current money growth to the policy rule in equation (5) produces

$$\text{(6) MP (with money):} \quad i_t = \gamma_0 + \gamma_{m1}[(M_t - M_{t-1}) - \bar{\mu}] + \gamma_{\pi 2}[(p_{t-1} - p_{t-2}) - \bar{\pi}] + \gamma_{x2} x_{t-1} + \varepsilon_t^{MP}.$$

This specification is close to the rule that Ireland (2000) estimates in an NK model. A policy rule like equation (6) carries two important implications. First, money is no longer an appendage to the NK model. Now interaction of supply and demand in the money market determines the money stock and the nominal interest rate simultaneously. This raises the tricky problem of separating money demand and monetary policy. Second, the dynamic IS and AS relationships imply that current inflation and output depend on the entire expected future path of policy. If money enters the policy equation, then it plays a role in forming expectations of policy. The reduced forms for IS and AS now must include the current money stock.

The presence of money in the IS equation raises a new identification problem. Now both IS and money demand include output, the price level, the nominal interest rate, and the money stock. Without further restrictions, IS and money demand are indistinguishable. Homogeneity restrictions play a prominent role in money demand regressions; in fact, the money demand relationship is usually written in terms of the demand for real money balances, reflecting one homogeneity restriction. Another restriction, which many general equilibrium models of money demand imply, is unitary income elasticity. Some VAR work has found it necessary to impose both homogeneity restrictions to model the money market (e.g., Cushman and Zha, 1997).

Estimated Models

We now illustrate some of these identification problems with estimated models. The reduced form for the NK model with three endogenous variables can be written as

$$\text{(7)} \qquad X_t' A_0 = C + X_{t-1}' A_1 + X_{t-2}' A_2 + \varepsilon_t',$$

where $X_t = (y_t, p_t, i_t, y_t^p)'$, C is a vector of intercept terms, and $\varepsilon_t = (\varepsilon_t^{IS}, \varepsilon_t^{AS}, \varepsilon_t^{MP}, \varepsilon_t^{yp})'$.[10] We take the

[9] In this case, $\pi_t = \lambda_1 x_{t-1} + \pi_{t-1}$, so the IS relationship implies $a = -1/\sigma(1-\lambda_1)$.

[10] We impose restrictions to express equations in terms of inflation or the output gap.

exogenous disturbances to be i.i.d. with $\varepsilon_t \sim N, (0, I)$. In the estimation, we follow tradition and treat potential output, y^p, as exogenous and estimate an AR(1) process for it. Adding y^p to the model in an exogenous block alters the order condition substantially: it buys us three zero restrictions while adding only one free parameter. Most NK work, however, includes the gap, rather than y and y^p separately. To keep in the spirit of that work, we assess the order condition as if we estimated the model in terms of (x, p, i).

All data are quarterly and all but the interest rate are seasonally adjusted. The estimation period in this section runs from 1959:Q1 to 2000:Q2; y is real gross domestic product (GDP) (chained 1996 dollars), p is the personal consumption expenditures deflator (chained 1996 dollars), i is the federal funds rate, y^p is the Congressional Budget Office's measure of potential GDP (chained 1996 dollars). We choose to estimate data in terms of levels, rather than growth rates, in order to connect the work more closely to the identified VAR literature. We impose all the linear restrictions implied by the NK model and execute maximum likelihood estimation.[11] All variables are logged except the funds rate, which is a percentage.

In the models reported below we display both the estimated parameters and the impulse response functions computed from

$$X_t = B(L)\varepsilon_t, \tag{8}$$

where $B(L) = (A_0' - A_1'L - A_2'L^2)^{-1}$.

Because many of our points are logical, illustrating the nature of identification problems, rather than statistical, we do not report standard errors or error bands for most of the estimated models.

Taylor's (1999b) Model. Taylor's model is described by

$$\begin{aligned} &\text{IS:} && x_t = -\frac{1}{\sigma}(i_t - \pi_t - r) + \varepsilon_t^{IS} \\ (9)\quad &\text{AS:} && \pi_t = \lambda_1 x_{t-1} + \pi_{t-1} + \varepsilon_t^{AS} \\ &\text{MP:} && i_t = \gamma_0 + \gamma_{\pi 1}\pi_t + \gamma_{x1} x_t + \varepsilon_t^{MP}, \end{aligned}$$

where $\pi_t = p_t - p_{t-1}$ is the inflation rate. In this model, π_t is inertial or predetermined, while x_t and i_t are determined simultaneously. Based on restrictions on A_0 alone, the model is not identified unless some additional restriction is imposed. Taylor imposes that the coefficients on the nominal rate and inflation in the IS equation are equal and of opposite sign.[12] Imposing that restriction yields the estimates[13]:

$$\begin{aligned} x_t &= 0.795\left[i_t - \left(p_t - p_{t-1}\right)\right] - 0.024 \\ (10)\quad p_t - p_{t-1} &= 0.077\, x_{t-1} + \left(p_{t-1} - p_{t-2}\right) \\ i_t &= 0.820\left(p_t - p_{t-1}\right) - 0.597\, x_t + 0.032. \end{aligned}$$

With the exception of the AS relationship, none of these parameters is reasonable. IS and MP relationships are confounded: the pattern of coefficients in IS makes more sense as a policy rule, and the pattern of coefficients in the policy equation makes more sense as an IS curve. Figure 1 confirms this interpretation.[14] The shock identified as IS raises the funds rate and lowers output (although only slightly), while the shock identified as MP raises output, the price level, and the funds rate. The latter is reasonable when interpreted as an endogenous response of policy to higher output and inflation; it is unreasonable when interpreted as an exogenous monetary policy contraction.

Without further restrictions, there appears to be no way to separate the two components of aggregate demand in the model. We turn now to two alternative solutions to this problem.

Calibration as Identification. Perhaps the most popular solution to identification problems is to impose parameter values obtained from other data sets or previous research. While this approach gained popularity initially in the real business cycle literature, its popularity has carried over to research using NK models. We show that transporting parameters from other studies certainly can solve the identification problems inherent in separating IS from MP. First we impose the intertemporal elasticity of substitution, $1/\sigma = 2$, which is within the range of values used in the literature,[15] and freely

[11] First we obtain maximum likelihood estimates of A_0; then we obtain estimates of (A_1, A_2, C), conditional on the MLE of A_0.

[12] In Taylor's model, the current output gap is excluded from AS. This exclusion restriction is necessary for identification from restrictions on A_0 alone. Without it, the model is underidentified for two reasons: because it adds the coefficient on x_t in AS and because, if x_t enters AS, then the restriction on IS that the coefficients on i and π be equal and of opposite sign no longer holds. See footnote 9.

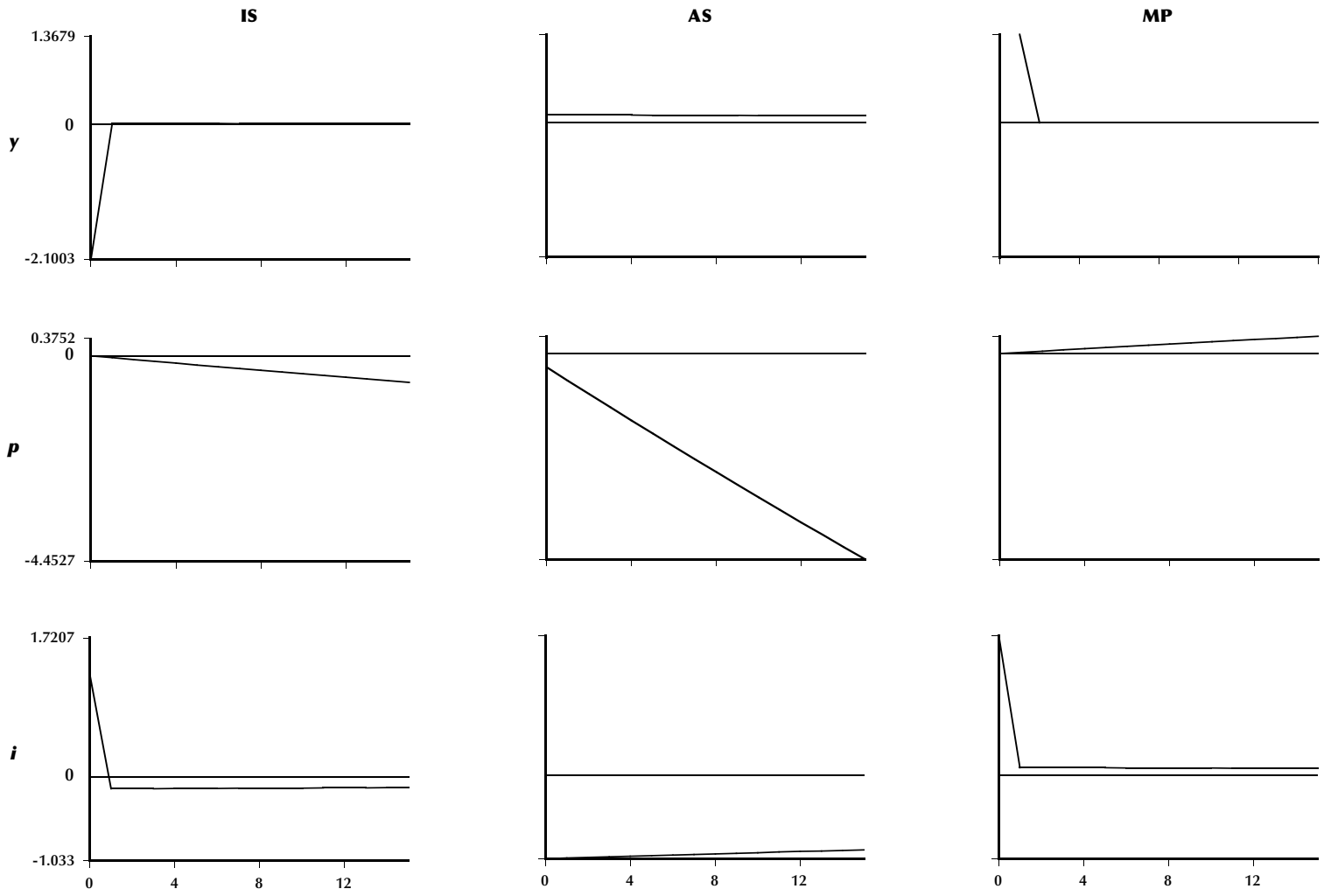
[13] The process for potential output is estimated to be $y_t^p = 0.0297 + 0.998\, y_{t-1}^p$.

[14] All figures depict impulse responses that have been multiplied by 100.

[15] Rotemberg and Woodford (1997) estimate $\sigma = 0.16$, producing an IS interest elasticity of –6.25, while McCallum and Nelson (1999) estimate $\sigma = 4.93$, making the interest elasticity –0.20. Clarida, Gali, and Gertler (2000) and Gali, Lopez-Salido, and Valles (2000) calibrate their models to log preferences, so $\sigma = 1$.

Figure 1

Confounding IS and MP in Taylor's Model



estimate the remaining parameters. Next we impose $\gamma_{\pi 1} = 1.5$ and $\gamma_x = 0.5$ in the Taylor rule and estimate the rest of the model. Both approaches produce sensible results, with monetary policy shocks having important effects on output. Inflation, however, appears to be entirely an aggregate supply phenomenon.

Imposing $1/\sigma = 2$ and not imposing Taylor's restriction on IS leads to the estimates

$$\begin{aligned} x_t &= -\underline{2.0}\, i_t + 1.658\left(p_t - p_{t-1}\right) + 0.063 \\ p_t - p_{t-1} &= 0.077\, x_{t-1} + \left(p_{t-1} - p_{t-2}\right) \\ i_t &= 1.027\left(p_t - p_{t-1}\right) + 1.535\, x_t + 0.030, \end{aligned} \tag{11}$$

where underlining indicates an imposed parameter value.

All the estimated parameters are reasonable. The IS elasticity with respect to inflation is positive, as one would expect if output depends on the real interest rate.

Most striking are the estimated policy parameters. The Fed raises the funds rate more than one-for-one with the inflation rate. It raises the funds rate about 150 basis points in response to a 1 percent increase in the output gap. A coefficient on inflation that exceeds 1 implies stabilizing policy, according to the standard interpretations of the policy rule (e.g., Taylor, 1999b, or Clarida, Gali, and Gertler, 1999). The system estimates in equation (11) are in sharp contrast to ordinary least-squares (OLS) estimates of the policy rule over this period:

$$\text{OLS:}\quad i_t = \underset{(0.07)}{0.86}\left(p_t - p_{t-1}\right) - \underset{(0.07)}{0.14}\, x_t + \underset{(0.003)}{0.03}, \tag{12}$$

which would seem to suggest that policy was *not* stabilizing on average since 1959. The substantive difference in estimates underscores the importance of estimating policy behavior and private behavior simultaneously. Inferences about policy behavior based on the system estimates in equation (10) are *qualitatively different* from those based on single-equation estimates in equation (12).

Figure 2 displays the system's responses to exogenous disturbances over four years. The third column shows that MP has important effects on output: a 100-basis-point exogenous contraction reduces output by 2 percent (as the calibrated value for σ implies), though the effects die out immediately. Policy disturbances matter for output, accounting for over a third of its variability. Exogenous shifts in policy, however, have little impact on inflation.

Policy is strongly endogenous. Policy disturbances account for 20 percent of the variation in the funds rate over the first year and for 10 percent over longer horizons. Endogeneity of policy shows up in the responses of the funds rate to IS and AS disturbances. An IS shock that increases the output gap and gradually raises the price level, brings forth a higher funds rate. An outward shift in AS persistently raises the output gap, permanently lowers the price level, and induces the Fed to lower the funds rate. Only very gradually does the Fed return the rate to its initial level.

Monetary policy shocks are the dominant source of output variation (75 percent over the four-year horizon), and AS disturbances are the sole source of price level movements (more than 98 percent over the horizon). AS shocks also account for three-quarters of funds rate variability at four-year horizons.

Figure 3 displays the time paths of structural shocks implied by the estimated model. With the exception of the AS shock, the estimated disturbances exhibit strong patterns of serial correlation, which arise from the absence of dynamics in the behavioral equations.[16] The time path of the monetary policy disturbances in the bottom panel of the figure resembles the "policy mistakes" that Taylor (1999a) reports in his historical analysis of policy rules. Taylor computes the gap between the actual funds rate and value of the rate implied by two policy rules. He concludes that when the gap was positive the funds rate was "too high" and when it was negative the funds rate was "too low." In Figure 3, a positive value of ε^{MP} is an exogenous tightening of policy and a negative value is an exogenous loosening. Unlike Taylor's work, the figure does not report that policy in the early 1960s was "too loose." It is consistent with Taylor's findings that in the second half of the 1960s and the 1970s policy was "loose," while in the early to mid-1980s policy was "tight." The figure is also generally consistent with Taylor in finding that through the 1990s "policy mistakes" were small.

We are unwilling, however, to draw the normative conclusions Taylor does. In the model, as Figure

[16] Clearly these errors are not i.i.d. as assumed in the estimation. Rather than estimate patterns of serial correlation for the shocks to render some residuals as white noise, we prefer to account for the data's persistence through behavioral relationships. Allowing serially correlated errors, as is common in the literature, would improve the fit, but would not contribute to the *economic* interpretation of the data.

Figure 2

Dynamic Responses in Taylor's Model

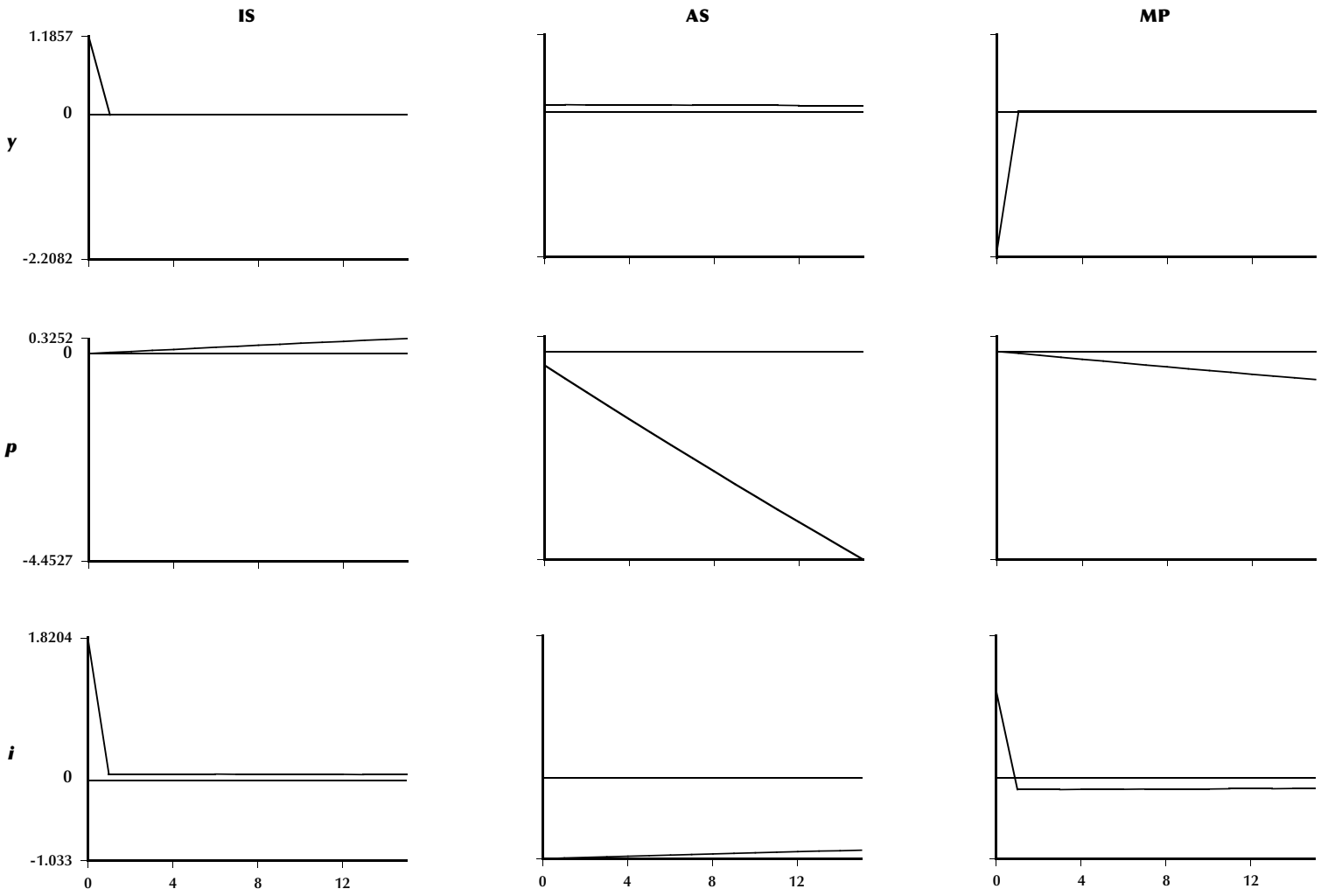
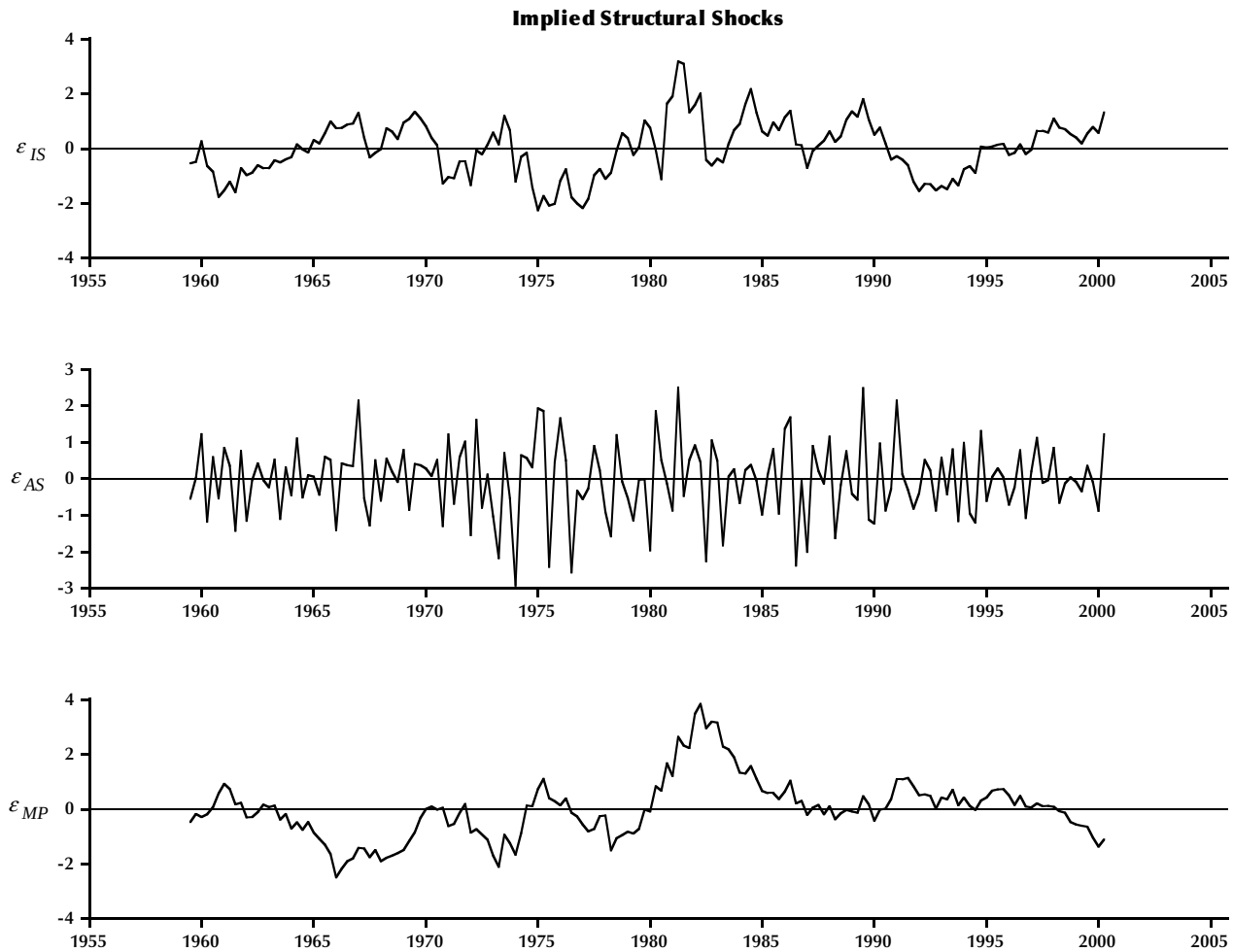


Figure 3

Implied Shocks in Taylor's Model



2 attests, exogenous shifts in policy have unrealistically large impacts on output and essentially no impacts on inflation. We prefer to link normative statements about policy to the impacts policy has on variables that affect private agents' welfare. If those estimated impacts are implausible, it seems premature to deduce how well policy has performed from the estimated pattern of residuals in the policy equation.

Not surprisingly, the data strongly reject equation (11), a model with severely pruned dynamics. Letting ξ denote twice the difference of the log likelihoods of the unrestricted and restricted models, $\xi = 2(\log(L_U) - \log(L_R))$, we find $\xi = 775.06$, which has a p-value of 0.00. Critical values for the Schwarz and Akaike criteria are 122.4 and 48.

Imposing that policy be set according to the parameters Taylor (1993a, 1999a) employs ($\gamma_{\pi 1} = 1.5$, $\gamma_{x1} = 0.5$) leads to the estimates

$$
\begin{aligned}
x_t &= -0.732\, i_t + 0.545\left(p_t - p_{t-1}\right) + 0.024 \\
p_t - p_{t-1} &= 0.077\, x_{t-1} + \left(p_{t-1} - p_{t-2}\right) \qquad (13) \\
i_t &= \underline{1.5}\left(p_t - p_{t-1}\right) + \underline{0.5}\, x_t + 0.009,
\end{aligned}
$$

where underlining indicates imposed parameter values. The qualitative impacts of the three disturbances are much like those depicted in Figure 2 and are not reported. Because the model's parameters are different, however, the quantitative implications differ somewhat. The estimated interest elasticity of IS is lower than in system (11), so the output

effects of policy disturbances in this model are smaller, accounting for no more than 35 percent of output variability. AS shocks explain one-quarter of output forecast error variance and 80 percent of funds rate variability over four-year horizons.

It is difficult to distinguish the reduced-form expressions for IS and MP that emerge from the NK model without introducing additional restrictions. We showed that imposing an interest elasticity of IS of –2.0, which is in the ballpark for calibrated NK models, can solve the identification problem. Estimates of policy behavior from the 1959:Q1 to 2000:Q2 period are consistent with the interpretation that the Fed has, on average, been stabilizing: it raised the federal funds rate more than one-for-one with inflation. This result comes from system estimates; OLS estimates of policy behavior produce a response to inflation that is substantially below 1.0. Although we solved the identification problem, the estimated models imply little role for monetary policy in influencing inflation.

III. INFERENCES ABOUT STABILITY BASED ON POLICY PARAMETERS

One piece of conventional wisdom to emerge from the NK work on MP is that policy is stabilizing when it raises the nominal interest rate more than one-for-one with the inflation rate. This increases the real interest rate, the argument goes, reduces aggregate demand, and counteracts the incipient inflation. In Taylor's policy rule, equation (4), this requires that $\gamma_{\pi 1} > 1$. Several authors have drawn inferences about how policy impacts the economy based on estimates of $\gamma_{\pi 1}$ (e.g., Clarida, Gali, and Gertler, 1999, 2000; Rotemberg and Woodford, 1997, 1999; and Taylor, 1999a).

Stability is a characteristic of an equilibrium and, as such, is an implication of a model. Much of the recent work on simple rules may give the impression that one can deduce this model implication merely by estimating a policy rule. Implicitly, many authors are conditioning their assertions about the magnitude of a particular policy parameter on the structure and parameter values of an entire model.

Two different but related interpretations of U.S. monetary policy behavior before the Volcker-Greenspan era stem from inferences about stability drawn from estimated policy rules. Taylor (1999b) argues in the following way that policy is "stabilizing" when $\gamma_{\pi 1} > 1$. Modify the AS relationship in his model, equation (9), to be

$$\text{(14)} \quad \text{AS:} \quad \pi_t = \lambda_1 x_{t-1} + \delta \pi_{t-1} + \varepsilon_t^{AS}.$$

Substituting MP into IS, and the resulting expression for x into AS, yields a first-order difference equation describing the evolution of equilibrium inflation:

$$\text{(15)} \quad \pi_t = \left(\frac{\lambda_1 (1-\gamma_{\pi 1})}{\sigma + \gamma_{x1}} + \delta \right) \pi_{t-1} - \frac{\lambda_1}{\sigma + \gamma_{x1}} \varepsilon_{t-1}^{MP} + \frac{\lambda_1 \sigma}{\sigma + \gamma_{x1}} \varepsilon_{t-1}^{IS} + \varepsilon_t^{AS}.$$

Taylor imposes $\delta = 1$. In that case, if $\lambda_1, \sigma, \gamma_{x1} > 0$, which are reasonable assumptions, then $\gamma_{\pi 1} > 1$ is necessary and sufficient for equation (15) to be a stable difference equation. Suppose the economy is hit by an adverse AS shock that increases inflation. A sufficiently strong policy response to the initial increase in inflation would raise the real interest rate, reduce output, and stabilize inflation. In the absence of a strong policy response, output might rise and, through the AS relationship, raise inflation still more in the future. The process can be explosive.

A second interpretation of the implications of $\gamma_{\pi 1} < 1$ comes from Rotemberg and Woodford (1997) and Clarida, Gali, and Gertler (2000). In a maximizing model with typical assumed values for private parameters, $\gamma_{\pi 1} < 1$ implies that sunspot equilibria cannot be ruled out. Expectations of higher inflation that arise for unexplained reasons can be self-fulfilling. Sunspot fluctuations may arise because economic agents rationally believe that the Fed will accommodate higher expected inflation by letting short-term real interest rates fall, stimulating aggregate demand, and raising inflation further. We do not pursue this interpretation in the present paper.

Both interpretations rely on estimates of $\gamma_{\pi 1}$ that are substantially below 1.0 in the United States before 1979.

We can see precisely the phenomenon that Taylor discusses when we reestimate the model in equation (10) over the sample 1959:Q1–1979:Q3. The estimates are

$$\text{(16)} \quad \begin{aligned} x_t &= -2.621\left[i_t - \left(p_t - p_{t-1}\right)\right] + 0.041 \\ p_t - p_{t-1} &= 0.105 x_{t-1} + \left(p_{t-1} - p_{t-2}\right) \\ i_t &= 0.671\left(p_t - p_{t-1}\right) + 0.571 x_t + 0.023. \end{aligned}$$

The critical policy parameter, the response of the funds rate to inflation, is substantially less than 1 at 0.671. According to conventional wisdom, policy

was not stabilizing. Impulse response functions in Figure 4 bear out the conventional wisdom. Although the short-run patterns make economic sense, the responses to AS shocks are explosive, with output, the price level, and the funds rate shooting off to negative infinity. Explosiveness stems from the source Taylor highlights: the eigenvalue of the model's difference equation in inflation in equation (15) exceeds 1.0.[17]

It may be surprising that an important ingredient in generating instability is the restriction that $\delta = 1$ in the AS specification in equation (14). We turn to the NK model laid out in Section II, but move away from the specific parameterization Taylor used. Instead, we consider an environment in which behavior in both the IS and the AS equations is dynamic.

To obtain restrictions motivated by the theoretical NK model, we calibrate and solve the model, deriving the theory's analogs to the (A_0, A_1, A_2, C) matrices in the estimated model, equation (7). We then apply the pattern of linear restrictions implied by the theory to our empirical model.[18] Let $X_t = (x_t, p_t, i_t)'$ and order the equations (IS, AS, MP). Let column j of A_i represent equation j, j = IS, AS, MP. As an example, the pattern matrices for Taylor's (1999b) model, specified in equation (9) and estimated in equation (10), are

(17)

$$A_0 = \begin{bmatrix} \times & 0 & \times \\ -\times_1 & \times_2 & \times_3 \\ \times_1 & 0 & \times \end{bmatrix}, A_1 = \begin{bmatrix} 0 & \times & 0 \\ -\times_1 & 2\times_2 & \times_3 \\ 0 & 0 & 0 \end{bmatrix}, A_2 = \begin{bmatrix} 0 & 0 & 0 \\ 0 & -\times_2 & 0 \\ 0 & 0 & 0 \end{bmatrix}$$

where $\times$ denotes a freely estimated coefficient, 0 denotes a coefficient that is excluded, and $\times_i$ denotes a coefficient estimated subject to a dynamic linear restriction within an equation. After estimation, setting the diagonal terms in A_0 to 1 normalizes the matrices. There are six freely estimated parameters in A_0, so the model is just identified.[19]

We now consider other versions of the NK model. IS and AS relationships are dynamic with both forward- and backward-looking behavior; policy follows the Taylor rule $i_t = 2 + 1.5\pi_t + 0.5x_t$.[20] The reduced form for this model implies the pattern matrices

(18) $$A_0 = \begin{bmatrix} \times & \times & \times \\ \times & \times & \times_1 \\ \times & 0 & \times \end{bmatrix}, \quad A_1 = \begin{bmatrix} \times & 0 & 0 \\ \times & \times & \times_1 \\ 0 & 0 & 0 \end{bmatrix}, \quad A_2 = \begin{bmatrix} 0 & 0 & 0 \\ 0 & \times & 0 \\ 0 & 0 & 0 \end{bmatrix}.$$

Now the model determines all three variables simultaneously. With eight free parameters in A_0, the model is not identified. We follow Taylor in forcing the effect of the output gap on inflation to occur with a one-period lag (making $A_0(1,2) = 0$ and $A_1(1,2) = \times$), and we add the restriction that the nominal interest elasticity of IS is –2.0: $\sigma = 1/2 (= A_0(3,1))$. The estimated model for the period 1959:Q1–1979:Q3 is

(19)

$$\begin{aligned} x_t &= -\underline{2.0}\, i_t + 1.126\left(p_t - p_{t-1}\right) + 1.495\, x_{t-1} + 0.060 \\ p_t &= 0.025\, x_{t-1} + 1.9615\, p_{t-1} - 0.9618\, p_{t-2} \\ i_t &= 0.690\left(p_t - p_{t-1}\right) + 0.517\, x_t + 0.023. \end{aligned}$$

Although we did not impose that the AS function can be written in terms of the inflation rate, the estimates are very close to $\pi_t = 0.025x_{t-1} + 0.962\pi_{t-1}$. As in system (16), we estimate that the policy response to inflation is well below one-for-one. This model, however, does not display the instability following AS shocks that appears in the previous model. Figure 5 displays the impulse response functions over a four-year horizon. All the responses look reasonable and converge after about 10 years. The absolute values of the largest eigenvalues of the estimated system are 0.996 and 0.997. Evidently, $\gamma_{\pi 1} > 1$ isn't necessary for stability.

Although the estimated response of policy to inflation is weak, policy behavior is strongly endogenous. Over forecast horizons of one to four years, over 20 percent of the fluctuations in the funds rate are due to IS shocks and 45 percent are due to AS shocks. Inflation is again estimated to be primarily an aggregate supply phenomenon, with 85 to 100 percent of price level variation due to AS shocks. Policy disturbances simply do not move the price level very much, though they are more important than IS shocks in accounting for output fluctuations

[17] The largest eigenvalue is estimated to be 1.036.

[18] Because we do not impose the cross-equation restrictions that the theory implies, the empirical model may be underidentified even when the theoretical model is not.

[19] As equation (17) makes clear, there are additional restrictions among coefficients across the A_i matrices. When we evaluate the order condition, we do not count these, and focus exclusively on restrictions on A_0. One could instead investigate achieving identification through the dynamic restrictions.

[20] We set $r = 2$, $\sigma = 1$, $\theta = 0.2$, $\kappa = 1$, $\lambda_0 = 0.3$, $\beta = 0.99$, $\psi = 0.2$, $\gamma_0 = 2$, $\gamma_{\pi 1} = 1.5$, $\gamma_{x1} = 0.5$, and the remaining parameters to zero.

Figure 4

An Unstable Equilibrium in Taylor's Model (1959:Q1–1979:Q3)

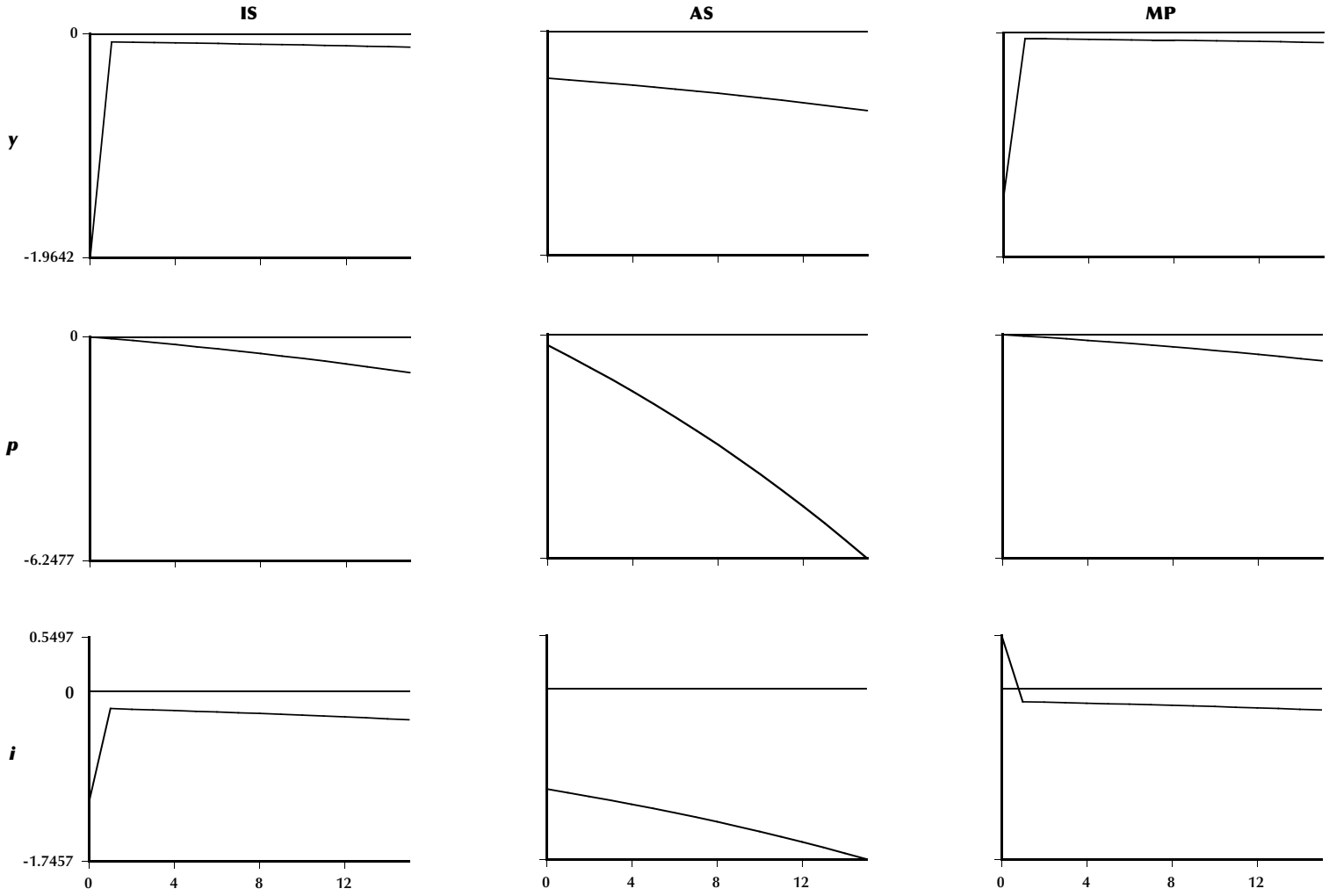
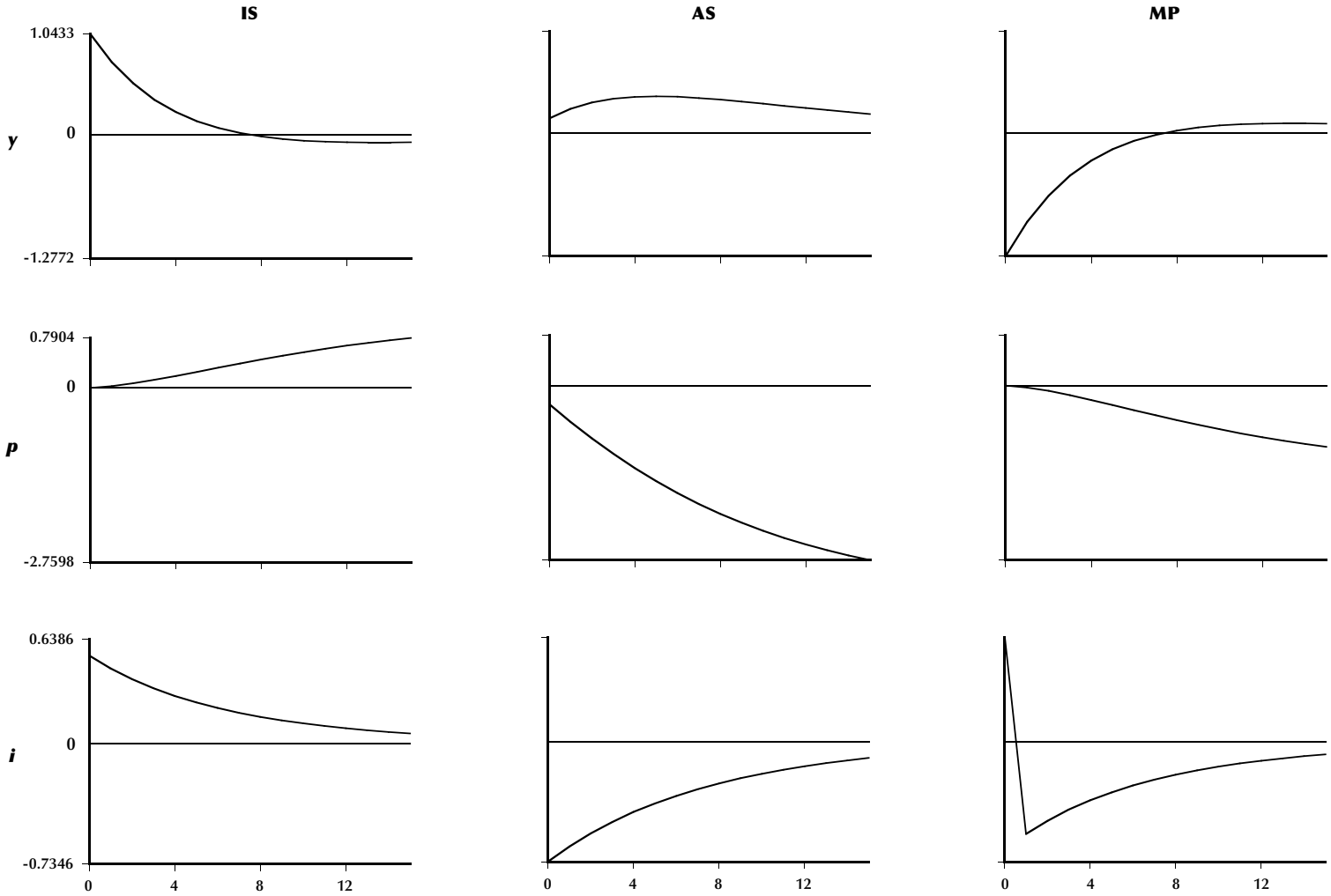


Figure 5

A Stable Equilibrium in Taylor's Model (1959:Q1–1979:Q3)



in the short run (60 percent versus 40 percent).

In spite of the widespread belief that the Fed raised the funds rate less than one-for-one with inflation in the period from 1959:Q1 to 1979:Q3, it is worthwhile estimating the same model with identification achieved by imposing the policy rule $i_t = \gamma_0 + 1.5\pi_t + 0.5x_t$. With these two restrictions on A_0, we now freely estimate the interest elasticity of IS ($A_0(3,1)$) and the contemporaneous effect of output on price setting behavior ($A_0(1,2)$). The model determines (x_t, i_t) simultaneously. The estimates are

(20)

$$\begin{aligned} x_t &= -0.555 i_t + 0.069\left(p_t - p_{t-1}\right) + 1.090 x_{t-1} + 0.027 \\ p_t &= 0.053 x_{t-1} + 1.979 p_{t-1} - 0.979 p_{t-2} \\ i_t &= \underline{1.5}\left(p_t - p_{t-1}\right) + \underline{0.5} x_t - 0.11 . \end{aligned}$$

Once again the model is stable, with largest eigenvalues equal to 0.997 and 0.999. Dynamic responses to exogenous disturbances look reasonable, as shown in Figure 6. The most notable quantitative difference between this model and the previous one, equation (19), is that now IS and MP disturbances are more important sources of inflation variation, accounting for 25 percent each over horizons of four years. AS shocks continue to be the dominant source of inflation in the short run, but over longer periods, aggregate demand is as important as aggregate supply.

We also estimated two versions of the NK model where AS behavior is forward looking only. IS continues to be forward and backward looking. Eliminating backward-looking price-setting behavior excludes current inflation from the IS equation. The resulting pattern matrices are

(21)

$$A_0 = \begin{bmatrix} \times & \times & \times \\ 0 & \times_1 & \times_2 \\ \times & 0 & \times \end{bmatrix}, \quad A_1 = \begin{bmatrix} \times & 0 & 0 \\ 0 & \times_1 & \times_2 \\ 0 & 0 & 0 \end{bmatrix}, \quad A_2 = \begin{bmatrix} 0 & 0 & 0 \\ 0 & 0 & 0 \\ 0 & 0 & 0 \end{bmatrix}.$$

One more restriction on A_0 is needed to identify the model. We considered (i) excluding x_t from AS and (ii) imposing an interest elasticity of –2.0 on IS. In both cases the estimated models were stable in spite of the policy's less than one-for-one response of the funds rate to inflation.

Whether or not monetary policy is stabilizing depends on policy and private behavior. We found that over the pre-Volcker-Greenspan era (1959:Q1–1979:Q3), Fed behavior appears not to be stabilizing when we impose the Taylor (1999b) restrictions on aggregate supply. In contrast, when we impose AS restrictions implied by the dynamic NK model, policy over the period appears to be stabilizing. In both cases we estimate that the Fed adjusted the funds rate less than one-for-one with inflation.

IV. THE DISAPPEARANCE OF MONEY FROM MONETARY POLICY ANALYSES

Money plays no role in NK models of monetary policy. To some observers this may seem odd. This section reviews and discusses the reasons for money's disappearance. The section then turns to some empirical implications of reintroducing money.

Why Money Disappeared

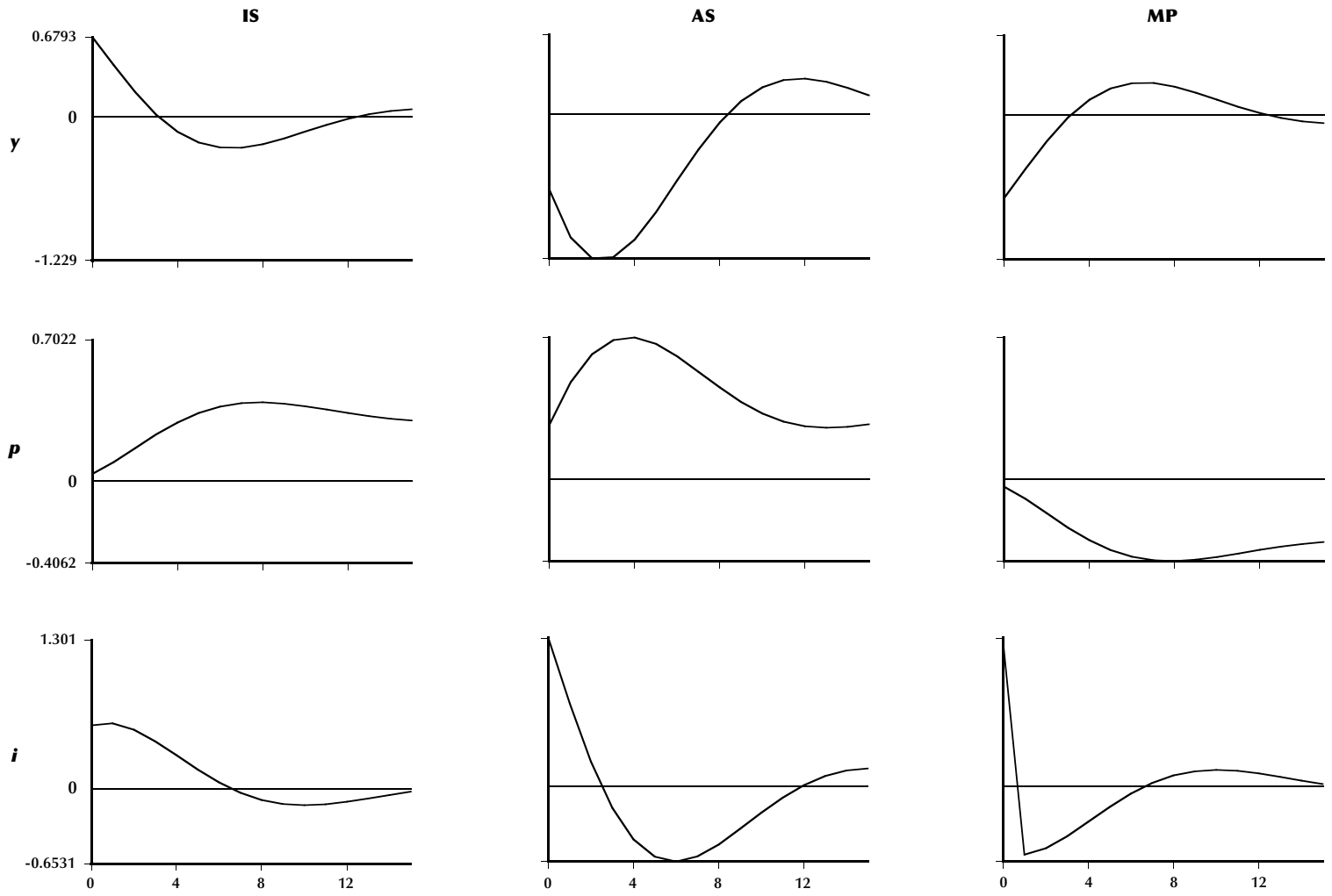
Money disappeared for both practical and theoretical reasons. Throughout the 1980s, the Federal Reserve paid fairly close attention to the growth of various monetary aggregates in setting its target for the federal funds rate. Target growth rates for aggregates were established and taken seriously by observers of monetary policy. A decade ago researchers at the Federal Reserve Board developed the "P-Star" model, which relied on stable long-run values of velocity and output growth, to use M2 growth to predict inflation (Hallman, Porter, and Small, 1991). Although doubts were raised at the time, any hope of exploiting M2 growth to forecast inflation evaporated when M2 velocity began to behave erratically in the early 1990s.[21] Since then, as a practical move, the Fed has deemphasized growth rates of aggregates as indicators of inflation. In 2000, the FOMC formalized this deemphasis, as the minutes from the June 27-28, 2000, meeting indicate:

> In contrast to its earlier practice, the Committee at this meeting did not establish ranges for growth of money and debt in 2000 and 2001. The legal requirement to set and announce such ranges recently had expired, and the members did not view the ranges as currently serving a useful role in the formulation of monetary policy. Owing to uncertainties about the behavior of the

[21] Christiano (1989) raised some doubts.

Figure 6

Another Stable Equilibrium in Taylor's Model (1959:Q1–1979:Q3)



velocities of money and debt, these ranges had not provided reliable benchmarks for the conduct of monetary policy for some years. Nevertheless, the Committee believed that the behavior of these aggregates retained value for gauging economic and financial conditions and that such behavior should continue to be monitored. Moreover, Committee members emphasized that they would continue to consider periodically issues related to their long-run strategy for monetary policy, even if they were no longer setting ranges for the money and debt aggregates.

Theoretical developments in the past decade reinforce the Fed's pragmatic response to unstable M2 velocity. Several authors showed that a nominal anchor need not come from control of a monetary aggregate: a policy rule that sets the nominal interest rate can uniquely determine the price level even in a rational expectations model.[22] This contradicted Sargent and Wallace's (1975) famous result that interest rate rules cannot determine the price level.[23] These developments initiated a literature about interest rate rules that continues to flourish.[24]

Several considerations arise from the absence of money in the analytical framework. First, even if the Fed ignores money when it sets the funds rate, this does not imply that money plays no role in the transmission of monetary policy or in individuals' and firms' consumption, investment, employment, and pricing decisions. In terms of the NK model, absence of money from the policy rule does not justify its absence from the IS and AS relationships. Interest rates need not be the only channel through which monetary policy affects economic activity.

Second, the fact that the Fed *can* ignore money without losing a nominal anchor does not imply the Fed *does* ignore it. The FOMC minutes leave open the possibility that the Fed may again choose to pay more attention to monetary aggregates. For example, it is hard to imagine that if M2 growth were to exceed 20 percent for four consecutive quarters that there would be no tendency for the FOMC to adjust its funds rate target in response.

Third, even if the Fed now ignores money, it certainly has not always ignored it. Historical interpretations of policy behavior that ignore money run the risk of seriously misinterpreting past policy actions.

Finally, if money plays any role at all in the FOMC's settings for the funds rate, then money is likely to enter private sector expectations of future funds rates. Money, therefore, will enter dynamic IS or AS relationships through the expectations terms, once expectations are solved out.

Adding Money

We add to the NK model a function that makes the demand for real money balances (MD) depend on the current nominal interest rate and current income. To focus on the marginal contribution of adding money to a model with simple policy rules, we adopt an agnostic view of the dynamics associated with IS, AS, and MD behavior. We posit the money demand function

$$\text{(22)} \quad \text{MD:} \quad M_t - p_t = \alpha_0 + \alpha_i i_t + \alpha_y y_t + lags + \varepsilon_t^{MD},$$

where M is a broad monetary aggregate, y is output (or income), and ε^{MD} is an exogenous disturbance to the demand for money. We exclude potential output entirely from MD. Money enters the econometric models in logged form.

Money is taken to be M2. Clearly, the federal funds rate is not the opportunity cost of M2. Based on the large models estimated in Leeper, Sims, and Zha (1996), modeling the details of the links between the markets for reserves and broad money complicate but do not substantively change the analysis. In addition, Gordon and Leeper (1994) found that correctly accounting for the own rate of return on M2 in computing the opportunity cost does not appreciably alter the conclusions that concern us here.

As discussed in Section II, there is the potential for confounding behavior described by IS with that described by either MD or MP. For the present purposes, we seek to minimize those identification problems by treating (y_t, p_t) as being determined in an "inertial" sector of the economy. This assumption

[22] Authors include McCallum (1981, 1983) and Leeper (1991). Related work falls under the rubric of the "fiscal theory of the price level" advocated by Sims (1994) and Woodford (1998).

[23] Sargent (1979) states the result as follows: "There is no interest rate rule that is associated with a determinate price level" (p. 362). Predecessors to Sargent and Wallace that do not impose rational expectations include Patinkin (1949) and Gurley and Shaw (1960).

[24] Analyses of the price level, inflation, and monetary policy without money are creeping into principles textbooks (see Romer, 2000 and Stiglitz and Walsh, 2000).

treats output and inflation as predetermined for monetary variables: disturbances to MD and MP behavior affect y and p with a one-period lag. By lumping output and price determination into a single sector, we can no longer claim to have identified behavioral IS and AS equations; instead, we now have "x" and "p" equations.

The empirical work in this section contrasts two assumptions about policy behavior: the conventional Taylor rule, as given by equation (4), and an even simpler rule in which the Fed's choice for the funds rate depends only on current money growth[25]:

$$\text{(23)}\quad \text{MP(M rule):}\quad i_t = \gamma_0 + \gamma_{m1}[(M_t - M_{t-1}) - \bar{\mu}] + \varepsilon_t^{MP}.$$

We have chosen to normalize this rule on the nominal interest rate, but it is equally consistent to imagine this as a rule that determines the supply of money, where that supply choice is sensitive to the nominal interest rate.

We order the equations "x," "p," MD, and MP and the variables x, p, M, and i. Common to both assumptions about policy behavior are the pattern matrices

(24)

$$A_0 = \begin{bmatrix} \times & \times & \times & \times_x \\ 0 & \times & \times_1 & \times_p \\ 0 & 0 & \times_1 & \times_M \\ 0 & 0 & \times & \times \end{bmatrix},\ A_1 = \begin{bmatrix} \times & \times & \times & 0 \\ \times & \times & \times & \times_p \\ \times & \times & \times & \times_M \\ \times & \times & \times & 0 \end{bmatrix},\ A_2 = \begin{bmatrix} \times & \times & \times & 0 \\ \times & \times & \times & 0 \\ \times & \times & \times & 0 \\ \times & \times & \times & 0 \end{bmatrix},$$

where either $\times_x$ and $\times_p$ are non-zero with $\times_M = 0$ (conventional Taylor rule) or $\times_M$ is non-zero with $\times_x = \times_p = 0$ (M rule). Coefficients denoted $\times_1$ reflect the homogeneity restriction making money demand the demand for real balances. The specification removes any dynamics from policy behavior.

We estimate the models from 1959:Q1 to 2000:Q2. For the model with the Taylor rule, estimates of the coefficients in A_0 imply

$$\text{(25)}\quad \begin{aligned} (M_t - p_t)^d &= -0.242 i_t + 0.204 y_t \\ i_t &= 0.873(p_t - p_{t-1}) - 0.045 x_t, \end{aligned}$$

where we suppress the lagged coefficients in money demand, all constants, and the coefficient on the output gap in the price equation.[26] It is clear that when the demand for money is appended to this model with a Taylor rule for policy, the variables can be solved recursively: the first equation yields x_t, which from the second equation implies p_t; together these yield i_t from the policy rule in equation (25), and M_t comes from the money demand equation in equation (25), given the value for exogenous potential output, y_t^p. Because M and i are not determined simultaneously, estimates of money demand have no effect on estimates of policy behavior.

The estimated parameters in equation (25) seem reasonable. The short-run semi-elasticity of money demand is negative, and the short-run income elasticity is positive. In contrast to what we found when estimating a model with severely restricted dynamics (see equation (11)), policy appears to adjust the funds rate less than one-for-one with inflation. This difference underscores the importance of *all* the model's identifying assumptions when drawing inferences about policy behavior from estimated policy rules. Model (11) determines x and i simultaneously through IS behavior; model (25) determines them recursively due to inertial behavior. In spite of the estimated policy behavior, the model is stable.

Figure 7 shows the Taylor rule applied to response functions over a four-year horizon. Responses to MP shocks are depicted in the fourth column: a policy contraction raises the funds rate substantially and reduces the money stock, generating a liquidity effect. Output has a strange positive blip in the quarter after the shock, but then declines, following a hump-shaped path. There is no effect on the price level. Policy disturbances explain, at most, 13 percent of output, 41 percent of M2, and, in the initial period, over 80 percent of the funds rate. After four years, only 40 percent of funds rate variability is due to MP disturbances.

The endogeneity of policy appears in the first three panels of the bottom row of the figure. An "x" shock, which reduces output and the price level, produces a modest response from policy, while a "p" shock, which moves output and inflation in opposite directions, engenders a stronger offsetting reaction. Over 40 percent of funds rate fluctuations at four-year horizons arise from reactions to "p" shocks.

Policy also responds to money demand disturbances. An MD shock lowers M2 on impact. This is

[25] In estimation, we annualize the growth rate of money, so $4(M_t - M_{t-1})$ appears in (23).

[26] All current and lagged coefficients in the output and price equations are identical between the two models with two different policy rules.

Figure 7

Model with Money and a Taylor Rule (1959:Q1–2000:Q2)

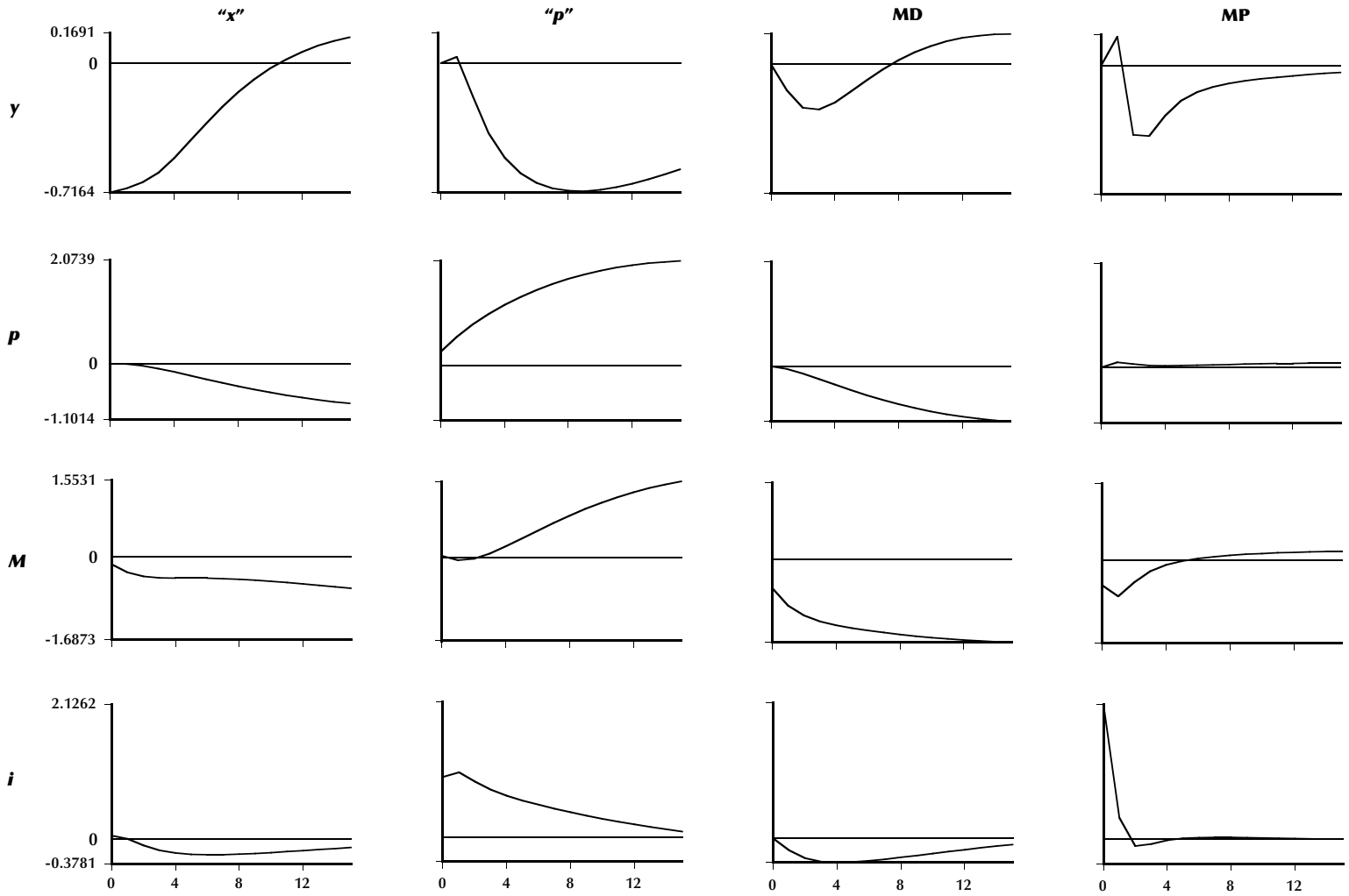
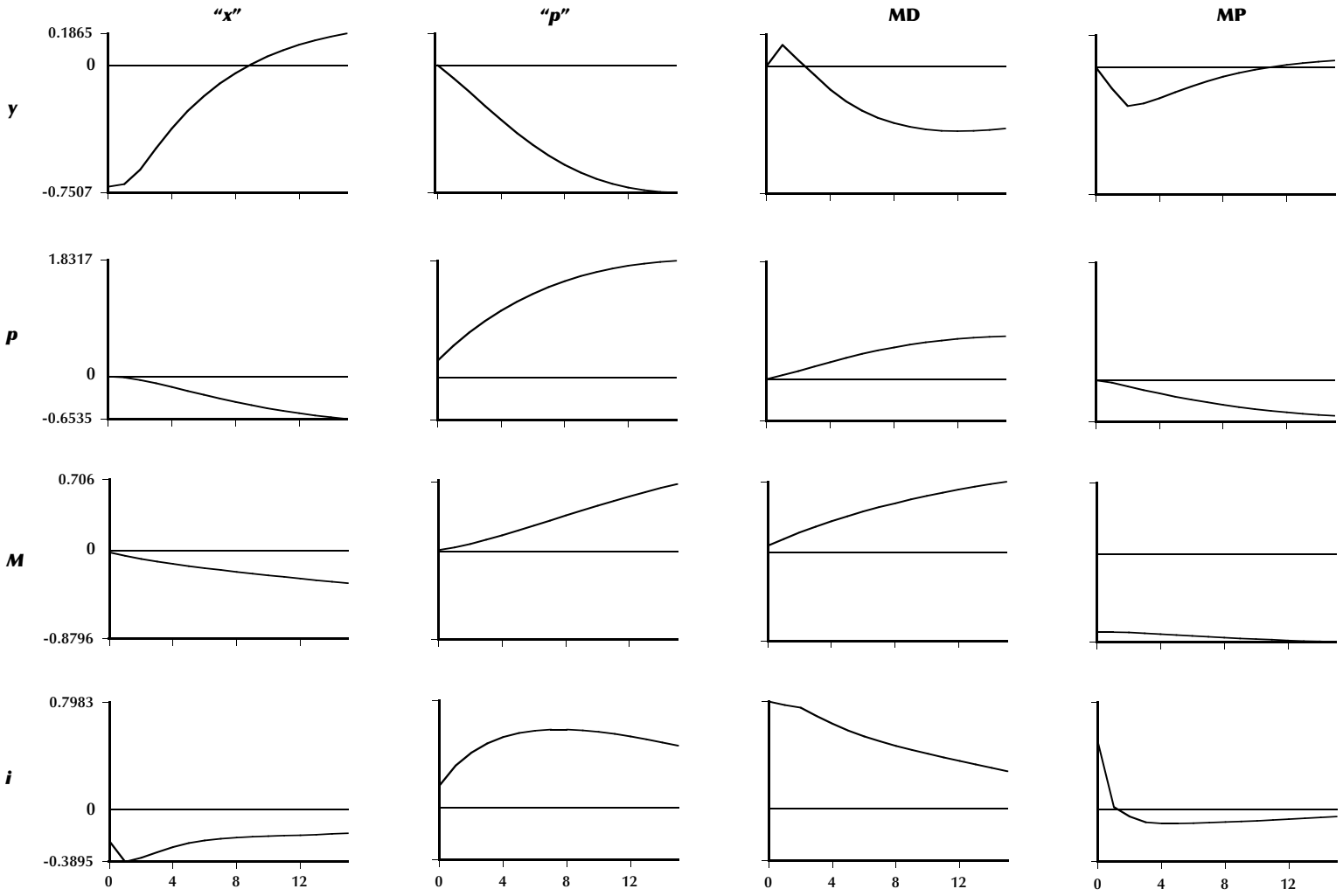


Figure 8

Model with Money and a Money Rule (1959:Q1–2000:Q2)



followed by falling prices and initially lower output; after about two years, output rises above its initial level.[27] Policy raises the funds rate smoothly, gradually returning it to its pre-shock level. The Taylor rule prevents the funds rate from jumping when MD shocks strike.

Estimates of the model with the alternative M rule in equation (23) yield[28]:

$$(26)\qquad \begin{aligned}(M_t - p_t)^d &= -1.571 i_t + 0.554\, y_t \\ i_t &= 2.913(M_t - M_{t-1}).\end{aligned}$$

The large estimated coefficient on money growth, by conventional wisdom, implies that policy was stabilizing.[29] The model is no longer recursive, as the equations in equation (26) simultaneously determine M and i. Note that the negative correlation between money and interest rates, which equation (25) attributes entirely to the interest elasticity of money demand, now gets split into a stronger negative demand elasticity and a positive supply elasticity.

Dynamic responses to the shocks in the "inertial" sector, shown in Figure 8, are similar to those in the model with a Taylor rule. From the standpoint of endogenous reactions to the disturbances that have occupied much of the attention of NK authors, the two policy rules are nearly indistinguishable. Some differences show up in the effects of exogenous shifts in policy: with an M rule a contraction generates a hump-shaped decrease in output and a smooth decline in the price level.

Three differences between the models are worth noting. First, comparing Figure 7 and Figure 8, a monetary contraction under a Taylor rule only temporarily changes the level of the money stock, while under an M rule it does so permanently. This implies that under a Taylor rule, the open market operation that initially raises the funds rate must be reversed to bring the money stock back to its original level. Second, the money stock appears to be more endogenous under an M rule: at most, 20 percent of the variation in M is attributed to exogenous MD shocks. With a Taylor rule, over 60 percent of M fluctuations are due to MD, providing a substantial role to exogenous factors in determining the money supply.

Finally, we formally test the overidentifying restrictions in the two models. The model with a Taylor rule imposes one less restriction than does the model with an M rule. We obtain:

Taylor rule	**M rule**
$\xi = 418.3$	$\xi = 207.4$
SC = 112.2	SC = 117.3
AC = 44	AC = 46
$p = 0.00$	$p = 0.00$

where SC is the Schwarz criterion and AC is the Akaike criterion. By any criterion the data reject both models. The test statistic in the M rule model is substantially less than in the Taylor rule model. These results suggest that a rule that makes the nominal interest rate respond to money growth—and nothing else—certainly fits no worse than a Taylor rule.

Estimates of identical models under two qualitatively different policy rules yield fairly similar results when judged by system properties like impulse response functions and stability. Based solely on estimated policy rules, however, the two models look very different.

The Recent Period

Much current research on Federal Reserve behavior draws sharp distinctions between the pre-1979 and the post-1979 periods. Rotemberg and Woodford (1997) focus on the 1980-95 period, Fuhrer and Estrella (1999) consider breaks in policy occurring in 1979:Q3, 1982:Q3, and 1987:Q2, while Taylor (1999a) estimates his rule from 1987:Q1 to 1997:Q3. We reestimate the two models in equations (25) and (26) over the period 1982:Q1–2000:Q2. The NK literature has concluded that during this period the Fed stabilized the economy by adjusting the funds rate strongly in response to inflation; it is also a period in which many authors believe no harm is done by ignoring money.

Estimates from the model with a Taylor rule are

$$(27)\qquad \begin{aligned}(M_t - p_t)^d &= -0.182 i_t + 0.303\, y_t \\ i_t &= 0.311(p_t - p_{t-1}) + 0.269\, x_t.\end{aligned}$$

System estimates do not recover a policy response to inflation that is even close to exceeding 1.0. In

[27] Textbook analyses typically have positive money-demand shocks lowering the price level. In simulations of the NK model, however, the pattern depicted in Figure 7 is common.

[28] Separate coefficients on M_t and M_{t-1} are estimated to be 2.913 and –2.902, so imposing equal and opposite coefficients does not move the estimates far from the peak of the likelihood function.

[29] In the NK model, this coefficient also eliminates indeterminacy of equilibria.

contrast, OLS estimates of the policy rule yield

$$\text{(28)}\quad \text{OLS:}\quad \underset{(0.19)}{i_t = 1.03}\left(p_t - p_{t-1}\right) \underset{(0.10)}{-0.13}\, x_t \underset{(0.006)}{+0.035},$$

which is consistent with conclusions of earlier authors that policy was stabilizing. Impulse response functions for the model estimate a small anomalous price response following an exogenous monetary policy contraction (not reported).

Estimates of the model with an M rule over the 1982:Q1–2000:Q2 period offer a different interpretation of policy behavior:

$$\text{(29)}\qquad \begin{aligned} (M_t - p_t)^d &= -4.503\, i_t + 1.442\, y_t \\ i_t &= 1.272\,(M_t - M_{t-1}). \end{aligned}$$

With the response of the funds rate to money growth exceeding 1.0, policy appears to be stabilizing. OLS estimates of the policy rule tell a different tale:

$$i_t = \underset{(0.072)}{0.344}\left(M_t - M_{t-1}\right) \underset{(0.005)}{+0.049}\ .$$

Responses to MP shocks in this model are not reported because they look very similar to those in Figure 8.

This section has presented evidence that the exclusion of money from NK empirical analyses is not innocuous. Substantive conclusions about the role of monetary policy and the behavior of the Federal Reserve can change when money is reintroduced in a way that generates interactions between MD and MP behavior. We also demonstrated that in practice it is difficult to distinguish a monetary policy that adjusts the nominal interest rate in response to inflation and output from a policy that adjusts the rate in response to the growth rate of the money supply. This raises some doubts that either specification of monetary policy—equation (27) or equation (29)—identifies policy decision rules. Instead, they may merely be alternative characterizations of equilibrium policy behavior.

In our results, the model with an M rule looks more reasonable than the model in which money is irrelevant to policy choice. These results seem to be at odds with Ireland (2000) and McCallum (2000). They show that money plays a quantitatively unimportant role in the class of general equilibrium models that includes NK models. The inconsistency between their quantitative-theoretic results and our empirical findings deserves further study.

V. STABILITY IN AN IDENTIFIED VAR FRAMEWORK

Section II estimated tightly parameterized behavioral relationships with simple policy rules. Section IV loosened the restrictions on dynamics in equations, describing private behavior while it maintained simple static policy rules. To complete the progression, this section allows also for freely estimated dynamics in policy behavior, leading to specifications in line with the approach taken in the identified VAR literature.

We show that when dynamics are left unrestricted, the models exhibit remarkable stability across subperiods. With the loss of parsimony comes increased sampling error and less precisely estimated parameters. To reduce sampling error we adopt the Bayesian methods developed by Sims and Zha (1998) and employed by Leeper, Sims, and Zha (1996) and Leeper and Zha (2001).

This section reports estimates from two weakly identified VARs. First we revisit the three-variable system consisting of output, the price level, and the federal funds rate. Although the system is fairly stable over time, it exhibits the price puzzle that has received attention in VAR work: an exogenous easing of policy lowers the funds rate, raises output, and *lowers* inflation.[30] The second model adds money to the system with two important effects: the price puzzle disappears and the responses to exogenous shifts in policy become more stable.

In choosing subperiods we face the usual problem that some "interesting" episodes may be too short to be informative. With brief time series, sampling error alone can dominate the estimates and produce misleading inferences. We check stability by estimating the system over three subperiods that coincide with ones frequently studied in work on Fed behavior: 1959:Q1–2000:Q2, 1959:Q1–1979:Q3, and 1959:Q1–2000:Q2 with 1979:Q4–1982:Q4 excluded. The models are estimated with four unrestricted lags and a constant term in each equation.[31] We found that adding potential GDP contributes little to the interpretation of results in this section, so we have dispensed with that variable.

[30] Sims (1992) and Eichenbaum (1992) discuss the price puzzle.

[31] In Sims and Zha's (1998) notation, the tightness of the prior is set as $\lambda_0 = 0.5$, $\lambda_1 = 0.4$, $\lambda_2 = \lambda_3 = 1.0$, $\lambda_4 = 0.2$, $\mu_5 = 1.0$, and $\mu_6 = 5$.

Three-Variable Model

As in Section IV, we treat output and inflation as determined in an inertial sector of the economy. This implies policy disturbances affect y and p with a one-period lag. We also take seriously the argument that an operational policy rule must make policy choice depend on observables. In the three-variable economy, where y and p are not observed contemporaneously, an operational rule sets the funds rate as a function of lagged values of all three series.

Because the VAR coefficients in this model are not interpretable, we move directly to the impulse response functions displayed in Figure 9. In each panel we report the three point estimates that correspond to the three subperiods and show a shaded region, which is a 68 percent error band associated with the model estimated from 1959:Q1 to 1979:Q3.[32] When, for the three subperiods, the responses to a shock fall within the error bands, the model makes stable predictions of the effects of that shock. For many policy purposes, this is sufficient evidence of stability.

Most responses over a four-year horizon fall within the error bands. Notable exceptions are the response of output and prices to a policy disturbance: the impacts of policy appear to weaken as more recent data are included in the sample. Another difference is that over the full sample, the funds rate responds more strongly to a "p" shock. This pattern is consistent with the view that, in the Volcker-Greenspan era, the Federal Reserve has placed increased emphasis on stabilizing inflation. Because in the model, both "p" and "y" shocks move output and prices in the same direction, they are both consistent with disturbances that shift aggregate demand. No shock in the model looks like aggregate supply.

Exogenous monetary expansions have strong and persistent effects on output. Even after four years, output remains well above its pre-shock level. The perverse response of the price level, though less pronounced in recent data, is consistent across subperiods. The stronger price puzzle exhibited in data up to 1979 conforms to Hanson's (2000a) findings in a different system of variables.

Figure 9 exhibits anomalies and enough instability that we are not comfortable with the identification of policy in the model. To address our concerns, we turn to a model with money.

Four-Variable Model

To identify policy behavior in the four-variable model with money, it is crucial to separate money demand from policy. We estimate the pattern matrix on contemporaneous variables

$$A_0 = \begin{bmatrix} \times & \times & \times & 0 \\ 0 & \times & \times & 0 \\ 0 & 0 & \times & \times \\ 0 & 0 & \times & \times \end{bmatrix}, \tag{30}$$

where the equations appear in columns in the order "y," "p," MD, MP, and variables appear in rows ordered y, p, M, i. We impose no additional restrictions on this matrix and no restrictions on lagged variables. Over the three sample periods, the estimates of A_0 are

(31)

$$A_0^{59-79} = \begin{bmatrix} 1 & 0.02 & -0.18 & 0 \\ 0 & 1 & -1.17 & 0 \\ 0 & 0 & 1 & -8.71 \\ 0 & 0 & 1.49 & 1 \end{bmatrix}, A_0^{59-00} = \begin{bmatrix} 1 & 0.02 & -0.43 & 0 \\ 0 & 1 & -0.59 & 0 \\ 0 & 0 & 1 & -9.93 \\ 0 & 0 & 1.44 & 1 \end{bmatrix},$$

$$A_0^{\text{no } 79-82} = \begin{bmatrix} 1 & 0.03 & -0.43 & 0 \\ 0 & 1 & -1.89 & 0 \\ 0 & 0 & 1 & 13.29 \\ 0 & 0 & 3.47 & 1 \end{bmatrix}.$$

In all periods, money demand is estimated to have a negative interest elasticity and positive price and income elasticities. Over the entire sample and over the period up to 1979, the Fed raised the funds rate in response to higher money growth. When the 1979-82 period is excluded, the policy response to current money growth changes from positive to negative. It may be tempting to infer that policy behavior changed in important ways. This parameter is one of many that describes policy behavior in the VAR. The implications of changes in that parameter must be gleaned from the entire model.

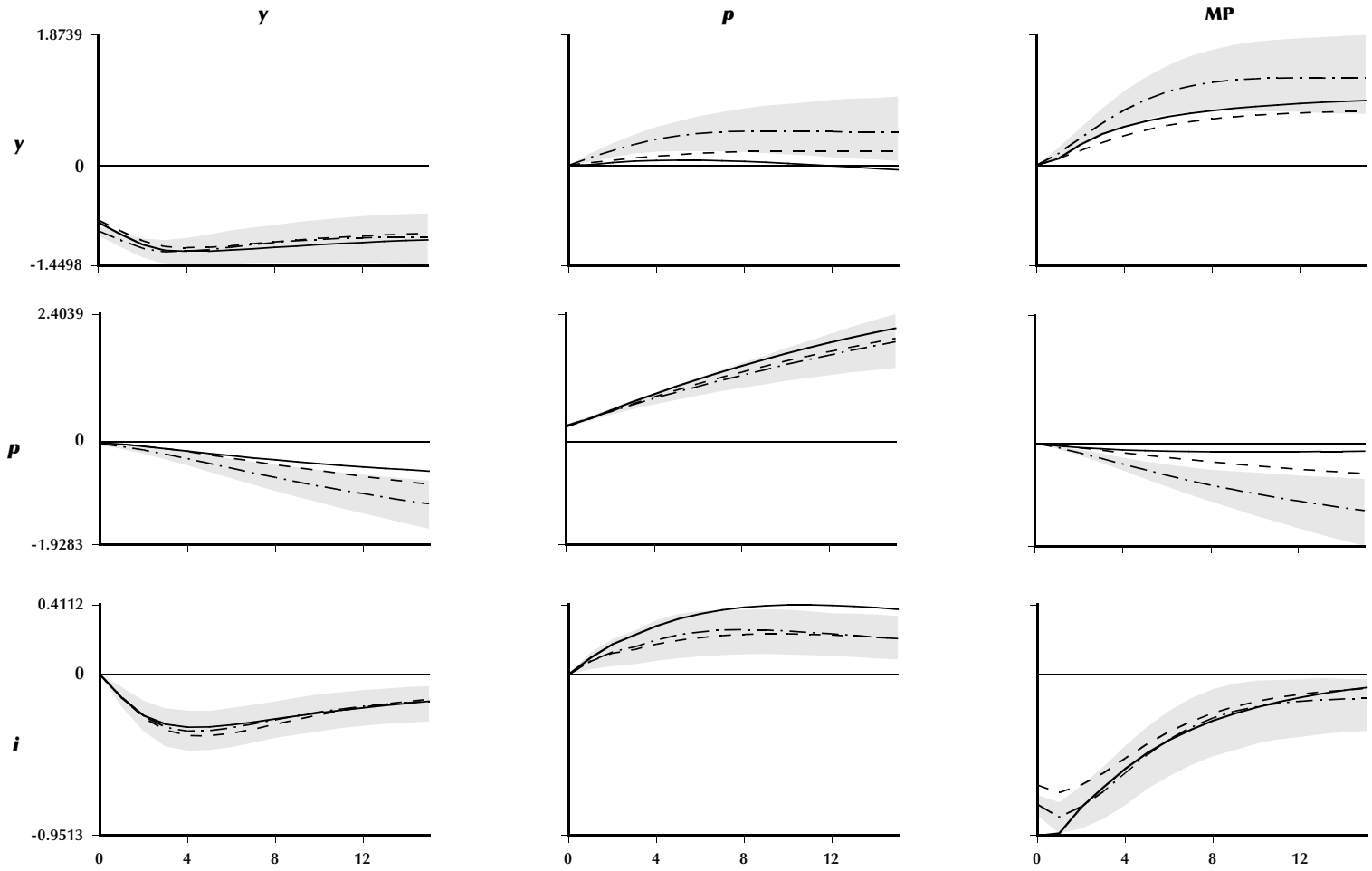
Figure 10 illustrates the pitfalls of inferring policy behavior from a single parameter in the policy rule. Dynamic responses to policy disturbances are remarkably stable. Point estimates from the three subperiods lie within the 68 percent error bands derived from the 1959-79 period. There is

[32] The error bands are computed from 50,000 draws using procedures developed by Sims and Zha (1999) and Waggoner and Zha (2000).

Figure 9

Three-Variable Identified VAR Over Various Sub-Periods

——1959:Q1–2000:Q2; — - 1959:Q1–1979:Q3; --- 1959:Q1–2000:Q2 excluding 1979:Q4–1982:Q4

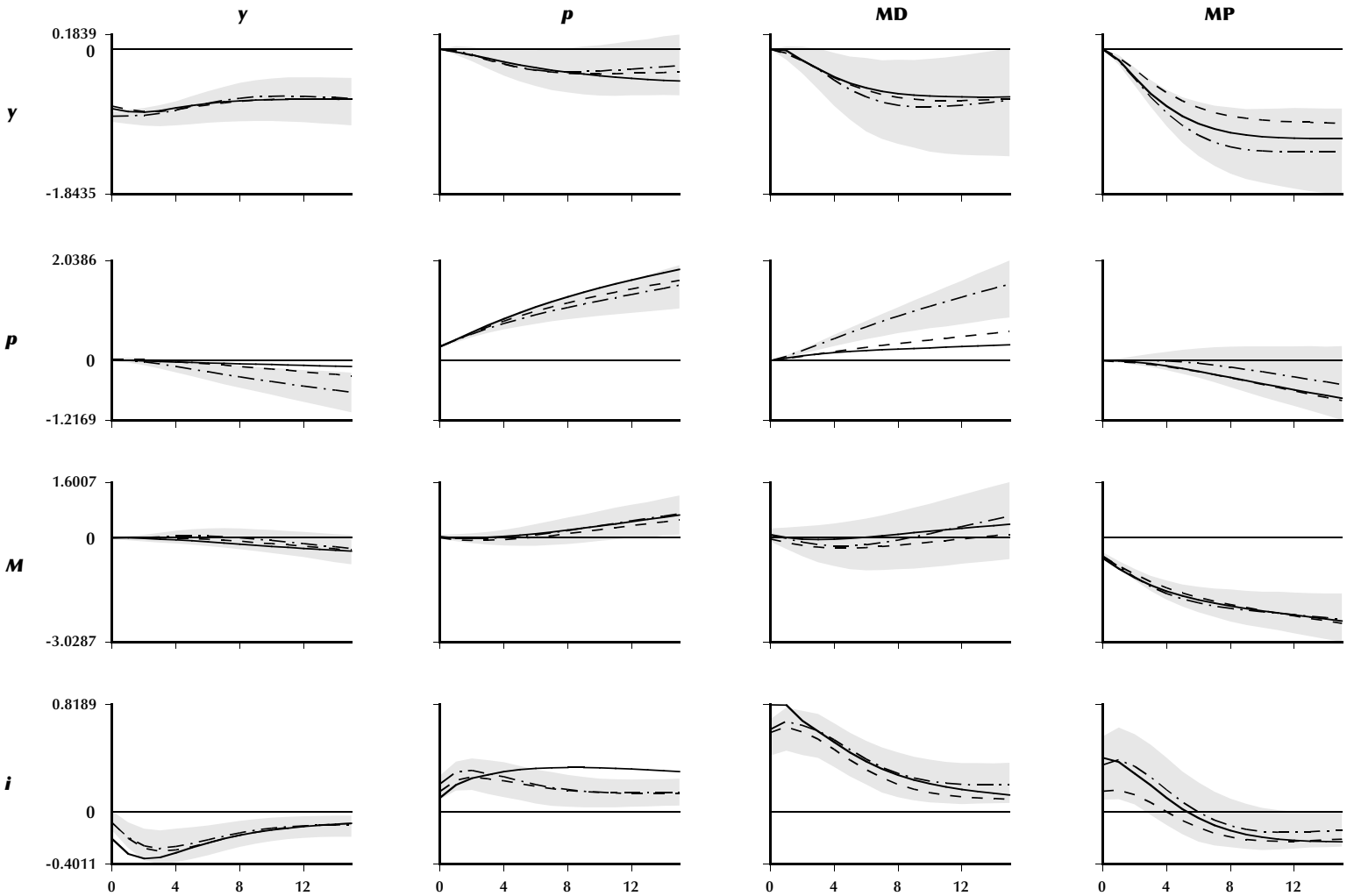


NOTE: The shaded region is a 68 percent error band associated with the model estimated from 1959:Q1 to 1979:Q3.

Figure 10

Four-Variable Identified VAR Over Various Sub-Periods

——1959:Q1–2000:Q2; — - 1959:Q1–1979:Q3; --- 1959:Q1–2000:Q2 excluding 1979:Q4–1982:Q4



NOTE: The shaded region is a 68 percent error band associated with the model estimated from 1959:Q1 to 1979:Q3.

some evidence in recent years that the output effects of policy shocks have weakened and the price effects have strengthened. In addition, much of the variance of the policy shocks over the entire sample derives from the 1979-82 period (note the smaller shock when that period is excluded). There is no evidence of a price puzzle: point estimates of price responses to a monetary contraction never rise, though the error band places some probability on a higher price path.

In contrast to the three-variable system, where both "p" and "y" shocks look like they shift aggregate demand, we now see distinct AS and AD disturbances. A "y" shock moves output and prices in the same direction, as one would expect from AD, while a "p" shock moves them in opposite directions, as would an AD disturbance. By separating the two kinds of aggregate shocks, the model allows a richer interpretation of policy behavior than can be gleaned from the model without money. When output and prices move together, policy responds to counteract the output effects. When output and prices move in opposite directions, policy tries to counteract the price effects. This pattern of policy responses is consistent with those found in the simple models with a Taylor rule (e.g., Figure 2), but they appear under very different identifying assumptions.

Some instability does show up in the model with money. The price effects of MD disturbances appear to be much weaker in models using recent data. And as in the three-variable system, the response of policy to a "p" shock is stronger in recent years, though now we can interpret the "p" shock as AS.

In contrast to the previous models, the four-variable VAR has only one overidentifying restriction. When estimated from 1959-79, the data do not reject the model by any criterion: the test statistic is $\xi = 1.04$; SC = 4.43, AC = 2.0, $p = .69$. Over the full sample there is more evidence against the model: $\xi = 4.92$; SC = 5.12, AC = 2.0, $p = .03$.

Adding money alters many of the inferences from an identified VAR. Money appears to stabilize the system across time, it eliminates the anomalous price puzzle following MP shocks, and it helps to distinguish aggregate supply from non-monetary policy aggregate demand disturbances. The instability of M2 velocity since the early 1990s, which has motivated some researchers to eliminate money from their analyses, does not appear to raise difficulties for identifying monetary policy behavior. Neither does it interfere with the stability of predictions about the dynamic impacts of exogenous shifts in policy.

Implications of VAR Estimates

NK analyses with simple policy rules consistently find that Federal Reserve behavior has been qualitatively different since 1979. Indeed, many authors attribute the superior performance of the U.S. economy over the past decade to superior policymaking. Central to this conclusion is that estimates of simple rules display substantial instability across time. We find no such instability in loosely identified VAR models with money. The contrast in our findings raises the possibility that some authors have overinterpreted the apparently shifting parameters in simple policy rules. The VAR literature, which does not attempt to reduce all policy behavior to two parameters, leads one to doubt the NK conclusions about policy.[33]

Views about the price puzzle in VARs have been influenced by Sims's (1992) argument. The Fed bases its choices on more information than small VARs contain, Sims argues, so what appears in a VAR to be an exogenous policy move is actually a response to extra-model information about aggregate supply disturbances. If this behavior is systematic, it can create a pattern of lower funds rates being followed by higher output and lower inflation. That view led to expanding VARs to include commodity prices, which serve as an "information variable" about supply developments, and thus diminishes or eliminates the price puzzle.

Hanson (2000a) questions the commodity price fix for the price puzzle. He shows that with Christiano, Eichenbaum, and Evans's (1999) recursive identification scheme, allowing the Fed to respond to current information in commodity prices does not resolve the price puzzle in data before 1979.[34] We cannot discuss these arguments

[33] Evidence from estimates of policy "reaction functions" supports the VAR findings. For example, Sims (1999) estimates a regime-switching equation describing the Fed's behavior. Although he finds that parameters describing systematic policy responses to the economy seem to shift across regimes, allowing for such shifts contributes little to the overall fit of the equation.

[34] The price puzzle is not mere VAR esoterica. Taking Hanson's findings as background, Barth and Ramey (2000) propose that the price puzzle actually is no puzzle. Through the "cost channel," a monetary contraction reduces "working capital" and impacts both aggregate demand and aggregate supply. Under certain elasticities, the equilibrium price level *should* rise after a contraction. They offer industry-level support for the view that monetary contractions reduce output and raise price-wage ratios.

in detail here. We are sympathetic to Hanson's view that the role of commodity prices in policy behavior is poorly understood and that they seem like a weak reed on which to rest identification of policy. We also believe that the role of commodity prices in helping to identify policy behavior has been overemphasized. In the four-variable identified VAR system, exogenous policy contractions never exhibit a price puzzle: the price level smoothly and strongly declines in all sample periods. These results obtain without the contrivance of commodity prices. Conditioning on commodity prices is neither necessary nor sufficient for resolving the price puzzle.

VI. CONCLUDING REMARKS

NK research offers little advice on how, at the frequency of FOMC meetings, the Fed should behave. Few authors suggest the FOMC should mechanically obey the simple rule assumed in the theory. The most detailed discussion of the practical application of a simple rule comes from Taylor (1993a), who suggests that policymakers use it to compare recent FOMC decisions to the rule. And forecasts could include those of the funds rate using the rule. This analysis, Taylor suggests, could include a range of forecasts corresponding to different coefficients in the rule. This suggestion is close to how Leeper and Zha (2001) use an identified VAR to conduct counterfactual policy analysis. Jansson and Vredin (2000) propose blending the two approaches. From the standpoint of a practical policy analyst, therefore, the two approaches could be applied in similar ways.

Applying the estimated NK models to the kind of policy analysis Taylor suggests leads to a quandary. In all the NK models we estimated, the inflation effects of policy disturbances—or deviations from the estimated policy rule—are minimal. By extension, changes in policy parameters, if private decision makers view them as temporary, will also have trivial impacts on inflation. It is not at all clear what monetary policy can do to stabilize inflation in the estimated models.

We introduced the paper by noting that policy analysts face tough choices. Our results do nothing to make those choices easier. But an analyst who wishes to base policy advice on a stylized model and a simple policy rule should be aware of the pitfalls of doing so. While the stories are compelling, they also appear to be fragile. The tradeoff between simplicity and robustness is an unpleasant reality of policy analysis.

To be sure, our analysis did not exploit all the structure embodied in the canonical IS-AS-MP NK model. Cross-equation restrictions implied by dynamic behavior may help resolve some of the identification problems we highlighted. On the other hand, experience suggests that those dynamic restrictions are precisely the ones most at odds with data.

It is a mistake to regard this paper as running a horse race between stylized models with simple rules and identified VARs with complex dynamics and loose behavioral interpretations. Each has its place in policy advising. For a model to help inform policy choice, though, its identifying assumptions should be robust and its fit—both in-sample and out-of-sample—should be respectable.

We have argued, and demonstrated in several ways, that it is treacherous to draw inferences about policy effects solely from policy rules estimated in isolation from a complete macro model. System estimates of policy parameters can differ substantially from single-equation estimates. And system properties do not align well with values of particular policy parameters.

A central theme of the NK literature is that the Fed's performance in the Volcker-Greenspan era is far superior to the Fed's performance in earlier periods. This dramatic conclusion is based on the following: policy parameters have changed across two subperiods; NK models predict that policy in the recent period produces more stable economic outcomes. We find that from the perspective of system estimation, instability of policy rules does not appear to be a serious concern. Even if particular policy parameters are unstable, when the dynamics of behavior are well modeled, the *equilibrium effects* of policy are quite stable. And it's the equilibrium effects that concern policymakers.

At a minimum our results argue forcefully that the bold NK conclusion, that U.S. monetary policy has improved dramatically in the past 20 years, deserves more careful scrutiny.

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Commentary

Kenneth D. West

I applaud the goal of this paper. I take this goal to be: compare tightly parameterized but easily interpretable "New Keynesian" (NK) models, with loosely parameterized, but more difficult to interpret, structural vector autoregressions (VARs). A systematic comparison about what each tells us about monetary policy is long overdue. I also applaud the technical and econometric skill of the analysis in the paper.

Unfortunately, despite the technical excellence of the paper, Leeper and Zha did not make quite as much progress towards this goal as I had hoped. The NK models in this paper differ in important ways from what seems to me to be common in the literature. So in the end I did not learn as much as I had hoped to learn about NK models. A crude summary of the paper's empirical message is that certain VARs say plausible and persuasive things about monetary policy while NK models usually do not. I do not find this message convincing. (I should stress that, in personal conversation, one of the authors [Eric Leeper] has emphasized that he does not interpret the empirical results this way. So my reading of the results differs from his.)

Let me begin by summarizing the paper. Let x_t denote the output gap, i_t a short-term interest rate under the control of the Fed, and π_t the inflation rate, $\pi_t = p_t - p_{t-1}$, where p_t is the log of the price level. Sections II and III in the paper analyze a three-equation NK model. (Actually, there is a fourth equation, for potential output, but since that is not central I will ignore it.) The three equations are: goods market equilibrium (IS), aggregate supply (AS) (Phillips curve), and monetary policy (MP). The following specification is broad enough to include virtually all the empirical work in these two sections of the paper:

(1) (IS) $x_t = -b_1 i_t + b_2 \pi_t + b_3 x_{t-1} +$ i.i.d. shock,

(2) (AS) $\pi_t = \lambda_1 x_{t-1} + \delta \pi_{t-1} +$ i.i.d. shock,

(3) (MP) $i_t = \gamma_{\pi 1} \pi_t + \gamma_{x1} x_t +$ i.i.d. shock.

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These notations match those in the paper except for the new symbols b_1, b_2, and b_3 in the IS curve.

The IS equation is most easily understood if one sets $b_3 = 0$, $b_1 = b_2 > 0$. Then we have

(4) (IS′) $x_t = -b_1(i_t - \pi_t) +$ i.i.d. shock.

Thus, the output gap is negatively related to the real interest rate, with current inflation proxying for expected inflation. This restriction is imposed in some of the empirical work. In some other parts, a value for b_1 was imposed and b_2 was estimated; this was done both with b_3 set to zero and with b_3 freely estimated.

The backward-looking AS function is familiar; in some of the empirical work, δ was set to unity, a restriction sometimes rationalized by the natural rate hypothesis. As well, the MP equation (3) is one version of the now-standard Taylor rule. In some of the empirical work, values for $\gamma_{\pi 1}$ and γ_{x1} were imposed.

Among the findings of Leeper and Zha:

- If the IS parameters b_1 and b_2 are unrestricted, IS cannot be distinguished empirically from MP: an identification problem.
- If one allows $b_1 \neq b_2$ or $\delta \neq 1$, then $\gamma_{\pi 1} > 1$ is not necessary for stability. This is a quantitative result.
- One can sometimes get sensible impulse response functions. A key requirement is that there is enough structure to separate IS from monetary policy.
- System and single equation estimations sometimes yield qualitatively different results, especially about $\gamma_{\pi 1}$.
- All specifications are strongly rejected by likelihood ratio tests.

Sections IV and V of the paper add money demand measured by M2, allow more dynamics, and experiment with an alternative money supply rule. The money demand equation is of the form:

(5) (MD) $M_t - p_t = \alpha_1 i_t + \alpha_2 y_t +$ unrestricted lags of M, p, i, and y + i.i.d. shock.

"More dynamics" comes first by allowing unrestricted second-order dynamics in "x" and "p" equations. The alternative money supply (MS′) rule is of the form:

(6) (MS′) $i_t = \gamma_m (M_t - M_{t-1}) +$ unrestricted lags of M, p, i, and y + i.i.d. shock.

Leeper and Zha find that parameters and impulse response functions are reasonable, especially under the alternative monetary policy rule, equation (6). Finally, the authors allow unrestricted fourth-order (rather than second-order) dynamics and find with the equation (6) money rule that impulse responses are not only reasonable, but are stable across subsamples.

This paper has some interesting logical results. It nicely illustrates a potential identification problem, namely, that a particular NK model implies an inability to use data to distinguish IS from MP. It also makes the important point that the economic system determines the behavior of macro variables simultaneously. This point is relevant to VAR studies, which often assume a recursive structure with little or no explanation. It is also relevant to any NK studies that attempt to define stability in terms of a single coefficient in the monetary policy reaction function. Finally, it reminds us that models without money are making some assumptions about money. One such assumption is that the money demand specification (whatever it is) is restricted in such a way that one can identify the equations under study. A second is that the monetary aggregates do not appear in the monetary policy rule.

But as I noted in my introductory paragraphs, I find the empirical results, and in particular the implicit contrast between NK (poor and often implausible fits) and VAR (good and often plausible fits) models unpersuasive. It is evident that the dynamics in the paper's NK models are generally more restricted than is consistent with the well-known persistence shown by macro variables. Recall that the paper takes seriously the notion that an IS curve specialized as in equation (4) can capture, at least roughly, the dynamics of the output–real interest rate relationship. The paper also presumes that a Taylor rule (equation (3)) without lags will pass econometric diagnostics. I am not surprised to find that neither notion finds support in the data.

In places, the dynamics of this paper's NK models are also more restricted than that of both the theoretical and empirical NK literature. In particular, the specification of the IS curve does not match what seems to me to be the current frontier of that literature. That frontier begins with the consumption–real interest rate relationship that is long familiar from asset pricing studies:

$$E_t\, c_{t+1} - c_t = (1/\sigma)(i_t - E_t\pi_{t+1}). \tag{7}$$

Here, I digress from the notation in the paper: c_t is log consumption. But as in the paper, σ is the intertemporal elasticity of substitution. This may be rewritten as

$$c_t = -(1/\sigma)(i_t - E_t\pi_{t+1}) + E_t c_{t+1}. \tag{8}$$

With what in my view is a bit of a stretch, the papers in the NK literature often substitute output or the output gap for consumption. This implies

$$x_t = -(1/\sigma)(i_t - E_t\pi_{t+1}) + E_t x_{t+1}. \tag{9}$$

If one sets $b_1 = b_2 = (1/\sigma)$, $E_t\pi_{t+1} = \pi_t$, this becomes $x_t = -b_1(i_t - \pi_t) + E_t x_{t+1}$. This reduces to equation (4) *only* when $E_t x_{t+1}$ is i.i.d., which is not the case in any of Leeper and Zha's models.

The paper is also inconsistent with much of the empirical NK literature. The paper makes frequent reference to Taylor (1999b). It is true that the model emphasized in Taylor (1999b) is a simple special case of equations (2), (3), and (4) (with $\delta = 1$). But in comments from the floor during the conference, Taylor stated that he used this model as an expository device, to illustrate empirical results actually found in a much larger and richer model. As well, existing studies allow richer dynamics either through lagged endogenous variables (e.g., McCallum and Nelson, 1999) and/or serially correlated disturbances (e.g., Rotemberg and Woodford, 1997).

A minor technical comment: Because the authors study overidentified models, it is not clear to me how to interpret the impulse responses and variance decompositions. In an overidentified model, some of the orthogonality conditions do not hold in sample. This might apply in particular to the conditions that state that disturbances are mutually uncorrelated. If so, the fitted model's residuals will be correlated, and one must somehow parcel out the correlation to underlying uncorrelated shocks. It would have been helpful for Leeper and Zha to tell us how they did so.

In sum: The paper by Leeper and Zha considers an interesting and important topic. It makes only a start on this topic. I very much look forward to a more comprehensive analysis in their future papers.

Measuring Systematic Monetary Policy

Kevin D. Hoover and Oscar Jordá

The financial press hangs on the words of every Governor of the Federal Reserve Board, every President of a Federal Reserve Bank, and most of all, of course, on the words of Chairman Alan Greenspan. In his testimony to the Senate Banking Committee on July 20, 2000, Greenspan said:

> Most recently we have needed to raise rates to relatively high levels in real terms in response to the side effects of accelerating growth and related demand-supply imbalances. Variations in the stance of policy—or keeping it the same—in response to evolving forces are made in the framework of an unchanging objective—to foster as best we can those financial conditions most likely to promote sustained economic expansion at the highest rate possible ... Irrespective of the complexities of economic change, our primary goal is to find those policies that best contribute to a noninflationary environment and hence to growth. The Federal Reserve, I trust, will always remain vigilant in pursuit of that goal.

Chairman Greenspan is well known for his inscrutability; yet the message here is exactly the one that the financial markets read into Federal Reserve policy: while ultimately it may aim to control inflation, it does so through contingent responses to inflation and real developments, and it expects its policy actions to affect the real economy systematically. The manner in which the Federal Reserve determines these contingent responses is central in the analysis of optimal policy-reactions function—provided, of course, that the Fed is right and that systematic monetary policy does have economically significant effects on the real economy.

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Starting in the early 1970s, new classical economists led by Robert Lucas began to question whether systematic monetary policy in fact had the required real effects. Over time, many macroeconomists have come to believe that, because of substantial frictions (e.g., price stickiness and limited participation in financial markets), systematic monetary policy does matter. Recent empirical analysis of monetary policy has typically used the econometric framework of vector autoregressions (VARs). Motivated in large measure by Lucas's argument that the coefficients of estimated macroeconomic relationships should not be invariant to changes in monetary-policy regime (the "Lucas critique"), practitioners have focused on unanticipated and unsystematic policy shocks. These shocks account for little of the variability of the instruments of monetary policy and, naturally, are of less interest to markets or politicians than is systematic policy. Yet the way in which VARs are interpreted implicitly assumes that Lucas's original argument—that systematic monetary policy is ineffective—is correct. There is a logical disconnection between the usual way in which VARs are interpreted and the belief that systematic monetary policy matters.

The aim of this paper is to analyze systematic monetary policy in a VAR framework in a way that is logically consistent. The key insight—originally by John Cochrane (1998)—is that the effect of systematic monetary policy depends on the balance of economic actors between those who behave as ideal new classical agents (frictionless competitors with rational expectations) and those who follow rules of thumb or face other frictions. Our own innovation is to suggest a method of using regime changes (the Lucas critique) to identify that balance empirically. Our purpose is both *critical* (we try to understand some of the recent literature in a common framework) and *positive* (we present an empirical assessment of U.S. monetary policy).

I. MONETARY POLICY AFTER LUCAS AND SIMS

Before the 1970s quantitative monetary-policy analysis had two important features. Orthodox monetary policy was typically viewed in a target-and-instruments framework in which the monetary authority sought to achieve a goal for inflation, gross domestic product (GDP), or unemployment using money or some more directly controllable monetary instrument as the means. Monetarists

and Keynesians debated the relative merits of fixed rules versus discretionary policies. Monetary economists investigated the relative merits of different ultimate and intermediate targets and of different instruments. The literature on optimal control suggested that rules need not be simple as, say, Milton Friedman preferred, but could be feedback rules allowing for nuanced responses to different contingencies. The second feature was that orthodox policy analysis was typically conducted in the context of large-scale macroeconometric models.

In the 1970s, the orthodoxy was attacked on two separate fronts. These two fronts are related, although their relationship is sometimes not clearly understood. The first front is the new-classical policy analysis of Robert Lucas and others. The second is the program of VARs initiated by Christopher Sims.

In the early 1970s, the early new classical school, especially as represented in the work of Lucas, attacked the logic of orthodox policy analysis. Before Lucas, economists typically analyzed the economy and the policymaker as *independent*. A policymaker who understood the economy (through a large macroeconometric model) could choose instrument settings and use the model to predict outcomes. Lucas (1972, 1976a) insisted that a sound analysis of policy required that the economy and the policymaker be seen as *interdependent*. The public based its behavior on its expectations of the policymaker's actions derived from an understanding of the rules that the policymaker followed. The rational-expectations hypothesis is a crisp implementation of the assumption that the public understands the implications of the policy rule. The public is modeled as having expectations that are consistent with the outcomes that would be predicted by the model of the economy itself. The policymaker cannot, then, model the public as being misled about the implications of systematic policies.

Lucas joined the rational-expectations hypothesis to the assumption of continuous market clearing and monetary neutrality to underwrite the "surprise-only" analysis of aggregate supply. On this view, money has no effect except when the public mistakes *neutral* changes in the price level for economically significant changes in relative prices; the *rational-expectations hypothesis* (Muth 1961) guarantees that such mistakes are short-lived.

Lucas not only undermined the conviction that systematic policy could be useful, he provided a basis for dismissing the usefulness of the typical macroeconometric models of the day as engines of policy analysis. The common practice circa 1970 was to use aggregative models with equations estimated conditionally on the existing monetary institutions and then to assume that the coefficients of the models remained constant as policymakers altered the values of monetary-policy variables and worked out their implications for GDP or other variables of interest. The surprise-only aggregate-supply function implied that this was a useless strategy. And Lucas predicted that one would find evidence of its uselessness in the instability of the coefficient estimates in macroeconometric models: as monetary regimes changed—that is, as the rules of systematic monetary policy changed—the coefficient estimates would also change.

The notion that changing the policy rule could not usefully affect real target variables became known as the *policy-ineffectiveness proposition* (Sargent and Wallace, 1975, 1976), while the conclusion that the coefficient estimates would change as policy regimes changed became known as the *policy-noninvariance proposition* or the *Lucas critique*. Although the Lucas critique is sometimes seen to be an attack on a modeling strategy (without rational expectations, the macroeconometric modeler cannot get it right), Lucas's point is not "if we only knew how the estimates would change, we could continue to use the old strategy of policy analysis." Rather, the point is that the estimates of the coefficients are merely shifting and useless correlations among macroeconomic aggregates. The real attack on macromodels is the policy-ineffectiveness proposition, which implies that there is no point in getting it right anyway: the surprise-only/rational-expectations hypothesis already implies that *no* predictable policy could work. Given policy ineffectiveness, the Lucas critique is a sideshow with respect to aggregate demand policies.

Over the past 25 years, the Lucas critique has become entrenched wisdom among macroeconomists. In contrast, the foundations of the surprise-only analysis of aggregate supply have been attacked in various ways. One approach challenges fundamental features of Lucas's analysis—particularly, the rational-expectations hypothesis or rapid market clearing. Lucas (1972) himself had pointed out that, if expectations were formed adaptively, systematic monetary policy would have real effects. Fischer (1977), Phelps and Taylor (1977), Taylor (1979), and, more recently, Fuhrer and Moore (1995)

and Rotemberg (1982, 1996), among others, have shown that, if prices can adjust only slowly because of contracts or because of costs of adjustment, then systematic monetary policy, not just monetary surprises, will have real effects.

A second approach developed more slowly out of the new classical analysis itself. Initial empirical tests of Lucas's analysis appeared to support the surprise-only hypothesis (Lucas, 1973, and Barro, 1977, 1978). Lucas (1973) himself realized that monetary surprises alone could not describe the serial correlation of output characteristics of the business cycle. He hypothesized non-monetary mechanisms involving optimal investment and intertemporal substitution in labor supply that would propagate an initial monetary shock through time (Lucas, 1975). More detailed investigations of the monetary-surprise hypothesis, however, revealed anomalies (Barro and Rush, 1980, and Gordon, 1990, p. 1135). Investigations of the stationarity of macroeconomic aggregates (Nelson and Plosser, 1982) convinced many macroeconomists that monetary shocks could not account for business cycles. If money were neutral in the long-run it could not induce permanent changes in real variables. Yet real output was, in fact, dominated by a non-stationary component, suggesting that real rather than monetary shocks were the cause of its movements. Kydland and Prescott's (1982) claim, that a business-cycle model with only real shocks better explained the data than a model with monetary shocks, bolstered this time-series result. In one of the main streams of macroeconomic development, monetary policy came to be viewed as irrelevant. The rational-expectations hypothesis and the principle of the Lucas critique were nonetheless now entrenched.

Sims (1980) criticized the orthodoxy of pre-1970s policy analysis along a second front. A central problem in isolating the independent causes of changes in macroeconomic aggregates is that the data are highly intercorrelated. The ideal of orthodox modelers was the strategy developed by the Cowles Commission in the late 1940s and early 1950s (see Morgan, 1990, and Hendry and Morgan, 1995, parts IV and VII and the correlative sections of the introduction). The hope was to isolate invariant (causal) structures using the implications of well-grounded economic theory to provide strong identifying restrictions. In practice, identifying restrictions were often imposed without firm justification. Sims (1980) played the honest boy gazing on the emperor of orthodox macroeconometric modeling, loudly declaring that he could see no clothes. The usual identifying assumptions were literally "incredible"—neither derivable from economic theory nor plausible on other grounds.

Sims's attack was not grounded in the new classical analysis or the Lucas critique, but was an independent criticism of the orthodox modeling. Nonetheless, one reaction to the Lucas critique can be seen as an attempt to answer Sims as well—to provide credible identifying restrictions. One way of dealing with coefficients that shift with changing monetary-policy regimes is to provide an accurate accounting of the actual behavior of the policymaker and the public in terms of the so-called "deep parameters" governing tastes and technology and economic constraints. The idea is to provide a detailed, structural model of the economy grounded in individual decision-making. Although this approach—at least in its representative-agent model form—dominates theoretical macroeconomics today, it has not been a notable empirical success. It founders because it makes a giant, unwarranted leap in concluding that what might be captured in a model of unique and differentiated individuals can be scaled up to an aggregate—the representative agent—that behaves just like one of the individuals, only on the scale of the whole economy. The identifying assumptions derived on such a basis are just as incredible as any palmed off by "structural" econometric modelers even before the dawn of the new classical macroeconomics.[1]

Sims's alternative program eschewed identification and worked instead with unrestricted reduced-form equations—VARs. Every variable in the VAR is regarded as endogenous. Each variable in the vector of endogenous variables is regressed on lagged values of itself and of all of the other variables. The VAR decomposes the observed variation in the economy into random errors and systematic responses. Since the variables are all endogenous, the action in the economy is attributed to the random-error terms. But Sims realized that these errors were not themselves exogenous as they were likely to be correlated with one another. Several simple algebraic transformations of the VAR could provide decompositions in which the errors were no longer intercorrelated by con-

[1] Hansen and Sargent (1980) provide a classic example of this strategy. See Kirman (1992), Hartley (1997), and Hoover (2001a, Lecture 3) for criticism of the representative-agent assumption.

struction. Then policy analysis could concentrate on the transformed error terms. These are the exogenous "shocks." In the transformed system, one could easily trace out the endogenous responses to the exogenous shocks in impulse-response functions or quantify their influence in variance decompositions.

The difficulty with this strategy is that the orthogonalizing transformations are not unique, but form an observationally equivalent class. To choose one of the transformations from the equivalence class is to impose structure on the model.[2] Each transformation defines quantitatively different shocks and different responses of endogenous variables to the shocks. The implications of a shock to monetary policy will generally be different depending on which transformation is chosen.

Noting the problem of observational equivalence, Cooley and LeRoy (1985) and Leamer (1985) argued that VARs were useful for policy analysis only to the degree that they were themselves structural. Sims conceded the point, but only by degrees. The key point in breaking down observational equivalence was to eliminate the correlation among the non-orthogonalized shocks. This required structural assumptions only about the contemporaneous variables. So-called "structural VARs" retain unrestricted lagged variables.[3] But even the contemporaneous order needs just as much justification as any other identifying assumption if it is not to be incredible. Yet the arguments that typically support the presumed order are informal or even casual. Credibility lies in the eye of the modeler.

The relationship of the program of VARs to the new classical macroeconomics was not completely worked out. Impulse-response functions modeled monetary surprises in a manner analogous to Lucas's surprise-only approach. Sims (1982, 1986) defended the structural VAR approach from the Lucas critique with the argument that the practice of monetary policy is captured in stable (if possibly complicated) contingent rules that change only rarely. Within any stable policy regime, the Lucas critique would not be an issue and concentrating on the response of the economy to shocks would be the appropriate strategy.

Most quantitative analysis of monetary policy is now conducted using VARs. But should it be? If Lucas was right in the first place, how does knowing the response of the economy to shocks help the policymaker when shocks cannot be systematically exploited? We cannot help but think that some practitioners want to have it both ways: to have a method that is immune to the Lucas critique because its VARs are estimated over periods in which, in fact, there have been no regime changes and, at the same time, to formulate advice for systematic policy on the basis of the impulse-response functions of these VARs.[4]

Recently policy analysis using VARs has moved past an exclusive focus on impulse-response functions and variance decomposition. Observing that only a small amount of the variability of the target variables in the economy can be credited to the shocks in monetary instruments no matter how they are identified, several economists have sought to shift the focus to systematic policy rules.[5] For example, one might ask how much of the response of the economy to a non-monetary shock (e.g., to an oil-price shock) is direct and how much is induced by the response of monetary policy to the shock?

Bernanke, Gertler, and Watson (1997), for example, estimate a structural VAR and then trace out the impulse responses when the equation governing the monetary instrument is modified to follow the desired rule. Sims (1999), in another example, imposes a structure that allows him to isolate the equations governing monetary policy in such a way that he believes he can legitimately transfer them from a VAR estimated on recent data to one estimated on data from before World War II. He asks whether the economy of the 1920s and 1930s would have developed differently if monetary policy in the earlier period had followed the

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[2] The transformations are observationally equivalent in the sense that, having the same reduced form, they also have the same likelihood. From the 1940s through the mid-1960s, whether systems of structural equations should be specified recursively or simultaneously was hotly debated (Wold 1960). The debate fizzled when Robert Basmann (1965) demonstrated the observational equivalence of recursive and simultaneous systems.

[3] That is, *unrestricted* aside from the practical necessity of a finite number of lags.

[4] See Cooley, LeRoy, and Raymon (1984) and LeRoy (1995) for an argument that the practice is, in fact, legitimate. Cf. Hoover (1988, Chap. 8, Section 4; 2001b, Chap. 7, Section 4).

[5] Some notable recent papers in this vein are Taylor (1995), Rotemberg and Woodford (1997, 1998), Bernanke, Gertler, and Watson (1997), and McCallum (1999). In the VARs reported in Section V, the usual variance decomposition attributes between 0.8 percent and 31 percent of the variance at a one-year horizon (depending on the estimation period) to the monetary shock. Because the variance attributable to each of the shocks mixes the direct effect of the random shocks with the indirect effects of the deterministic structural elements of the VAR, these estimates form an upper bound on the real effects attributable to unsystematic monetary policy.

typical patterns of the later period. Both examples are extensions that do not fundamentally change the structural-VAR framework. But inconsistencies remain. If Lucas's earlier assessment of monetary policy analysis is correct, which appears to be the unstated assumption of the emphasis on shocks (see Sections II and IV), then the counterfactual experiments are pointless. On the one hand, if the economy is really governed by a surprise-only aggregate-supply function and people have rational expectations, systematic policy, regardless of how it reacts to exogenous shocks, is ineffective. On the other hand, if policy regimes change, then the structural VARs should be just as subject to the Lucas critique as earlier "structural" macroeconometric models.

Our goal is to take a step to deal with these inconsistencies, to assess monetary policy in a VAR framework in a way that accounts for (or, if appropriate, dismisses) Lucas's analysis of policy. Our approach is based on an empirical implementation of Cochrane's (1998) structural interpretation of VARs. A by-product of the exercise is a framework for a critical assessment of the ways VARs are used for policy analysis.

II. A TAXONOMY OF MONETARY POLICY

The VAR and the Structural VAR

Monetary policy analysis involves at least two connected distinctions: systematic versus unsystematic policy and anticipated versus unanticipated policy. To clarify the relationships among these distinctions and their role in policy analysis, we develop a stripped-down VAR model and relate it to an equally stripped down macromodel.

We begin with the VAR. Two variables, which we think of as output (y_t) and money (m_t), depend on the histories of both variables and on random errors $\boldsymbol{\omega}_t' = (\omega_{yt}, \omega_{mt})$. For simplicity, we consider only one lag of the variables:

(1) $$y_t = \theta_{yy} y_{t-1} + \theta_{ym} m_{t-1} + \omega_{yt}$$

(2) $$m_t = \theta_{my} y_{t-1} + \theta_{mm} m_{t-1} + \omega_{mt}.$$

The covariance matrix of these errors is:

(3) $$E(\boldsymbol{\omega}_t \boldsymbol{\omega}_t') = \begin{bmatrix} var(w_{yt}) & cov(w_{yt}, w_{mt}) \\ cov(w_{yt}, w_{mt}) & var(w_{mt}) \end{bmatrix}.$$

Since there is no reason to believe that the error terms in equations (1) and (2) are not correlated, the off-diagonal terms are in general not zero. This poses a problem for policy analysis since it implies that we cannot distinguish the shocks: a shock, say, to the money equation would in general be associated with a correlated shock to the output equation. A properly transformed system, however, would fit the data equally well and have independent error terms. For example, multiply equation (2) by $\mu = \text{cov}(\omega_{yt}, \omega_{mt})/\text{var}(\omega_{mt})$ and subtract it from equation (1) to yield:

(4) $$y_t = \Pi_0 m_t + \Pi_1 m_{t-1} + \Pi_2 y_{t-1} + \xi_{yt}$$

where

(4a) $$\Pi_0 + \mu$$

(4b) $$\Pi_1 = \theta_{yy} - \mu\theta_{mm}$$

(4c) $$\Pi_2 = \phi_{ym} - \mu\phi_{mm}$$

(4d) $$\xi_{yt} = \omega_{yt} - \mu\omega_{mt}.$$

Equations (2) and (4) constitute a transformed model for which the random-error terms

$$\boldsymbol{\xi}_t' = (\xi_{yt}, \omega_{mt})$$

are no longer correlated:

(5) $$E(\boldsymbol{\xi}_t \boldsymbol{\xi}_t') = \begin{bmatrix} var(\omega_{yt}) - 2cov(\omega_{yt}, \omega_{mt}) + \mu^2 var(\omega_{mt}) & 0 \\ 0 & var(\omega_{mt}) \end{bmatrix}.$$

This transformation is the bivariate version of the *Choleski factorization*. It is characterized by the transformation of the variance-covariance matrix into a diagonal form and by the recursive structure of the contemporaneous variables known as the *Wold causal order*. Money is causally ordered ahead of output because m_t appears in equation (4) and helps to determine y_t, whereas y_t does not appear in equation (2). The lack of correlation between the random-error terms in the transformed system means that each equation may be shocked independently. The Wold causal order means that shocks to money transmit immediately to output, but shocks to output transmit to money only with a lag.

Unfortunately the Choleski factorization is not unique. If instead we had multiplied equation (1) by $\delta = \text{cov}(\omega_{yt}, \omega_{mt})/\text{var}(\omega_{yt})$ and subtracted it from

equation (2), we would have ended up with a system with a diagonal variance-covariance matrix, albeit with different error terms, and a Wold causal order in which output was ordered ahead of money. This is the problem of observational equivalence.

The VAR setup here is easily generalized to longer lags and more variables. Higher dimensional VARs will have as many Choleski factorizations as there are permutations of the variables. Furthermore, the restrictions that diagonalize the variance-covariance matrix need not be Choleski, and the equations of the transformed model need not be strictly recursive. The rule is that the system is identified so long as there are at least as many restrictions as the number that would be imposed by a Choleski decomposition, that is, $n(n-1)/2$. When the modeler claims that the particular order of the contemporaneous variables is the true one, the model is known as a *structural VAR*.[6] If the claim is warranted, then we have the pleasant result of having isolated the true independent shocks to the variables. When it is unwarranted, the shocks—even though uncorrelated by construction—are linear combinations of the true shocks.

The word "structural" here refers only to the relatively weak notion of an ordering of contemporaneous variables. The lagged variables remain unordered (and, in a commonsense usage, nonstructural). Yet, "structural VAR" in this sense has become cast-iron idiom. We will distinguish between structural VARs and *structural macroeconomic* (or *macroeconometric*) *models* in which the invariant parameters are identified.

Each shock to the VAR system has several effects: (i) a direct effect on the current value of the dependent variable in its own equation; (ii) an effect mediated through that variable on any equation lower in the causal order; and (iii) an infinite sequence of effects on the future values of both variables, since the values of today become the lagged values of tomorrow. The impulse-response function for the system of equations (2) and (4) captures the net result of all three effects. It is calculated by repeatedly substituting the lagged values of one equation into the other to eliminate the variables, leaving only the error terms going infinitely far back. This is the *moving-average representation* of the system. Although, in principle, the moving-average representation involves infinitely many shock terms, it is, in practice, truncated to some finite number. Typically, the impulse-response function is represented by a graph of the effect of a single-period unit shock on one of the variables over time. In principle, impulse-response functions are not invariant to the causal order of the structural VAR, so that getting the causal order correct is critical.

The Structural Macroeconometric Model

We begin the exposition of the structural macroeconometric model with a textbook Lucasian model. Output measured as a deviation from the mean is governed by a surprise-only aggregate-supply curve in which output at time t (y_t) depends on the deviation of prices (p_t) from prices that were expected at $t-1$ to hold at t (p_t^e):

$$y_t = \alpha\left(p_t - p_t^e\right) + \varepsilon_{yt}, \tag{6}$$

where ε_{yt} is a white-noise error term, and all variables are expressed in logarithms. Aggregate demand is assumed for convenience to be governed by a quantity equation with velocity normalized to unity:

$$p_t = m_t - y_t, \tag{7}$$

where m_t is the money stock, which is assumed to be in the control of the monetary authorities and governed by a monetary-policy rule (or reaction function):

$$m_t = \gamma_1 m_{t-1} + \gamma_2 y_{t-1} + \varepsilon_{mt}, \tag{8}$$

where ε_{mt} is a white-noise error term independent of ε_{yt}. This policy rule is a feedback rule that allows the monetary authorities to react to the state of the real economy, as well as to past values of the money stock. The model is closed by assuming that price expectations are formed rationally on the basis of all the information available up to time $t-1$ (Ω_{t-1}):

$$p_t^e = E\left(p_t \mid \Omega_{t-1}\right). \tag{9}$$

The distinction between systematic and unsystematic monetary policy refers to the *policymaker*. In the reaction function, equation (8), *systematic* policy is characterized by the choice of policy parameters, γ_1 and γ_2, while *unsystematic* policy is

[6] In the simple two-variable example, there are just two Choleski orderings and one further *overidentified* ordering in which the contemporaneous value of neither variable appears in the two equations (i.e., the ordinary VAR turns out to be the structural VAR). In larger systems, even when just identified or overidentified, there may be blocks of simultaneous equations.

Table 1

A Taxonomy of Monetary Policy

		Policymaker	
		Systematic	Unsystematic
Public	Anticipated	Known policy-reaction function	Credible announcement of a transitory, atypical setting of a policy instrument
	Unanticipated	Surprise change to new known policy-reaction function	Random shock to policy-reaction function

characterized by the error term, ε_{mt}. The distinction between anticipated and unanticipated monetary policy refers to the *public*. In equation (6), a policy is *unanticipated* if it delivers a non-zero price surprise ($p_t - p_t^e \neq 0$); it is *anticipated* if it delivers no price surprise ($p_t - p_t^e = 0$).

The two distinctions can be thought of as forming a two-by-two matrix as in Table 1. Each cell provides an example of the type of policy that exemplifies it. The fact that the cells along the main diagonal illustrate the most common monetary-policy actions explains the frequent equivocation between systematic and anticipated policies, on the one hand, and unsystematic and unanticipated policies, on the other. Policies are usually paired in those ways. Yet, surprisingly, neither of the off-diagonal cells is empty. One might think at first blush that unsystematic monetary policy could not be anticipated. Yet, if we regard the systematic elements of the reaction function as capturing what the monetary authorities typically do, we can imagine a transitory policy action which is both atypical—and, therefore, unsystematic—and yet widely expected. Past and future Federal Reserve behavior, for example, may in the current circumstances point to a 0.25 point rise in the federal funds rate; yet, the Federal Reserve may raise rates 0.5 points, as might have been widely expected, thus causing no surprise. The injection of liquidity in anticipation of the so-called "Y2K" demand may provide a less generic example. We can distinguish between anticipated and unanticipated policy according to whether the policy is fully captured in Ω_{t-1}. If a transitory policy were captured in ε_{mt}, but ε_{mt} was in fact effectively an element of Ω_{t-1}, there would be no price surprise in equation (6).

The usual solution to a model such as that in equations (6) through (9) presumes that only information dated at t–1 or before can be part of the information set, Ω_{t-1}. On that assumption, the model can be solved for y:

$$(10) \quad y_t = \left(\frac{1}{1+\alpha}\right)\left(\alpha m_t - \alpha y_1 m_{t-1} - \alpha\gamma_2 y_{t-1} + \varepsilon_{yt}\right).$$

Despite the apparent influence of m over y, systematic policy is ineffective. To see this, substitute equation (8) into (10), to yield:

$$(11) \quad y_t = \left(\frac{1}{1+\alpha}\right)\left(\alpha\varepsilon_{mt} + \varepsilon_{yt}\right).$$

Only the real shock to aggregate supply and ε_{mt}, the unsystematic part of monetary policy, and not the systematic part (governed by γ_1 and γ_2) matter.

Equations (8) and (10) can be regarded as a structural VAR with m (Wold-)causally ordered ahead of y. Equation (11) can be regarded as a (degenerate) form of the moving-average representation of this VAR. The impulse-response function for y to a unit monetary shock follows from equation (11) and takes the value $(1 + \alpha)^{-1}$ at period t, and zero thereafter. In this case, it is easy to see that the usual method assumes the monetary-policy action is both unsystematic and unanticipated. This case illustrates the policy-ineffectiveness proposition.

Contrast this to the case in which the monetary-policy action is unsystematic and anticipated. As we already noted, an unsystematic, anticipated policy is equivalent to Ω_{t-1} including ε_{mt}. When ε_{mt} is not known at t–1, the rational expectation of m_t is $\gamma_1 m_{t-1} + \gamma_2 y_{t-1}$; but, when ε_{mt} is known at t–1, it is simply m_t. An easy way to derive the consequence for y of an anticipated, unsystematic policy is to replace $\gamma_1 m_{t-1} + \gamma_2 y_{t-1}$ with m_t in equation (10) to yield:

$$(10') \quad y_t = \left(\frac{1}{1+\alpha}\right)\varepsilon_{yt}.$$

The impulse response to an unsystematic shock to

monetary policy is now zero for all current and future times because the impulse is itself eliminated, that is, $(p_t - p_t^e = 0)$.

Mapping Between the Structural Macroeconometric Model and the Structural VAR

Turning to systematic policy, notice that an econometrician typically would not estimate a regression of the form of equation (10). Rather he would estimate something like equation (4). The coefficients of equation (4) can be related to the parameters of the structural macroeconomic model:

(4a′) $$\Pi_0 = \frac{\alpha}{1+\alpha}$$

(4b′) $$\Pi_1 = \frac{\alpha\gamma_1}{1+\alpha}$$

(4c′) $$\Pi_2 = \frac{\alpha\gamma_2}{1+\alpha}$$

(4d′) $$\xi_{yt} = \frac{1}{1+\alpha}\varepsilon_{yt}.$$

If equations (6) through (9) are the true model, then the two sets of equations (4a) through (4d) and (4a′) through (4d′) together provide a mapping between the true underlying parameters of the structural macroeconomic model and the reduced-form coefficients of the ordinary VAR. Given the ethos of the structural VAR program, the assumption one *knows* that equations (6) through (9) are the true model is too strong to be believed. Yet, without this assumption, the constituent parameters (α, γ_1, γ_2) cannot be identified separately.[7] A change in systematic policy, a change in the value of γ_1 or γ_2, will change the value of one or more of the Π_i's. This is the Lucas critique: the estimated macroeconometric relationship is not invariant to changes in systematic policy. Such a policy change, however, could have a real effect only to the degree that it was unanticipated. If the change were credibly announced in advance, then like other anticipated policies it would have no effect. If it were sprung on the public by surprise, it would act like an unanticipated shock until it was incorporated into the public's expectations (see the lower left-hand cell of Table 1).

The Lucas critique might appear to threaten the usefulness of the VAR methodology in the case of changing monetary regimes—since every shift of systematic policy would alter not only the monetary-policy equation (equation (8)) but also the rest of the system (here equation (4) with the coefficients defined by (4a′) through (4d′)). While it is true that the coefficients (the Π_i's) will change (that is, the structural VAR is non-invariant), both equations (11) and (10′) (the moving-average representation of the structural VAR) are invariant to the policy regime. They depend only on the parameter α, which is not a parameter of the policy-reaction function. The invariance of the moving-average representations implies that the impulse-response functions are themselves invariant. The VAR is immune to the Lucas critique not because regimes do not in fact change but because of the assumptions that real variables respond only to monetary surprises and that expectations are rational. Since equation (11) is drawn from a transformation of the structural VAR, the invariance result depends on having the correct structural VAR. Getting the right causal structure of the contemporaneous variables is, therefore, crucial.

III. RULE-OF-THUMB AND PARTIAL RATIONAL MODELS

Although it is not often remarked, the manner in which VARs are usually evaluated makes sense on the assumption that Lucas's analysis of monetary policy is correct. What is more, only the case of unsystematic, unanticipated monetary policy is typically analyzed. This is the case of the policy-ineffectiveness proposition in full force. An older tradition saw monetary policy as having real effects, either because expectations were not formed rationally or because prices did not move quickly to clear markets. To incorporate this view into the model of the last section, we could replace equation (9) with an adaptive-expectation scheme. This would be analogous to Lucas's non-natural rate model, the foil for the rational-expectations model of his 1972 paper. Or we could add a price adjustment equation, which is the approach implicit in the overlapping contract models and explicit in the models of Fuhrer and Moore (1995) and Rotemberg (1982, 1996), among others. A less specific approach would be to model anticipated monetary policy more simply as directly affecting output. Instead of equation (6), aggregate supply is modeled as

(12) $$y_t = \beta m_t + \varepsilon_{yt}.$$

Now equations (8) and (12) form a structural VAR.

[7] If we are willing to assume the truth of the model, then $\gamma_1 = \Pi_1/\Pi_2$, $\alpha = (1 + \Pi_0)^{-1}$, etc.

In the new system, the distinction between anticipated and unanticipated monetary policy is moot—both have an identical effect on y. Similarly, the Lucas critique is inapplicable since equation (12) indicates that expectations are not an element in people's supply responses.

The two systems (equations (8) and (10) and equations (8) and (12)) represent extreme cases. Cochrane (1998) considers an intermediate case in which there are two types of agents in the economy: Lucasian agents, who have rational expectations and act according to equations (6) and (9), and those who do not have rational expectations and act according to equation (12). Following his conjecture in a simplified case, we characterize an economy with both types as a simple mixture of equations (6) and (12), where λ is the mixing parameter:

$$(13) \qquad y_t = \lambda\beta m_t + (1-\lambda)\alpha\left(p_t - p_t^e\right) + \varepsilon_{yt}.$$

Intuitively, we can regard $(1-\lambda)$ as the proportion of people in the economy who form rational expectations and λ as the proportion who follow a rule of thumb.[8] But this is not quite right since those forming rational expectations would rationally take account of the responses of those who follow a rule of thumb, so that λ would properly correspond to the proportions of the two groups only if they lived on separate islands. It may nonetheless satisfactorily capture the relative importance of their behaviors (if not their numbers) and, when properly selected, may adequately account for the properties of the impulse-response functions of the estimated VAR.[9]

The complete model consists, then, of equations (7), (8), (9), and (13). The new system is given by the solution for y (equation 14) and the policy rule in equation (8), namely,

$$(14) \qquad y_t = \left[\frac{\lambda\beta + (1-\lambda)\alpha}{1+(1-\lambda)\alpha}\right] m_t - \left[\frac{(1-\lambda)(1-\lambda\beta)\alpha}{1+(1-\lambda)\alpha}\right]\left(\gamma_1 m_{t-1} + \gamma_2 y_{t-1}\right) + \left[\frac{1}{1+(1-\gamma)\alpha}\right]\varepsilon_{yt}$$

$$(8) \qquad m_t = \gamma_1 m_{t-1} + \gamma_2 y_{t-1} + \varepsilon_{mt}.$$

When $\lambda = 1$, equation (14) reduces to equation (12); and when $\lambda = 0$, it reduces to equation (10). In general, when $0 < \lambda < 1$, equations (8) and (14) form a structural VAR.

Because of the interaction between agents with rational expectations and those for whom the distinction between anticipated and unanticipated monetary policy is meaningless, the Lucas critique has real bite, except in the two extreme cases. Not only are the estimates of the structural equations (8) and (14) not invariant, the responses of y to monetary impulses are complicated functions of the mixing parameter, λ, as well as the policy parameters, γ_1 and γ_2. What is worse for purposes of empirical analysis is that, in a single regime (that is, for a particular setting of the policy parameters), λ is not identified and cannot be estimated directly. The inability to measure λ renders Cochrane's analysis critical rather than empirical. Cochrane (1998) illustrates the consequences of different assumptions about the value of λ but provides no means of getting an empirical handle on it.

To make some headway, notice that, when policy is anticipated and we can, therefore, replace $\gamma_1 m_{t-1} + \gamma_2 y_{t-1}$ with m_t, equation (14) reduces to

$$(15) \qquad y_t = \lambda\beta m_t + \left(\frac{1}{1+(1-\lambda)\alpha}\right)\varepsilon_{yt}.$$

Equation (15) is invariant to monetary-policy regime but depends on the mixing parameter, λ. If we assume that λ is itself invariant to regime changes as seems reasonable, then regime changes themselves can be used to identify λ. Intuitively, the identification strategy works like this: A structural VAR (analogous to equations (8) and (14)) is estimated in two or more regimes. In general, the impulse-response functions can be worked out from the moving-average representation of the structural VAR only if we assume a value for λ; and, thus, the impulse-response functions will be different for every different λ. But since, according to equation (15), the impulse response to an *anticipated monetary shock* should be identical across regimes, we

[8] This is similar to Campbell and Mankiw's (1989) consumption function in which one part of the population follows the permanent-income hypothesis and another part follows a rule of thumb.

[9] The interaction in which the rational agents take account of the responses of the non-rational agents is part of Haltiwanger and Waldman's (1989) analysis. In work on the consumption function, Haase (1998) demonstrates that the likely difference between a proper analysis of the interaction and a pure mixing process is likely to be small, so that in this case λ is probably very close to the actual proportion of the population that does not exhibit rational-expectations/surprise-only aggregate supply behavior.

search for that value of λ that delivers the required identity. The details of our implementation strategy are set out in the next section.

IV. MEASUREMENT STRATEGY

Deeper Structures

The model of the last section was simplified to illuminate the taxonomy of monetary policy more clearly. To carry the strategy just sketched to the data, we need a more complicated model. Closely following Cochrane (1998), we generalize the model in two ways: first, we allow interactions with a set of macroeconomic variables orthogonal to the monetary shocks; second, we allow for the richer dynamics characteristic of the VAR approach. The final setup is designed to accommodate any macroeconomic model that can be well represented empirically by a VAR.

The main focus of our approach is Cochrane's (1998) equation (8), reproduced here with a slight change of notation as

(16)

$$y_t = A(L)\left[\lambda m_t + (1-\lambda)\left(m_t - E\left(m_t|\Omega_{t-1}\right)\right)\right] + \boldsymbol{B}(L)\boldsymbol{\varepsilon}_{wt} ,$$

where y_t denotes an output measure that belongs to a vector of non-monetary variables $\boldsymbol{w}$, m_t denotes a monetary-policy indicator, $E(.|.)$ is the conditional expectations operator, $\boldsymbol{\Omega}_{t-1}$ is the information available at time $t-1$, and $\boldsymbol{\varepsilon}_{wt}$ is a vector of orthogonalized, non-monetary innovations (which include the output innovation itself). The term $A(L)$ is a polynomial in the lag operator L (e.g., $L^{\alpha}x_t = x_{t-\alpha}$). $\boldsymbol{B}(L)$ is a polynomial vector in the lag operator. The parameter λ takes values between 0 and 1.

Equation (13) is a particular case of equation (16). Equations (7) and (9) can be used to eliminate the term $(p_t - p_t^e)$ in equation (13), replacing it with the term $(m_t - m_t^e)$. Equation (13) has no intrinsic dynamics. The dynamics of the system of equations (7) through (9) and (13) come entirely from the dynamics of the monetary-policy rule (equation (8)). In contrast, the lag structure $A(L)$ provides equation (16) with its own dynamics. The parameters α and β in equation (13) are subsumed in $A(L)$. The form of the equation reflects a conception of the influence of monetary policy in which *either* an anticipated shock (λm_t) *or* an unanticipated shock ($(1-\lambda)(m_t - m_t^e)$) is propagated by exactly the *same* mechanism: $A(L)$. Non-monetary impulses propagate according to their own mechanism: $\boldsymbol{B}(L)$.

Note that, although we have been drawing a parallelism between equation (13) and equation (16), the latter is silent with regard to the causal structure of the underlying model (despite the presence of the term λm_t). It is a general expression that does not restrict the modeler's choice of structural identification assumptions in any way. This point is made transparent in the derivation of equation (20) below.

Equation (16) can be rewritten as

(17)

$$y_t = \lambda A(L)E(m_t \mid \Omega_{t-1}) + A(L)\left[m_t - E(m_t \mid \Omega_{t-1})\right] + \boldsymbol{B}(L)\boldsymbol{\varepsilon}_{wt} ,$$

which makes its economic interpretation easier. The term $\lambda A(L)$ describes the dynamic response of output to anticipated monetary policy, $E(m_t|\boldsymbol{\Omega}_{t-1})$. In a Lucasian economy in which everyone has rational expectations, $\lambda = 0$ and the first term of the right-hand side of equation (17) vanishes. The term $A(L)$ describes the dynamic response to unanticipated monetary policy ($m_t - E(m_t|\boldsymbol{\Omega}_{t-1})$).

Equation (16) captures the spirit of early aggregate new classical models. Yet, it falls short of the quest initiated with Lucas's (1976a) "critique" for a model grounded in tastes and technology—a true microfoundational model. That quest, we believe, is quixotic. Either it is hopelessly intractable—theoretically and empirically—if one seeks to model millions of individuals, or it is of dubious empirical relevance if one falls back on the representative-agent (or even agents) assumption. To bring the representative-agent model to aggregate data in the form of maintained identifying assumptions is to impose restrictions that implicitly claim that the aggregates behave as if they were governed by an individual scaled-up to the size of the economy. Such a claim beggars belief; it is far more incredible than anything assumed by the macroeconometric modelers of the 1960s.[10]

We believe that the Lucas critique can be dealt with only pragmatically. We cannot seek invariance at the deepest microfoundational level, but we must rather seek *relative* invariance a level or two below the aggregate macroeconomic phenomena. The parameters of a structural VAR are, we believe, unlikely to be invariant. Through some plausible, but by no means certain, assumptions, we hope to account for the most important causes of non-

[10] See Hoover (2001a) for a critical account of the methodological foundation of the quest for microfoundations.

invariance. The only test of our assumptions is whether or not they appear to succeed in practice.

Cochrane (1998) also considers a model of costly price adjustment due to Rotemberg (1982, 1996). There can be other possibly more realistic models as well. We prefer the model of equation (16) to the Rotemberg model or any of the alternatives since it makes relatively weaker (coarser) restrictions. There is a vast number of alternative models of sticky price adjustment or models using differing nonrational-expectations schemes of expectations formation in which the fine details are likely to matter a good deal. The fact that in equations (16) and (17) the anticipated/unanticipated monetary policy distinction is summarized in the single parameter, λ, is a virtue.

The key steps in our strategy of identification are to establish the links between equation (16) and the structural moving-average representation of $\boldsymbol{w}_t$. One would first estimate the structural VAR (with the presumed-correct causal order) and then invert it to obtain

$$(18) \qquad y_t = C_{ym}(L)\varepsilon_{mt} + \boldsymbol{C}_{yw}(L)\boldsymbol{\varepsilon}_{wt},$$

where ε_{mt} is the structural monetary innovation and $\boldsymbol{\varepsilon}_{wt}$ is as defined above.[11] The term $C_{ym}(L)$ is an infinite polynomial, while $\boldsymbol{C}_{yw}(L)$ is an infinite polynomial vector in the lag operator. Each of their elements, $c_{i,j,k}$, associates movements of variable i to a shock in variable j that took place k periods ago. Note that, because equation (18) is a moving-average representation derived from a structural VAR, the terms $c_{i,j,0}$ are in general non-zero for $i \neq j$. The typical Wold causal orderings that result from Choleski factorizations impose the condition $c_{i,j,0} = 0$ for all $i > j$. The observationally equivalent class is then defined by the permutations of the variables indexed by i and j. Similarly, the moving-average representation of m_t is,

$$(19) \qquad m_t = C_{mm}(L)\varepsilon_{mt} + \boldsymbol{C}_{mw}(L)\boldsymbol{\varepsilon}_{wt}$$

Equating terms in expressions (17), (18), and (19), we have

(20)

$$y_t = C_{ym}(L)\varepsilon_{mt} + \boldsymbol{C}_{yw}(L)\,\boldsymbol{\varepsilon}_{wt} =$$

$$A(L)\left\{\lambda\left[C_{mm}(L)\varepsilon_{mt} + \boldsymbol{C}_{mw}(L)\,\boldsymbol{\varepsilon}_{wt}\right] + (1-\lambda)c_{mm0}\varepsilon_{mt}\right\}$$

$$+\boldsymbol{B}(L)\boldsymbol{\varepsilon}_{wt},$$

so that in general

$$(21) \qquad C_{ym}(L) = \lambda A(L)C_{mm}(L) + (1-\lambda)A(L)c_{mm0}$$

and similarly,

$$(22) \qquad \boldsymbol{C}_{yw}(L) = \lambda A(L)\boldsymbol{C}_{mw}(L) + \boldsymbol{B}(L).$$

In a Lucasian economy, for which $\lambda = 0$, only the unanticipated component of monetary policy has real effects resulting in

$$(23) \qquad C_{ym}(L) = A(L)c_{mm0}.$$

The factor c_{mm0} appears as a normalization that sets the 0th order coefficient on m_t in the money equation of the VAR representation to its conventional value of unity. Equation (23) says that the structural coefficients $A(L)$ correspond to those of the usual impulse-response function of output to an orthogonalized monetary shock. Also note that $\lambda = 0$ implies $\boldsymbol{C}_{yw}(L) = \boldsymbol{B}(L)$, that is, the response of output to each non-monetary shock characterized in the structural model by the elements of the polynomial vector $\boldsymbol{B}(L)$, correspond to the usual collection of impulse-response functions in $\boldsymbol{C}_{yw}(L)$. In this Lucasian case, the monetary shock is all there is to the influence of monetary policy: it starts a dynamic process going but has no further effect.

The fact that the impulse-response functions for the structural macroeconometric model in the Lucasian case of $\lambda = 0$ correspond to the usual impulse response functions from the structural VAR highlights a point only infrequently acknowledged: the usual methods of assessing the implications of structural VARs implicitly assume Lucas's surprise-only economy. Looking at the other extreme case in which $\lambda = 1$ demonstrates how misleading this can be. When $\lambda = 1$, the equivalent to equation (23) is

$$(24) \qquad C_{ym}(L) = A(L)C_{mm}(L)$$

and, similarly,

$$(25) \qquad \boldsymbol{C}_{yw}(L) = A(L)\boldsymbol{C}_{mw}(L) + \boldsymbol{B}(L).$$

This corresponds to a traditional (pre-Lucas) structural macroeconometric model in which coefficients of the VAR representation are invariant structural parameters. Here, in contrast to the Lucasian case, a

[11] Equation (18) is one row of the inverted structural VAR. Other variables are represented *mutatis mutandis* by other rows. The derivations that follow do not require that we make specific distinction among the elements of ε_{wt}, mainly the output shock and any other non-monetary shocks.

monetary shock has a *direct effect* as in the Lucasian case and an *indirect feedback effect* in which the monetary-policy-reaction function captured in equation (19) systematically affects real variables (indicated by the terms in square brackets on the right-hand side of equation (20)).

In the intermediate case in which $\lambda \neq 0$, where some people react to anticipated monetary policy and with rational expectations integrate the behavior of these rule-of-thumb agents into their own expectations, equations (21) and (22) themselves are the equivalent to (23). In this case, the conventional impulse response of output to a shock to a variable other than money or output ($\boldsymbol{C}_{yw}(L)$) can be decomposed: the direct effect is measured by the polynomial vector $\boldsymbol{B}(L)$, and the effect of the endogenous response of the monetary authority is measured by the polynomial vector $\lambda A(L)\boldsymbol{C}_{mw}(L)$.

Equations (21) and (22) yield a set of conditions that govern the correspondence between the coefficients of the estimated impulse-response functions, derived from the moving-average representation of the VAR ($\boldsymbol{C}_{ij}(L)$) and the structural parameters (λ, $A(L)$, and $\boldsymbol{B}(L)$). These conditions are

$$
(26)\quad
\begin{aligned}
c_{ym0} &= a_0 c_{mm0} \\
c_{ym1} &= a_0 \lambda c_{mm1} + a_l c_{mm0} \\
&\vdots \\
c_{ymk} &= a_0 \lambda c_{mmk} + \ldots + a_{k-1} \lambda c_{mm1} + a_k c_{mm0} \\
&\vdots \\
c_{yy0} &= a_0 \lambda c_{my0} + b_{0y} \\
c_{yy1} &= a_0 \lambda c_{my1} + a_1 \lambda c_{my0} + b_{1y} \\
&\vdots \\
c_{yyk} &= a_0 \lambda c_{myk} + \ldots + a_k \lambda c_{my0} + b_{ky} \\
&\vdots \\
&\left.\begin{aligned}
c_{yw0} &= a_0 \lambda c_{mw0} + b_{0w} \\
c_{yw1} &= a_0 \lambda c_{mw1} + a_1 \lambda c_{mw0} + b_{1w} \\
&\vdots \\
c_{ywk} &= a_0 \lambda c_{mwk} + \ldots + a_k \lambda c_{mw0} + b_{kw}
\end{aligned}\right\} \text{for each element in } w \text{ except } y. \\
&\vdots
\end{aligned}
$$

In principle, there is an infinity of such conditions; in practice, they would be truncated at some arbitrary $k = K$. Then, for any given value of λ between 0 and 1, one can compute the terms a_k and b_{ik} for $i \in \boldsymbol{w}$ and $k = 0,1,2,\ldots$, although λ itself cannot be separately identified from the set (26).

Identification Strategy

Since λ measures the relative importance of output responses to anticipated policy, one might quibble that more transparent and, therefore, more accurately anticipated operating procedures for monetary policy might affect the value of λ. These concerns are probably secondary. In 1994, the Federal Reserve began announcing the policy actions of the Federal Open Market Committee immediately after its meetings. Demiralp and Jordá (2000) provide evidence that financial markets already anticipated Federal Reserve policies reasonably accurately even in the period in which the actions were meant to be secret.

Although it is natural to assume that the response of output to a monetary impulse, $A(L)$, or to non-monetary shocks, $\boldsymbol{B}(L)$, would remain invariant across policy regimes, one cannot rule out other sources of parameter instability unrelated to monetary policy. Such instability is a matter of econometric specification and, unlike the instability highlighted by the Lucas critique, not a matter of endogenous economic behavior. In any case, the assumption of the invariance of these dynamic structures is no stronger than the invariance assumptions typical in the literature on structural VARs.

Starting with the identifying assumptions, we must first identify distinct monetary policy regimes. Let $t = 1, 2,\ldots, T$ index the sample period of a time series. Let $T_1,\ldots, T_H$ denote the dates that partition the sample into $H + 1$ distinct regimes corresponding to the subsamples $t = T_{h-1} + 1,\ldots, T_h$ for $h = 1,\ldots, H$. (By convention $T_0 = 0$.) Practically, the smallest of these subsamples must afford adequate degrees of freedom to estimate a monetary VAR, the specification of which is common to all subsamples but the parameters of which are estimated within each subsample. In practice, the VAR would contain the variables y_t and m_t, along with the other variables in the vector $\boldsymbol{w}_t$ that are customarily included in monetary VARs. To simplify the notation and the exposition in the derivations below, we shall treat the VAR as including only y and m. It is straightforward to generalize our results to higher order systems.[12]

Estimates of each subsample VAR produce a set of impulse-response functions similar to those in expressions (18) and (19), namely,

[12] Although the notation is meant to suggest output and money as the variables, in Section V, y shall correspond to employment and m to the federal funds rate.

$$(27)\qquad \begin{aligned} y_t &= C^h_{ym}(L)\varepsilon_{mt} + C^h_{yy}(L)\varepsilon_{yt} \\ m_t &= C^h_{mm}(L)\varepsilon_{mt} + C^h_{my}(L)\varepsilon_{yt} \end{aligned} \qquad h = 1,\ldots,H$$

and consequently $H+1$ sets of conditions similar to those in expression (26). To solve for the parameters of interest λ, a_k and b_{ik} for $k = 0,1,\ldots,K$, consider solving the system of $K\times 2$ equations implied by our subsample estimates. In particular, define $\boldsymbol{C}_y = [C_{ym0},\ldots, C_{ymK}, C_{yy0},\ldots, C_{yyK}]'$, where

$$(28)\qquad \underset{(H+1)\times 1}{C_{ymk}} = \begin{bmatrix} c^1_{ymk} \\ \vdots \\ c^{H+1}_{ymk} \end{bmatrix}; \quad \underset{(H+1)\times 1}{C_{yyk}} = \begin{bmatrix} c^1_{yyk} \\ \vdots \\ c^{H+1}_{yyk} \end{bmatrix} \quad k = 0,1,\ldots,K.$$

That is, the vector $\boldsymbol{C}_y$ collects the $H+1$ estimates for each of the $K\times 2$ right-hand side terms in expression (26). Similarly, define

(29)

$$\underset{(H+1)\times(K+2)}{\boldsymbol{X}_k} = \begin{bmatrix} c^1_{mm0} & \ldots & c^1_{mmk} & 0 & \underset{K-k}{\ldots} & 0 & 1 \\ \ldots & \ldots & \ldots & \ldots & \ldots & \ldots & \ldots \\ c^{H+1}_{mm0} & \ldots & c^{H+1}_{mmk} & 0 & \ldots & 0 & 1 \end{bmatrix}$$

$$k = 0,1,\ldots K$$

$$\underset{(H+1)\times(K+2)}{\boldsymbol{Z}_k} = \begin{bmatrix} c^1_{my0} & \ldots & c^1_{myk} & 0 & \underset{K-k}{\ldots} & 0 & 1 \\ \ldots & \ldots & \ldots & \ldots & \ldots & \ldots & \ldots \\ c^{H+1}_{my0} & \ldots & c^{H+1}_{myk} & 0 & \ldots & 0 & 1 \end{bmatrix}$$

with

$$(30)\qquad \boldsymbol{W} = \begin{bmatrix} X_0 & \ldots & 0 & 0 & \ldots & 0 \\ \ldots & \ldots & \ldots & 0 & \ldots & 0 \\ 0 & \ldots & X_K & 0 & \ldots & 0 \\ 0 & \ldots & 0 & Z_0 & \ldots & 0 \\ 0 & \ldots & 0 & \ldots & \ldots & \ldots \\ 0 & \ldots & 0 & 0 & \ldots & Z_K \end{bmatrix}.$$

Rewriting expression (26) in terms of the auxiliary definitions (28) through (30) we have,

$$(31)\qquad \boldsymbol{C}_y = \boldsymbol{W}\boldsymbol{\beta} + \boldsymbol{v}.$$

Equation (31) represents a system of $K\times 2$ equations based on a sample of size $h+1$, where the parameter vector $\boldsymbol{\beta}' = [\beta_1,\ldots, \beta_K,\ldots, \beta_{K\times 2}]$ contains a high number of cross-equation restrictions implied by the relations in equation (26) and where $\boldsymbol{v}$ represents the vector of computational error terms for a given $\boldsymbol{\beta}$. For example, the subvectors $\beta_k' = [a_k, \lambda a_{k-1},\ldots, \lambda a_0, 0,\ldots, 0]$ for $k = 1,\ldots, K$, and $\beta_k' = [\lambda a_k, \lambda a_{k-1},\ldots, \lambda a_0, 0,\ldots, b_{ky}]$ for $k = K+1,\ldots, K\times 2$. In order to calculate the $\boldsymbol{\beta}$ that satisfies equation (31), one would want to minimize the terms in $\boldsymbol{v}$ with some sensible loss function. A natural candidate is the minimized sum of the squared deviations, which converts the problem of calculating the parameters $\boldsymbol{\beta}$ in equation (31) into one of estimating a panel of $K\times 2$ equations over $H+1$ periods with cross-equation restrictions.

To summarize our empirical strategy: first, locate the structural breaks in the monetary-policy equation; second, estimate a structural VAR for each subsample; third, use these VARs to retrieve the coefficients of the moving-average representations of the VARs (equation (27)); fourth, use these in equation (31) to estimate jointly the a_ks, b_{ik}s and λ (explicit in equation (26)); finally, having recovered an identified structural model from the unidentified structural VAR, we are in a position to conduct quantified policy analysis.

V. RECOVERY OF THE STRUCTURAL MODEL

The Structural VAR

We draw on the extensive literature on the analysis of monetary policy using linear VARs. We considered specifications based on monthly data that would afford a sufficiently large sample. Even though we believe that there are serious questions about the cogency of the arguments used to justify the identification of the causal ordering of the structural VAR and the typical, albeit not universal, assumption of a Choleski factorization, we want a specification with an established tradition in the literature to avoid lengthy discussions regarding variable choice and structural identification assumptions, which would distract the reader from the main focus of this paper. We hope to revisit this issue in later work. From the available alternatives, we chose the system originally proposed in Christiano, Eichenbaum, and Evans (1996) and slightly modified and used *inter alia* by Evans and Marshall (1998) and Hamilton and Jordá (2000).[13]

[13] The slight modifications consist of using log employment rather than log real GDP, log personal-consumption-expenditure deflator rather than log GDP price deflator, and the ratio of nonborrowed reserves to total reserves rather than log nonborrowed reserves.

Our data sample runs from January 1960 to January 1999. The data vector is given by $\boldsymbol{S} = [EM, P, PCOM, FF, NBRX, \Delta M2]'$, where EM denotes the logarithm of nonagricultural payroll employment; *P* denotes the logarithm of the personal consumption expenditures deflator (1996 = 100); *PCOM* denotes the annual growth rate of the index of sensitive materials prices issued by the Conference Board; *FF* denotes the federal funds rate; *NBRX* denotes the ratio of nonborrowed reserves plus extended credit to total reserves; and *ΔM2* denotes the annual growth rate of the monetary aggregate M2. Data are seasonally adjusted. All logged data are multiplied by 100 so that impulse responses can be interpreted directly as percent deviations. The inclusion of the variable *PCOM* has now become customary in monetary VARs to mitigate the anomalous responses of the price-level to monetary-policy shocks (the so-called "price puzzle" described in Sims, 1992, and Eichenbaum, 1992). The contemporaneous variables are causally ordered through a Choleski factorization in which the Wold causal order is the order in which the variables are written in the vector *S*. To conserve parsimony and degrees of freedom, we experimented with several lag length specifications. A lag length of 4 seemed adequate (for example, the Durbin-Watson statistic for the *FF* equation had a value of 2.0267) to capture the dynamic properties of our problem.

Monetary Policy Regimes

In the analysis that follows, employment (*EM*) will be regarded as the real output measure (y in our theoretical exposition) and the federal funds rate (*FF*) as our monetary-policy instrument (m in the theoretical exposition).

The first task required by our methodology is to identify dates of monetary policy regime shifts. Detailed institutional accounts can be found *inter alia* in Meulendyke (1998) and Strongin (1995). That the period from October 1979 to October 1982 is a monetary-policy regime distinct from what came before or after is, perhaps, the most commonly agreed fact about monetary policy regimes in the United States. This regime targeted nonborrowed reserves, while the immediately preceding and following regimes essentially targeted interest rates. The dates October 1979 to October 1982 are frequently identified by purely statistical methods as structural breaks in short-term nominal interest rates (see, for example, Garcia and Perron, 1996, and the references therein). There is, however, considerably less agreement with regard to the dates of other regimes.

We rely on a combination of institutional knowledge and the sup *F* tests for structural breaks at unknown dates due to Bai and Perron (1998).[14] These tests are a generalization of the well-known test of Andrews (1993). In particular, we test the policy-reaction equation of the VAR described in the previous section in its structural form, namely,

$$FF_t = \boldsymbol{\Gamma}_t' \boldsymbol{\beta}_h + u_t \quad h = 1, \ldots, H+1, \tag{32}$$

where $\boldsymbol{\Gamma}_t'$ includes a constant; up to four lags of the vector *S* described in the previous subsection, and the contemporaneous values of *EM*, *P*, *PCOM*. The index h refers to the number of breaks which delivers $H + 1$ possible regimes. We allow for the possibility that all of the coefficients, and not just the intercept, may change.

The first stage of the test requires that we calculate the unknown break dates T_h, $h = 1, \ldots, H$ along with the unknown coefficients $\boldsymbol{\beta}_h$. The method suggested by Bai and Perron (1998) requires that we specify a maximum number of possible break points (which we set at eight, corresponding to at most nine distinct regimes) as well as a minimum size partition τ such that $T_h - T_{h-1} > \tau T$. We choose $\tau = 10$ percent, constraining the minimum subsample size to 46 observations (this choice afforded a reasonable number of degrees of freedom for estimation of equation (32)).

It is important to highlight this particular feature because it conditions the candidate dates for possible breaks. In particular, the beginning and end of the nonborrowed-reserves targeting regime (October 1979 to October 1982) are separated by fewer than 46 observations, and, therefore, the regime does not afford the minimum number of observations needed for computation. As we shall see, however, this did not constitute a significant impediment. The test detected breaks at June 1978 and at April 1982, which are separated by exactly the minimum 46 observations and bracket a period that includes nearly all of the nonborrowed-reserves targeting period.

The method of estimation is based on the least-squares principle. For each h partition ($T_1, \ldots, T_H$), the associated least-squares estimates for $\boldsymbol{\beta}_h$ are obtained by minimizing the sum of squared residuals

[14] We thank Jushan Bai for generously providing us with the code used in this paper to run the structural break tests.

Table 2

Structural Break Tests of the Monetary-Policy Equation

Number of breaks under the alternative, *H*	sup *F*(*H*|0)	5% Critical value	sup *F*(*H*+1|*H*)	5% Critical value	AIC	BIC
1	106.59	64.69	—	—	0.2320	0.3027
2	***125.92***	58.56	119.92	68.12	0.1318	***0.2244***
3	117.20	55.52	96.80	70.21	0.1254	0.2785
4	106.32	53.16	96.80	71.09	0.1222	0.3540
5	123.22	50.93	***90.20***	71.84	***0.1178***	0.4452
6	124.74	48.77	70.62	72.59	0.1193	0.5882
7	102.92	46.29	46.76	73.83	0.1267	0.8151
8	88.84	42.83	52.22	74.83	0.1395	1.1707

Breaks	Dates							
1				1980:6				
2				1978:6	1982:4			
3		1970:4		1978:6	1982:4			
4		1970:4		1978:6	1982:4	1986:2		
5		*1970:6*	*1974:6*	*1978:6*	*1982:4*	*1986:2*		
6	1965:5	1970:9	1974:5	1978:6	1982:4	1986:2		
7	1965:5	1970:9	1974:5	1978:6	1982:4	1986:2		
8	1965:5	1970:9	1974:5	1978:6	1982:4	1986:2	1989:12	1994:8

NOTE: The 5 percent critical values are extrapolated from Bai and Perron (1998, Table I).

$$\sum_{h=1}^{H+1} \sum_{t=T_{h-1}+1}^{T_h} \left[FF_t - \boldsymbol{\Gamma}_t' \boldsymbol{\beta}_h \right]^2 . \tag{33}$$

Denote the resulting sum of squared residuals $S_T(T_1,..., T_H)$, and the estimated break points $\hat{T}_1,..., \hat{T}_H$ are such that

$$\hat{T}_1,...,\hat{T}_H = arg\,min_{T_1,...,T_H} S_T(T_1,...,T_H), \tag{34}$$

where the minimization is taken over all partitions $(T_1,..., T_H)$ such that $T_h - T_{h-1} > \tau T$.[15]

Once the break dates have been estimated for the different values of H, we apply the sup F test of the null of no structural breaks ($H = 0$) versus the alternative hypothesis that there are H (for $H = 1$, 2,..., 8) breaks as well as the sup F of the null that there are $H + 1$ structural breaks versus the alternative of H breaks. The F test consists essentially of the appropriately normalized ratio of the sum of squared residuals under the null to the sum of squared residuals under the alternative hypothesis.[16] The tests are flexible enough that they permit lagged dependent variables as regressors as long as the residuals are serially uncorrelated.[17] Table 2 summarizes the results of the test. The tests suggest that the null hypothesis is strongly rejected for all values of H, although the maximum value of sup $F(H|0)$ is attained for $H = 2$ (125.92) with $H = 5,6$ in close proximity (123.22 and 124.74, respectively). The sup $F(H + 1|H)$ indicates that for $H < 6$, the null hypothesis is easily rejected, indicating that a value of $H = 5$ would be appropriate. These results seem to be confirmed by the information criteria: Akaike information criterion (AIC) selects a more generous specification with five breaks, whereas Bayesian

[15] The algorithm used to compute expression (16) is discussed in Bai and Perron (1996).

[16] The specific form of these tests is described in Bai and Perron (1998, Section 4).

[17] Recall the Durbin-Watson statistic for the *FF* equation is 2.0267.

information criterion (BIC) would suggest a more conservative specification with two breaks. We choose $H = 5$ because, for the purposes of our exercise, there is no bias in having redundant regimes (although there may be some loss of efficiency in the estimates of the impulse responses). The estimated break dates match relatively well with institutional developments at the Federal Reserve, even for values of h that are larger than our final selection. For instance, in 1970 the Federal Reserve revised its Regulation Q to eliminate interest-rate ceilings on bank certificates of deposit, and it formally adopted monetary targets with the intention to reduce inflation. Further developments in 1972 included the introduction of new required-reserve ratios and the introduction of targets for the growth of money over a 6-month horizon. We have already commented on the nonborrowed-reserves targeting regime spanning October 1979 to October 1982, which loosely coincides with the June 1978 to April 1982 break dates detected. The February 1986 date can be associated with the end of Volcker's chairmanship. Other dates that were not selected but related to institutional changes at the Fed include the beginning of free reserves targeting in 1966, which corresponds well to the detected May 1965 break date. The December 1989 break date coincides with the "Thanksgiving 1989 effect" described in Hamilton and Jordá (2000) and Demiralp and Jordá (2000). The August 1994 break date can be tied to the practice, instituted by the Fed in February of that year, of publicly announcing changes in the federal funds rate target after FOMC meetings.

At this juncture, we considered two alternatives. The dates detected by the structural break tests can be connected broadly to conventional monetary-regime shifts for which we have relatively accurate documented shift-dates. In the end, however, rather than imposing our own priors, we decided to maintain the break dates detected by Bai and Perron's (1998) test for $H = 5$ under the view that, in practical terms, these well-known shifts may have exerted their true effects with some lead or lag.

Estimating λ

Once we have determined the dates of monetary regime shifts, it is straightforward to compute the impulse responses of *EM* to shocks in *FF* (that is, y_t and m_t in the notation of previous sections) as well as shocks in *P*, *PCOM*, *NBRX*, and *ΔM2* for each of the $H + 1$ subsamples determined by the breaks in Table 2. Each subsample VAR has the same structure. For each subsample we compute a set of impulse-response functions such as those described in expression (27), which we then used to set up the system described by equations (29) to (31).

Estimation of this system yields an estimate of $\lambda = 0.57$ with a standard error of 0.14.[18] The estimate of λ provides a direct measure of the existence of the Lucas critique. The fact that we find breaks in the monetary policy rule confirms the results of Estrella and Fuhrer (1999). Yet to underwrite their conclusion that the Lucas critique is not quantitatively relevant, we would also have to have found that $\lambda = 1$. Instead, we found a value rather closer to the value for the mixing parameter between permanent-income and rule-of-thumb consumers in Campbell and Mankiw's (1989) test of the consumption function.

To understand what the estimate of $\lambda = 0.57$ means consider Figure 1.[19] The top panel shows the output effect of an *unanticipated* impulse to *FF* on *EM* for three values of λ. An unanticipated impulse occurs when the monetary surprise term in square brackets in the middle term on the right-hand side of equation (17) takes a non-zero value. Figure 1 plots the coefficients of the polynomial $A(L)$ that provide the dynamic response to that shock.[20] The lower curve (solid line) shows the function for the Lucasian case, $\lambda = 0$. Except for the scale factor (c_{mm0}) this is the ordinary impulse response function for the structural VAR (see equation (20)). The upper curve (gray line) shows the function for the rule-of-thumb case, $\lambda = 1$. The quantitative responses are much more moderated in this case since systematic monetary policy acts to offset the monetary surprise. Finally, the middle curve (dotted line) shows the intermediate case in which the mixing parameter takes the estimated value, $\lambda = 0.57$. Although this value is near to the midpoint of the range of possible values for λ, the quantitative

[18] Caution should be used in interpreting this standard error. Note that the regressors of the system used to estimate λ have been generated from the subsample VARs, and therefore their sampling variation should be incorporated when evaluating the precision of the estimated λ.

[19] This figure is the analogue of Cochrane's Figure 6 (1998, p. 294). Our real variable is employment; his is output.

[20] It is worth recalling that one of our identifying assumptions $A(L)$ is constant for a given λ whatever the monetary regime, but that, in general, $A(L)$ will be different for different λ's. Because the coefficients of the monetary policy rule do not enter into the dynamics reported here, Figure 1 is the same for all regimes.

responses are much closer to the rule-of-thumb case than to the Lucasian case, which corresponds closely to Cochrane's (1998) conclusion that the economy acted more like an economy with no rational agents when even a relative few followed a rule of thumb.

The lower panel of Figure 1 shows the output responses of *EM* to an *anticipated* impulse to *FF* for the three values of λ. It shows the *direct* effect only—that is, it corresponds to the first term on the right-hand side of equation (17) rather than to the impulse-response function (calculated from equation (20)) that incorporates the *indirect* feedback of monetary policy on *EM*. Each curve in the lower panel is a scaling of the corresponding curve in the upper panel by the appropriate value of λ. When $\lambda = 0$ (straight line), there is no response at all: in the Lucasian world only monetary surprises have real effects. When $\lambda = 1$ (gray line), the curve is the same as the corresponding curve in the upper panel. The two messages of the panel are as follows: first, except in the Lucasian case, anticipated policy has real effects; and, second, the direct effects of anticipated policy are uniformly more moderate than had the same policy action been unanticipated.

VI. EVALUATING SYSTEMATIC MONETARY POLICY

With estimates of equation (16) in hand, we are now in a position to quantify the effects of systematic monetary policy. We do this in three ways: first, we present a decomposition of the effects of monetary policy into systematic and unsystematic responses to shocks to monetary policy and to shocks to employment; second, we compare the estimated systematic impulse-response functions from our identified structural macroeconometric model to the counterfactual experiments that Sims and Zha (1998) suggest as measures of systematic policy; and, finally, we consider counterfactual simulations of the structural macroeconometric model in the spirit of Sims's (1999) counterfactual simulations.

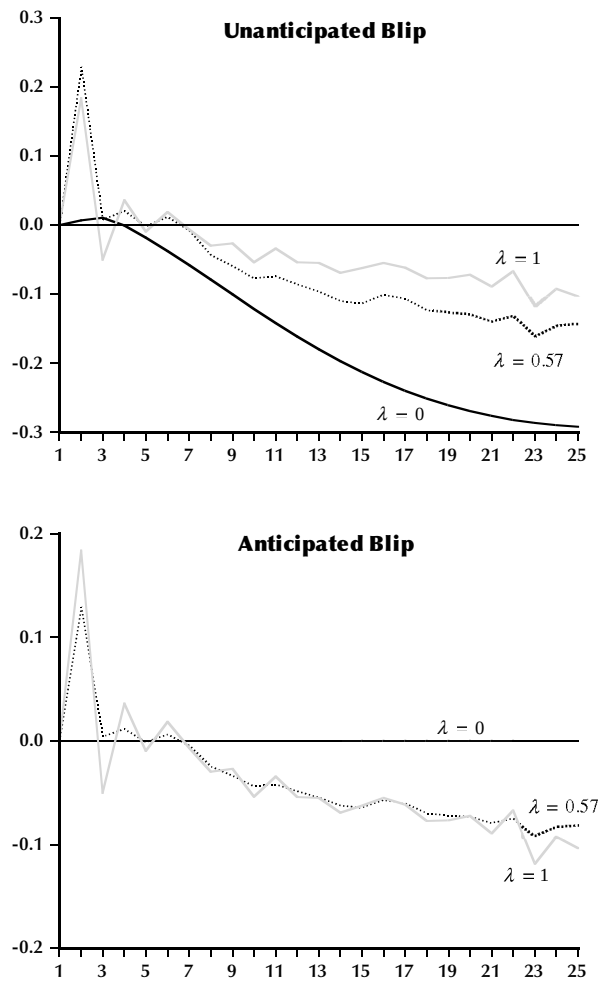
The Systematic Monetary-Policy Component of Impulse-Response Functions

For each policy regime, the top panel of Figure 2 plots $\lambda A(L)C_{mm}$, which according to equation (20) is the systematic component of the impulse-response function for *EM* to a 1 percent positive shock to the federal funds rate (ε_{mt}). Similarly, the bottom panel plots $\lambda A(L)C_{my}$, which is the systematic component of the impulse-response function of *EM* to a 1 percent negative shock EM itself (ε_{yt}).

An unanticipated positive shock to the federal funds rate can be regarded as inadvertent tightening. The top panel of Figure 2 shows that in every regime the initial effect is to lower employment. In all but the 1974-78 regime, monetary policy sustains that tightening for some time, although in three of the regimes it is neutralized or reversed within two years. However, for the 1960-70 regime, policy follows a pattern of sustained tightening so that

Figure 1

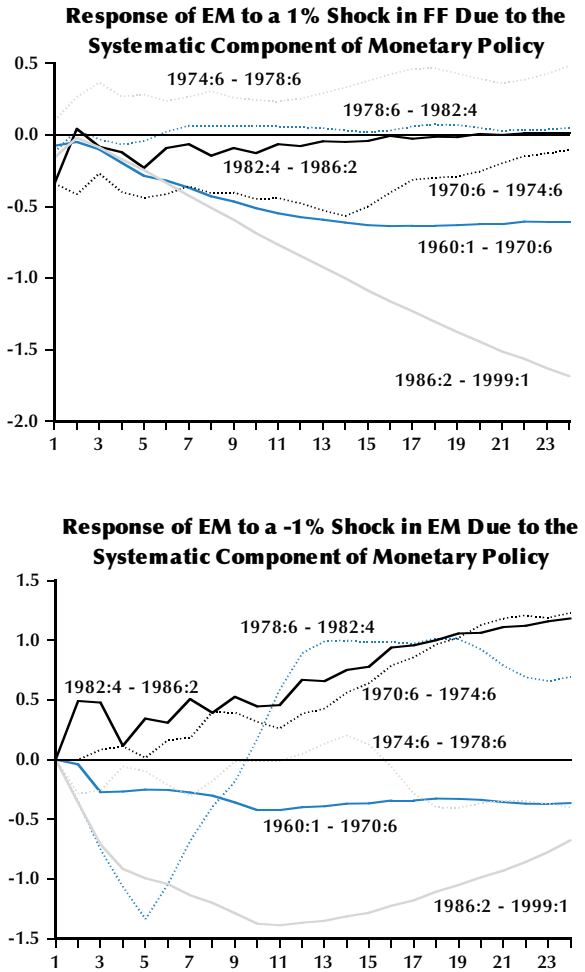
Response of Employment to the Components of a Monetary Shock



NOTE: This decomposition mirrors that in Cochrane (1998, page 294, Figure 6).

Figure 2

The Effects of Systematic Monetary Policy Across Regimes



employment is 0.5 percent lower after two years. A similar, but more substantial tightening occurs in the 1986-99 regime: a nearly 2 percent fall after two years.

The lower-panel is perhaps more interesting for policy as it shows the systematic monetary response to an unanticipated real development. A fall in employment is a surprise cooling of the economy. In two of the regimes (1970-74 and 1982-86), policy more than reverses the shock—reaching a 1 percent *increase* at the end of two years. In two of the regimes (1960-70 and 1986-91) policy accelerates the drop in employment—reaching around 0.5 percent further reduction after two years. In the remaining two regimes, the behavior is mixed. In both the 1974-76 and 1978-82 regime, the initial effect is to further reduce employment. In the 1974-78 regime the employment shock is neutralized by nine months and reversed by one year. Still, by the end of two years the net effect is about 0.5 percent further reduction. In the 1978-82 regime, there is a similar reversal by nine months, but at the end of two years employment has been raised by 0.5 percent. The behavior of the Federal Reserve does not correspond in any regime exactly to William McChesney Martin's description of the Fed as taking away the punch bowl just as the party is getting started. Faced with a dull party, the Fed, in three of the regimes, spikes the punch and, in the other three, laces it with poison. In every regime, systematic monetary policy is important.

A Comparison with Sims and Zha's Counterfactual Experiment

A number of recent papers employ counterfactual methodologies to assess the effects of systematic monetary policy. Sims and Zha (1998) modify the equation that they identify as the policy-reaction function while maintaining the rest of the equations in a structural VAR unchanged. The authors concede that this would be an objectionable procedure, given the Lucas critique, since it ignores the endogenous response of the public to the new policy regime. They argue, however, that the public would require time to learn about the new policy regime, so that the Lucas critique is unlikely to be operative over a relatively short forecast horizon. Bernanke, Gertler, and Watson (1997) refine this defense of the counterfactual procedure with the argument that the Lucas critique is more important for some markets than for others. Their counterfactual experiments are similar to those of Sims and Zha.

In practice, Sims and Zha's procedure is to replace the equation for the federal funds rate in the structural VAR with a constant. This amounts to a firmly maintained target for the federal funds rate. The rationale for ignoring the Lucas critique in this case is equivalent to the assumption that $\lambda = 1$, at least over the horizon of interest to the policymaker. Yet, there is an inconsistency since, as we demonstrated in Section IV, using the usual structural VAR to assess the effect of monetary policy is equivalent to assuming that $\lambda = 0$. In any case, the evidence of this paper is that λ is neither 1 nor 0. The Lucas critique cannot be completely

ignored, although as Figure 1 suggests it may not be quantitatively as important as when all agents have rational expectations. But neither should we assume that the economy is affected only by surprises in the monetary instrument that have real effects as the usual impulse-response functions imply.

Our argument is that the attempt to isolate the systematic effects of monetary policy through the sort of counterfactual experiment suggested by Sims and Zha (1998) is likely to be misleading. How misleading can be seen in Figure 3 in which we compare estimates for each policy regime of an impulse-response function for a negative 1 percent shock to *EM* using their methodology to calculate the systematic effect of monetary policy on *EM* reported previously in Figure 2. In every case, our estimates of the systematic effects of monetary policy are smaller in absolute value—typically period-by-period and always by the end of two years—than those of Sims and Zha. For the 1978-82 regime, even though the two estimates end up at nearly the same value, the paths are nearly mirror images of each other. For the 1986-99 regime, Sims and Zha's method suggests that a negative employment shock is met with an increasingly large, systematically countervailing policy response. In contrast, our estimates suggest a small response ratifying the shock in the midterm but offsetting it almost completely by the end of two years.

A Counterfactual Simulation

Sims (1999) conducts a counterfactual experiment in which he takes the monetary policy equation from the postwar periods and inserts it into a structural VAR for the prewar period. He then feeds the actual shocks from the prewar VAR into the new "chimera" with the intent of seeing how the economy would have fared during the Great Depression had it had the postwar monetary policy in place—would the modern Fed have done better than the Fed in fact did in the period between the two World Wars? This counterfactual is open to the same objection that we raised against Sims and Zha's other counterfactual experiment: it is an inadequate and inconsistent response to the Lucas critique. One problem is that the "structural" VAR is not structural enough to sustain the counterfactual experiment in the face of the evidence that the Lucas critique is in fact operative to some degree. And, as before, the usual impulse-response function makes sense only in the surprise-only Lucasian economy of $\lambda = 0$.

In contrast, to the degree that our identifying assumptions are correct, we can perform a legitimate counterfactual experiment. Like Sims, we consider a turbulent time, but one closer to our own time—the period at the end of the 1970s through the double-dip recessions of the early 1980s. This is the period of the Federal Reserve's nonborrowed-reserves operating regime. Our counterfactual experiment uses the shocks from the period 1978:7 to 1982:4, which is the period indicated by our break tests that essentially overlaps the period of recessions and volatile interest rates. We refer to this period as "Volcker (actual)," designating it by the Federal Reserve chairman in office through most of it. The shocks and appropriate initial conditions are fed into the structural macroeconometric models identified for the regimes 1974:6 to 1978:6 ("Burns-Miller") and 1986:2 to 1999:1 ("Greenspan")—each also designated by the corresponding Chairman of the Federal Reserve.[21]

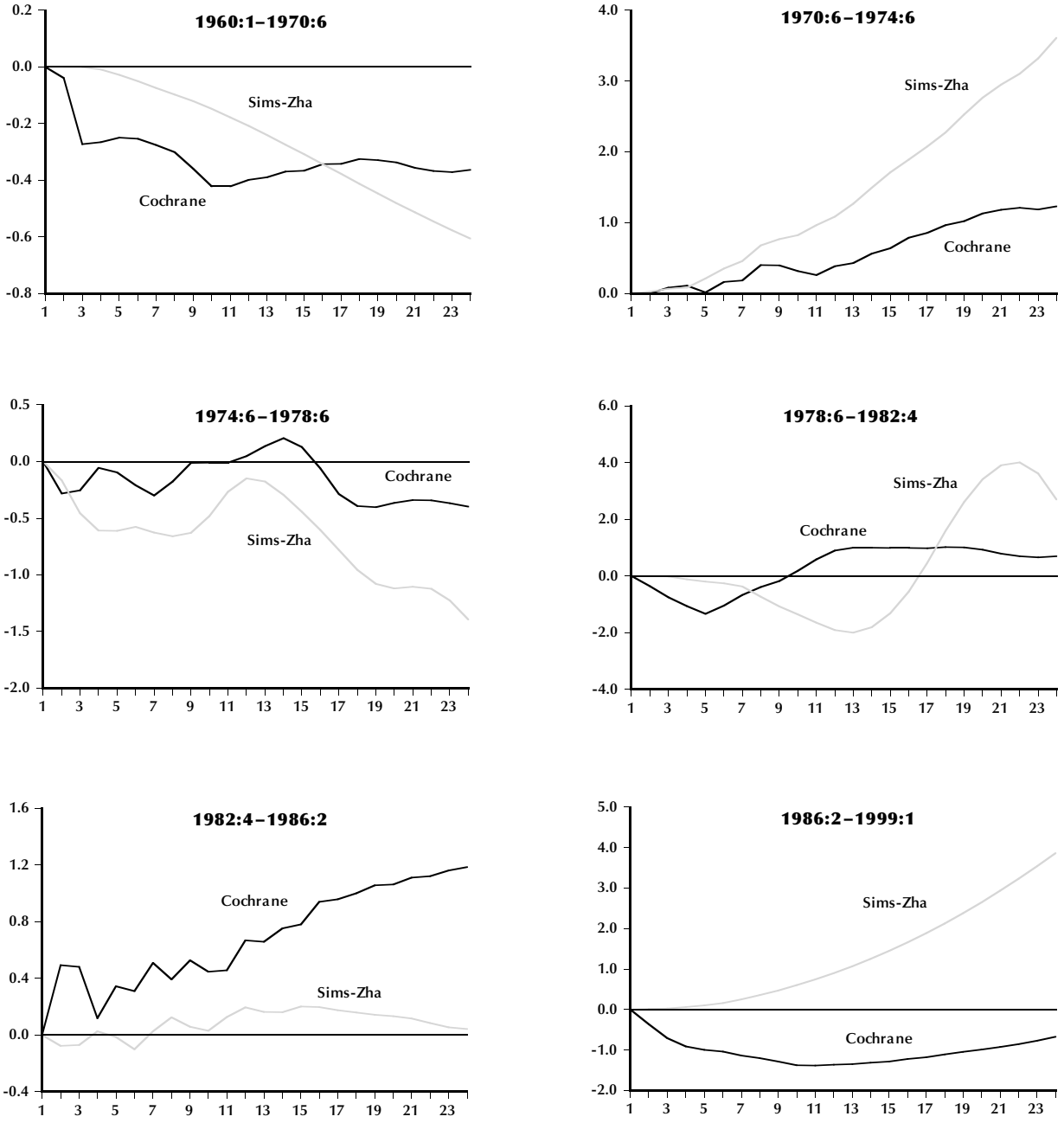
The results of the counterfactual experiment presented in Figure 4 are interesting—if not completely intuitive. The shaded area displayed on the six panels in Figure 4 corresponds to the July 1980–July 1981 recession and the original variables of the VAR are appropriately transformed to be more readily interpretable.[22] At the start of the recession, the decline in employment growth is similar under the stewardships of Greenspan and Volcker (somewhat more dramatic under the Burns-Miller scenario). However, the decline in the real federal funds rate a couple of periods into the recession is more dramatic under Volcker's chairmanship than it is under Greenspan's. This has the effect of reducing the fall in employment growth much more quickly, with employment growth attaining positive values eight months into the recession. By contrast, the real federal funds rate declines much less markedly under Greenspan (the trough of this decline is eight months into the recession) and Burns-Miller (the trough is reached five months

[21] Recall that to identify the structural macroeconometric model, $A(L)$ and $B(L)$ (and, of course, λ) are assumed to be constant across regimes. Feeding the shocks from one regime in the model for another amounts, then, only to changing the monetary rule. This is a little different from Sims's (1999) experiment. Sims assumes that the prewar and postwar periods are characterized by different real structures as well as different monetary rules.

[22] The annual growth rates of employment, M2, and the nonborrowed reserve ratio as well as inflation, and inflation in sensitive commodities, are calculated as the 12th difference of the logarithms of the levels. The real federal funds rate is calculated as the nominal rate minus the inflation rate.

Figure 3

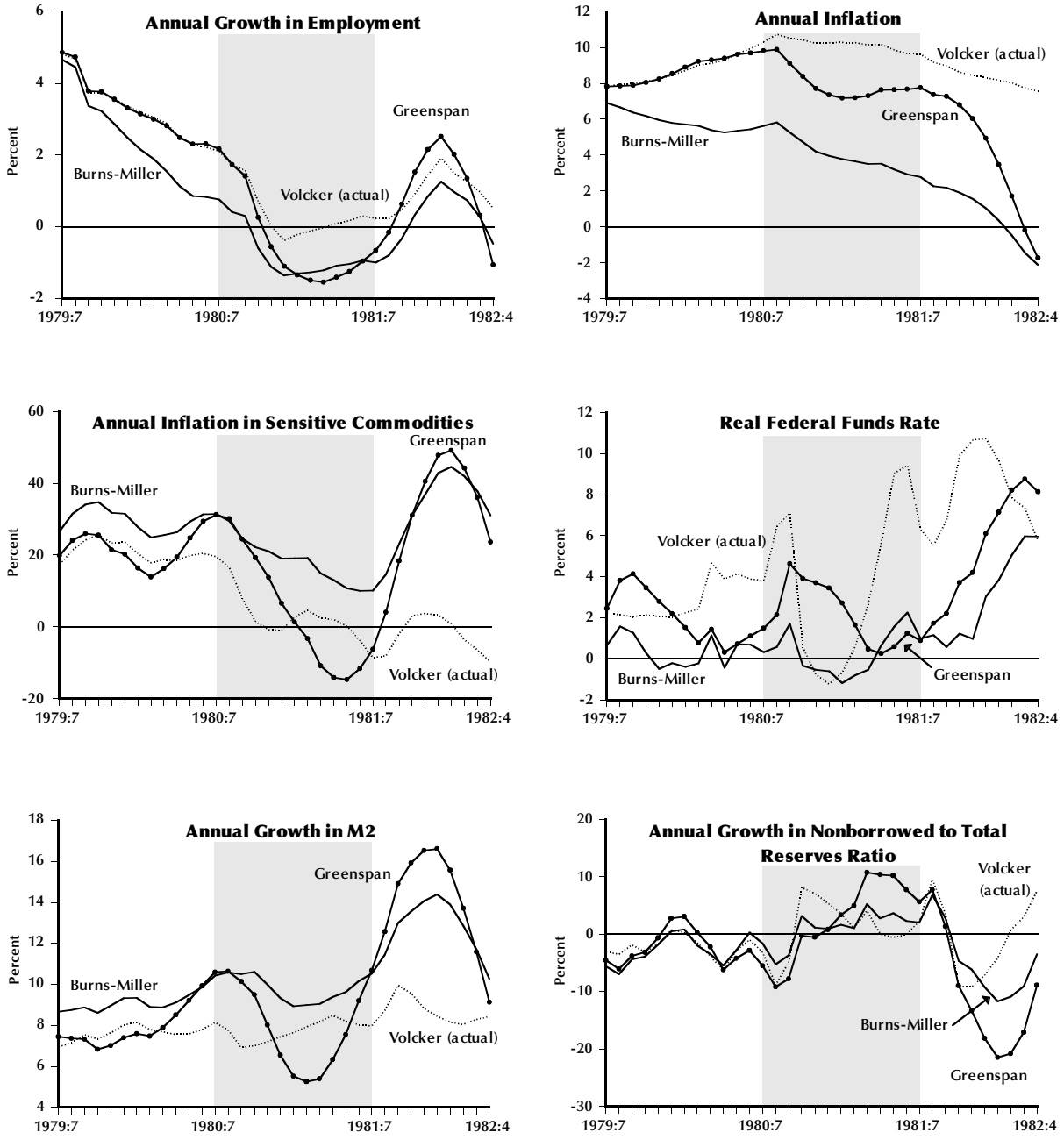
The Monetary Amplification/Mitigation of a –1% EM to Employment: A Comparison Between Sims and Zha, and Cochrane's Decompositions



NOTE: Cochrane denotes the difference between the traditional impulse response function from the VAR and the orthogonal component of the response to monetary policy obtained on the basis of equation (22). Sims and Zha denote the difference between the traditional impulse response function from the VAR and the response one would obtain if the funds variable is maintained at a constant level throughout.

Figure 4

Counterfactual Simulations
Fed Chairmen Performance During the 1979:7–1982:4 Period



NOTE: The simulations replace the FF equation of the VAR estimated over the 1978:7–1982:4 period (Volcker chairmanship), the 1974:6–1978:6 period (Burns-Miller chairmanship), and the 1986:2–1999:1 period (Greenspan chairmanship). Shaded area depicts the July 1980–July 1981 recession.

into the recession instead) and over a longer period of time. This causes a deeper and more protracted decline in employment growth, and a subsequent longer recovery relative to Volcker. Upon exiting the recession, both Greenspan and Burns-Miller aggressively ratchet up the federal funds rate but with an eight month lag relative to Volcker's actions.

The policies of both Greenspan and Burns-Miller would have held the federal funds rate at a substantially lower level than was actually the case under Volcker. More important, perhaps, is that both Greenspan and Burns-Miller would have delivered a much less volatile federal funds rate during the recession than Volcker actually did. As a result, they would have had substantially more difficulty in hitting the M2 monetary targets that were one of the guides to monetary policy at the time, even though there are few appreciable differences in the behavior of the ratio of nonborrowed reserves to total reserves.

Perhaps the most unintuitive result of this experiment has to do with the behavior of inflation and inflation in sensitive materials. In particular, a rate of inflation for consumer goods higher under Volcker than under the others is consistent with a relatively quicker recovery from the recessions. What is puzzling is why the rate of inflation for sensitive materials should remain low under the Volcker regime but not under the others while general inflation accelerated.

The real outcomes are mildly surprising since Paul Volcker was widely regarded as running a tighter monetary policy than had his predecessors, Arthur Burns and William Miller. The Volcker regime exhibits greater employment than would have been the case under either the Burns-Miller or the Greenspan regimes. The correlative consequences for inflation—consistent with the Phillips curve—would have been lower prices in the two regimes with lower employment and indeed would have turned into disinflation by the end of the period.

The difference between the configurations of employment, inflation, and interest rates produced by Volcker's policy compared with the alternatives is consistent with the Federal Reserve having adjusted the federal funds rate in a manner that adapted to the higher inflation—in effect building higher expectations of inflation into policy. What remains difficult to understand, however, is why the prices of sensitive commodities would rise substantially under the apparently tighter regimes of Greenspan and Burns-Miller.

VII. CONCLUSION

Our goal was not only to understand the nature and significance of systematic monetary policy but also to make some sense of the voluminous literature based in structural VARs. The great tension in the empirical analysis of monetary policy is between the need for a structural account that can support the kind of counterfactual analysis needed for policy analysis and the need for modesty on the part of macroeconometricians in their claims for empirical warrant (or even credibility) for the assumptions used to identify that structure. Lucas's original program set a high standard for the requisite structural detail. Sims's original VAR program advised a high degree of modesty. Macroeconomics has tried to steer between their competing claims ever since. Cochrane (1998) was a warning to the structural VAR camp to steer toward the Lucas light. What we hope to have achieved in this paper is to demonstrate that there is deeper water in that direction.

The results of this study are preliminary. We have followed other recent studies in adopting a particular contemporaneous causal ordering of the structural VAR. But this is a highly contested area. Different causal structures could significantly affect our results. Similarly, there remain questions about the identification of monetary policy regimes. And there are questions not really addressed here at all of how to characterize monetary policy. We assumed that the federal funds rate was the policy instrument. It would be worth exploring whether a larger block of monetary-policy variables could be analyzed in a similar manner. Finally, there are, of course, many counterfactual experiments that we could address beyond the particular case of the early 1980s. What we have accomplished—even in this preliminary form—is to show that there is an empirically workable template on which a more refined and comprehensive study can be based.

Although preliminary, our results are interesting. First, the economy is best characterized as being composed of a mixture of agents, some of whom operate according to the new classical paradigm (rational expectations, short-run neutrality of money) and some of whom appear to follow rules of thumb. The estimate of $\lambda = 0.57$ could be read as saying Lucas was 43 percent right in his early new-classical model of the macroeconomy. Second, even with only half the economy in the rule-of-thumb camp, the economy behaves quantitatively and qualitatively substantially as if Lucas had been

wrong altogether about the unimportance of systematic and anticipated monetary policies. Third, Lucas is correct, nonetheless, that the aggregate reactions of the economy are conditioned on policy regimes and the analysis of what happens when a regime changes—in practice as well as in theory—requires some structural knowledge. The key assumption of this paper is that the coarse structural knowledge suggested in Cochrane's decomposition of the effects of monetary policy into anticipated and unanticipated components is sufficient—and very likely the best that we can practically accomplish—to reach substantive results.

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Commentary

Valerie A. Ramey

In their paper, Hoover and Jordá have tackled a very difficult task: the analysis of the effect of systematic monetary policy. As the paper's critical review of the literature outlines, during the last several decades the literature has focused almost exclusively on the effects of unanticipated monetary shocks rather than on the effects of systematic monetary policy. Hoover and Jordá argue that this exclusive focus should be altered, that monetary analysis should also study the effects of systematic monetary policy.

The second half of the paper offers new methods and results for the analysis of monetary effects. Hoover and Jordá propose a new way to identify the relative effects of anticipated versus unanticipated monetary policy in the context of Cochrane's (1998) mixed model. They then use the estimates to analyze the implications for systematic monetary policy.

This paper deals with many subtle issues of identifying monetary policy. Because an understanding of the subtleties is essential for appreciating the contribution of this paper, I will first review the identification issues involved. I will then expand on Hoover and Jordá's arguments for why we should study the effects of systematic monetary policy. Focusing on the second half of their paper, I will analyze Hoover and Jordá's new procedure and then finish by assessing its strengths and weaknesses.

IDENTIFYING THE SEPARATE EFFECTS OF ANTICIPATED VERSES UNANTICIPATED MONETARY POLICY

Sargent (1976) first pointed out that without additional information, one cannot separately identify the effects of anticipated versus unanticipated monetary policy. The problem is that many different theories of the effects of monetary policy are consistent with the history of monetary policy, as captured by a vector autoregression (VAR). Thus, without further identifying assumptions, a VAR can only reveal the effect of a monetary shock *if it is followed by the usual policy actions*.

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It is useful to consider two polar cases and an intermediate case:

- **Polar Case 1: Only unanticipated money matters.**
  This is the case usually assumed (implicitly) in the VAR literature. If only unanticipated money matters, the impulse response function (IRF) reveals the structural coefficients of the model. Furthermore, the impulse response function is invariant to changes in monetary regimes. Why is this the case? Because none of the propagation of a monetary shock to output operates through the subsequent effect on anticipated money, the effects do not depend on the coefficients of the monetary feedback rule.
- **Polar Case 2: All money matters equally.**
  In this case, the coefficients in the regression of output, y, on money, m, and its lags are invariant to changes in the policy rule. This result occurs because a unit increase in money always has a certain effect on output, regardless of whether the increase was caused by a monetary shock or a systematic increase in money. The impulse response function, however, is not invariant to changes in the policy rule. If policy changes so that movements in money become more persistent, the response of output will be different.
- **Intermediate Case: Anticipated and unanticipated money matter differently.**
  This middle case is the most difficult case to analyze because nothing from the VAR is invariant to policy. Without further identifying information, it is hopeless to try to distinguish the two types of effects.

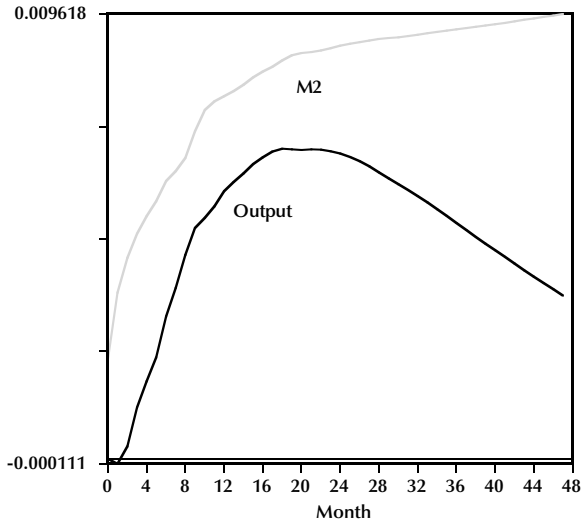
WHY STUDY THE EFFECTS OF SYSTEMATIC MONETARY POLICY?

Until recently, most researchers were content to examine only the effects of unanticipated monetary shocks because they assumed that systematic monetary policy does not matter. As pointed out above, impulse response functions from the VAR are the natural method for analyzing these effects because they are invariant to changes in policy regimes.

Why have Hoover and Jordá, as well as a few papers in the recent literature, decided to tackle the very difficult problem of analyzing systematic policy? I would argue that there are four key reasons.

Figure 1

Response of Output and M2 to an M2 Shock



The first reason is the realization in the last decade that shocks to monetary policy do not explain much of the variance of output (e.g., Cochrane, 1994). Many researchers have strong prior beliefs that money matters a lot. In order to reconcile that belief with the small role of shocks, one must naturally turn to systematic monetary policy as a candidate.

The second reason is the realization of the potential role of systematic monetary policy as a propagation mechanism. By ignoring the possible systematic effects of monetary policy we may be inappropriately attributing its effects to other variables. For example, Bernanke, Gertler, and Watson (1997) argue that many of the observed effects of oil on the economy may be due to the endogenous response of monetary policy to oil shocks.

The third reason that the literature has become more open to the idea that systematic monetary policy is important is the great amount of recent research on the importance of monetary policy rules. If systematic monetary policy has no effects, then there is little point in debating the merits of various rules, except for the central bank's role in price stabilization.

Finally, as Cochrane (1998) points out, if only unanticipated monetary policy matters, then "the striking similarity of the output and monetary responses is a pure and unlikely coincidence" (p. 295). Figure 1 shows the response of the log of industrial production and the log of M2 to a shock to M2.[1] Output and money follow very similar paths in the first few years after a shock. If we believe that only unanticipated money matters, then we should not expect such similarities in the path. The similarities displayed offer incriminating evidence that part of the impact of money on output is propagated through the endogenous response of money.

HOOVER-JORDÁ IDENTIFICATION METHOD

Hoover and Jordá offer a very clever idea for distinguishing the effects of anticipated versus unanticipated money. They begin with Cochrane's hybrid model, which is a "structural" model that allows anticipated money to matter. The hybrid equation is as follows:

$$y_t = \lambda A(L)\, E(m_t | \Omega_{t-1}) + A(L)[m_t - E(m_t | \Omega_{t-1})] + \boldsymbol{B}(L)\varepsilon_{wt}\,,$$

where ε_{wt} is a vector of orthogonalized nonmonetary innovations, which includes the output innovation. The first term captures the effect of anticipated money and the second term captures the effect of unanticipated money. If $\lambda = 0$, then only unanticipated money matters. If $\lambda = 1$, there is no distinction between anticipated and unanticipated money. If λ is between 0 and unity, then unanticipated and anticipated money matter, but they matter differently.

Cochrane gives no economic interpretation of λ, but Hoover and Jordá suggest one. They suggest that λ be interpreted as the fraction of nonrational agents in the economy, much as Campbell and Mankiw (1989) interpreted a comparable coefficient in their study of consumption behavior. I will discuss the plausibility of this interpretation below.

The identification problem is best seen by considering the relationship between the structural parameters λ and the parameters of $A(L)$ on the one hand and the coefficients from the reduced-form moving-average representation (MAR) of the VAR on the other. The MAR is given by equations (18) and (19) of the paper, which are shown here:

[1] The impulse response functions are computed from a VAR with industrial production, the consumer price deflator, and M2, all in logarithms. The VAR has 12 lags and is estimated over the period 1960:1–1999:12.

(18) $$y_t = C_{ym}(L)\varepsilon_{mt} + \boldsymbol{C}_{yw}(L)\varepsilon_{wt}$$

and

(19) $$m_t = C_{mm}(L)\varepsilon_{mt} + \boldsymbol{C}_{mw}(L)\varepsilon_{wt}\,.$$

The relationship between the structural coefficients and the MAR coefficients is given by equation (26) of the paper. Here is the first set of equations in equation (26):

(26) $$\begin{aligned} c_{ym0} &= a_0 c_{mm0} \\ c_{ym1} &= a_0 \lambda c_{mm1} + a_1 c_{mm0} \\ &\vdots \\ c_{ymk} &= a_0 \lambda c_{mmk} + \ldots + a_{k-1}\lambda c_{mm1} + a_k c_{mm0} \\ &\vdots \end{aligned}$$

(The rest of the equations in equation (26) involve the coefficients such as c_{yy0}, c_{yw0}, etc. and the b's from the polynomial $\boldsymbol{B}(L)$.) Consider the algebra behind the identification problem. The c coefficients are estimated from the MAR of the VAR and thus can be treated as "known" for the purposes of identification. Given the c's, can we back out the λ and a's? No. Inspection of the equations shows that the number of equations is greater than the number of unknowns: there are $k+1$ equations in $k+2$ unknowns. Adding the remaining equations in the system does not solve the problem.

Because the system is not identified without further assumptions, Cochrane only analyzes what the effects of monetary policy would be if λ took on various values. Hoover and Jordá, in contrast, tackle the identification problem head-on.

Hoover and Jordá solve the identification problem by making assumptions that allow them to convert this system of algebraic equations into a system of regressions. Rather than solve algebraically for the λ, a's, and b's, they instead figure a way to *estimate* them. The required assumptions are as follows:

1. The monetary policy regime switches at least once during the sample, so there are at least two sets of MAR coefficients (the c's).
2. λ is invariant to switches in monetary policy regimes.
3. $A(L)$ and $\boldsymbol{B}(L)$ are invariant to switches in monetary policy regime.
4. Anticipated and unanticipated shocks are propagated by the same mechanism: $A(L)$.

To see how these assumptions solve the identification problem, rewrite the equations in system (26) as a system of *regressions* rather than algebraic equations:

$$\begin{aligned} c_{ym0,t} &= a_0 c_{mm0,t} + \mu_{0,t} \\ c_{ym1,t} &= a_0 \lambda c_{mm1,t} + a_1 c_{mm0,t} + \mu_{1,t} \\ &\vdots \\ c_{ymk,t} &= a_0 \lambda c_{mmk,t} + \ldots + a_{k-1}\lambda c_{mm1,t} + a_k c_{mm0,t} + \mu_{k,t} \\ &\vdots \end{aligned}$$

I have added t subscripts on the c coefficients because now they are treated as variables (with at least two observations). I have also added error terms, μ, which I will discuss shortly.

Now, identification becomes much easier. In fact, the system is recursive. The first equation regresses $c_{ym0,t}$ on $c_{mm0,t}$ and produces a regression coefficient estimate for a_0. With an estimate of a_0 from the first equation, one can then use the regression given in the second equation to estimate λ and a_1. One can estimate each of the a's recursively. In short, Hoover and Jordá identify the structural parameters by turning a system of algebraic equations into a system of regressions.

To implement the idea, Hoover and Jordá use the following procedure. They begin with the Christiano, Eichenbaum, and Evans (1996) VAR to describe monetary policy and its effect on the economy. Hoover and Jordá use Bai-Perron tests for structural breaks on the monetary reactions function (the federal funds rate [FFR] equation in a Christiano et al. system). The test statistics favor two breaks, but they choose five breaks since the test statistic is almost as high and it gives more observations for each c in the system above. In the next step, they estimate the entire VAR separately on each regime, taking regime breaks as given. This step yields six sets of estimates of the MAR coefficients.

They then treat each MAR coefficient as a variable with six observations to create the set of regressions outlined above. They use a minimum-distance estimator to estimate λ and the parameters of $A(L)$ and $\boldsymbol{B}(L)$, imposing the nonlinear cross-equation restrictions.

This procedure yields several surprising results. First, λ is estimated to be 0.57 with a standard error of 0.14! This estimate of λ is eerily similar to Campbell and Mankiw's (1989) estimate of the fraction of rule-of-thumb consumers. Are we to believe that Hoover and Jordá's nonrational agents and Campbell and Mankiw's rule-of-thumb consumers are one and the same?

A second result, which is not so surprising, is that the response of money to a shock depends significantly on the value of λ. This result confirms what Cochrane found when he analyzed monetary policy under different assumptions about λ. The response of output to an unanticipated shock becomes smaller as λ rises because we attribute more and more of the subsequent response of output to lagged anticipated money, which also changes in response to the initial unanticipated shock.

Perhaps the most surprising result is the analysis of monetary policy over the various regimes. Hoover and Jordá find a wide variety of responses of monetary policy to real shocks over the different regimes. Among the more curious results found are the estimates that imply that Volcker was much more of an inflation dove than Burns-Miller and Greenspan. These results are difficult to interpret.

CRITIQUE

While I think that the Hoover and Jordá identification method is a clever device that can probably be used in a variety of contexts, I think that there are several weaknesses of this particular implementation. These weaknesses lead me to question the robustness of the empirical results.

My first concern is that the authors are applying a very clever idea to a very shaky foundation. I am uncomfortable with their interpretation of the Cochrane hybrid model as a model with a certain fraction of nonrational agents. Abandoning Lucas's idea that only unanticipated money matters does not mean we must abandon rigorous methodology as well. We should be serious about deviations from rational expectations. There is an important area of literature that studies learning and adaptive behavior. Some of the more realistic models are those with agents that have bounded rationality but which display active cognition.

In a serious model of bounded rationality, we would expect λ to change when the monetary regime changes. In particular, we might expect λ (the fraction of nonrational agents) to rise immediately after the shift in a monetary regime because it is difficult to form rational expectations when the structure of the economy has suddenly shifted.

I am also concerned about the plausibility of some of the other identifying assumptions. One key assumption is that the lag structure coefficients for both anticipated and unanticipated money are the same, even though they are assumed to have different effects on the economy. Why would we expect them to have the same lag structure in the propagation mechanism? In short, a better structural model would help us assess the reasonableness of the identifying assumptions.

Identifying assumptions aside, there are other aspects of the implementation that cast some doubt on the precision of the results. First, as acknowledged by Hoover and Jordá, the true standard error on λ is probably much higher than 0.14. That standard error is based on an estimation method that assumes that the c's are known, rather than estimated. In fact, they are estimated. Moreover, some of the sets of c's are estimated over sample periods with very few observations. For example, for the monetary regime from 1978:6 to 1982:3, they use 46 observations to estimate 25 VAR parameters. It's no wonder that monetary policy looks so different across regimes: the sampling error must be very large. A related concern is the source of the error terms introduced into the system when it is converted from an algebraic system to a regression system. The authors talk about "computational errors," but can we be certain that these errors have the properties required for consistency of the estimates?

Another reason we might not have much confidence in the results is that there are no tests of over-identifying restrictions. It seems it would be preferable to incorporate the notion of regime changes directly in the structural model in order to derive the implications. One could then estimate the six sets of MAR coefficients jointly with λ, $A(L)$, and $\boldsymbol{B}(L)$, using the theoretical restrictions.

A final quibble is the point that finding five breaks using the Bai-Perron test does not imply six different regimes. In principle, monetary policy could be switching back and forth between two distinct regimes.

Given these potential concerns, it seems that it would be good for the authors to test a more straightforward implication of their model—one that does not require so many identifying restrictions. As discussed above, if λ is equal to unity, meaning that there is no distinction between anticipated and unanticipated money, the coefficients on money in the output equation are invariant to regime shifts. The advantage of setting λ equal to unity as the null hypothesis is that in this case it is natural to believe that the $A(L)$ coefficients are the same across anticipated and unanticipated money.

Thus, as a first pass, one could use the following

Table 1

Results of Chow Tests (*p* values)

Regimes compared	Money equation	Output equation
1 versus 2	0.000	0.470
2 versus 3	0.003	0.377

procedure. First, test the set of coefficients in the money equation for stability. If stability is rejected, then there is evidence of monetary regime shifts. Conditional on the regime switch, a test of the stability of the coefficients on money in the output equation is a test of λ equal to unity.

In order to illustrate this procedure, I estimated a VAR with the log of employment, log of M2, and log of the consumption deflator. The VAR contains seven lags. I use two breaks, which implies three regimes, because this is the result with the highest sup F in the Bai-Perron test. The first regime extends from 1960:1 to 1978:5, the second from 1978:6 to 1982:3, and the third from 1982:4 to 1999:1.

The results of Chow tests on the coefficients in the money equation versus the output equation are given in Table 1. Table 1 shows that the Chow test supports the Bai-Perron test of a structural break in the monetary policy reaction function. In contrast, one cannot reject stability of the coefficients on money in the output equation. These results suggest that the data are not at odds with the assumption that anticipated and unanticipated money matter equally. Of course, these results need to be tested in more complicated systems, but, if they hold up, they have important implications for the analysis of monetary policy.

CONCLUSIONS

Hoover and Jordá set their sights on an ambitious goal when they decided to analyze the effects of systematic monetary policy. In trying to analyze this problem, they have introduced a clever new identification technique. While I have raised concerns about several aspects of the implementation, the paper nonetheless makes two important contributions. First, it provides further support for the importance of shifting the research agenda to analyzing systematic monetary policy. Second, the potential contribution of their new technique is not limited to this particular implementation.

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Monetary Policy Analysis in Models Without Money

Bennett T. McCallum

It has recently become common practice—indeed, virtually standard practice—for monetary policy analysis to be conducted in models that include no reference to any monetary aggregate.[1] Although there have been a few protests,[2] this general tendency is true of research conducted by both central bank and academic economists.[3] The purpose of the present paper is to consider whether there is anything fundamentally misguided about this practice.

The paper begins by specifying a small prototype model that reflects today's standard approach and asking whether its adoption implies that monetary policy has little or nothing to do with money. The answer developed here is "no." Next it is argued that the prototype model excludes monetary aggregates only because of an assumption concerning the monetary transactions technology that seems unjustifiable in principle. We go on to consider whether elimination of this assumption, and the implied inclusion of a monetary aggregate, would be of quantitative importance in business cycle analysis. Again the apparent answer is "no."

The paper's third main topic involves *indeterminacy* issues that have been prominent in recent discussions of models and interest-rate policy rules of the type under consideration. Here it is argued that the undesirability of basing policy on forecasts of future inflation rates has been overstated in the theoretical literature, which has emphasized nonfundamental solutions that may be irrelevant empirically. As a related matter, the paper takes up the type of indeterminacy implied by a rule that does not respect the Taylor principle (i.e., that interest rates should be made to increase by more than point-for-point with inflation). It is argued that the nature of the problem in this case is different and of genuine importance. Finally, it is noted that although these answers suggest that policy analysis in models without money is not fundamentally misguided, they do not imply that conducting policy in this manner is necessarily a desirable strategy.

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ARE MODELS WITHOUT MONEY NON-MONETARY MODELS?

Let us begin by writing down a simple schematic prototype model without money of the sort that has been popular recently. Here and in the rest of the paper we use y_t to denote the logarithm of real output during period t with $\bar{y}_t$ being the natural-rate (i.e., flexible price) value of y_t so that $\tilde{y}_t = y_t - \bar{y}_t$ is the output gap. Also p_t is the log of the price level so that Δp_t is the inflation rate, while g_t represents the log of real government purchases and R_t is the short-term nominal interest rate that the central bank uses as its instrument. Then the prototype model is:

(1)
$$y_t = b_o + b_1\left(R_t - E_t\Delta p_{t+1}\right) + E_t y_{t+1} + b_2\left(g_t - E_t g_{t+1}\right) + v_t$$
$$b_1 < 0,\ b_2 > 0$$

(2)
$$\Delta p_t = \beta E_t \Delta p_{t+1} + \alpha\left(y_t - \bar{y}_t\right) + u_t$$
$$0 < \beta < 1,\ \alpha > 1$$

(3)
$$R_t = \mu_0 + E_t\Delta p_{t+1} + \mu_1\left(E_t\Delta p_{t+j} - \pi^*\right) + \mu_2\left(y_t - \bar{y}_t\right) + e_t.$$
$$\mu_1,\ \mu_2 > 0$$

Here, equation (1) represents a forward-looking expectational IS function of the type that can be justified by dynamic optimization analysis, as explained by Kerr and King (1996), McCallum and Nelson (1999b), Woodford (1995, 1999), and many others. The stochastic disturbance v_t represents the effects of taste shocks and is assumed to be exogenous, as are the "cost push" and policy shocks u_t and e_t. Relation (2) is a price adjustment specifica-

[1] For some prominent examples, see Clarida, Gali, and Gertler (1999), Rotemberg and Woodford (1997), and most of the papers in Taylor (1999).

[2] For example, Meltzer (1999) and Nelson (2000).

[3] The similarity of research strategies being used by central bank and academic economists can be seen by perusal of Taylor (1999) and the June 1999 issue of the *Journal of Monetary Economics*, which include papers from two major conferences on the topic of monetary policy rules.

tion of the Calvo-Rotemberg type, which is the most nearly standard of those currently in use.[4] For present purposes it will suffice to treat $\bar{y}_t$ as exogenous, as is usually done in small models, although Casares and McCallum (2000) show that it is not too difficult to endogenize investment and therefore $\bar{y}_t$.[5]

Finally, equation (3) represents a policy rule of the Taylor (1993) type, which has the effect of adjusting upward the real interest rate $R_t - E_t\Delta p_{t+1}$ when current or expected inflation (depending on the value of j) exceeds its *target* value π^* and/or the output gap is positive. For best performance the central bank will choose the parameter μ_0 to equal $\bar{r}$, the long-run average real rate of interest, which in turn will equal $-b_0/b_1$, presuming that $E(g_t - E_t g_{t+1}) = 0$ (i.e., that we are abstracting from growth in $\bar{y}_t$ and g_t). Often an additional term in R_{t-1} is included in the policy rule to represent interest rate smoothing.

Clearly, the system given by equations (1) through (3) includes no monetary aggregates. Yet it is complete, in the sense that the three relations govern time paths for the three endogenous variables, y_t, Δp_t, and R_t. It would be possible to add to the system a (base) money demand relation such as

$$(4)\quad \log M_t - p_t = c_0 + c_1 y_1 + c_2 R_t + \eta_t, \qquad c_1 > 0,\ c_2 < 0$$

but the latter would be superfluous, in the sense that it would not affect the behavior of y_t, Δp_t, or R_t.[6] Its only function would be to specify the amount of base money that is needed to implement the policy rule (3). Thus policy analysis involving y_t, $\bar{y}_t$, Δp_t, and R_t can be carried out without even specifying a money demand function such as equation (4) or collecting measurements on the stock of money, M_t.

Nevertheless, it would be wrong to view this system without any monetary aggregate, equations (1) through (3), as representing a non-monetary model. For 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. If some entity other than the central bank could exogenously manipulate the path of M_t, then (4), (1), and (2) would determine paths for y_t, Δp_t, and R_t with (3) being overruled.

Of course, the preceding statement presumes institutional arrangements much like those in existence today, in which it is appropriate to assume that the central bank has (monopoly) control over the issue of base money. Some writers, such as Friedman (1999), have suggested that technological progress in the payments and information processing industries may critically alter this situation in coming years and leave central banks with no control over the short-term interest rates that matter for macroeconomic purposes. Most of the Summer 2000 special issue of *International Finance* is devoted to a discussion of that possibility. In my opinion, Goodhart (2000) is correct to argue that, because central banks can be supported by governmental powers of regulation and taxation, they are unlikely to lose their control over interest rates in the foreseeable future.

In any event, there is an additional way to express the basic point at issue. This is to ask whether, according to the model (1) through (3), inflation is viewed as a non-monetary phenomenon, governed by the Phillips curve relationship (equation (2)). It is my contention that such a suggestion would be unjustified. Specifically, the primary issue in this regard is what controls the long-run average rate of inflation. And in the prototype model, that rate is controlled entirely by the central bank—the monetary authority. For this argument let us assume that the long run average value of $\tilde{y}_t$ is zero, i.e., that $E(y_t - \bar{y}_t) = 0$ as is implied by the strict version of the natural rate hypothesis.[7] Then from equation (1) we have that, in the absence of growth, $E(R_t - \Delta p_{t+1}) = -b_0/b_1$ so equation (3) implies that

$$(5)\qquad -b_0/b_1 = \mu_0 + \mu_1\left(E\Delta p_{t+j} - \pi^*\right) + \alpha E\tilde{y}_t.$$

Thus it follows that, with $E\tilde{y} = 0$,

$$(6)\qquad E\Delta p_t = \pi^* - \left(\left(b_0 / b_1\right) + \mu_0\right)\left(1/\mu_1\right).$$

Consequently, if the central bank sets μ_0 equal to the average real interest rate, $-b_0/b_1$, as a sensible central bank would, then the average inflation rate will equal the central bank's chosen target value π^*. And if it errs by, for instance, ε in setting μ_0, the average inflation rate will differ from π^* by ε/μ_1, which becomes smaller as the policy rule's response to

[4] There has recently been much controversy over the adequacy of the Calvo-Rotemberg specification since it itself supplies no persistence to the inflation rate; see Estrella and Fuhrer (2000) and Gali and Gertler (1999) for an introduction to the controversy.

[5] This, of course, requires that equation (1) be replaced by a set of equations representing an "expectational IS sector."

[6] Here M_t is the nominal money stock. For present purposes we can think of it both as base money and as the relevant monetary aggregate that facilitates transactions.

[7] This version is due to Lucas (1972). For more discussion, see McCallum and Nelson (1999a).

discrepancies of inflation from its target becomes stronger. Basically, then, the average inflation rate is determined by central bank behavior. Parameters α and β from the Phillips relationship (equation (2)) play no role.

There is an apparent problem with the foregoing demonstration, namely, that the Calvo-Rotemberg relation (2) becomes $E\pi = \beta E\pi + \alpha E\tilde{y}$ in the steady state, which implies (with $\beta < 1$) that $E\tilde{y} > 0$. The latter condition, however, violates the natural rate hypothesis, in contradiction with the assumption made above. In my opinion this points to a flaw in the usual formulation of the Calvo-Rotemberg model.[8] Instead of a derivation pertaining to the costs or impossibility of changing prices in relation to the previous period's level, a rational version of the model would be concerned with changes relative to the previous period's level plus the average one-period inflation rate, as in the version of the model used by Ireland (2000). In that case, there would be no problem with the foregoing argument. Furthermore, although the inflation rate would depend upon Phillips curve parameters if one were to insist on retaining (2), the same would be true if the central bank were to control the monetary base as its instrument variable, assuming that the policy feedback rule was one that involved both $\pi - \pi^*$ and $\tilde{y}$ as objectives.

IS NEGLECT OF MONEY QUANTITATIVELY IMPORTANT?

The objective of this section is to look into the theoretical foundations for the prototype model (equations (1) through (3)) and follow up with a quantitative analysis.[9] Actually, the focus will be only on equations (1) and (4) because the policy rule (3) is simply being taken as an object of investigation, whereas issues relating to the price adjustment specification (2) are quite distinct and beyond the scope of this paper.[10] Thus we begin by reviewing the optimizing rationale for (1) and (4), which is reasonably familiar from the references mentioned on p. 1.

For simplicity, suppose that capital is treated as a constant, k. Then a typical household, which supplies inelastically one unit of labor per period, seeks at time $t = 1$ to maximize

$$\sum_{t=1}^{\infty} \beta^{t-1} u(c_t, \zeta_t),$$

where c_t represents Dixit-Stiglitz consumption bundles and ζ_t is a stochastic shock to preferences. The household's budget constraints for $t = 1,2,\ldots$ are

$$(7)\quad \begin{aligned} &Y_t^A\left(P_t / P_t^A\right)^{1-\theta} - tx_t - w_t(n_t - 1) = \\ &c_t + m_t - (1+\pi_t)^{-1} m_{t-1} + (1+r_t)^{-1} b_{t+1} - b_t + \psi(c_t, m_t). \end{aligned}$$

Here Y_t^A is aggregate per-household demand while P_t/P_t^A is the price of the household's specialized output relative to the implied Dixit-Stiglitz price index of goods in general. Also, tx_t is lump-sum taxes paid (net of transfers); n_t is labor employed in production so with a real wage of w_t we have $w_t(n_t - 1)$ as the household's net payment to hired labor; m_t is real money balances held at the end of period t; $\pi_t = (P_t^A/P_{t-1}^A) - 1$ is the inflation rate; and b_{t+1} is the number of real bonds purchased, at a real price of $1/(1 + r_t)$, during t. Finally, $\psi(c_t, m_t)$ represents the resources used in conducting transactions (i.e., in shopping for the precise bundle of consumption goods that the household chooses). The household produces output subject to the production function $Y_t = f(A_t n_t, k)$, where A_t is a technology shock, and its amount produced is equal to the quantity demanded:

$$(8)\quad f(A_t n_t, k) = Y_t^A\left(P_t / P_t^A\right)^{-\theta}.$$

As is common, we assume $0 < \beta < 1$, $\theta > 1$, and that f is well behaved. The transaction technology is such that $\psi_1(c_t, m_t) > 0$ and $\psi_2(c_t, m_t) \leq 0$.

In this setup the household's first order conditions are as follows for $t = 1,2,\ldots$, with λ_t and ξ_t being the Lagrange multipliers on (7) and (8):

$$(9)\quad u_1(c_t, \zeta_t) - \lambda_t\left[1 + \psi_1(c_t, m_t)\right] = 0$$

$$(10)\quad -\lambda_t w_t + \xi_t A_t f_1(A_t n_t, k) = 0$$

$$(11)\quad -\lambda_t\left[1 + \psi_2(c_t, m_t)\right] + \beta E_t \lambda_{t+1}(1 + \pi_{t+1})^{-1} = 0$$

$$(12)\quad -\lambda_t(1 + r_t)^{-1} + \beta E_t \lambda_{t+1} = 0$$

$$(13)\quad \lambda_t Y_t^A(1-\theta) P_t^{-\theta} / \left(P_t^A\right)^{1-\theta} + \xi_t Y_t^A \theta P_t^{-(\theta+1)} / \left(P_t^A\right)^{-\theta} = 0.$$

Thus equations (7) through (13) determine the household's choices of c_t, m_t, n_t, b_{t+1}, P_t, λ_t, and ξ_t

[8] My own preferred price adjustment scheme is the "P-bar" model used by McCallum and Nelson (1999a).

[9] The issue at hand has been addressed previously by Ireland (2001), McCallum (2000), and Nelson (2000). Here we take a somewhat different approach.

[10] Details concerning price adjustment are, in other words, basically unrelated to this paper's central concern.

in response to exogenous, market-given, or government-specified values of w_t, A_t, π_t, r_t, Y_t^A, and P_t^A.

For general equilibrium, the additional relations are

$$n_t = 1 \tag{14}$$

$$m_t = M_t/P_t \tag{15}$$

$$G_t - tx_t = m_t - \left(1+\pi_t\right)^{-1} m_{t-1} + \left(1+r_t\right)^{-1} b_{t+1} - b_t \tag{16}$$

$$\pi_t = \left(P_t^{\ A} / P_{t-1}^{\ A}\right) - 1. \tag{17}$$

Then with M_t, G_t, and tx_t given exogenously by policy, and assuming symmetry among households so that $P_t = P_t^A$ and $Y_t^A = f(A_t n_t, k)$, the system also determines endogenously the values of w_t, r_t, P_t, and π_t.

This last statement presumes that the monetary authority—part of the "government"—exogenously controls M_t. But if we introduce the nominal rate of interest R_t defined by the Fisher identity,

$$1 + R_t = \left(1+r_t\right) E_t \left(1+\pi_{t+1}\right), \tag{18}$$

then we could reverse the roles of R_t and M_t, making the latter endogenous and the former policy-governed.

Also, we could introduce stickiness of nominal product prices. Suppose we did so by adding another relation such as

$$\Delta p_t = \beta E_t \Delta p_{t+1} + \alpha\left(y_t - \bar{y}_t\right), \tag{19}$$

which is the same as (2). Here $p_t = \log P_t$ and again $y_t = \log Y_t$ with $\bar{y}_t$ being the flexible-price value of y_t that would be produced if there were no price stickiness, that is, $\bar{y}_t = f(A_t 1, k)$. The introduction of (19) requires, in order to prevent overdetermination, that one of the previously prevailing conditions be eliminated. The simplest option, and in my opinion the most realistic, is to eliminate the condition for labor market equilibrium—equation (14). Then labor and output will both be demand determined.

The crucial issue, in the present context, is whether a relation such as (1)—with no money term involved—can be justified by our optimizing equilibrium analysis. In that regard we solve (9) for λ_t and substitute into (12):

$$\frac{u_1\left(c_t, \zeta_t\right)}{\left[1+\psi_1\left(c_t, m_t\right)\right]} = \left(1+r_t\right)\beta E_t \left\{\frac{u_1\left(c_{t+1}, \zeta_{t+1}\right)}{\left[1+\psi_1\left(c_{t+1}, m_{t+1}\right)\right]}\right\}. \tag{20}$$

Now, if $\psi(c_t, m_t)$ were separable, so that ψ_1 did not involve m, equation (20) would include only c_t, c_{t+1}, and r_t (plus shock terms). It would then be possible to write a log-linearized approximation of the form

$$\log c_t = b_0' + E_t \log c_{t+1} + b_1' r_t + v_t', \tag{21}$$

where v_t' represents $\zeta_t - E_t \zeta_{t+1}$. The latter is a familiar consumption Euler equation, although it differs from the most common version by incorporating the influences of the transactions term $\psi(c_t, m_t)$. Continuing, a log-linearized approximation to the overall resource constraint for the economy at hand, with constant capital, is

$$\log Y_t = \left(c/Y\right) \log c_t + \left(G/Y\right) \log G_t, \tag{22}$$

where (c/Y) and (G/Y) are steady-state shares. Substitution of (21) into (22) then yields

$$\log Y_t + E_t \log Y_{t+1} + b_0 + b_1 r_t + b_2\left(g_t - E_t g_{t+1}\right) + v_t, \tag{23}$$

where $g_t = \log G_t$. This relation is of the form of equation (1), so the latter can be justified. But this justification relies upon the assumption that $\psi(c_t, m_t)$ is separable, and in fact that seems implausible. Much more likely, I would think, would be a $\psi(c_t, m_t)$ function that made the cross partial derivative negative, so that the marginal benefit of holding money—i.e., the reduction in transaction costs—increases with the volume of consumption spending.

In McCallum (2000a), I proposed the following as a first-guess specification for ψ:

$$\psi\left(c_t, m_t\right) = c_t a_1 \left(c_t/m_t\right)^{a_2}. \qquad a_1, a_2 > 0 \tag{24}$$

Furthermore, it is shown there that the resulting implication for (23) is that it should then include an additional term, namely,

$$\begin{aligned} & b_3\left(\log m_t - E_t \log m_{t+1}\right) \equiv \\ & \left(c/Y\right)\phi\left(\phi+\sigma\right)^{-1}\left(\log m_t - E_t \log m_{t+1}\right), \end{aligned} \tag{25}$$

with $\phi = a_1(1 + a_2)\, a_2 (c/m)^{a_2}$, where σ is the inverse of the intertemporal elasticity of substitution in consumption. This equation can then be written

$$\begin{aligned} y_t = {} & b_0 + b_1\left(R_t - E_t \Delta p_{t+1}\right) + E_t y_{t+1} \\ & + b_2\left(g_t - E_t g_{t+1}\right) + b_3\left(\log m_t - E_t \log m_{t+1}\right) + v_t. \end{aligned} \tag{1'}$$

Numerical values for a_1 and a_2 will be considered shortly.

Another implication of the assumed specification (24) concerns the implied demand for money

function. We see that (11) and (12), together with the Fisher identity, imply

$$1+\psi_1\left(c_t, m_t\right)=\left(1+R_t\right)^{-1} \tag{26}$$

or, approximately,

$$-\psi_1\left(c_t, m_t\right)=R_t. \tag{27}$$

But with (24) this gives

$$a_1 a_2\left(c_t/m_t\right)^{1+a_2}=R_t \tag{28}$$

or

$$\log m_t = \left[\left(\log a_1 a_2\right)/\left(1+a_2\right)\right]+\log c_t-\left(1/\left(1+a_2\right)\right)\log R_t. \tag{29}$$

Thus we have a money demand function with a constant elasticity with respect to R_t of $-1/(1 + a_2)$ and an elasticity with respect to spending of 1.0. This relation can now be added to our model, which becomes (2), (3), (29), and (1′); these govern the behavior of y_t, Δp_t, R_t, and m_t.

Furthermore, equation (29) (or (28)) is useful in assigning values to a_1 and a_2, i.e., in calibration. Let us begin by assuming a money demand elasticity of –0.2; that choice implies $a_2 = 4$. Also, for a quarterly model let us assume an average interest rate of 0.0125 (i.e., 5 percent per year) and an average c/m ratio of 1.25. Then (28) becomes $a_1(4)\,(1.25)^5 = 0.0125$, implying $a_1 = 0.00102$. Then the crucial parameter ϕ becomes

$$\phi=0.00102\,(5)\,(4)\,\left(1.25\right)^4 = 0.0204\left(2.44\right) = 0.0498, \tag{30}$$

and the slope coefficient b_3 in (25) is, assuming $\sigma = 2$ and $(c/Y) = 0.7$,

$$\left(c/Y\right)\phi\left(\phi+\sigma\right)^{-1}=\left(0.7\right)\left(0.0498\right)/\left(2.0498\right)=0.017. \tag{31}$$

The remainder of our calibration is more standard: $\beta = 0.99$, $\alpha = 0.05$, $\theta = 5$, and the policy rule parameters $\mu_1 = 0.5$, $\mu_2 = 0.1$, $\mu_3 = 0.8$. The latter value refers to an extension of the Taylor rule to reflect interest rate smoothing:

$$R_t = \left(1-\mu_3\right)\left[\mu_0+E_t\Delta p_{t+1}+\mu_1\left(E_t\Delta p_{t+1}-\pi^*\right)+\mu_2 E_{t-1}\tilde{y}_t\right] +\mu_3 R_{t+1}+e_t. \tag{3′}$$

Note that in the latter we have used $E_t\Delta p_{t+1}$, setting $j = 1$ in (3). Also, v_t is white noise and we have generated $\bar{y}_t$ by the process $\bar{y}_t = 0.95\ \bar{y}_{t-1} + a_t$, with a_t white noise. Finally, we have made one more modification to the model at hand, in an attempt to have fairly realistic specifications for parts of the system not under scrutiny. This modification is to replace the price adjustment relation (2) with the following:

$$\Delta p_t = 0.5\beta\left[E_t\Delta p_{t+1}+\Delta p_{t-1}\right]+\alpha\,\tilde{y}_t+e_t. \tag{2′}$$

Here we have included the Δp_{t-1} term to reflect inflation inertia that appears to exist in many developed economies.

The model at hand consists, then, of relations (1′), (2′), (3′), and (4), specified to approximate (28).[11] The object is to see if the inclusion of the term (25) in (1′) substantially affects the behavior of Δp_t and y_t—i.e., whether theoretically incorrect exclusion of money from the system is of quantitative importance. For this purpose, impulse response functions are very well suited. As a tool for comparing a model with actual economic behavior, impulse response functions are of dubious value because of the need for shock identification. But they are more sensitive than autocorrelation functions to model specification, so are highly appropriate for the purpose at hand. Consequently, Figure 1 shows impulse response functions for y_t, Δp_t, and R_t for unit shocks to v_t, e_t, and a_t. In the top half of the figure we have included the m_t terms in (1′), whereas in the bottom half they are excluded. It is obvious that there is no appreciable difference.

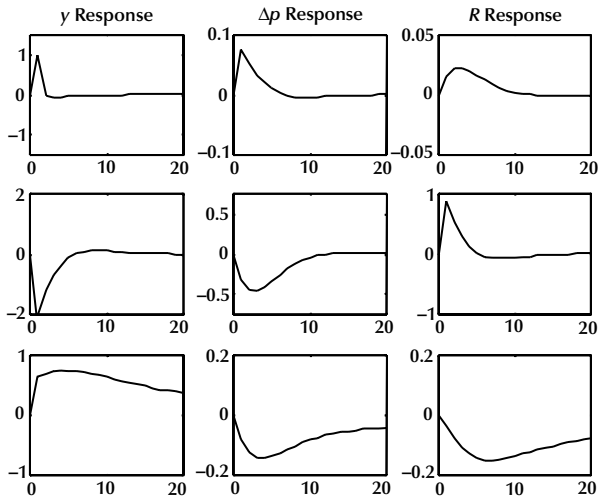
That there would be an appreciable difference, if the coefficient in (25) were larger, is illustrated in Figure 2. There we take the coefficient to be 0.2, holding everything else (including c_2) unchanged. Now the output and inflation responses are noticeably different, especially in response to a monetary policy shock.

In addition, as a quick robustness check we assume that the money demand elasticity is –0.1, rather than –0.2, implying that $a_2 = 9$ instead of 4. This change yields $b_3 = 0.033$ and $c_2 = -8$. The results, shown in the bottom half of Figure 2, are scarcely different from those in Figure 1. Finally, we note that decreasing rather than increasing a_2 (making money demand more elastic) would make the effect even smaller than in our initial case.

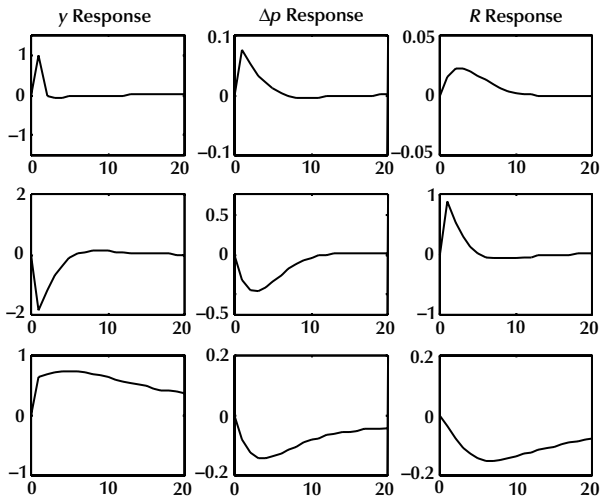
Our investigation suggests, then, that although it is theoretically incorrect to specify a model without money, the magnitude of the error thereby

[11] Thus c_2 in (4) equals $-1/(R(1 + a_2))$, which equals –16 when $a_2 = 4$.

Figure 1

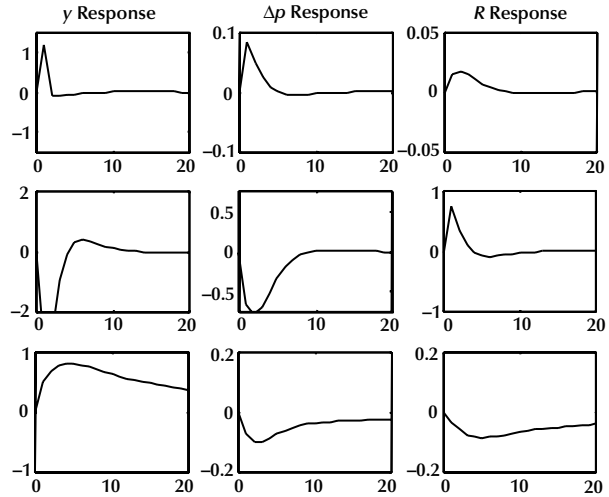


NOTE: Unit shocks to IS (top), policy rule (mid), and tech with money (b_3=0.017).

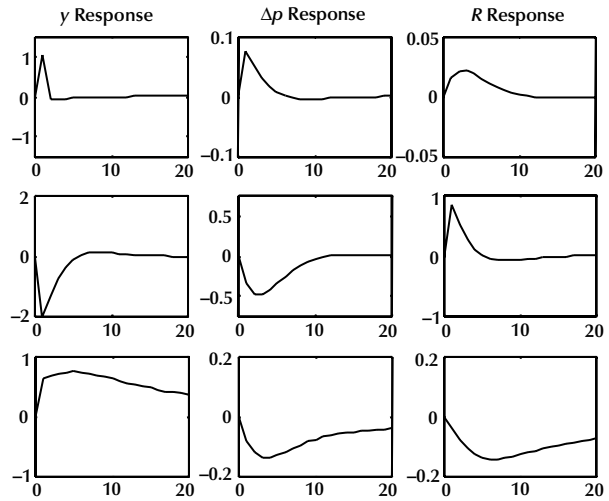


NOTE: Unit shocks to IS (top), policy rule (mid), and tech without money (b_3=0.0).

Figure 2



NOTE: Unit shocks to IS (top), policy rule (mid), and tech with money (b_3=0.2).



NOTE: Unit shocks to IS (top), policy rule (mid), and tech with money (a_2=9).

introduced is extremely small. This finding is basically consistent with those of Ireland (2001), who finds that econometric estimates of a parameter analogous to b_3 are insignificantly different from zero.[12]

ARE RATIONAL-EXPECTATION INDETERMINACIES IMPORTANT?

Models without money are typically ones in which an interest rate serves as the policy instrument that is adjusted in response to macroeconomic conditions, prominently including inflation. Central bankers and practical analysts stress the need to move *preemptively*, i.e., to adjust the policy stance when inflation forecasts get out of line without waiting for realized inflation to depart strongly from its target path (see, e.g., Goodfriend, 1997, and Svensson, 1997). There is, however, a line of theoretical analysis that warns of the danger of

[12] Ireland (2001) shows that, for his preferred rationalization of sticky prices, a term involving m_t also appears in the price adjustment relation.

"indeterminacy" if central banks' R_t policy rules respond too strongly to rational forecasts of *future* inflation, even if the same responses to current inflation would not be problematic. This argument was first developed by Woodford (1994) and has subsequently been promoted, or discussed with apparent approval, by Bernanke and Woodford (1997); Kerr and King (1996); Clarida, Gali, and Gertler (1997); Svensson (1997); Christiano and Gust (1999); Carlstrom and Fuerst (2000); Isard, Laxton, and Eliasson (1999); and Bullard and Mitra (2000). Its main message, that variants of inflation-forecast targeting are likely to generate undesirable outcomes, seems rather surprising in light of the descriptions of actual policy procedures used by the Bank of England, Reserve Bank of New Zealand, and Bank of Canada.[13] Note in this regard that for very large values of μ_1, in a policy rule like (3), the implied policy is virtually the same as exact targeting of an expected inflation rate, as promoted by Svensson (1997) and others. Thus the argument seems to deserve scrutiny. The present section extends and elaborates on an alternative argument briefly outlined in McCallum (1999, pp. 634-35), which suggests that the danger identified by the line of analysis in question represents a theoretical curiosity that is probably not of practical relevance.

Let us begin the discussion by noting the way in which the term "indeterminacy" is used in this body of literature. The term first became prominent in monetary economics from a series of writings by Patinkin—beginning with (1949) and culminating with (1961) and (1965)—that grew out of observations made by Lange (1942) about a putative logical inconsistency in classical monetary theory. Some of Patinkin's conclusions were disputed in a notable book by Gurley and Shaw (1960), and the resulting controversy was prominently reviewed in an influential survey article by Johnson (1962). In all of this earlier literature, it must be noted, the form of indeterminacy under discussion was "price level indeterminacy" such that the models in question fail to determine the value of *any* nominal variable, including the money supply. That type of failure occurs basically because of postulated policy behavior that is entirely devoid of any nominal anchor—i.e., there is no concern by the central bank for nominal variables.[14] Since rational private households and firms care only about real variables, according to standard neoclassical analysis, the absence of any "money illusion" by them *and* by the central bank must imply that no agent (in the model) has any concern for any nominal variable. Thus there is in effect no nominal variable appearing anywhere in the model, so naturally it cannot determine the value of such variables.

Arguably, a dynamized, rational expectations version of this type of price-level indeterminacy, in the context of an interest rate policy rule, was developed by Sargent and Wallace (1975) and exposited in Sargent's influential textbook (1979, pp. 362-63). But of course this type of indeterminacy disappears if the central bank provides a nominal anchor, as was recognized by Parkin (1978) and McCallum (1981), even in the presence of rational expectations and the complete absence of private money illusion.

The type of indeterminacy under discussion in the current literature cited at the beginning of this section is very different. Instead of a failure to determine any nominal variable (without any implied problematic behavior for real variables), the recent Woodford-warning[15] literature is concerned with a multiplicity of stable equilibria in terms of *real* variables.[16] This type of aberrational behavior stems not from the absence of any nominal anchor (a static concept) but from the (essentially dynamic) fact that various paths of real money balances can be consistent with rational expectations under some circumstances. In order to avoid possible semantic confusions, McCallum (1986) proposed that different terms be used for the two types of aberrational behavior—nominal indeterminacy and solution multiplicity, respectively.[17] It is necessary to report, however, that this proposal has not met with widespread acceptance.

Of what importance is the distinction emphasized in the last paragraph? As an example of the sort of confusion that can arise if the distinction is not recognized, let us refer to the analysis of "price level indeterminacy" under an interest rate rule in the famous *JPE* paper by Sargent and Wallace (1975) mentioned above. It has long been my own belief

[13] See, for example, descriptions by King (1999), Archer (2000), and Freedman (2000).

[14] See Patinkin (1965, p. 309).

[15] This term is due to Lars Svensson.

[16] It is dynamically stable equilibria that are relevant because explosive paths of real variables are normally ruled out by transversality conditions that show them to be suboptimal for individual private agents.

[17] The adjective "nominal" was omitted from my original proposal, but seems clearly to be desirable.

that the Sargent-Wallace (1975) paper was concerned with nominal indeterminacy—see McCallum (1981, 1986). Woodford (1999, Chap. 2), by contrast, interprets this Sargent and Wallace discussion as pertaining to solution multiplicity. My position is strengthened by the fact that the only substantive reference cited by Sargent and Wallace is Olivera (1970), which is clearly concerned with nominal indeterminacy. But there is something to be said for Woodford's position: under his interpretation the Sargent-Wallace result is valid, whereas under mine it is invalid (see McCallum, 1986, p. 148). Possibly Sargent and Wallace were undecided in their own thinking about which of the two concepts was being considered. In any event, their paper and the writings that have followed illustrate clearly the importance of making the distinction.

Let us now consider the substance of the Woodford warning of multiple solutions when policy is based on rational forecasts of future inflation.[18] It can be illustrated in a model such as our prototype (1) through (3) presented above. For convenience, let us rewrite the model here, omitting for simplicity the g_t term and treating $\bar{y}_t$ as a constant normalized to zero. Also, let us ignore constant terms that are tedious and for present purposes uninteresting. Finally, let us suppose that $E_t\Delta p_{t+1}$ is the inflation-forecast variable to which the policy rule pertains. Then the system can be written as

$$y_t = b_1\left(R_t - E_t\Delta p_{t+1}\right) + E_t y_{t+1} + v_t \tag{32}$$

$$\Delta p_t = \beta E_t \Delta p_{t+1} + \alpha y_t \tag{33}$$

$$R_t = \left(1+\mu_1\right) E_t \Delta p_{t+1} + \mu_2 y_t + e_t. \tag{34}$$

Here we suppose that u_t is absent from (2) while e_t in (3) is white noise, but that v_t in (1) is generated by a first-order autoregressive process—denoted AR(1)—as follows:

$$v_t = \rho_1 v_{t-1} + \varepsilon_{1t}. \tag{35}$$

Here ε_{1t} is white noise and the AR parameter satisfies $|\rho_1| < 1$.[19]

In this model the unique minimum-state-variable (MSV) rational expectations solution is of the form[20]

$$y_t = \phi_{11} v_t + \phi_{12} e_t \tag{36}$$

$$\Delta p_t = \phi_{21} v_t + \phi_{22} e_t. \tag{37}$$

Then we have $E_t y_{t+1} = \phi_{11}\rho_1 v_t$ and $E_t\Delta p_{t+1} = \phi_{21}\rho_1 v_t$; consequently, standard undetermined coefficient calculations yield

$$\phi_{11} = 1/\left[1-\rho_1 - b_1\mu_2 - \left(\alpha b_1 \mu_1 \rho_1\right)/\left(1-\beta\rho_1\right)\right] \tag{38a}$$

$$\phi_{12} = b_1/\left(1-b_1\mu_2\right) \tag{38b}$$

$$\phi_{21} = \alpha/\left[\left(1-\beta\rho_1\right)\left(1-\rho_1-b_1\mu_2\right) - \alpha b_1\mu_1\rho_1\right] \tag{38c}$$

$$\phi_{22} = \alpha b_1/\left(1-b_1\mu_2\right). \tag{38d}$$

It is easy to verify that $\phi_{11} > 0$, $\phi_{12} < 0$, $\phi_{21} > 0$, and $\phi_{22} < 0$—i.e., that both y_t and Δp_t respond positively to a demand shock and negatively to a random, policy-induced blip in R_t. Thus the MSV solution suggests that there is no problem with the inflation-forecast targeting rule (34).

Suppose, however, that a researcher looks for non-MSV solutions of the form

$$y_t = \phi_{11} v_t + \phi_{12} e_t + \phi_{13}\Delta p_{t-1} \tag{39}$$

$$\Delta p_t = \phi_{21} v_t + \phi_{22} e_t + \phi_{23}\Delta p_{t-1}, \tag{40}$$

where the extraneous state variable Δp_{t-1} is included. These expressions imply $E_t y_{t+1} = \phi_{11}\rho_1 v_t + \phi_{13}(\phi_{21} v_t + \phi_{22} e_t + \phi_{23}\Delta p_{t-1})$ and $E_t\Delta p_{t+1} = \phi_{21}\rho_1 v_t + \phi_{23}(\phi_{21} v_t + \phi_{22} e_t + \phi_{23}\Delta p_{t-1})$. Then undetermined coefficient reasoning implies that the values for the ϕ_{ij} are given by six relations analogous to (38) among which are

$$\phi_{13} = b_1\mu_1\phi_{23}^2 + b_1\mu_2\phi_{13} + \phi_{13}\phi_{23} \tag{41}$$

and

$$\phi_{23} = \beta\phi_{23}^2 + \alpha\phi_{13}. \tag{42}$$

From these ϕ_{13} can be solved out, yielding the cubic equation

$$\phi_{23} = \beta\phi_{23}^2 + \alpha b_1\mu_1\phi_{23}^2/\left(1-b_1\mu_2-\phi_{23}\right). \tag{43}$$

Inspection of the latter indicates that one solution

[18] Note that I am not disputing the different point that central banks need to base policy on their own information and structural models, also discussed by Woodford (1994) and Bernanke and Woodford (1997).

[19] It will be observed that the current system is somewhat simpler than the one used in the third section of the paper ("Is the Neglect of Money Quantitatively Important?"). The reason is to have one in the current section that permits some analytical results to be obtained, so that more understanding of the numerical results will be possible. The basic results also pertain to more general models.

[20] The MSV concept is discussed at length in McCallum (1999), where it is interpreted as the unique solution that includes no bubble or sunspot components. A solution procedure is there proposed that generates a unique solution by construction in a very wide class of linear RE models.

is provided by $\phi_{23} = 0$, which implies $\phi_{13} = 0$. This, of course, gives the MSV solution obtained previously. But (43) is also satisfied by roots of the quadratic

$$\beta\phi_{23}^{2} - \left[1+\beta+\alpha b_1\mu_1 - b_1\mu_2\beta\right]\phi_{23} + \left(1-b_1\mu_2\right) = 0, \tag{44}$$

i.e., by

$$\phi_{23} = \frac{d \pm \left[d^2 - 4\beta\left(1-b_1\mu_2\right)\right]^{0.5}}{2\beta}, \tag{45}$$

where d is the term in square brackets in (44). Therefore, for some values of the parameters α, β, b_1, μ_1, and μ_2 there may be other real solutions in addition to the MSV solution.[21]

To keep matters relatively simple, let $\mu_2 = 0$ so that the policy rule responds only to expected inflation. Then d becomes $1 + \beta + \alpha b_1\mu_1$, and there will be two real roots to (44) if $\mu_1 < 0$ or $\mu_1 > \mu_1^c \equiv [2\beta^{0.5} + 1 + \beta]/(-b_1\alpha)$. Furthermore, while one of the ϕ_{23} values in (44) will exceed 1.0 in absolute value when $\mu_1 > \mu_1^c$, the other will not—it will be a (negative) stable root. Consequently, there will be no transversality condition to rule out that root's implied trajectory as a rational expectations (RE) equilibrium. Thus there is, for $\mu_1 > \mu_1^c$, an infinite multiplicity of stable RE solutions indexed by the initial start-up value of Δp_{t-1}. In such cases, moreover, "sunspot" solutions are also possible in the sense of not being ruled out by the conditions of RE equilibria.[22] This is the danger pointed out by the Woodford warning, and it is made more likely when values of μ_2 exceed zero.[23]

I now wish to argue that the foregoing danger may not be of any practical significance, for it is entirely possible that non-MSV—i.e., bubble and sunspot—solutions are empirically irrelevant.[24] That such is the case is a cogent and plausible hypothesis, which to my knowledge has not been convincingly contradicted by any empirical tests, despite the enormous amount of interest shown by researchers over the past 25 years. In support of this position, I will offer two infrequently-stated lines of argument.

The first line of argument, in favor of the proposition that only MSV solutions are of empirical relevance, concerns the nature and role of the RE hypothesis. No one, I would think, believes that the orthogonality conditions for RE literally obtain precisely in real world economies, any more than do the conditions for exact profit and utility maximization. The hypothesis is extremely fruitful and attractive nevertheless because it points to a powerful force, the desirability (for purposeful agents) of eliminating any systematic component of their expectational errors. And it keeps analysts from constructing models in which it is possible for agents to repeatedly commit the same type of expectational error, over and over again. But to suggest that the "expectation function,"[25] which describes actual expectational behavior, can jump from one specification (e.g., the MSV form $\Delta p^e_{t+1} = \phi_{21}\rho_1 v_t$) to another (e.g., the non-MSV form $\Delta p^e_{t+1} = \phi_{21}\rho_1 v_t + \phi_{23}[\phi_{21}v_t + \phi_{22}e_t + \phi_{23}\Delta p_{t-1}]$ with different ϕ_{ij} values) at any point of time—without any particular stimulus—seems downright whimsical. Much more plausible, I would contend, is the idea that such expectation functions are uniquely given at any point of time for any specified economy and policy regime. This does not imply, of course, that expectations themselves, e.g., Δp^e_{t+j}, cannot jump abruptly from one period to the next.

In this regard, the theoretical work of Evans (1986) and Evans and Honkapohja (1999, 2001) is in my opinion predominantly supportive of the hypothesis that the unique MSV solution is relevant for macroeconomic analysis.[26] With respect to the model at hand, for example, it is shown by Bullard and Mitra (2000, Figure 3) that the MSV solutions are *E-stable*, and therefore learnable by a real-time least-squares learning procedure, for the cases with large μ_1 and/or μ_2 values.[27] Bullard and Mitra do not analyze the E-stability/learnability properties of the non-MSV solutions, but very closely related cases have been analyzed by Evans (1986, pp. 150-53) and

[21] An analysis is provided by Bullard and Mitra (2000, p. 26).

[22] By a sunspot solution I mean one that includes random variables (of a martingale difference variety) that have no connection with other elements of the model.

[23] See, e.g., Bullard and Mitra (2000).

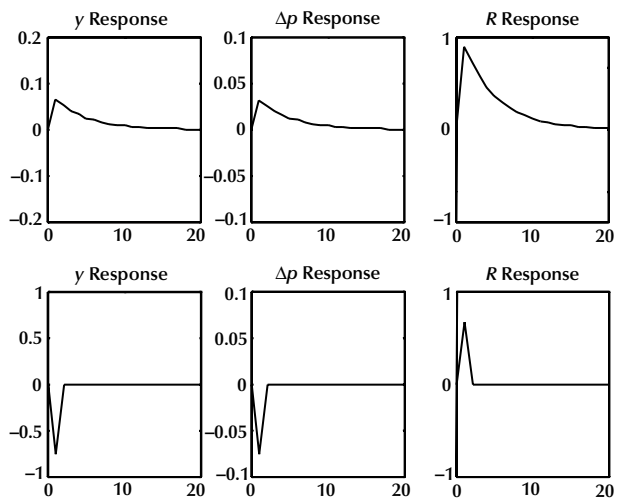
[24] At least, in macroeconomic contexts.

[25] An expectations function is a formula relating an expectational variable, such as the period-t expectation of z_{t+j}, denoted ${}_t z^e_{t+j}$, to observable variables.

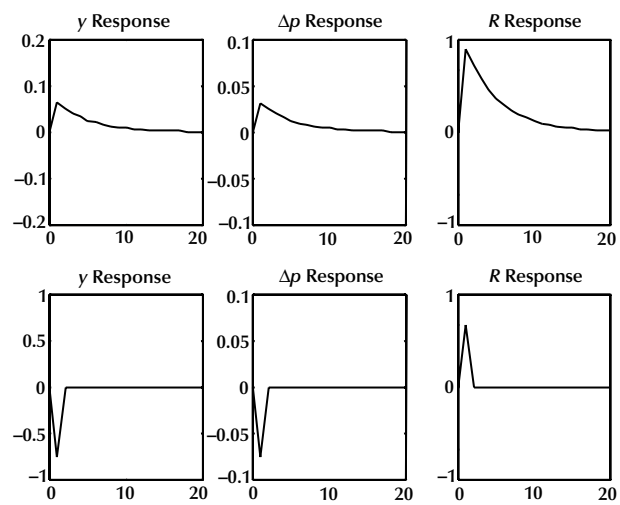
[26] Evans and Honkapohja themselves might not agree. In any event, their recent terminology differs from mine in that they use the term MSV to refer to solutions that in some cases obtain in addition to the one that is specified by my concept of the MSV procedure. (See Evans and Honkapohja, 1999, p. 488; 2001, Chaps. 8-10.)

[27] E-stability pertains to the convergence of meta-time iterations that may or may not drive non-RE expectations functions to their RE values. Evans and Honkapohja (1999, 2001) show that in the cases at hand E-stability implies convergence of a real-time least-squares learning process like that of Marcet and Sargent (1989). For a useful introduction, see Bullard and Mitra (2000).

Figure 3



NOTE: Responses to unit shocks to IS (top) and policy rule (bottom) with μ_1=53.0.



NOTE: Responses to unit shocks to IS (top) and policy rule (bottom) with μ_1=53.1.

Evans and Honkapohja (1999, pp. 487-506; 2001, Chap. 10). These results indicate that their method of determining an expectation function would lead to the MSV solution in the case at hand.

The second line of argument to be developed here is to emphasize (i) that the unique MSV solution is available in the high-μ_1 cases pointed to by the Woodford warning and (ii) that this solution is well behaved in the sense of experiencing no discontinuity when passing through the critical values that delineate the region of multiple stable solutions. Specifically, impulse response functions for the MSV solution are virtually indistinguishable for μ_1 values just above and just below the μ_1^c critical value at which solution multiplicity sets in. By contrast, the non-MSV solutions are highly different for the same pairs of μ_1 values (i.e., just above and just below μ_1^c).[28]

To illustrate this, let us take a numerical example in which $b_1 = -0.75$, $\beta = 0.99$, $\alpha = 0.1$, and $\rho_1 = 0.8$. In this case the critical value of μ_1 will be

$$\mu_1^c = \left[2\left(0.99^{0.5}\right) + 1.99\right] / (0.75)(0.1) = \left[1.99 + 1.99\right] / 0.75 = 53.07. \tag{46}$$

For values of μ_1 less than 53.07, there will be a single stable solution; for values above 53.07 there will be multiple stable solutions.[29] But for values of μ_1 close to 53.07, the behavior of the MSV solution will be virtually identical regardless of whether μ_1 is slightly below, equal to, or slightly above 53.07. This is demonstrated in Figure 3, where impulse response functions for y, Δp, and R are shown for unit shocks to the IS function (i.e., $v_t = 1.0$) and the policy rule (i.e., $e_t = 1.0$). The plots with $\mu_1 = 53.0$ and $\mu_1 = 53.1$ are, it seems fair to say, virtually indistinguishable.

More generally, properties of the MSV impulse response functions change continuously with values of μ_1. This is illustrated in Figures 4 and 5. In the former, we have cases with $\mu_1 = 10$ and $\mu_1 = 20$, both of which imply unique stable solutions. The response peaks in y and Δp are reduced smoothly in size as μ_1 increases from 10 to 20 to 53. Then these reductions continue to obtain, in a smooth manner, as μ_1 is increased further to 80 and 200; see Figure 5.

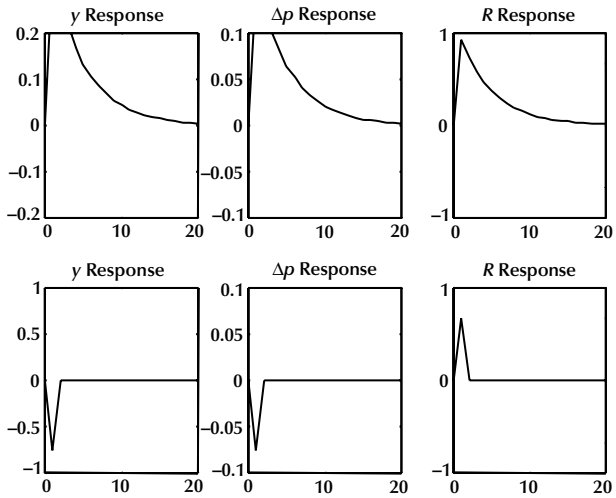
Behavior of the non-MSV solution contrasts sharply, and is distinctly non-continuous. For $0 < \mu_1 < 53.07$, roots to (44) are complex so (38) gives the only solution. Then with $\mu_1 = 53.1$, we obtain a stable non-MSV solution (in addition to the stable MSV solution) as shown in the top panel of Figure 6.[30] Since the responses to e_t involve coefficients in which the denominator equals the AR(1)

[28] I am indebted to Doug Laxton for suggesting comparisons based on impulse response functions.

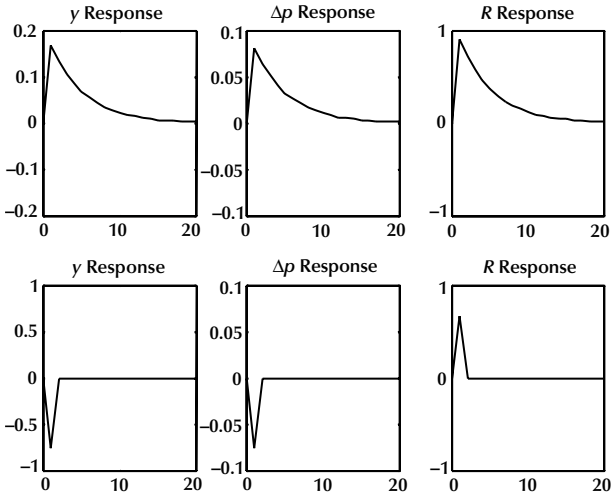
[29] This is verified by Matlab calculation of solutions using my modification of Klein's (2000) QZ algorithm.

[30] These non-MSV solutions are obtained by adding to the Matlab file mentioned previously a subroutine written by Christopher Sims, qzswitch.m, to generate the solution implied by a different ordering of the system's generalized eigenvalues, in the manner mentioned on p. 633 of McCallum (1999).

Figure 4

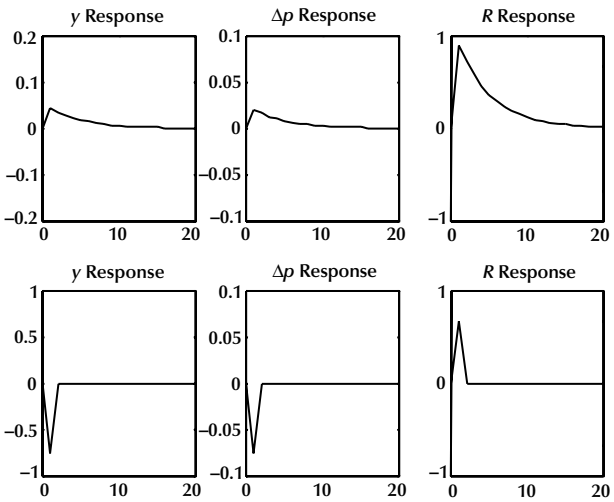


NOTE: Responses to unit shocks to IS (top) and policy rule (bottom) with μ_1=10.0.

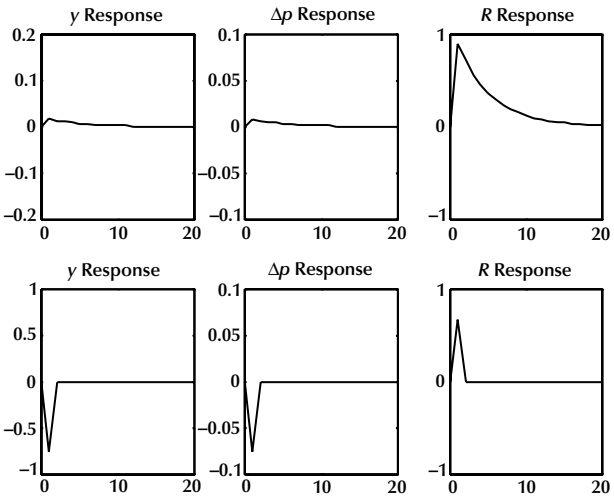


NOTE: Responses to unit shocks to IS (top) and policy rule (bottom) with μ_1=20.0.

Figure 5



NOTE: Responses to unit shocks to IS (top) and policy rule (bottom) with μ_1=80.0.

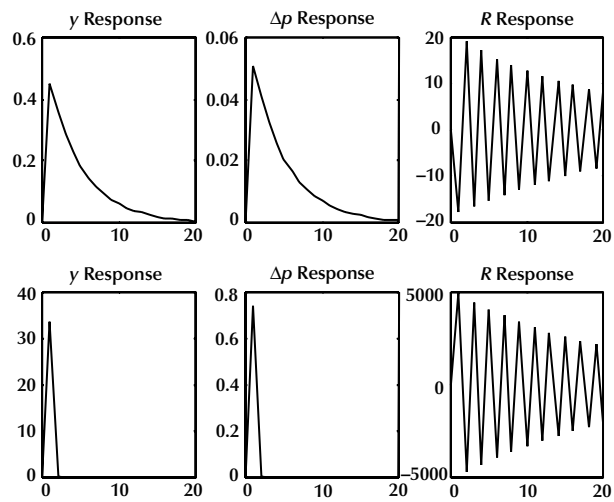


NOTE: Responses to unit shocks to IS (top) and policy rule (bottom) with μ_1=200.0.

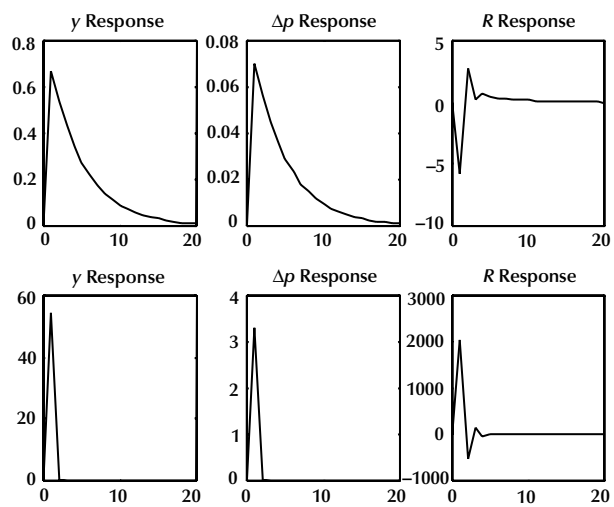
parameter for the e_t process, these responses are "infinite" when e_t is white noise. Consequently, a value of 0.01 is used for this AR(1) parameter in the model for Figure 6. A comparison with Figure 3 shows, not surprisingly, that this non-MSV solution is not at all similar to the MSV solution with μ_1 = 53.0. Next, consider the bottom panel of Figure 6, where μ_1 = 80. It can be seen that the increase in μ_1 *decreases* the responsiveness of R_t and leaves larger peaks for y_t and Δp_t in response to both shocks. Also note that y_t and Δp_t blip *upward* in response to a surprise increase in R_t and that the responses of y_t and R_t are huge. Finally, if we let the autoregressive parameter generating e_t be 0.04 instead of 0.01, the direction of the y_t and Δp_t responses to e_t flips over to become negative. (This case is not shown.)

These results illustrate, for one representative set of parameter values, the well-behaved nature of the MSV solution and the erratic nature of the non-MSV (bubble) solutions. Such results also obtain for other parameter values and clearly suggest the desirability of considering the MSV solutions as the sole economically relevant solution. If this strategy is adopted, i.e., if the MSV solution is taken to rep-

Figure 6



NOTE: Non-MSV solution; unit shocks to IS (top) and policy rule (bottom) with μ_1=53.1.



NOTE: Non-MSV solution; unit shocks to IS (top) and policy rule (bottom) with μ_1=80.0.

resent implied behavior for the model at hand, then there is no compelling reason to believe that large μ_1 values will generate undesirable behavior. In that case, preemptive inflation forecast targeting with an R_t instrument will not be subject to the dangers mentioned above.

ON THE VALIDITY OF THE TAYLOR PRINCIPLE

As a related matter, let us now consider the consequences of having a policy rule that fails to satisfy the Taylor principle, i.e., that an interest rate policy rule should respond by more than point-for-point to inflation or its expectation. Both Taylor (1999) and Clarida, Gali, and Gertler (1999) emphasize that this requirement, which translates into $\mu_1 > 0$ in our model (32) through (34), implies that the *real* rate of interest will be moved upward (tightening policy) when inflation exceeds its target (and vice versa). Our analysis of the previous section was principally concerned with cases in which $\mu_1 > \mu^c$, but it was shown using (44) that solution multiplicity also obtains when $\mu_1 < 0$. Thus it might be thought that our argument for downplaying the importance of solution multiplicity would also apply to cases with $\mu_1 < 0$, thereby contradicting the Taylor principle. Such is not the case, however, and it is the task of this section to explain why. In other words, we will determine what is the true problem posed, from the MSV perspective, by $\mu_1 < 0$.

For our argument to be different for "indeterminacies" with $\mu_1 < 0$, as compared with those with $\mu_1 > \mu^c$, it must not be the existence of solution multiplicities per se that is considered problematic when $\mu_1 < 0$. Instead, there must be some other condition that prevails when $\mu_1 < 0$ and represents a problem that is, according to the present argument, of genuine importance.

Once again this problem is the absence of E-stability. Bullard and Mitra (2000) show that $\mu_1 < 0$ implies the absence of E-stability, and therefore the failure of least-squares learning, for separate cases in which π_t, π_{t-1}, and $E_{t-1}\pi_{t+j}$ enter the policy rule.[31] This can be seen by inspection of their Figures 1 through 3. Since E-stability of an MSV solution enhances the attractiveness of that solution by indicating that it may be of empirical relevance, its absence for $\mu_1 < 0$ suggests that policy rules with $\mu_1 < 0$ should be avoided.

An interesting application of this argument concerns the special case in which $\mu_1 = -1$ with Δp_t in the rule and $\mu_2 = 0$, i.e., the case of a pure interest rate peg: R_t = constant. Then the Bullard-Mitra results imply that E-stability does not prevail. This constitutes a version, with an optimizing model, of the argument of Howitt (1992). Note that it applies to a maintained interest rate peg, not to the use of an interest rate instrument.

An alternative argument for the case with $-1 < \mu_1 < 0$ can be developed as follows. Consider a model in which the endogenous variable x_t is generated by the relationship

[31] Here I am discussing cases with $\mu_2 = 0$. Bullard and Mitra's results are more general, since they also consider $\mu_2 > 0$.

(47) $$x_t = a_0 + a_1 E_t x_{t+1} + a_2 x_{t-1} + z_t,$$

where z_t is a stationary exogenous forcing variable with an unconditional mean of zero. Now apply the unconditional expectation operator to get

(48) $$Ex_t = a_0 + a_1 Ex_{t+1} + a_2 Ex_{t-1} + 0.$$

Assuming stationarity, we then have

(49) $$Ex_t = a_0 / (1 - a_1 - a_2).$$

Clearly, as $a_1 + a_2$ approaches 1.0 from below, the unconditional mean of x_t approaches $+\infty$, whereas if $a_1 + a_2$ approaches 1.0 from above, then the mean of x_t approaches $-\infty$. Thus there is an infinite discontinuity at $a_1 + a_2 = 1$. So to be well formulated, the model needs to include a parameter restriction that rules out $a_1 + a_2 = 1$. From a purely mathematical perspective, $a_1 + a_2 > 1$ would do as well, but for economic plausibility the preferred restriction is $a_1 + a_2 < 1$. Note that if $a_2 = 0$, this amounts to $1 - a_1 > 0$.

To see the relevance of the foregoing for the model (32) through (34), with $\mu_2 = 0$, write the system as

(50)
$$\begin{bmatrix} 1 & 0 \\ -\alpha & 1 \end{bmatrix} \begin{bmatrix} y_t \\ \Delta p_t \end{bmatrix} = \begin{bmatrix} 1 & b_1\mu_1 \\ 0 & \beta \end{bmatrix} E_t \begin{bmatrix} y_{t+1} \\ \Delta p_{t+1} \end{bmatrix} + \begin{bmatrix} 1 & 0 \\ 0 & 1 \end{bmatrix} \begin{bmatrix} v_t \\ e_t \end{bmatrix}$$

or

(51) $$x_t = AE_t x_{t+1} + Cz_t,$$

where $x_t = [y_t\ \Delta p_t]'$, $z_t = [v_t\ e_t]'$, and

(52) $$A = \begin{bmatrix} 1 & 0 \\ -\alpha & 1 \end{bmatrix}^{-1} \begin{bmatrix} 1 & b_1\mu_1 \\ 0 & \beta \end{bmatrix} = \begin{bmatrix} 1 & b_1\mu_1 \\ \alpha & \alpha b_1\mu_1 + \beta \end{bmatrix}.$$

Now the counterpart of $1 - a_1 > 0$ in the previous example is that $\det(I - A) > 0$. If that condition does not hold, the model is not well formulated and the unconditional mean of x_t passes through an infinite discontinuity. But the value of $\det(I - A)$ in the case at hand is $-\alpha b_1 \mu_1$. Thus with $\alpha > 0$ and $b_1 < 0$, the requirement for this model to be well formulated is $\mu_1 > 0$. This follows the original development and promotion of the MSV solution, in which McCallum (1983, p. 160) points out "the desirability of specifying admissible parameter values as an integral part of the model."

The main conclusion of this section is that the Taylor principle is basically correct. If μ_2 is positive, then μ_1 can be negative to some extent without losing E-stability; according to Bullard and Mitra (2000), the necessary and sufficient condition for E-stability is $\alpha\mu_1 + (1-\beta)\mu_2 > 0$. But that is a modification, not a fundamental contradiction, of the principle. Its basic logic is sound.

CONCLUDING REMARKS

In the foregoing sections the following arguments have been developed: (i) models without monetary aggregates do not imply that inflation is a non-monetary phenomenon and are not necessarily non-monetary models; (ii) theoretical considerations suggest that such models are misspecified, but the quantitative significance of this misspecification seems to be very small; (iii) arguments based on "indeterminacy" findings, e.g., regarding policy rules that respond strongly to expected future inflation rates, are of dubious merit: there are various reasons for believing that findings of solution multiplicity are theoretical curiosities that have little or no real world significance; (iv) monetary policy rules that violate the Taylor principle, by contrast, possess another characteristic—the absence of E-stability and least-squares learnability—that suggests undesirable behavior in practice.

These points are mostly supportive of the notion that policy analysis in models without money, based on interest rate policy rules, is not fundamentally misguided. It is important, consequently, to mention explicitly that they do not imply that policy rules with an interest rate instrument are necessarily preferable to ones based on a controllable monetary aggregate, such as total reserves or the monetary base. My own preference has been, for many years, for base instrument rules. Furthermore, my recent (2000b) study of the counterfactual historical performance of alternative rules for the United States, the United Kingdom, and Japan suggests that—for reasons that are not entirely clear—base instrument rules would have provided better policy guides than interest instrument rules over 1965-98. But the topics considered in the present paper are ones of considerable fundamental interest, and it is important in choosing among different types of rules—i.e., different ways of conducting policy—that central bankers not be misled by dubious economic analysis.[32]

[32] In that regard, the analysis in the fourth section of the paper ("Are Rational-Expectation Indeterminacies Important?") also suggests that some results used to argue in favor of an interest rate instrument, rather than the monetary base—see, e.g., Woodford (1999)—are also dubious.

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Monetary Transmission Lags and the Formulation of the Policy Decision on Interest Rates

Charles A.E. Goodhart

I. INTRODUCTION

Lags and Optimal Control Methods of Setting Interest Rates

Although it is a generally accepted stylized fact that, as Milton Friedman noted, the monetary policy transmission mechanism has "long and variable lags," most of the early theoretical work on these issues, e.g., the rules versus discretion debate, proceeded on the assumption that monetary policy measures were instantly transmitted to inflation. (See Barro and Gordon, 1983, and Cukierman, 1992.) Thus in his basic model, Cukierman (1992, Chap. 3, p. 28) states that "Abstracting from real shocks, growth and changes in velocity, the rate of inflation is equal to the rate of monetary growth m. Hence inflationary expectations are equal to expected money growth m^e, and the short-run Phillips relation…can be restated as $N-Nn=\alpha(m-m^e)$, where N is employment and Nn the natural rate of employment."

Since then analysis has become more realistic. Most theorists now accept that the policy instrument that central banks actually adjust is the short-term rate of interest, and not a monetary aggregate (though some still regret this fact). Moreover, the standard work-horse models now in current use, especially Rudebusch and Svensson (originally 1997, now 1999a), do incorporate monetary transmission lags. The Monetary Policy Committee (MPC) addressed this issue, of the lags in the transmission mechanism, in its report, "The Transmission Mechanism of Monetary Policy," (Bank of England, 1999)—a publication intended for an audience beyond academic experts in the subject. The existence of such lags raises questions about how interest rates should be set currently at time t to achieve future objectives at time $t+n$. Typically it is supposed that interest rates do not affect current goal variables instantaneously; and, in so far as they can affect certain (intermediate) variables immediately (e.g., exchange rates), they may only do so at the cost of destabilizing other goal variables subsequently (e.g., output).

Academic studies of the way in which interest rates might be set in such a forward-looking context involve a number of dimensions. The three dimensions required for study are the model, the structure of shocks, and the loss function. In such cases, the optimal policy horizon over which inflation returns to target is determined endogenously within the model, as we will discuss subsequently when I report work by Batini and Nelson from the Bank of England. In practice, however, very few central banks, or monetary authorities, set monetary policy by such optimal control methods.

There are no doubt various reasons why this is so. As will be described in Section II, which reports work undertaken within the Bank, such optimal control horizons appear very sensitive to the precise model/shock specification applied. Moreover, all the models on which such exercises have been attempted have been small and simplified. As will be discussed, it remains unclear how successfully, if at all, optimal control techniques could be applied in the context of the larger models used in practice for forecasting purposes.

Last, but not least, such an approach requires specification of a loss function, for which the appropriate discount rate must be included. Because of the complexity and difficulty of such an exercise, governments have not been prepared to do so. Instead they have usually restricted themselves to giving primacy to the objective of price stability, while accepting that some conditions may exist in which the monetary authorities should not aim to return inflation to target excessively quickly, since that might impart undue volatility to output.

In the case of the United Kingdom, the Chancellor set out the proposed target in his letter,

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June 3, 1998, to the Governor of the Bank of England, writing

> The operational target for monetary policy remains an underlying inflation rate (measured by the 12 month increase in the RPI [retail price index] excluding mortgage interest payments) of 2½ per cent. The inflation target is 2½ per cent at all times: that is the rate which the MPC is required to achieve and for which it is accountable… The framework takes into account that any economy at some point can suffer from external events or temporary difficulties, often beyond its control. The framework is based on the recognition that the actual inflation rate will on occasions depart from its target as a result of shocks and disturbances. Attempts to keep inflation at the inflation target in these circumstances may cause undesirable volatility in output.

If the government is not prepared to define the loss function, a central bank without goal independence is not constitutionally in a strong position to do so on its own. So, for a combination of reasons central banks have generally given weight to a variety of operational procedures, which do not involve conscious optimization conditional on an assumed loss function. This will involve another set of dimensions, specifically that choice of operational technique (e.g., some kind of feedback rule) and what forecast horizon to set for operating this rule (since the horizon now becomes a choice variable as well). This is discussed further in Section III.

The Rudebusch/Svensson Model and Its Findings on Alternative Policy Horizons

In their excellent paper, Rudebusch and Svensson (R/S) (1999b) consider a variety of such operational techniques in the context of a simple calibrated two-equation model of the U.S. economy, and various alternative versions of their assumed loss function. Then they seek to compare the loss from each procedure with the loss from using the benchmark optimal control approach. The operational techniques that they examine include (i) the Taylor rule, (ii) a forward- (eight quarter) looking Taylor rule, (iii) an instrument rule that responds to a rule-consistent inflation forecast, (iv) a strict inflation target (where output deviations are given no weight), and (v) a flexible inflation target (where such deviations are given weight). In all cases such techniques are examined both with and without interest rate smoothing.

In the case of the forward-looking Taylor rule, the horizon was, somewhat arbitrarily, set at 8 quarters; while for instrument rule (iii) above and for the strict and flexible inflation targets (iv and v above), the horizons are again somewhat arbitrarily set at 8, 12, and 16 quarters, respectively.[1] R/S look at losses relative to the benchmark optimal control (OC) case in five tables (5.3 through 5.7) relating to various configurations of parameters in the loss function. Out of the 25 permutations (five techniques with differing horizons, five tables with differing loss functions), the shortest horizon gave the *worst* outcome in 21 cases and the best in 2; the 12-quarter horizon gave the worst in 0 cases and the best in 8 cases; and the 4-year, 16-quarter horizon (the longest) was the worst in 4 cases and the best in 15.

Although R/S do not focus strongly on the optimal horizon length, the implication of their findings appears to be that overall economic (adjustment) costs are reduced if the horizon for inflation targetry is considerably longer than normally practiced, at least in the U.K., where the horizon for the inflation fan chart is two years.[2] There may be several reasons for this difference between their U.S. analytical results and U.K. practice. One might be that the monetary policy transmission lag is somewhat shorter in more open economies, since exchange rates typically adjust faster than real output in response to an interest rate change. Another reason could be that the R/S model is backward-looking; in models with forward-looking elements the transmission process is generally speedier.

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[1] The word "horizon" looks simple enough, but, as Svensson has demonstrated in several papers, semantic complexities abound in this field. Horizon can merely mean the end of the forecast, whereas "policy horizon" is usually taken to imply the date at which the objectives of policy are approximately obtained; and "forecast horizon" implies the future date at which the policy makers react to deviations of the objective(s) from their desired level. As described in more detail in Section II, the forecast and policy horizons often differ, and can obviously diverge from the length of the published forecast. In the above case, the R/S paper is examining forecast horizons.

[2] As described further in Section II, the evidence suggests that this is the MPC's policy horizon, as well as the end-point of the published fan chart.

The Structure of the Remainder of this Paper

Be that as it may, in Section II of this paper I shall review and summarize some current work being done in the Bank on this subject of the optimal *horizon* for monetary policy.

Another feature of U.K. procedures is that the inflation forecast is based on (i.e., conditional on) the assumption that interest rates are held constant thereafter over the remaining two years. As R/S note (p. 13), this was originally done at the Bank because (before operational independence in 1997) it could not presuppose future policy changes by the government. That particular constraint has now gone. In Section III I shall offer a few speculative comments on whether that procedure remains appropriate or whether some alternative *time-path* might instead be adopted. So Section II will concentrate on Bank work on horizons, and then Section III on some less substantial personal musings on time-paths.

A Digression on the Periodicity of Decision-Making Meetings and Forecasts

Before moving on to these issues, however, I want to digress for a moment on the subject of periodicity. The R/S model is a clever combination of annual averages and quarterly data points. In their paper, the periodicity of both the forecasting process and the decision-making process is implicitly assumed to be quarterly. In practice in almost all countries, the periodicity of the decision-making process is higher (more frequent) than that of the forecasting process. This is partly because the decision-makers can subjectively update the forecasts in the light of incoming news at less expense in terms of the use of skilled resources; so it is probably optimal to reconsider decisions with a higher frequency than formal, comprehensive forecasts. Nevertheless formal forecasts *could* be undertaken at almost any frequency, as also could policy decisions.

There seems, on the basis of casual empiricism, to be more international uniformity amongst developed nations on the preferred periodicity of formal forecasting processes for monetary policy purposes (quarterly) than on the preferred frequency for decision-making. The Federal Open Market Committee (FOMC) meets eight times a year, but can (and does) make inter-meeting policy changes; the Bank of England and the Bank of Japan meet once a month (without inter-meeting changes so far, since the grant of operational independence); and the European Central Bank meets twice a month, but will normally only consider the monetary policy decision in depth at one of these meetings. Given that the frequency of both these processes can be chosen, and is not ineluctably and exogenously determined, there has been notably little analysis on what might be the optimal periodicity for either process. Instead, the frequencies seem to have evolved by practice, convention, and shared experience without much (any?) formal (e.g., cost/benefit) analysis. Perhaps this is desirable?

II. HORIZONS

The Horizon in the U.K.

The Chancellor's letters to the MPC, specifying the target (2.5 percent for RPIX, where RPIX is the standard retail price index excluding X, which are the components of the index directly affected by interest rate changes, specifically the mortgage cost component of housing services prices) that the MPC seeks to achieve, have not set any finite horizon for the achievement of that target, thus requiring it to be met indefinitely. Nevertheless the wording of these letters indicates that some short-run flexibility, to allow for the occurrence of supply shocks and to limit excessive short-run volatility in output, would be both acceptable and desirable. (The relevant wording in the letter of June 3, 1998, was noted earlier in Section I.)

In practice, the MPC has published inflation (and output) fan charts that extend two years into the future. Moreover, observers will have noted that, without exception, all the fan charts for inflation published since the MPC was established have shown RPIX very close to target in the quarters near the terminal date, but deviating somewhat more (though rarely by much) in the prior quarters. The fan charts for inflation for the forecasts in February and May 2000 are shown in Figures 1 and 2. The implication of these findings would seem to be that a horizon of about 18 to 24 months is also the policy horizon of the MPC.

In the meantime, staff at the Bank of England have been exploring in a more formal analytic mode the factors that might determine the choice of an appropriate (perhaps optimal) horizon. The first of the two papers that I shall describe here is

Figure 1

February 2000: Current RPIX Inflation Projection Based on Constant Nominal Interest Rates at 6 Percent

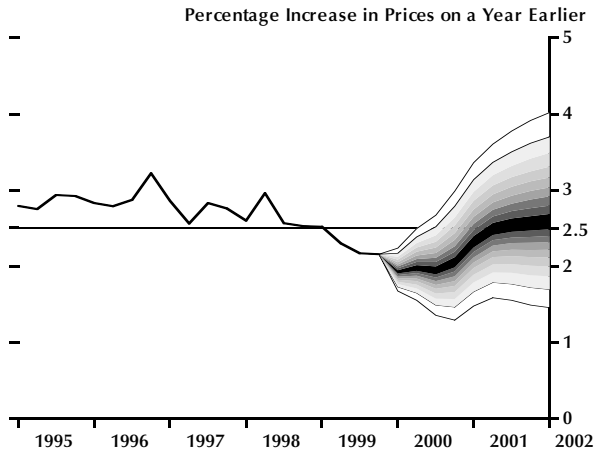
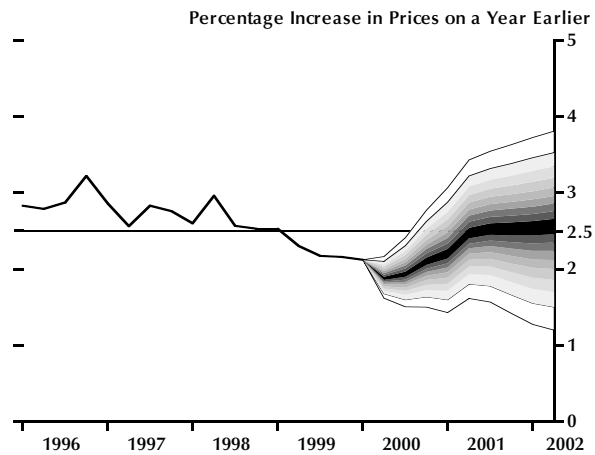


Figure 2

May 2000: Current RPIX Inflation Projection Based on Constant Nominal Interest Rates at 6 Percent



by Batini and Haldane (B/H) (1999).

The Batini/Haldane Study

In this study B/H establish and calibrate a five-equation model for the U.K.; an IS curve with real interest rate and real exchange rate terms; an LM demand for money function; an uncovered interest parity condition for the nominal exchange rate; a supply side equation based on a staggered contracting model, and an equation in which overall inflation relates both to domestic and to imported inflation (import pass through). Against the background of this (numerically calibrated) model, they then explore the (simulated) implications of imposing a class of relatively simple inflation forecast-based rules of the form:

$$r_t = \gamma r_{t-1} + (1-\gamma) r_t^* + \theta \left[E_t \, \pi_{t+j} - \pi^* \right]$$

where r_t denotes the short-term *ex ante* real rate of interest, $r \equiv i_t - E_t \pi_{t+1}$, where i_t is the nominal interest rate; r_t^* denotes the equilibrium value of the real interest rate;

$$E_t(.) = E(.|\Phi_t),$$

where Φ_t is the information set available at time t and E is the mathematical expectations operator; π_t is inflation ($\pi_t \equiv p_t^c - p_{t-1}^c$ where p_t^c is the log of the consumer price index); and π^* is the inflation target.

According to the rule, the monetary authorities control deterministically the nominal interest rate (i_t) so as to hit a path for the short-term real interest rate (r_t). The short real rate is in turn set relative to some steady-state value, determined by a weighted combination of lagged and equilibrium real interest rates. The novel feature of the rule, however, is the feedback term. Deviations of *expected* inflation—the feedback variable—from the inflation target—the policy goal—elicit remedial policy actions.

The policy choice variables for the authorities are the parameter triplet $\{j, \theta, \gamma\}$. The parameter γ measures the degree of interest rate smoothing (see Williams, 1997). So, for example, with $\gamma = 0$ there is no instrument smoothing. θ is a policy feedback parameter. Higher values of θ imply a more aggressive policy response for a given deviation of the inflation forecast from its target. Finally, j is the targeting horizon of the central bank when forming its policy. For example, in the United Kingdom, the Bank of England feeds back from an inflation forecast around two years ahead (King, 1997). The horizon of the inflation forecast (j) and

> the size of the feedback coefficient (θ), as well as the degree of instrument smoothing (γ), dictate the speed at which inflation is brought back to target following inflationary disturbances. Because they influence the transition path of inflation, these policy parameters clearly also have a bearing on output dynamics. (B/H, pp. 8-9)

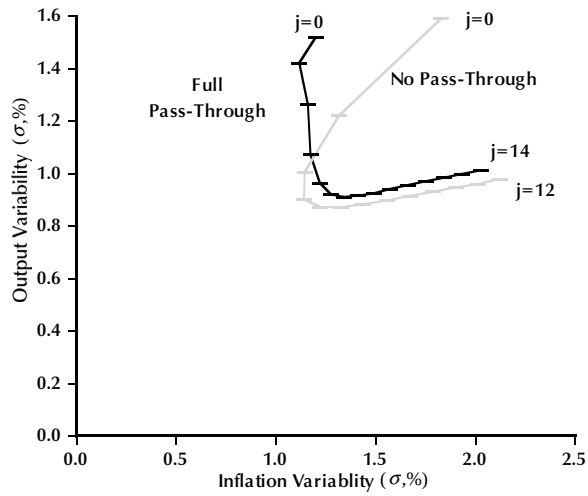
Of particular interest, both to the authors B/H and in this context, is what happens to the economy as the coefficient *j* (the forecast horizon) is varied. One feature of the model (and of the actual experience of open economies) is that the speed of the effect of monetary policy on inflation is very sensitive to the impact of interest rates on exchange rates and thence on imported inflation. So, as B/H state, they experiment with two versions, one "assuming full and immediate import-price pass-through (a shorter transmission lag); the other, no immediate pass-through (a longer transmission lag)." With the other two choice parameters, (γ, θ) set at their assumed 0.5 baseline values, and the calibrated values for the equations in the model, B/H obtain the resulting outcomes for output variability and inflation variability shown in their Chart 2, reproduced here as Figure 3.

As B/H state:

> Several points are clear from Chart 2. First, irrespective of the assumed degree of pass-through, the optimal forecast horizon is always positive and lies somewhere between three and six quarters ahead. This forecast horizon secures as good inflation performance as any other, while at the same time delivering lowest output variability. The latter result arises because three to six quarters is around the horizon at which monetary policy has its largest marginal impact. The (integral of the real) interest and exchange rate changes necessary to hit the inflation target is minimised at this horizon. So too, therefore, is the degree of output destabilisation (the integral of output losses). At shorter horizons than this, the adjustment in monetary policy necessary to return inflation to target is that much greater—the upshot of which is a destabilisation of output. Once we allow for the fact that central banks in practice feed back from annual inflation rates,

Figure 3

j-Loci: Full and No Pass-Through Cases



> whereas our model-based feedback variable is a quarterly inflation rate, then the optimal forecast horizon implied by our simulations (of three to six quarters) is rather similar to that used by inflation-targeting central banks in practice (of six to eight quarters).[3]
>
> Second, taking either pass-through assumption, feeding back from a forecast horizon much beyond six quarters leads to worse outcomes for *both* inflation and output variability. This is the flip-side of the arguments used above. Just as short-horizon targeting implies "too much" of a policy response to counteract shocks, long-horizon targeting can equally imply that policy does "too little," thereby setting in train a destabilising expectational feedback. (p. 28)

They also note further, in their conclusions:

> An inflation forecast-based rule, with an appropriately chosen targeting horizon, naturally embodies a degree of output-stabilisation. Moreover, any degree of output-smoothing can be synthetically

[3] This comparison is also not exact, because the two definitions of horizon are different: the feedback horizon in the rule and the policy horizon in practice (the point at which expected inflation is in line with the inflation target) are distinct concepts.

> recreated by judicious choice of the parameters entering an inflation forecast-based rule. There is no need for any *explicit* output terms to enter this rule. That is evidence of the output-encompassing nature of inflation targeting based around inflation forecasts. (p. 41)

A feature of this exercise that will be explored further in the next section is that the time path of interest rates (both nominal and real) that emanates (and can be predicted to result) from this approach (i.e., a rule-based response of deviations of forecast from target inflation at some future horizon) will neither be constant (i.e., unchanged from the present) nor necessarily, or probably, optimal. B/H did not attempt to specify a loss function, which the authorities might seek to minimize. It is, however, worth noting that a choice of horizon around four to six quarters, based on (annualized) quarterly (rather than average annual) inflation rates, minimized both output and inflation volatility. Using a quarterly, rather than an annual, inflation measure tends to lead to a shorter implied lag measure.

The Batini/Nelson Study

Subsequently in 1999, Batini and Nelson (B/N) have further extended this exercise in their work in progress at the Bank of England ("Optimal Horizons for Inflation Targeting," Draft July 1999). I hope that a revised version of this will be ready for circulation as a working paper reasonably soon. Unlike B/H, B/N do employ a loss function in which deviations of the output gap and of inflation from its target value enter, in the benchmark case with equal weight; it also incorporates a specific discount factor. To explore the optimal horizon question, they then look at several small models: a one-lag four-equation quarterly vector autoregression (VAR) and several variants of the calibrated model used in B/H.[4]

They approach the question of appropriate horizons using two alternative criteria: First they minimize the loss function to obtain an optimal policy horizon. Here the optimal horizon is not a choice variable, but is determined by the loss minimization exercise. The second approach, rather more akin to B/H, is to examine a simpler feedback rule (i.e., simpler than the OC approach) where interest rates are set in relation to the deviation of forecast future inflation from the target at period k; thus,

$$i_t = \Psi p\left(E_{t-1}\pi_{t+k} - \pi^*\right)$$

(see B/N, p. 24).

The question then is which is the best value of k, i.e., the value which will minimize the loss function (when a simple rule of this kind is being followed). But the choice of k obviously depends on Ψp, so that they term optimal feedback horizons (OFH) the choice of k when Ψp is also optimal and feedback horizons (FH) when Ψp is not optimal.

The loss that will be suffered in the economy depends on the nature of the shock that disturbs it (e.g., whether a demand, supply, or exchange rate shock) and on the assumed model, in particular whether the economy is supposed to be backward looking, as in most VAR models, or forward looking, as in some variants of the calibrated B/H model. This means, unfortunately, that the optimal policy horizon appears highly sensitive to shock-specific and model-specific factors; see for example their Table 4. By contrast the OFH works off the deviation of forecast from target inflation (whatever may cause that deviation) and so is in a sense immune to the identification of the disturbing shock, though it too is highly sensitive to the specific model of the economy adopted.[5] Using one of their earlier criteria for this, B/N then examine by what date (policy horizon [PH]) the economy, which follows an OFH, will achieve a convergence of inflation to the target. They find, in their Table 9, that the OFH is, in all cases, shorter than the PH; or, in simpler terms, if you want inflation to converge to target at $t+x$, you need, when using a simple rule adjusting for forecast deviations (from target), to work off deviations at $t+x-q$, where $q>0$.

It is *not*, however, also necessarily the case that the OFH is shorter than the *optimal* policy horizon (OPH). This OPH depends on the nature of the disturbing shock which has to be identified exactly to estimate an OPH, whereas in the estimation of the PH, B/N assume that the disturbance reflects the

[4] In assessing (optimal) policy horizons in such a model-based context, any stabilizing rule for interest rate decisions will bring inflation close to, but not exactly equal to, a point target. Consequently B/N define the horizon in various ways, either as the period when inflation converges to within a specified range around a point target, or when a given fraction of the initial shock is permanently eliminated.

[5] Essentially, the criterion used for deriving the OFH makes B/N select optimal k in the presence of all three shocks simultaneously in stochastic simulations, with the importance of each shock depending both on how strongly it enters the model's structural equations and on the shock variance-covariance matrix.

historical average of all the various shocks. The OPH depends on both the model and the shock. In general, the OFH is again shorter than the OPH in the face of demand or supply shocks, but in three of the four models it is much longer in response to exchange rate shocks.

Preliminary Conclusions

My own reaction to this work included some concern over (i) the multiplicity of variants, some of which (e.g., whether or not interest rate smoothing might be adopted) have been glossed over here, and (ii) the extent to which the numerical results seemed sensitive to the precise specification (e.g., of model, shock, and loss function). These concerns are not that the work is wrong, rather that it seems doubtful how much it can illuminate and ease the difficult decisions facing policymakers. In practice it is rarely clear how far an *actual forecast* deviation of inflation from target is due to one or another shock. How far such an exercise, whether to examine OPHs or OFHs, could be applied to the much larger and more complex models actually used in most real-life forecasting exercises, including those in the Bank, remains an open question. Whereas the B/H exercise gave some comfort (to me at least) that the policy processes in the Bank were pretty much on course, the B/N paper suggests (to me at least) that selection of (optimal) horizons is so model/context specific that little advance can be made unless such studies can be brought to apply to the specific model/context under consideration as used in practice by the MPC. Whether that can be done in practice has yet to be seen.

III. THE PROSPECTIVE FUTURE TIME PATH OF INTEREST RATES

The Prima Facie Case Against a Constant Time Path

As is well known, the MPC's forecast is conditioned on the assumption that, between the starting date of the forecast and the two-year horizon, the short-term nominal policy–determined interest rate will be held constant. Initially this assumption was adopted in some large part because the forecast was done by the Bank, but the future decisions were taken by the Chancellor, and the Bank could not be seen publicly to second-guess what the Chancellor might do in future.

But that quasi-constitutional constraint has gone. In formal constitutional terms the MPC can, if it so wishes, indicate its own future expectations. Meanwhile continuing to adopt a (formally non-binding) assumption might seem to limit artificially the range of options that could be considered, at least in public. It would only be by extraordinary accident that the best, optimal, plan would be to consider a change of interest rates today and then intend to hold rates constant at this (new) level for the next eight quarters.

Moreover, we know that holding nominal interest rates constant tends to lead to Wicksellian instability.[6] In practice, the rate of change of most variables visible at the two-year horizon in the Bank's forecast generally (though not invariably) tends to persist, and on occasion to accelerate, in the third and subsequent years. Consequently, any simulation, or other model exercise, with a horizon (much) beyond two years requires bolting on to the initial premise of rates being unchanged for two years—an auxiliary assumption that at some point (after the two-year horizon) the unchanged interest rate starting-point is shifted to the adoption of some stabilizing rule for nominal interest rates (e.g., a Taylor rule with appropriately chosen coefficients). Such a joint system might be described as spatchcocked, or at least inelegant (a term of some considerable opprobrium in academic circles!).[7] There are, however, other reasons to wonder whether the width of the fan charts is quite correct (e.g., it is based largely on historical averages). My own feeling is that, whereas the above criticism is formally valid, it is not a matter of great significance. I can happily live with it.

Finally, it is not surprising that *market expectations* are often for a future time path of interest rates that is far from constant. Not only will an assumed future constant path in the MPC forecast often not be seen as the most likely, or highly

[6] In the U.K., government expenditure plans are specified in nominal terms. Hence any shift in inflation implies offsetting projections for real government expenditures. This conditioning assumption can strongly dampen any tendency towards Wicksellian instability.

[7] A minor variant of the same criticism is that the inflation/output fan charts are misleadingly wide, especially in the second year. If inflation did appear to be diverging from target so much, surely the MPC would respond and such response would dampen the divergence (e.g., Flemming, 1999)? That criticism could, in principle, be met by applying a "rule"-based response to divergences (beyond a certain range). But, again, such a combination of a constant-path assumption for the modal (most likely) projection and a rule-based path for divergences could also be described as spatchcocked and inelegant.

credible outcome, but it also causes internal complications for the MPC's own forecasting process. Wherever current behavior depends on future expectations of interest rates, as is the case notably with exchange rates (e.g., under any form of the uncovered interest rate parity [UIP] hypothesis), then the MPC's forecast involves the prediction that the market's expectations will be systematically falsified, based on the assumption that short-term interest rates do remain constant. That means that the MPC forecast must contain a tricky assumption about how the market would react to a (systematic) string of falsified expectations—not an easy matter.

The MPC is fully aware that its conditioning assumption of constant future interest rates does not accord, most of the time, with the implicit forward rate expectations of the market, as for example derivable from a market yield curve. Therefore, it also publishes a fan chart of the projected inflation and output outcomes that it forecasts would result if the markets' (calculated) expected time path for interest rates was to take place (e.g., as shown in the February 2000 Inflation Forecast, Figure 4). Casual observation will, however, show that the deviation of forecast inflation in the MPC's own forecasts from its (2.5 percent) target is generally wider at the 18- to 24-month horizon for the forecast with (calculated) market rates than with the MPC forecast conditioned on constant rates. The implication is that policy making rests on the MPC's conditioned forecast, with the market rate forecast presented also to help inform outside observers of what might happen if the market's implied predictions were to occur.

So the forecast, conditioned on constant interest rates, is the more important for U.K. policy-making purposes. The case against the constant interest rate assumption has several facets: it is unnecessary; it would seem to imply often (if taken as a binding constraint rather than as a conditioning assumption) a probably suboptimal decision; it is often hardly credible as the most likely outcome; and it involves various forecasting and simulation problems.

As the first draft of this paper was being written, this criticism surfaced in an International Monetary Fund working paper by Martijn and Samiei (1999). They argue that

> The problem [of the credibility of the Bank's inflation forecast] is compounded by the absence of an explicit assessment of the likely future path of the interest rate in the Report. While more recently there has been an effort to emphasize alternative views held by MPC members, the primary inflation forecasts are made under the assumption of unchanged interest rates. In principle, there are many interest rate profiles that could deliver an inflation of 2.5 percent two years ahead. Clearly, and as discussed in the previous section, there is no reason to suppose that a policy that holds interest rates unchanged and delivers a two-year ahead inflation of 2.5 percent is necessarily superior to other policies.
>
> The Inflation Report, therefore, appears to lack transparency and credibility in relation to its inflation forecast. It is not obvious to what extent the inflation forecast based on constant interest rates is an expositional or an operational construct. If it is the latter, the MPC indeed does not intend to smooth interest rates, and consistently expects to hit the target at the newly set rate. In that case the framework is transparent, but, given a history of interest rate smoothing, it is not considered credible by market participants, as is evidenced by the deviating market forecasts…On the other hand, the constant interest rate assumption may merely be an expositional tool. In this case, the MPC in fact considers that further interest changes are likely to be necessary, even in the absence of news, implying that the MPC's forecasts lacks transparency as well as credibility. (pp. 15-16)

But What Alternative Procedure Would Be Better?

While the constant rate assumption undoubtedly has certain shortcomings, the question then becomes, What are the alternatives that might be proposed and are they preferrable?

One alternative would be to have the MPC decide, and vote, not just on the change in interest rates this month but also on the whole prospective path up to some (arbitrary) horizon in a discretionary mode. But there is a virtually infinite set of possible time paths, delivering convergence to the inflation target at a wide range of policy horizons. The space of choice becomes so great that it is hard to see how a committee could ever reach a

majority for any particular time path. A great advantage of restricting the choice to what to do now, this month, is that it makes the decision relatively simple, even stark. Given the difficulties involved already in achieving majority agreement in the MPC on this simple decision, the idea of trying to choose a complete time path by discretionary choice seems entirely fanciful and counterproductive.

That means that the future time path would have to be determined in some other fashion. A standard academic approach would be by optimal control procedures, i.e., minimizing a loss function applied to the forecast. There are a variety of problems with this. First, formally establishing such a loss function, unless it was agreed by the Chancellor, might be seen as the MPC abrogating the right to select its own (short-term) goals; it could be thought to involve a "democratic deficit." Against that, it could be argued that the Chancellor's letter, as outlined previously, does provide some tightly limited room for discretion by the MPC to apply a (short-run) loss function, recognizing that "the actual inflation rate will on occasions depart from its target as a result of shocks and disturbances. Attempts to keep inflation at the inflation target in these circumstances may cause undesirable volatility in output." That discretion is limited by the requirement to write a letter to the Chancellor if inflation deviates from target by more than 1 percent. If, in such circumstances, the MPC proposed to return inflation to target more or less rapidly (than the Chancellor wished), given the projected effects of that on the prospective path of output, the Chancellor could tell the MPC to adjust its plans according to his own preferences.

Second, it might be difficult for a committee to agree on any formal functional representation. The coefficients in the function would be somewhat arbitrary (and what would be done about the standard central bank practice of interest rate smoothing?). Moreover, membership of the committee is time-varying, and existing members may find that their views about the (short-run) loss function shift as the context changes. In short, choosing a formal loss function might need to be revisited on each relevant occasion, running into exactly the same problems of complexity for decision-making that were outlined above.

Third, it is not clear that optimal control procedures could be applied in practice to larger, messy forecasting models incorporating a wide variety of subjective assumptions, residual adjustments, and such other discretionary adjustments as the MPC applies to its own forecast. It is far from clear that such techniques can make the large jump from small (two to six) equation models, with often styilized shocks, to real live forecasting models with a much messier context. That still has to be seen.

Fourth, if such optimal control procedures were applied to the forecast, the resulting outcome of time paths for interest rates, inflation, etc., would become a hideously complex interaction of forecast and OC procedure. It is already difficult enough for MPC members to understand all the nuances of their own forecast, even when conditioned on relatively simple assumptions. Introducing OC procedures as well might lead MPC members to regard the whole exercise as a mysterious "black box" whose entrails were only comprehensible to a tiny number of staff academic specialists.

Fifth, if the MPC should find it more difficult to understand how the resultant outcome for the relevant variables had been determined, how would it be possible to explain it to the public, or to justify the decisions that would hang in part from it? To say that we have done what our model told us was best to do is not very convincing, especially given the track record of fancy economic models.

Those considerations suggest that, for the time being, a no-change assumption for interest rates could *not* be replaced by a full optimal control exercise, but would need to be replaced by some simpler rule, e.g., a forward-looking Taylor rule. That runs into the difficulty that there are a plethora of such potential rules. The R/S paper described in Section I mentions at least six types of rules (inflation deviation, the Taylor rule, a forward-looking Taylor rule, an instrument rule working off a rule-consistent inflation forecast, a strict inflation target, and a flexible inflation target). Each rule would need auxiliary decisions on parameters, including interest-rate smoothing and horizon. The optimality of any rule is almost certainly model dependent. Using a simplified model in the U.S., R/S have done some work on what might be the best buy amongst such rules. What might be the best choice of rule for the U.K. in the context of our actual forecasting model is far from clear. Much more work on how the time paths of the relevant variables (including interest rates) might look if various alternative rules were to be adopted would seem to be required before

one could be confident that one was not jumping out of the frying pan into the fire, if one was to replace the constant assumption by a future "rule."

Moreover the adoption of any such "rule" would not eliminate some of the criticisms applied to the constant interest rate assumption. For example, the "rule"-related interest rate path would still normally deviate from market expectations, thereby requiring some kind of expectation correction mechanism to be applied to the exchange rate forecast. Second, in some circumstances a "rule"-based forecast for the time path of interest rates might have as little, or less, credibility as a constant forecast, e.g., in the aftermath of an asset price shock. Consider for example whether a "rule"-based forecast would have given a credible projection in autumn 1998 in the aftermath of the Russian default, the Long Term Capital Management (LTCM) hedge fund crisis, and the weakening of business confidence.

Moreover a "rule"-based forecast shares some of the potential faults of an optimal control forecast. It adds to the complexity of the forecast and makes the outcome less transparent to all concerned. It thereby makes it harder to explain and to justify to the general public. Nevertheless the constant conditioning assumption does play some part in guiding the decision on interest rates; at least I know that it did so in my own case. As already noted, there are other paths for interest rates that would not only achieve the target at the 18- to 24-month horizon, but also would at the same time potentially improve some subsidiary objective.

Let me consider two cases. First the time path for output might be projected to be unstable. If interest rates were planned to be, say, lower initially, and then subsequently higher (than the constant) path, it could simultaneously achieve both the inflation target and a smoother path for output. Second, certain asset prices might cause concern because of the potential effect of "boom and bust" on both the economy and on the financial system. Say housing prices were rising much too fast. Then a policy of higher interest rates in the short run, followed by (an expectation of) lower rates thereafter, might be desirable. So the question of whether one should necessarily condition the forecast on an assumption of future constant (short-term) interest rates actually maps into (overlaps with) the issue of whether, and how, one might take asset prices into consideration in determining interest rate policy (see Cecchetti et al., 2000).

In this context, simplicity is a virtue. One simple approach would be to augment the forecast based on the constant conditioning assumption, with two other modal forecasts for output and inflation.[8] In these two latter forecasts, the short-term interest rate would first be 0.5 percent lower/higher, respectively, for say the next three quarters,[9] and then raised over the following five quarters to whatever level was calibrated as necessary to hit the inflation target on average between 18 and 24 months hence.[10]

The publication of three alternative interest rate paths, each of them consistent with achieving the inflation target at the 18- to 24-month horizon, might lessen the pressures to act *now*, since even with no further action immediately in prospect the target could still be hit. As we shall discuss further later, this reduces the discipline on the MPC itself. In return, and in exchange, it would provide the MPC with more flexibility to pursue subsidiary objectives (as already noted). Finally, although it is a simple and almost Spartan proposed mechanism (involving Charts A through C, a mock-up of which is shown in Figure 4) that is based on simple (but arbitrary) conditioning assumptions, it could still be subject to the criticism of fine-tuning.

Flexibility and Commitment

If one adopts a "rule," there is some implication that one will follow that path. The constant, no change, assumption is clearly exactly that—an assumption not a rule. No one infers any commitment from the MPC to abide by that assumption in the future, nor is the credibility of the MPC damaged when, having made this assumption in a forecast one month, it decides to change interest

[8] One could reasonably assume that the variance and skews would be much the same in all three cases.

[9] Two quarters would, perhaps, be too short to show much relevant difference. Four quarters would probably require too sharp a subsequent reversal to hit the inflation target given lags in the transmission mechanism.

[10] It should, however, be noted that this proposal, which emphasizes the future prospective *reversal* of a current move, is almost the exact opposite of Woodford's (2000) proposal for emphasizing inertial prospective continuations. He places weight on the importance of influencing long rates by commitment to a series of moves in short rates. If the above prospective reversals were credible, then the temporary prospective move in short rates would have very little, or no, effect on long rates. Whether that matters depends, in some large part, on whether the transmission mechanism runs largely via changes further out along the yield curve.

rates even in the next month if the "news" should appear to warrant it.

There *is* something special about zero, no change. Any indication that the MPC is formally indicating a future specific change in rates (e.g., as driven by a "rule"-based formula) would be taken to indicate some degree of commitment. It is my view that the few experiences in the past where the MPC has given *any* measure of commitment (e.g., in August 1997) were soon felt to be burdensome and unhappy.

Moreover, the extent of any such commitment may be interpreted differently by the market and even by different members of the monetary authority itself (Thornton and Wheelock, 2000). It appeared, from a distance, to be a problem for the FOMC in the summer of 1999 that its statements about bias were at times misinterpreted by the market, requiring subsequent speeches by the Chairman to try to rectify.[11] Some observers also say that the fact that the Fed is not required to announce the bias at the end of each meeting has led the markets to interpret excessively the importance of the decision to announce bias. Economists say the statement has almost no utility as a predictive device since it is likely to give an indication of the next move in interest rates only 30 to 50 percent of the time. It was for reasons such as this that the MPC responded to the Treasury Select Committee of the House of Commons that it had no wish to publish a statement about future bias.[12]

A bias statement is, I would suggest, much less binding on future decisions than a quantified projection for a future path of interest rates. Unforeseen events (shocks) are likely to make the MPC depart from any such prefigured path. This may well be seen by markets as reneging on a prior commitment, and lead to accusations of misleading the market. It would be extraordinarily difficult to decide amongst ourselves, or to explain to the markets, just what extent of commitment was involved in following a rule-based path. This was less of a problem for the Reserve Bank of New Zealand since they projected a Monetary Conditions Index (MCI), which specified a complex combination of future expected developments, without implying any specific future path either for interest rates or for the exchange rate itself.[13] The authors of the IMF working paper, Martijn and Samiei, are aware of this problem, but do not, in my view, give it sufficient weight; they assume this difficulty away when they state (p. 16), "Obviously, the Bank would have to make it clear it was not committing itself to a particular path, so that without loss of credibility it might revise its projection at a later date as new information becomes available."[14]

By contrast, a constant interest rate assumption is generally perceived as involving no forward commitment whatsoever. So it imposes no constraints on the MPC's future decisions, leaving the MPC to respond with maximum flexibility to unforeseen events as they occur.

There is a clear division of views about the degree of forward signaling of, and market

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[11] This was underlined, even as the first draft of this paper was being written, by a report that the "Fed [was] to examine policy bias announcements" in the *Financial Times*, December 14, 1999, p.11, by S. Fidler in Washington who reported as follows: "A policy step taken by the Federal Reserve earlier this year aimed at increasing the openness about its decision-making has confused financial markets, the central bank has admitted…Despite the aim to increase transparency about the Fed's activities, and therefore to calm market volatility, the bias statement is widely acknowledged to have had the opposite effect. Critics have suggested that this is because of widespread confusion about what the bias statement means, confusion that appears to be shared by some Fed officials."

[12] In any case the arguments in the minutes of the MPC meetings and the recorded individual votes give a considered view of the current inclinations of the MPC. For example the minutes of the April 2000 meeting (¶ 29-32) report the differing views of the members of the MPC. For example ¶ 31 states that "On one view, it would be better not to raise rates this month. The news over the month as a whole was inconclusive, with falls in manufacturing production, retail sales and consumer confidence, and oil prices, but with the determinants of domestic demand, both private and public, remaining robust. Against that background other factors were also important. First, the extent of price pressures stemming from the labour market required further analysis, not least in disentangling the effects of bonuses and other elements of wage drift from that of settlements. Second, the analysis undertaken for the *Inflation Report*, and the opportunity this provided to set out the Committee's thinking in detail, were valid reasons not to move this month unless there was a strong case to do so. Third, for some, the volatility in equity markets introduced a possible downside risk which might mean that any increase in rates this month would need to be reversed soon afterwards. Finally, the imbalances in the economy, manifested in another fall in manufacturing production, seemed to have worsened. An increase might exacerbate these imbalances and it was possible that the weakness in some sectors might feed through into the rest of the economy. With inflation running below target, and expected to continue to do so for a while, there was no pressing reason to raise rates straightaway. For these members, no change in the repo rate was needed this month, although for some of them it was more likely than not that there would need to be an increase in rates in due course."

[13] While the Reserve Bank of New Zealand has, subsequently, abandoned the use of MCI, it has replaced this with a quantitative indication of the expected future path of short-term interest rates. Whether financial markets will feel aggrieved when, and if, interest rates depart from this prefigured path has yet to be seen.

[14] In the next sentence they state that "The Bank could also include outside projections of inflation in the Inflation Report." We already do, and the Inflation Report is the responsibility of the MPC, not the Bank alone; coverage of the two differs.

Figure 4

Chart A
Interest Rates

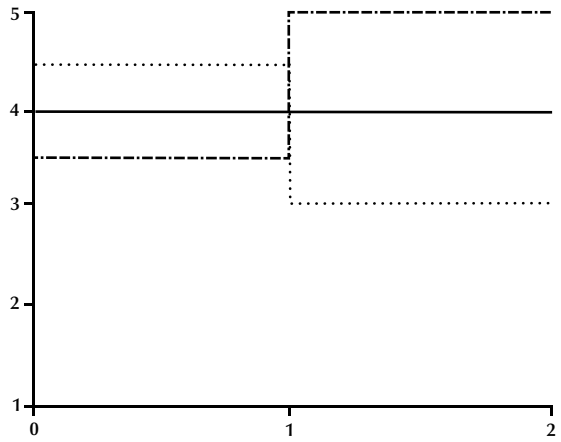


Chart B
% Growth Output

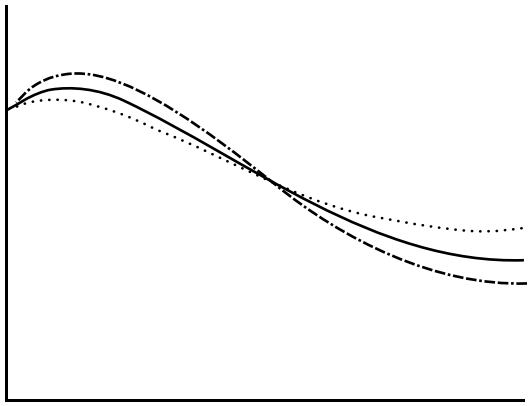
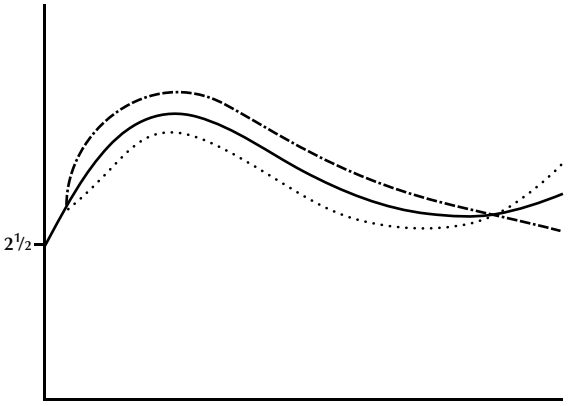


Chart C
% Level of Inflation



preparation for, future interest rate changes that (independent) central banks regard as suitable. The MPC certainly wants the market to be able to predict its future moves, and I believe that its members were not unhappy by a report that a computer had been programmed on an in-sample basis successfully to do so. (The program's out-of-sample subsequent predictive ability was less good!) Indeed the MPC wants monetary policy to be seen as "boring." But the MPC wants the basis of such predictability to be a considered assessment of its "reaction function" based on published accounts of members' actual votes and detailed minutes of the reasons for such votes, rather than more speculative comments in advance of the actual decision-making meetings of how (individual) members might vote. Given the large weight placed on individual accountability in the MPC and the inherent unpredictability of economic "shocks," any attempt to foreshadow in individual comments the prospective future decisions of the MPC as a whole would be seen as potentially embarrassing hostages to fortune.

In contrast, the Fed and ECB appear to consider the preparation of the market for future interest rate changes, for example by public statements by the Chairman, to be often desirable. This may reflect in part a tendency of the Fed and the ECB to have a more collegiate, consensual bias to decision-making, whereas the MPC places more weight on individual responsibility. It would be an interesting exercise to try to ascertain whether central banks differ significantly in their propensity to signal future interest rate moves, and, if so, what might be the causes and consequences of such differences. But that exercise has not yet been done.

How one might define "a signal" would itself be difficult to determine. In the absence of such an exercise, further discussion of "signaling" would take me beyond the self-imposed limits of this paper.

What Are the Consequential Incentives for Behavior?

Publishing the forecast for inflation, together with the assumption of constant interest rates from the forecast date to the horizon, has the effect of inducing the MPC to act now to adjust expected inflation at the appropriate horizon into line with the target. If the MPC could choose a time-varying path for interest rates, it might be more inclined to

defer interest rate changes until it was possible to learn more about evolving conditions.

There is a developing debate in the academic literature between those who suggest gradual changes in interest rates (for example, because of uncertainty about key parameters in the model [Brainard uncertainty], about possible errors in the data, and about opportunities for learning) and those who argue for more aggressive and preemptive action (e.g., the need for robust policy responses and minimizing potentially large errors). It is my conjecture that the choice of a constant versus a time-varying interest rate path has similar implications. The assumption of a constant interest rate path (plus a published forecast) may be thought to induce the MPC into early preemptive action. Whether or not "gradualism" or "preemptive" action is generally preferable is too large a subject to enter into here. My conjecture is that our procedure tilts us in the direction of early, preemptive actions, and my personal viewpoint is that this is desirable.

The working paper from the IMF is less than appreciative, or fair, in this respect. The authors do not seem to realize that publishing the forecast in this way forces us to act *now* on interest rates in order to drive inflation to its target level 18/24 months hence. Instead they question whether the forecasts are "credible." Thus they state:

> Since the MPC took over the job of monetary policy decision making, the analysis and the inflation forecasts reported in the Inflation Report cannot be treated as those of an independent reviewer: the body that makes interest rate decisions also assesses these decisions. As a result, it would be difficult to envisage a situation where the forecasts suggest that the two-year ahead inflation, on which public scrutiny tends to focus, would (in probabilistic terms) be missed, because then the report would in fact be questioning the committee's own policy decisions. Indeed, since the Bank became independent, two-year ahead inflation has always been around the target. While this is possible logically, it raises questions as to the credibility of the Bank's inflation forecast. (p. 15)

In so far as this suggests that, rather than adjust interest rates to hit the target, we manipulate the forecast to appear to be hitting the target, it is a slur which members of the MPC would find offensive. Moreover in the large scale exercise involved in such a forecast, involving many Bank staff as well as all the MPC, such cosmetic manipulation could not get done without notice and huge risk of becoming publicly known.

One syndrome that I sometimes seem to perceive (both in myself and occasionally also in others) is the belief that the quantum of uncertainty (fog) will be reduced by waiting (e.g., to make a decision). Current shocks, implausible data points, and unintelligible anomalies are always very clear. But the *mean* expectation of future shocks is zero; hence, some of us may tend to assume implicitly that they will actually turn out to be zero. If that were so, uncertainty would indeed reduce over time. But subject to a qualification below, the distribution of shocks is *constant* over time; so, the best expectation of the future quantum of uncertainty is that it will be *constant*. If, as I believe, uncertainty myopia exists, then a procedure forcing an early decision may well be desirable.[15]

When I was a member of the MPC I thought that I was trying, at each forecast round, to set the level of interest rates so that, without the need for future rate changes, prospective (forecast) inflation would on average equal the target at the policy horizon. This was, I thought, what the exercise was supposed to be. But, if so, one might have expected short-term interest rates to follow, at least to a reasonable approximation and from quarter to quarter (*not* month to month), a random walk. But, in practice, changes in interest rates appear to have been just as serially correlated and gradual thus far under the MPC as in previous regimes and in other countries. If, however, we recognize that the appropriate periodicity is quarterly, then there have been only 12 observations of the MPC period, which is short for any formal testing of whether there was any significant shift in the extent of serial correlation.

Nevertheless my (casual) observation suggests there was not. Why? My preliminary hypothesis is that, during this period at least, there was some

[15] There may be some seasonal patterns in uncertainty. There is some tendency in the U.K. for large negatively correlated movements in consumption in December and January, so assessment of consumption trends may be better left till late winter. Again, wage settlements and bonus payments tend to be concentrated in the winter and spring, so there is more uncertainty in December-June than in July-November. There are other instances of time-varying uncertainty relating to holidays, budget dates, etc. More research could be done to ascertain whether such seasonal patterns in uncertainty really do exist; it would be a nice simple exercise.

Table 1

Error Made in Forecasting

Date of: Forecast	Date of: Outturn	Output	Inflation
November 97	Q1:98	–0.58	0.08
February 98	Q2:98	0.64	0.32
May 98	Q3:98	0.42	0.20
August 98	Q4:98	–0.58	–0.03
November 98	Q1:99	–0.10	–0.03
February 99	Q2:99	0.67	–0.23
May 99	Q3:99	0.91	–0.12
August 99	Q4:99	1.05	–0.03
November 99	Q1:00	–0.18	0.09

NOTE: Positive values imply an outcome greater than forecast.

serial correlation in the forecasting process itself, i.e., that, whenever inflationary pressures were on the upward tack, or alternatively the downward tack, the forecasters initially underestimated the extent of such pressures. If so, the forecasters were playing "catch up," and that led the MPC into a series of auto-correlated sequential decisions, although the MPC was not itself consciously trying to be "gradual" or inertial.

There is some plausibility in this hypothesis. By definition forecasters cannot predict shocks, such as that which hit in the late summer of 1998 or the sharper than expected recovery in 1999. Uncertainty, herd instinct among forecasters, and extrapolation from the past are all likely to make the expected (forecast) path smoother than the actual path. How far the serial correlation in interest rates derives from serial correlation in forecast errors, rather than from a conscious decision-making process, has not (yet) been properly researched, and I did not have the time/capacity to do it for this paper.

There would be considerable difficulties in doing this exercise in any case. If you take, say, the forecast *two years* hence and compare it with the *ex post* outcome, then, in the interval, both monetary and fiscal domestic policies will have changed, often sharply, from those that conditioned the original forecast. Should one do a counter-factual by trying to adjust the outcome for the intervening policy changes? But those policy changes will, in turn, have partly (largely) been in response to shocks that the original forecasters could not have foreseen. On the other hand, if you take a very short-term forecast, say, over the coming quarter, there is usually sufficient inertia in the system that it is not a "model" forecast at all. Anyhow, in the one simple exercise that I asked to be done, the emphasis has been put, rightly or wrongly, on the *two-quarter*-ahead error in the forecast.

This is much more difficult to do for output than for the RPIX series, because the latter never gets revised, and the MPC puts weight on the latest actual figure. For output, the data get continuously revised, and the latest datum point at the time of the forecast is, itself, provisional. Moreover the quarterly output series are very noisy, more so than the annualized RPIX series. Even so, I asked the Bank staff to work out the MPC's forecast errors for output and inflation two quarters hence. Given the short period, noise in the system, etc., the results can be at most suggestive. They are noted in Table 1. Note that there is some slight suggestion of inflation predictions being revised upward in the earlier part of the period *despite* a series of interest rate increases; then, this same pattern reverses in the middle of the period. This is consistent with my hypothesis that "gradualism"—the auto-correlation of interest rate movements—was, at least during this period, partly a function of the forecasting process and *not* of the decision-making process. It would, however, take much more careful empirical work to discover whether this was also the case in other regimes and countries. I just do not know, but I think it an exercise worth exploring.

IV. ON SKEWS AND ASSET PRICES

When the Bank was preparing its inflation forecast as background for the Chancellor's decision on interest rates, the Bank could consider risks to its forecast in an unconditional way and to the best of my belief did so. But once the Bank obtained operational independence, the situation changed, since the question of what risks it would take into account would affect the decision itself. Let me first outline why it would affect the decision and then go on to argue that one would want the current decision to be affected in different ways, in some cases not at all, depending on the particular characteristics of each kind of risk.

When the probability distribution is symmetric, all three measures of central tendency (mode, median, and mean) are the same. In such cases the variance of the distribution, the extent of

uncertainty in the forecast, tends to play virtually no role in the interest rate decision. Discussion about forecast variances in the MPC were usually brief and uncontroversial; in this respect there was a kind of certainty equivalence. Pure variance was an academic issue.

This was not so for risk, because when we (as MPC members) think of risk, we are generally concerned with asymmetric possible outcomes. Asymmetry implies skew, and skew drives the measures of central tendency apart. The question of which measure of central tendency one should focus upon depends on one's own individual loss function.

Most forecasts, including that of the Bank, concentrate on the most likely outcome, i.e., the mode, and usually consider "the risks to the forecast" at the tail end of the exercise. But the interest rate decision should be taken only on the basis of the modal outcome if the occurrence of *any* deviation from the actual modal forecast, however small, is taken to be catastrophic. The usual academic assumption is that the loss function is quadratic in deviations from the desired outcome, in which case the focus should be on the mean forecast (not the mode). In my own case, I could never see why a 2 percent deviation from desired outcome was four times as bad as a 1 percent deviation, rather than just twice as bad. So I tended to concentrate on the median forecast, while others of my colleagues focused on the mean.[16] But in either case the existence of a skew would affect our decision on the appropriate interest rate. Unlike uncertainty and variance, skew and risk mapped directly into the interest rate decision. So, discussions about risks were policy-relevant and not just academic.

But should all perceived risks affect policy now? I would argue not. Let me take some examples. Assume that there has been an ongoing "bubble" in housing prices, which have been driven far out of line with (perceived) fundamentals. The inflationary effect of this is, I assume, offset by other deflationary forces (say higher productivity) so the modal forecast is in line with the target. The risk is that the bubble will burst, and when it does this will be deflationary. So, if you take account of this risk in the forecast, the mean is driven below the mode. So the implication is that interest rates should now be cut!

This strikes me as a nonsensical response. The contrary argument is that faced with such an asset price bubble, interest rates should be (temporarily) raised in order to check the misalignment and to prevent it from getting further out of hand and presumably collapsing with an even bigger detonation in the future.

This is *not*, alas, a hypothetical problem. Housing prices in 1988-90 in the U.K., equity prices in the U.S., and exchange rates in the U.K. all raise aspects of this question of how to deal with risk and skew. The MPC was criticized in the press after its May 2000 inflation forecast for not including the biggest risk of all, that of a sharp fall back in the exchange rate (notably against the euro), in its specific quantitative forecast. But if it had, that by itself would have implied that interest rates should have be tightened immediately.

An astute reader will by now have noted that this subject relates back to the earlier issue of conditioning on a constant future interest rate assumption. The point is that there are some categories of risk which should not involve countervailing action now, but only if and when they actually occur. In the meantime there is a case for nudging rates in the opposite direction, if that should help to reduce the misalignment and the risk and scale of the subsequent hard-landing collapse. This latter case was made in Cecchetti et al. (2000); it remains highly contentious. It is not the purpose of this paper to take sides on that (and anyhow I am ambivalent). All I want to argue is that there exists a sizeable category of risks whose future potential effects should be ignored in setting interest rates today.

Note that, so long as the inflation forecast continues to be conditioned on future unchanged interest rates, quantitative account of such risks are better omitted from the fan charts of output and inflation. The Bank was actually right to do just this in its May 2000 forecast. If *all* risks are to be included in the inflation forecast unconditionally, then the quid pro quo would have to be some discussion of future, non-constant paths for interest rates. As earlier described, this has some serious drawbacks and disadvantages. But the choice seems clear enough to me. Either condition on constant interest rates and exclude a certain category of

[16] Commentators might expect the nature of the (individual member's) loss function to be affected by the 1 percent deviation from target, acting as a trigger for writing a letter of explanation and intent to the Chancellor. I can only say that in my own case it did not. Perhaps this was because inflation remained during these three years so close to the target, both currently and at the forecast horizon, that the possibility of having to write such a letter remained more hypothetical than immediate.

risk from the (formal) forecast or include all risks unconditionally and describe how the future path of interest rates might change over time in response.

What are the characteristics of the risks that should *not* lead to countervailing interest rate adjustments now? The first, and perhaps most important, is that the event of the risk coming into effect should be clearly observable. Examples are political or natural disasters (war in the Middle East, earthquakes in Tokyo, hurricanes in Washington, Liberal Democrats getting elected in the U.K. [only joking], etc.), and sharp changes in asset or commodity prices (exchange rates, oil, equity and housing prices, perhaps certain food prices).

Examples where the event is not observable, observable only with great difficulty, or observable too long after the event to be useful are changes in the non-accelerating inflation rate of unemployment, in future (trends in) productivity, in competitive pressures on margins (e.g., from the Internet), on international competitiveness, etc., etc. In so far as there are thought to be asymmetric risks attached to these, you need to take them into account now 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.

A second, main characteristic of the risks that policy should *not* try to preempt is that they are low-probability events with a high payoff, if they should transpire at all.[17] Fortunately the list of clearly observable risks and low-probability/high-payoff risks overlap rather closely.

Let me again take some examples. You should not run a systemically mildly inflationary policy because there is a non-zero risk of a 1929 (or a Japanese) collapse in asset prices. Nor should you run a systematically mildly deflationary policy because there is a non-zero chance of an exchange rate collapse or of entering an inflationary war.[18] Similarly the distinction between observable and not-observable risks is not so easy to draw.

As far as I know the academic economics profession has not yet provided much help on the question of how to respond to asymmetric risk. Error terms, whether related to additive or multiplicative uncertainty, are universally taken to be symmetrically distributed. Standard practice is to run a forecast as if it was the mean of a symmetric distribution and then to add an asymmetric risk as an ad hoc afterthought.

Help on how better to treat asymmetric risks would be much appreciated. Until such assistance arrives, I stand by my view that some such risks, i.e., clearly observable, low probability/high payoff risks should *not* induce current countervailing action. This conclusion has, as already noted, some implications for the conduct and public presentation of the forecast process itself.

V. CONCLUSION

It will certainly be desirable to do further research on what either an optimal control or an assorted rule-based model would imply in the context of U.K. forecasting systems. But that should initially be for internal use only. Whether or not such further research might lead the MPC to change its forecasting assumption about a constant interest rate path is far from clear. Until such time, for the reasons set out above, there seem to me to be good grounds for maintaining present procedures in this respect. Nevertheless I would see some merit in exploring the presentation of an alternative, but equally arbitrary, set of scenarios, as in the charts in Section III (i.e., Figure 4).

Unless some such move is made away from the constant assumed time path of interest rates, those risks which should only evoke a policy response in the (unlikely) event of them occurring will have (to continue) to be excluded from any quantified inclusion in the inflation forecast. Only if the time path of interest rates can be presented as "conditional" can risks be included in an "unconditional" way.

A proper research exercise should be commissioned to examine whether the auto-correlation observed in interest rates has been due to the forecasting process rather than to the decision-making process.

In so far as the assumption of a constant time-path for interest rates was taken as an effective

[17] Low-probability, low-payoff risks will get ignored.

[18] Considerations of this kind make me somewhat unhappy also about the arguments of those who argue for "robust" policies (e.g., Sargent, 1999, and Stock, 1999). They suggest setting policy so as to minimize the likelihood of having a really bad outcome (a form of minimax policy). Since the worst outcome, from the point of view of a central bank, is to find oneself powerless to prevent worsening deviations from target, their policy prescription is to be more aggressive. But if I give a (very) low subjective probability, say 2 percent, to such an outcome (especially if I can observe it should it happen), does it make sense to aim consciously for a poor outcome (98 percent of the time) in order to offset a real disaster (2 percent of the time)?

constraint, rather than just a conditioning assumption, it would have the effect of inducing the monetary authority to choose a probably suboptimal and often noncredible path. But if correctly interpreted as just a conditioning assumption, it simplifies the decision-making process, making it easier to understand and to explain publicly; provides the greatest flexibility as well as minimum forward constraint on the MPC's future actions; and encourages the MPC to act in a robust, preemptive fashion.

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Commentary

Laurence H. Meyer

It is a pleasure for me to participate in this conference in honor of Darryl Francis. In the late 1960s and early 1970s, when Darryl Francis was president of the Federal Reserve Bank of St. Louis, I had recently arrived at Washington University. I had just completed my graduate work at MIT, primed during those years for combat with the monetarists at the Federal Reserve Bank of St. Louis. Needless to say, I engaged in many vigorous debates, often with Leonall Anderson and Jerry Jordan, and often at conferences presided over by Darryl Francis. I can only imagine the degree to which Darryl Francis enlivened the FOMC meetings during this period.

It is also a distinct pleasure to comment on this stimulating and thought-provoking paper by Charles Goodhart. It is just what many of us hoped we would be treated to when Charles returned to full-time academic pursuits after his experience as a member of the Monetary Policy Committee (MPC) at the Bank of England—an insightful assessment of the strategy and process of monetary policy-making by the MPC.

Charles covers a lot of ground in this paper, challenging the ability of even a fast-talker like me to do justice in a short comment. I will focus on three questions that Charles poses, with most of my attention on the first two questions, which form the centerpiece of his paper.

1. Is the MPC's implicit two-year policy horizon the optimal one for their inflation targeting approach?
2. Is the MPC's practice of conditioning its forecast on a constant path for the nominal short-term interest rate appropriate?
3. Under what circumstances should policy today respond to asymmetric risks in the outlook?

THE POLICY HORIZON

Charles starts by asking what the relationship is between the lag in the response of the economy to monetary policy actions and the policy horizon in the inflation targeting approach at the MPC. My immediate response was that this was an interesting question, and I looked forward to the analysis and answer. Unfortunately, Charles did not follow up in this direction very effectively. He could, for example, have varied the length of the lag of the response of the economy to monetary policy in some of the models he considered and traced the effect on the optimal policy horizon.

Instead he reviewed a series of studies, each of which offered multiple simulations, leaving us with a smorgasbord of results. This led Charles to conclude, correctly I believe, that the appropriateness of the two-year policy horizon of the MPC is unclear because the optimal horizon is so sensitive to the shocks, the model, and the loss function.

This should not suggest that Charles's analysis was a dead-end. The point, it seems to me, is that one cannot determine a single optimal policy horizon for an inflation targeting approach precisely because the optimal horizon should be variable, depending on the source and size of shocks, which affect the speed of the desired return to the inflation target. I do not read this as the conclusion that Charles reaches, but it seems it is a conclusion that follows from his careful analysis and survey of the literature.

The fundamental problem with the selection of an arbitrary policy horizon in an inflation targeting approach is its inability to deliver on its raison d'être. The policy horizon in an inflation-targeting approach is a device for taking into account the unacceptable output variability that would follow from an excessively rapid return to the inflation target following a departure from the target. Charles approvingly quotes Batini and Haldane: "[A]ny degree of output stabilization can be synthetically recreated by judicious choice of parameters entering an inflation-targeting formula. There is no need for any *explicit* output terms to enter this rule."

This may be true on average over some period, but it is clearly not true episode by episode, depending on the source and size of the shocks. The ability of forecast-based rules that exclude the output gap to achieve any desired level of output stabilization by adjusting the forecast horizon is not robust across models, and there are several cases in which the cost of excluding the output gap are quite significant.

It seems to me that the interesting question here is, Why use an imperfect, synthetic device to

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allow for output stabilization when you can use the real thing, that is, take output variability directly into account? The answer I sometimes hear—usually from policymakers at inflation-targeting central banks—is that admitting that a central bank carries out its monetary policy in part to stabilize output would undermine the public's confidence in its commitment to price stability. But this would be a strange answer from a central bank that prides itself on its transparency.

Let me compare the MPC's well-articulated inflation targeting approach to the Fed's less well-articulated strategy. First, the Fed's mandate, given by the Congress, is to promote full employment and price stability, an explicit dual mandate, with a clear directive to worry about output stabilization. Given that policymakers inevitability face a trade-off between output and inflation variability and the public cares about both, this seems to me the right way to frame a central bank's objectives. In addition, I don't see any reason to saddle the Federal Open Market Committee (FOMC) with an arbitrary horizon for returning inflation to the target—that requirement that would force us to deviate from the optimal policy response to a given shock that balances the two objectives.

THE INTEREST RATE PATH IN AN INFLATION FORECAST

The next question tackled by Charles was the appropriateness of the fixed short-term interest rate path in the MPC's inflation forecast. I thought Charles did an admirable job of presenting the prima facie case against this approach. I was convinced.

This approach is unnecessary, suboptimal, and may undermine the credibility of the MPC's inflation forecast. It reflects a historical precedent that no longer applies, as well as institutional inertia and an over-emphasis on simplicity.

Let's model the constant nominal interest rate path in this inflation targeting exercise as a variant of the Taylor rule. It sets the real interest rate as a function of the (expected) inflation rate with a coefficient of –1, assuming there is some variation in (expected) inflation over the policy horizon. This leads to an immediate insight. It would be better to set the coefficient to zero. So a still-simple, but I believe clearly improved, approach would be to set a constant real interest rate.

Still better, it seems to me, would be for the MPC to set the entire path for the real interest rate as consistent with its forecast and policy objectives. Charles's conclusion is that such an approach, while desirable in principle, simply would not be practical, given the likely difficulty the committee would face in reaching a consensus about the full path of interest rates. But one has to be concerned if the policy process is set up in such a way that it potentially distorts the current policy decision as well as the communication to the market about prospective policy.

Let me now shift to the FOMC practice, because the staff has to take into account the interdependence between the forecast and policy process. The FOMC makes forecasts only twice a year, when a central tendency is reported; the forecast is, at most, two years out based on members' forecasts of fourth-quarter-to-fourth-quarter inflation and output growth and the fourth-quarter level of the unemployment rate. The staff makes the forecast presented at each FOMC meeting. The FOMC is a more eclectic group than the MPC—fewer economists and fewer members who would be comfortable putting together a forecast. We have an implicit deal with the staff. We let them put together the forecast without interference and we don't promise to use their forecast to make our policy decisions. I am not sure it makes sense, in fact, to require the policy committee to reach a consensus on the forecast. Policy decisions of the respective members should reflect their individual judgment about the outlook as well as their individual policy preferences.

The staff, on the other hand, faces a dilemma similar to that faced by the MPC before the Bank of England received its operational independence. How is the staff to make a forecast without some assumption about the path of the policy rate? How can the staff specify the path for the policy rate when this would appear to prejudge the outcome of the policy decision?

This dilemma is sometimes resolved by the staff conditioning its forecast on a constant path of the nominal funds rate, at the initial setting for the policy rate, so that it represents no change in policy. This is the simplest way to separate the forecast from the policy decision, as long as you don't mind that the staff's forecast is not its best judgment about the outlook over the forecast horizon. Still, this simple approach has proved a very effective device for the staff to provide the FOMC with information about what the outlook

might be in the absence of any change in the policy rate. This can be a powerful motivator to action.

Sometimes, however, this assumption seems so out of line with the committee's long-term objectives that the staff cannot—and, I believe, should not—resist a more activist approach. The first fallback is the constant real rate assumption, aligned to the prevailing real rate. But, at times, the staff will go still further and condition the forecast on a rising or falling real federal funds rate path, though never with a change assumed as the outcome at the current meeting.

But the MPC situation is quite different. It publishes a forecast that extends a couple of years. If the MPC wants this forecast to be credible, it has to condition it on a credible path of interest rates. Charles says that it would be a slur on the MPC to imply that they fudge the forecast relative to the path of interest rates so that the forecast should be credible even if the path of interest rates is not. I apologize for any offense taken, but I have had my doubts about the credibility of the MPC's forecast because of the restrictions under which it is made.

Charles then suggests that the decision to condition the forecast on a constant nominal interest rate has the desirable consequence of encouraging policymakers to be more preemptive than they otherwise would have been. What was missing from this discussion was an explanation of why the committee would be less preemptive than would be optimal if it were freed from the restraint on its forecast procedure. What is it that biases the MPC to be suboptimally responsive to changing economic conditions? Actually, this might be an interesting line of inquiry because I suspect that there is a bias toward waiting longer than is appropriate to change policy. This arises perhaps from the view that it would be undesirable to move rates in one direction and then have to reverse course in short order, as would be more likely to be the case if policy were the random walk that Charles talked about. To avoid this outcome, a high hurdle is set for rate moves, especially at turning points. The result may well be a bias toward holding rates too long.

Charles then proceeds to offer an alternative to the current procedure of a constant policy rate in the MPC forecast. I was, however, a little confused about the alternative approach Charles suggested. After all, he had just concluded that there were constructive incentives that followed from the current approach and that the forecast was in fact fully consistent with the rate path. Why, based on this assessment, even offer an alternative? At any rate, Charles recommends that, in addition to the forecast conditioned on a constant nominal interest rate, the MPC publish two alternatives. One alternative would have higher rates and the other would have lower rates, initially, but in each case rates would be set later in the forecast to ensure the same conformity of inflation with the target at the policy horizon. It seems to me that this option would leave the MPC with three arbitrary, potentially suboptimal forecasts instead of one. I view Charles's solution, therefore, as multiplying the problems with the existing approach rather than resolving them.

I have struggled to come up with some alternatives of my own, abiding by the restriction that the alternatives maintain the concept of a policy horizon. First, the MPC could set a constant real interest rate, as discussed above. Second, the MPC could set the current policy rate, as it does today, but then allow the remainder of the rate path to be set according to a simple feedback rule. It might be relatively easy, in such a practice, to limit the perception of commitment to the rules-based path and therefore preserve a high degree of flexibility for the MPC with respect to future policy actions. This might be a better "device" to reconcile the forecast with the absence of a fully agreed to policy path. On the other hand, it would likely be difficult to get agreement on a specific rule, even though that rule might not carry a clear policy commitment. Third, the MPC could continue to set a constant nominal path but not insist that inflation be aligned with the target at the 18- to 24-month horizon, as appears to be the case today. This would remove the possible perception that the current interest rate path is the MPC's best judgment about the path consistent with achieving the inflation target at the policy horizon. On the other hand, it would give the market a reading on the likely direction of future policy actions and therefore be a highly explicit statement of a policy bias related directly to prospects for future policy.

SKEWS AND MONETARY POLICY

Charles then asks whether and when policymakers should explicitly take account of asymmetric risks in the forecast. That is, should policy respond to only the mode of the forecast probability distribution—presumably the point forecast—or to the mean, when the two are differ-

ent on account of an asymmetric probability distribution of forecast outcomes.

I have often felt there were asymmetric risks in the outlook. I nevertheless have suspicions when others tell me that *their* forecasts have asymmetric risks! I usually suggest that they to go back and work on their forecasts until they can return with a symmetric probability distribution. When they tell me the risks are to the downside, for example, I wonder if they just got lazy about changing the forecast and didn't line it up on their true mode.

But as I said, the risks to the forecast do on occasion appear asymmetric, and Charles's question is therefore a most interesting one. Should policy with symmetric risks be different from policy with asymmetric risks if the modal forecast is the same in the two cases?

Charles's answer is, It depends. This is one of my favorite answers to interesting questions, so I thought this was a promising start. But as I always told my students, you also have to tell me what it depends on when you choose this answer.

Charles did precisely that. He believes that it depends on whether the risks are observable or not. Don't respond to observable risks, because you will know when they come into play and you can wait to respond then. On the other hand, if they are unobservable, respond immediately, because you will never be in a better position to clarify their relevance. Charles also argues that you should not take into account low-probability, high-payoff risks such as earthquakes and wars.

I thought the discussion of asymmetric risks related to asset prices was particularly interesting. Otherwise, I was not very satisfied with his criteria for when policymakers should or should not respond to asymmetric risks. I would have thought that the issue was not whether the risks are observable but instead whether timely action is needed. For example, if an upside risk is that the stock market may start to surge again, there may be no need to react today to such a possibility. The impact of an increase in the stock market on the economy is gradual and policymakers will have time to react. But I am only scratching the surface of a most interesting question. I judge Charles's answers here to be quite preliminary ones and I hope he will return to this topic in future work.

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