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To our readers:

The Federal Reserve Bank of St. Louis has recently embarked on an initiative called Branching Out, which will increase the Bank's participation in the geographic region of the Federal Reserve System's Eighth District. Specifically, the St. Louis Fed will make greater contributions to understanding and promoting economic development of its branch cities—Little Rock, Louisville, and Memphis—and the surrounding region.

CRE8

As part of this initiative, the Research Division of the St. Louis Fed established its Center for Regional Economics—8th District (CRE8) to provide and facilitate rigorous economic analysis of policy issues affecting local, state, and regional economies—particularly those in the Eighth District. CRE8 will also organize policy forums, conferences, and symposia that highlight economic research done outside the St. Louis Fed.

BERG

The goals of Branching Out are also being accomplished through another channel: the newly established consortium known as the Eighth District Business and Economics Group (BERG). BERG will provide a forum for researchers who have a detailed knowledge of the sub-areas of the Eighth District. It is composed of CRE8 and university-based centers for business and economics research in the states of the Eighth District:

Institute for Economic Advancement, *University of Arkansas at Little Rock*

Center for Business and Economic Research, *University of Arkansas at Fayetteville*

Center for Business Development and Economic Research, *Jackson State University, Mississippi*

Sparks Bureau of Business and Economic Research, *University of Memphis*

Center for Business and Economic Research, *University of Tennessee, Knoxville*

Center for Business and Economic Research, *University of Kentucky*

Economic and Policy Analysis and Research Center, *University of Missouri at Columbia*

Louisville Economic Monitor, *University of Louisville*

Center for Economic and Business Research, *Southeast Missouri State University*

Delta Center for Economic Development, *Arkansas State University*

Business and Economic Research Center, *Middle Tennessee State University*

Simon Center for Regional Forecasting, *Saint Louis University*

Regional Economic Development

The primary channel for organizing and distributing the research generated from these varied sources and events is our newly formed journal, *Regional Economic Development*. With the CRE8 staff as the editorial board, this journal will publish the proceedings from these events on the St. Louis Fed's Research Division web site: <http://research.stlouisfed.org/publications>. Although the contents of the journal will be accessible to a wide audience, beyond purely academic circles, it will nevertheless offer serious economic analysis, addressing and solving practical policy issues.

Upcoming events whose proceedings will be published in *Regional Economic Development* include a symposium on November 4, 2005, which CRE8 is co-hosting with the Wiedenbaum Center at Washington University in St. Louis, titled "Challenges to Public Education Financing Facing Missouri and the Nation." Also, on March 29 and 30, 2006, CRE8 will co-host "TED2006," a conference on transportation and economic development organized by the Institute for Economic Advancement at the University of Arkansas at Little Rock.

The inaugural issue of *Regional Economic Development* contains the proceedings of the first annual conference of BERG, held in St. Louis on May 6, 2005.

As always, we welcome your questions and comments.

William T. Gavin
Editor
October 1, 2005



How Predictable Is Fed Policy?

William Poole

This article was originally presented as a speech at the University of Washington, Seattle, Washington, October 4, 2005.

Federal Reserve Bank of St. Louis *Review*, November/December 2005, 87(6), pp. 659-68.

Day in and day out, all of us depend on a high degree of predictability in the nation's monetary arrangements. Consider, for example, the counterfeiting problem. The problem is in fact small enough that we rarely examine our paper money closely. Through careful design of the currency, and careful monitoring and enforcement, the Treasury and Federal Reserve together maintain a highly reliable currency—its usefulness to us is almost completely predictable.

Historically, the least predictable aspect of our monetary system has been monetary policy. Monetary policy mistakes led to the Great Depression of the 1930s and the Great Inflation of the 1960s and 1970s. Unpredictability can have high costs. The Great Inflation and its correction led to the failure of hundreds of savings and loan associations (S&Ls). The problem was that S&Ls made long-term mortgage loans at interest rates reflecting expectations of modest inflation, but those expectations proved to be too low again and again as inflation rose after 1965. As inflation rose, interest rates on outstanding long-term fixed-rate mortgages did not, creating enormous losses for S&Ls. Later, as inflation expectations became embedded, many borrowed at interest rates that turned out to be unsustainably high; purchases of farmland, for example, financed at high interest

rates turned out to be extremely unwise as inflation fell unexpectedly in the early 1980s. Many farmers and agricultural banks that lent to them failed.

Inflation predictability is the most obvious, and probably most important, consequence of predictable monetary policy. Nevertheless, most economic decisions depend, directly or indirectly, on the predictability of monetary policy. Monetary policy decisions can create surprises that affect outcomes from household decisions as to what jobs to take and where to live. Similarly, business firms find that their decisions on hiring and investment in physical capital may turn out well or poorly depending on the course of monetary policy and its effects on the economy.

If you follow the financial press at all, or watch national TV news following a Fed policy decision, you know that financial markets have an intense interest in what the Federal Reserve does or does not do. Financial market behavior provides an opportunity to study the predictability of monetary policy with some precision. That is my topic today, organized around my title theme: How Predictable Is Fed Policy?

Before proceeding, I want to emphasize that the views I express here are mine and do not necessarily reflect official positions of the Federal Reserve System. I thank my colleagues at the Federal Reserve Bank of St. Louis for their com-

William Poole is the president of the Federal Reserve Bank of St. Louis. The author appreciates comments from colleagues at the Federal Reserve Bank of St. Louis. Robert H. Rasche, senior vice president and director of research, provided special assistance. The views expressed are the author's and do not necessarily reflect official positions of the Federal Reserve System.

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ments. Robert Rasche, senior vice president and director of research, provided special assistance. However, I retain full responsibility for errors.

EVOLUTION OF FOMC COMMUNICATION DURING THE GREENSPAN ERA

Since 1989, the FOMC has adopted many practices that improve the transparency of its policy actions. Enhanced transparency is important for improving the Fed's political accountability, but from an economic perspective transparency is essential if markets are to understand Fed policy and therefore be more successful in predicting policy adjustments. Fed policy is implemented through decisions by the Federal Open Market Committee (FOMC) that set the target, or "intended," federal funds rate. The funds rate is the interest rate in the interbank market on overnight (one-day) loans. The funds rate is the fulcrum, or anchor, for all other interest rates in the market.

Here are some milestones of changes in FOMC practices that have enhanced transparency:

- August 1989: Policy changes in the target federal funds rate are limited to multiples of 25 basis points. Prior practice of changing the rate in other increments often created market uncertainty as to exactly what the Federal Reserve's intention was.
- February 1994: Starting with this FOMC meeting, the Committee released a press statement describing its policy action at the conclusion of any meeting at which the Committee changed the target funds rate. Prior to this practice, the market had to infer from Fed open market operations whether, and how, the Fed's policy stance had changed. Consequently, the market was often uncertain as to the current setting of Fed policy.
- August 1997: Public acknowledgment that policy is formulated in terms of a target for the funds rate. The market had come to believe that the federal funds rate was the

policy target but all uncertainty about this issue disappeared after this time.

- August 1997: A quantitative target federal funds rate is included in the Directive to the System Open Market Account Manager at the Trading Desk of the Federal Reserve Bank of New York (the "Desk"). Previously, the Fed often discussed policy in terms of the "degree of pressure on reserve positions" in the money market. A clear focus on a quantitative target ended the ambiguity.
- May 1999: A press statement following the conclusion of every FOMC meeting includes the target federal funds rate and the policy "bias." The bias indicated that the Committee was leaning toward an increase or decrease in the funds rate target but had not yet decided to actually change the target.
- December 1999: In its press statement, the FOMC replaces the policy bias language with "balance of risks" language in an effort to lengthen the horizon of its statement and provide a summary view of its outlook for the economy.
- January 2002: The vote on the Directive and the names of dissenting members, if any, are included in the press statement. Previously, this information was not available to the market until the meeting minutes were released following the subsequent FOMC meeting, six to eight weeks later.
- August 2003: The FOMC introduces "forward-looking" language into its post-meeting press statement.¹ This language suggested the probable direction of the target federal funds rate over the next one or more meetings.

¹ "In these circumstances, the Committee believes that policy accommodation can be maintained for a considerable period." Federal Reserve *Press Release*, August 12, 2003 (www.federalreserve.gov/boarddocs/press/monetary/2003/20030812/). In the press release of January 28, 2004, the language was modified: "...the Committee believes that it can be patient in removing its policy accommodation" (www.federalreserve.gov/boarddocs/press/monetary/2004/20040128/default.htm). Subsequently, in the press release of May 4, 2004, a second modification of the language was introduced: "the Committee believes that policy accommodation can be removed at a pace that is likely to be measured" (www.federalreserve.gov/boarddocs/press/monetary/2004/20040504/).

- January 2005: Release of minutes of FOMC meeting advanced to three weeks after the meeting (and before the next scheduled FOMC meeting).

The purpose of these changes, which have gone a long way toward lifting the traditional veil of secrecy over monetary policy, is to increase transparency of policy, improve accountability, and provide better information to market participants about the future direction of policy. In several earlier speeches and papers written jointly with members of the St. Louis Fed Research Division, I examined how changes in Fed transparency have affected market behavior, especially after 1994. Today, I will review some of those findings and add new findings on market behavior over the past two years, when the most recent innovations were introduced.²

ARE MARKETS IN SYNCH WITH THE FOMC?

On a number of occasions I have stressed my view of the importance of markets being “in synch” with the FOMC. Accumulating evidence has shown that, judging by the reaction of the federal funds futures market, market participants have been increasingly accurate in predicting FOMC policy actions as steps toward more transparency were implemented. The basic theme of this work is that the economy will function more efficiently if the markets and the Fed are interpreting incoming data the same way. If the Fed and the markets have the same view as to the policy implications of new information, then the market will be able to predict Fed policy adjustments accurately.

Those analyses were made prior to the introduction of “forward-looking” language in the post-FOMC meeting press releases, beginning in August 2003. An appropriate question is how the “forward-looking” language has affected market

perceptions of future FOMC policy actions. Figures 1 and 2 replicate and extend the corresponding figures from Poole and Rasche (2003; see footnote 2).

First, a little background. The federal funds market trades continuously during the day and the rate may fluctuate minute by minute. At the conclusion of each day, the Fed publishes the “effective” rate, which is the rate at which most transactions took place. Also trading continuously during the day, on the Chicago Board of Trade, are futures contracts in federal funds. The maturing futures contract is settled at the end of the month based on the average effective federal funds rate during the month. Thus, the federal funds futures market is a direct bet on the FOMC’s target federal funds rate in the future. The number of contracts traded has changed over time, as has the level of trading activity in the market.

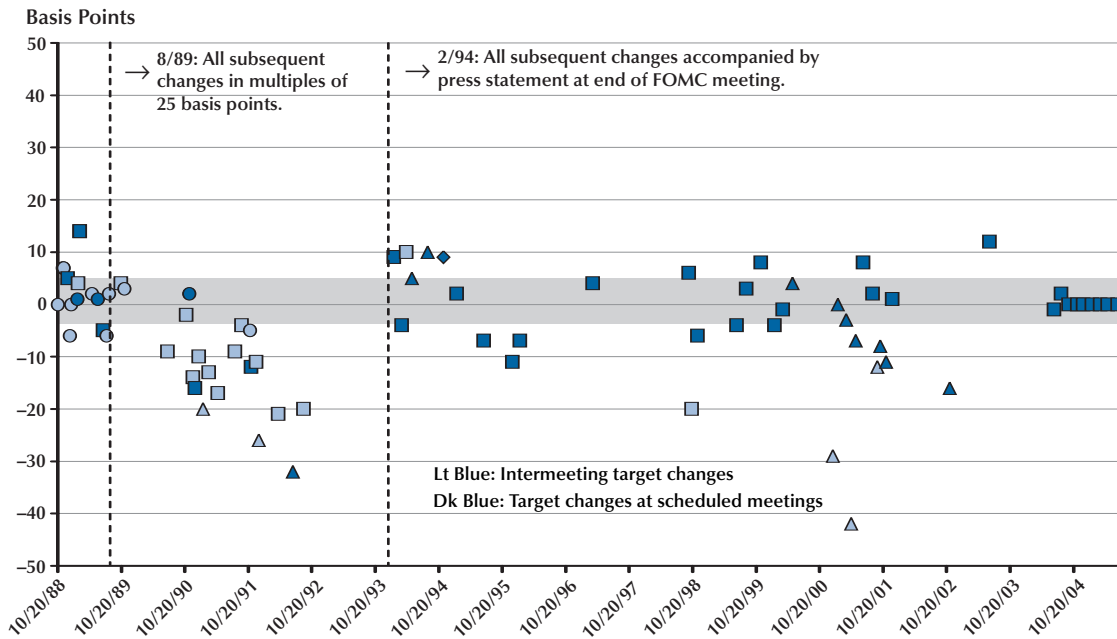
Over the course of a month, data become known and the market trading is based on that information plus expectations as to the federal funds rate during the remaining days of the month. There is also trading in the 1-month-ahead contract—for example, in October 2005, trading in a November 2005 contract. The Chicago Board of Trade lists additional contracts as far as a year or more out, but trading volume is trivial much beyond the 5-month-ahead contract. The distant contracts, because they tend to have thin volume, are not necessarily reliable measures of market expectations of the federal funds rate in the future. However, the 1-month-ahead contract is pretty active and provides an excellent measure of market expectations for that month. In my analysis, I will focus on the 1-month-ahead contract and certain other contracts, such as the 4-month-ahead contract—for example, in October 2005, the contract for February 2006.

Now look at Figure 1. The period covered starts when trading in federal funds futures commenced in October 1988. The data shown are daily changes (close-of-business to close-of-business) in the yield on the 1-month-ahead federal funds futures contract on days of scheduled FOMC meetings and days when the FOMC changed the target funds rate between regular meetings. The area shaded in gray, between plus and minus 5

² For a review of the effect of the earlier improvements in FOMC transparency see W. Poole and R. H. Rasche, “The Impact of Changes in FOMC Disclosure Practices on the Transparency of Monetary Policy: Are Markets and the FOMC Better ‘Synched’?” *Federal Reserve Bank of St. Louis Review*, January/February 2003, 85(1), pp. 1-9.

Figure 1

Changes in Funds Futures Rate When FOMC Changed Target Funds Rate



NOTE: □ 25-basis-point change, △ 50-basis-point change, ◇ 75-basis-point change, ○ change that market was not immediately aware had occurred (Poole, Rasche, and Thornton, 2002).

basis points, indicates a region that I have defined as insignificant “noise” in this market.

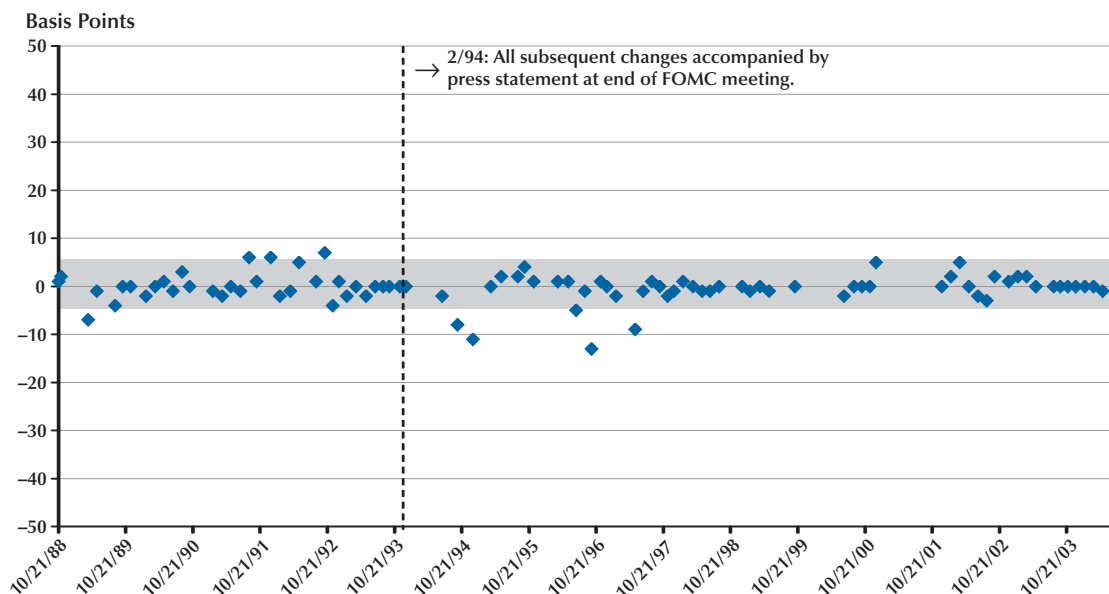
In Figure 1, the points plotted with a square show days on which the FOMC changed the target federal funds rate by 25 basis points. For example, the second dark blue square from the left shows that on that day the 1-month-ahead futures contract rose by about 0.12 percentage points, or 12 basis points. Thus, on that day, the market had predicted about half of the actual change of 25 basis points in the target funds rate. Looking farther to the right in Figure 1, you can see that most of the dark blue squares fall within the gray band. That means that on days of scheduled FOMC meetings, most of the time the market correctly predicted the Committee’s action of changing the target funds rate by 25 basis points. Remember that the data show the market’s trading the day of the FOMC meeting. The Committee’s decision is not generally predicted accurately weeks or months in advance.

Also in Figure 1, points plotted with a dark blue triangle indicate days of scheduled meetings when the FOMC changed the target funds rate by 50 basis points. There is one point, November 15, 1994, plotted with a dark blue diamond, showing the single case in which the FOMC changed the target rate by 75 basis points. If you look carefully at the dark blue triangles, you can see that the futures market did a pretty good job of predicting changes of 50 basis points. The largest error, in 2002, was about -0.18 , or 18 basis points. One way of interpreting that error is that the market was betting on a decline in the target rate of 25 basis points and putting some probability on a decline of 50 basis points. Looking at all the dark blue points, the market has done a pretty good job of predicting rate changes, especially after February 1994.

Now look at the light blue points in Figure 1. These show days on which the FOMC took policy actions between regularly scheduled meetings.

Figure 2

Changes in Funds Futures Rate on FOMC Meeting Dates With No Change to Target Funds Rate



As with the dark blue points, squares indicate federal funds target changes of 25 basis points and triangles indicate changes of 50 basis points. Although there are few light blue points after February 1994, it is clear that FOMC policy actions on days other than scheduled meetings are not well predicted. That result is hardly surprising, given that such meetings are not announced in advance. The very existence of such a meeting, as well as the change in the target funds rate, necessarily takes the market by surprise.

Figure 2 shows days when the FOMC met and made a policy decision not to change the target federal funds rate. As can be seen in the figure, there were times before 1998 when the market put some probability on a rate change, and when the FOMC did not change the funds rate target that meant that the futures market adjusted to reflect that outcome. However, most of the time the points fall in the gray band, meaning that the futures market did not change by much because the market had correctly predicted that the FOMC would not change the target rate.

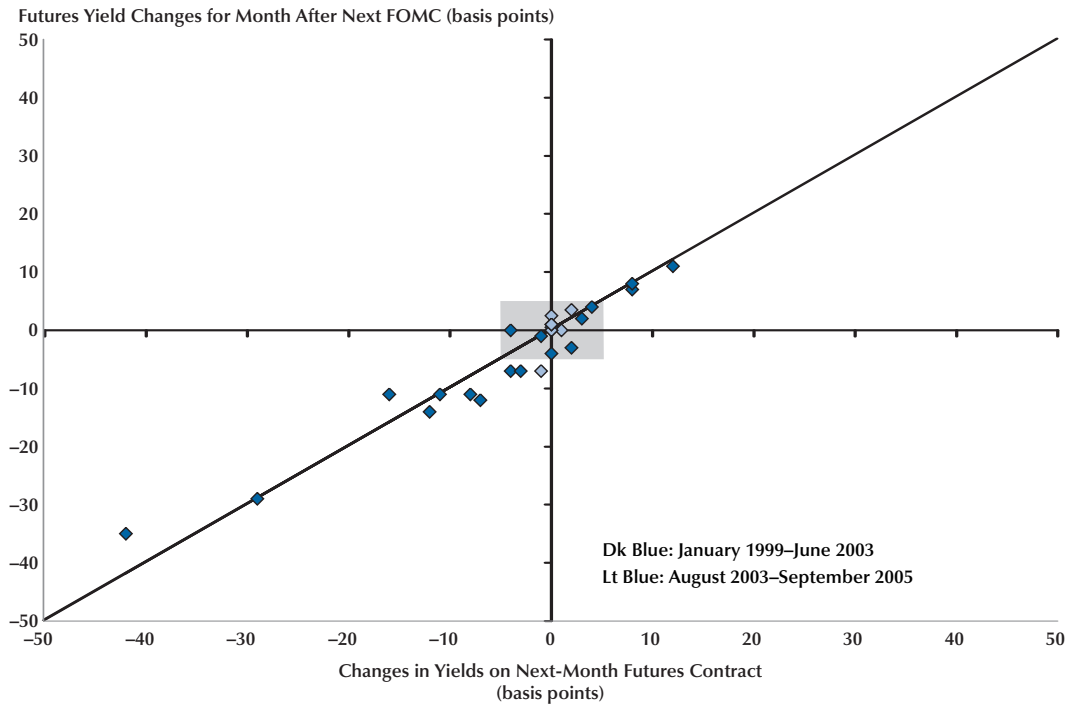
We can summarize these results as follows:

Particularly since February 1994, policy decisions taken at regularly scheduled FOMC meetings, whether or not they’ve involved a federal funds target change, have generated little if any news in the federal funds futures market. Such decisions have been well anticipated by market participants.

As you have been looking at Figures 1 and 2 you have probably wondered why the points at the end of the period, starting with August 2003, fall almost precisely on zero, indicating no error at all in predicting FOMC decisions. Starting with the statement issued after its meeting of August 12, 2003, the FOMC has included “forward-looking” language that has facilitated nearly perfect market forecasts of the FOMC decision at its next meeting. Initially, the language indicated that “policy accommodation can be maintained for a considerable period.” That language suggested that the FOMC would not change the target funds rate at its next meeting. After the meeting of January 28, 2004, the language was modified to say that “the Committee believes that it can be patient in removing its policy accommodation.” The market read that language as suggesting that the period of an

Figure 3

Changes in Funds Futures Rate When FOMC Changed Target Funds Rate



unchanged target funds rate was coming to an end, but was not yet over.

Finally, the statement issued after its meeting of May 4, 2004, said that “the Committee believes that policy accommodation can be removed at a pace that is likely to be measured.” That language then appeared in every statement through the most recent statement issued after the meeting on September 20, 2005. The market came to read the language as indicating that the FOMC would raise the target funds rate by 25 basis points at its following meeting. The FOMC in fact did so at every meeting through the most recent one, and the market prediction errors were negligible. In particular, none of the policy actions taken since the introduction of this language—that policy accommodation can be removed at a measured pace—has generated any large (greater than 5 basis points) change in the yield on the 1-month-ahead funds futures contract on the day of the FOMC action. On each of these occasions the

futures market has made an almost perfect forecast of the 25-basis-point increase in the target funds rate.

Bob Rasche and I, in our earlier work, found that the futures market, although predicting FOMC decisions quite accurately a day in advance, usually did not predict accurately several months in advance. But now we have a new question to explore: If the content of the FOMC press releases since August 2003 is signaling the policy decision at the next FOMC meeting so clearly, how well are markets now predicting the more distant trajectory of the target federal funds rate?

Addressing this question is a bit more complicated than it might seem. The FOMC and market participants understand that monetary policy cannot be locked down long in advance because an unpredictable economic event might make a policy adjustment highly desirable. Since June 2004, the press release has clearly indicated that, while the Committee intends to proceed at a meas-

ured pace, its intention is conditioned on future information about the state of the economy.³ A second question concerns the information that is generating adjustments of market expectations.

On the vertical axis of Figure 3 are changes in the futures yield for the contract of the month subsequent to the next FOMC meeting. These changes are plotted on the date when an FOMC policy action was announced. Because there are eight scheduled meetings per year, sometimes there are meetings in adjacent months and sometimes not, which means that the futures contract studied is sometimes two months ahead and sometimes three months ahead. An example may help in understanding what data were used. In the case of the meeting on May 15, 2001, the market knew that there was a scheduled meeting on June 26-27, 2001. Thus, here we examined the change on May 15 in the July 2001 futures contract—the 2-month-ahead contract. However, the next scheduled meeting following the one on June 26-27, was on August 21; there was no meeting scheduled for July. Thus, the relevant change in the federal funds futures market on June 27 was for the September 2001 contract—the 3-month-ahead contract.

In Figure 3, on the horizontal axis are changes in the yield on the 1-month-ahead futures contract, and on the vertical axis are changes in the appropriate 2- or 3-month-ahead contract as just explained. The points plotted in dark blue are for the period from the beginning of 1999 through June 2003. During this period, yields on the two futures contracts essentially moved one-for-one as indicated by the tight scatter of the points around the 45-degree line. That is, if the 1-month-ahead contract changed by 30 basis points, so also did the appropriate 2- or 3-month-ahead contract. The points plotted in light blue are from the succession of 25-basis-point moves during the period of the “measured pace” language. All but one of these points fall into the gray box that designates changes of less than 5 basis points in absolute

value. Hence the changes in the target funds rate during the measured pace regime have generally not been surprises, nor have they generated revisions to market expectations of the policy action at the immediate future FOMC meeting.

If we examine all the daily data, and not just data for days of FOMC meetings, from the beginning of July 2003 through mid-September 2005, there were only two days when the yield on the 1-month-ahead funds futures contract changed by 5 basis points or more. Those two days were June 15, 2004, and September 1, 2005. On the first of these days Chairman Greenspan testified at the hearings for his renomination as Chairman of the Board of Governors. On that day the July 2004 futures yield decreased by 5 basis points. On the other day, September 1, 2005, the October 2005 futures yield decreased by 6 basis points in the aftermath of Hurricane Katrina. Thus, since July 2003 there is simply not much information in the 1-month-ahead contract on how market expectations are reacting to news. This finding is in sharp contrast to earlier Poole-Rasche findings, which I do not have time to discuss in any detail here, that economic news such as employment reports often triggered significant changes in the 1-month-ahead futures contract.

However, over the past two years, the 4-month-ahead futures contract is more informative. Between June 2003 and mid-September 2005 there were 24 occasions when the yield on this contract changed by 5 or more basis points. One was the occasion of the announcement of a 25-basis-point increase in the target funds rate on June 30, 2004. On that date the yield on the October 2004 funds futures contract decreased by 8 basis points. At first glance this market reaction might suggest some confusion about FOMC intentions.

In fact, two pieces of information became available with the FOMC press release on that day. First, the increase of 25 basis points in the target funds rate was the initial policy action under the forward-looking language of removing policy accommodation “at a pace that is likely to be measured.” On the previous day, the October 2004 funds futures contract had closed at 1.90. Since there were only two scheduled FOMC meet-

³ “Nonetheless, the Committee will respond to changes in economic prospects as needed to fulfill its obligation to maintain price stability.” Federal Reserve *Press Release*, June 30, 2004 (www.federalreserve.gov/boarddocs/press/monetary/2004/20040630/default.htm).

Poole

ings during the July-October 2004 period, this yield implied a market expectation of a cumulative 75-basis-point move and roughly a 0.6 probability of an additional 25-basis-point move over three FOMC meetings in June through September. The August and September futures contract yields were quite consistent. The August contract yield implied a probability of about 0.4 of a 50-basis-point move at the August FOMC meeting conditional upon a 25-basis-point increase at the June meeting. The September contract yield implied a probability of about 0.5 of a 50-basis-point move at the September FOMC meeting conditional upon a 25-basis-point increase at both the June and August FOMC meetings.

The revelation of a 25-basis-point move at the June 2004 meeting solidified market impressions that “measured pace” meant a succession of 25-basis-point increases in the target funds rate. At the close of trading after the June 30 policy action, the October futures yield of 1.83 implied two additional increases of 25 basis points through September, but only about a 0.3 probability of a move larger than 50 basis points during that period. Consistently, after the announcement, the probability of a 50-basis-point move at the August FOMC meeting implied by the August contract fell to around 0.25. The probability of a 50-basis-point move at the September FOMC meeting implied by the September contract, conditional on a 25-basis-point move in August, also fell to around 0.25.

The other information that became available in the June 2004 press release was introduction of the qualification to the measured pace language that “the Committee will respond to changes in economic prospects as needed to fulfill its obligation to maintain price stability.” On July 2, 2004, the initial estimate of payroll employment growth for June 2004 was only 112,000 new jobs, compared with survey predictions on the order of 250,000. After this news, a second large downward adjustment to the October futures yield occurred, reducing the expectation of any policy action in excess of 25 basis points at either of the next two FOMC meetings to zero. The yields on the August and September contracts also implied approximately zero probability of anything other than

25-basis-point moves at the two forthcoming FOMC meetings.

Of the remaining 23 “large changes” since June 2003, 12 (52 percent) occurred on days when the employment data were released. Three “large changes” occurred in the immediate aftermath of Hurricane Katrina (August 30–September 1, 2005.) Eight changes occurred on days when economic data were released, but in none of these cases were there multiple releases of any single statistical series.⁴ Hence, since the introduction of “forward-looking” language, market expectations appear to have reacted systematically only to employment data.

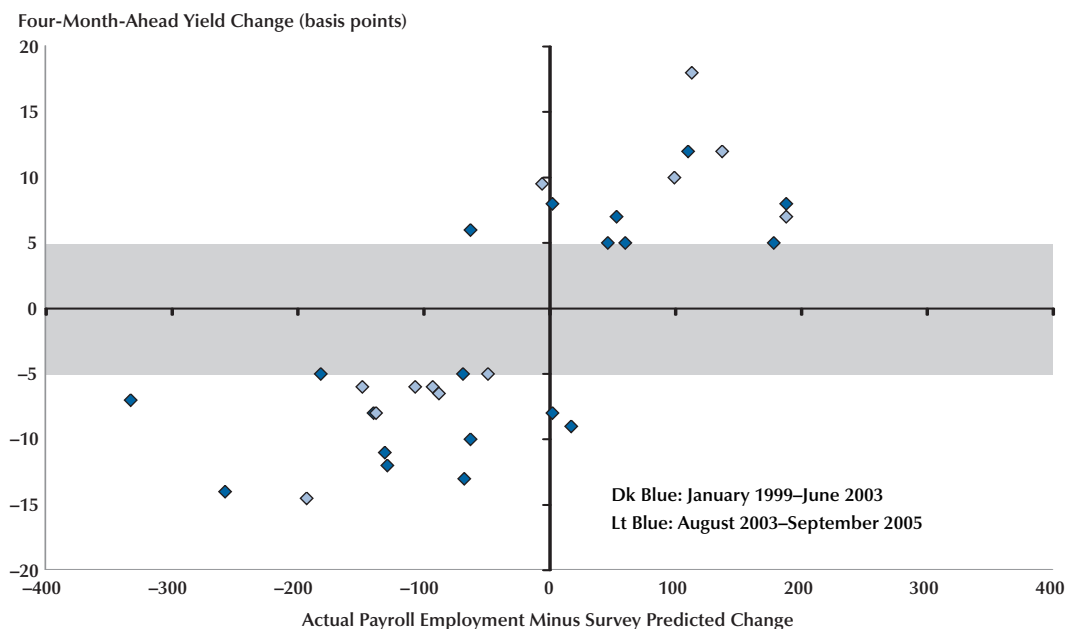
Are the observed market reactions suggestive that markets expect a systematic reaction by the FOMC to employment data? Economic theory and ample empirical investigation in many markets indicate that the appropriate concept is the employment surprise, measured by the difference between the initial estimate of the change in payroll employment and survey predictions of the employment change. Figure 4 presents a scatter plot of changes in the yield on the 4-month-ahead futures contract and employment surprises. The 12 points plotted in light blue indicate the futures market reaction to the employment prediction errors. Since I consider only “large changes” in the futures yield, there are no observations in the gray shaded area of 5 basis points.

With one exception, all of the light blue points fall in the first and third quadrants of the graph and are roughly consistent with a linear relationship between the changes in the futures yield and the employment survey prediction error. Apparently, market expectations of the future level of the funds rate since the FOMC introduced “forward-looking” language into the press release are adjusted in the same direction as the employment surprise. This finding is consistent with an understanding that the short-run strategy of the FOMC involves adjustments in the future path of the target funds rate when incoming data suggest that the risks to employment growth have changed.

⁴ The releases that occurred on these eight dates were wholesale inventories (9-Jun-04), retail sales (14-Jun-04), CPI (15-Jun-04), personal income (28-Jun-04), industrial production (15-Apr-05), leading indicators (21-Apr-05), ISM (6-Sep-05), and productivity (7-Sep-05).

Figure 4

Large 4-Month-Ahead Funds Futures Changes and Employment Surprises



The points plotted in dark blue in Figure 4 indicate “large changes” in the 4-month-ahead fund futures rate on days that the payroll employment data were released between January 1999 and the introduction of “forward-looking” language in August 2003. There are 19 such events of a total of 131 “large change” events during this period (15 percent). Again almost all of the points lie in the first and third quadrants of the graph. Also, there does not appear to be any systematic difference between the scatters of points before and after mid-2003. Thus, at the horizon of the 4-month-ahead futures contract, the adjustment of market expectations to news about payroll employment does not appear to have been systematically influenced by the introduction of “forward-looking” language in the press release.

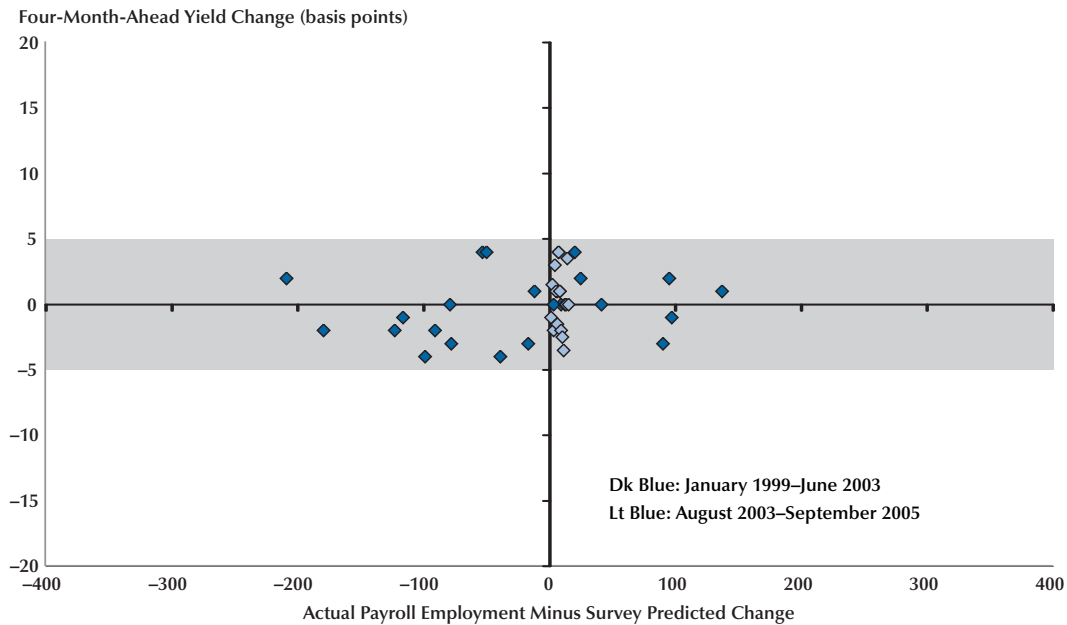
To complete the analysis, it is necessary to examine the nature of market reactions to employment surprises on those dates when the release of the employment statistics was not accompanied by large changes in the yield on the 4-month-ahead funds futures contract. These data are shown in

Figure 5. As before, the points in light blue are for the period since the introduction of “forward-looking” language in August 2003. The points in dark blue are from January 1999 through mid-2003. In contrast to Figure 3, the difference between the data from the past two years and the earlier period is startling. In the recent period, all of the small changes in the futures yield occurred when the employment forecast was highly accurate. Hence, during this period there is a clear distinction in the response of the futures yield to the employment data: When there are large employment surprises the futures rate adjusts; when there are no surprises (or very small surprises) there is little movement in the futures yield. However, in the period from 1999 through mid-2003 there were many occasions when employment surprises did not generate any appreciable reaction in the funds futures yield.

My conclusion from these observations is that market sentiment has coalesced around the view that news about employment growth is a significant influence on the path of the target funds rate in the foreseeable future.

Figure 5

Small 4-Month-Ahead Funds Futures Changes and Employment Surprises



I'll finish with two observations. First, my emphasis on market reactions to employment surprises does not mean that the market ignores inflation. What has happened in recent years is that core inflation—inflation excluding effects of food and energy—simply has not generated significant surprises. The Fed has emphasized that it focuses on core inflation so that monetary policy does not react to energy and food prices, which tend to be highly volatile. I have no doubt that both the FOMC and the market would respond to surprises in core inflation that seemed likely to be persistent and to indicate a developing inflation problem.

Second, recent changes in the federal funds futures rate in response to rapidly changing events connected with hurricanes Katrina and Rita will be interesting to examine carefully in the future. However, these events are too recent to be good candidates for careful analysis now, and I'll forgo an effort at instant analysis.

CONCLUSION

The federal funds futures market, and other markets I have not discussed here, provide a rich source of information to better understand the effectiveness of the Fed's changes in disclosure policies over the Greenspan era. It is quite clear that the markets understand Fed policy to a much greater extent than before. My own view is that the market's improved understanding, and the Fed's efforts to improve clarity of monetary policy decisions and decision processes, have much to do with the economy's improved stability. Recessions have become milder, and core inflation more stable. Maintaining these gains is important to economic welfare. I would not claim that we have enough evidence to say that the gains are permanent, but we do have enough to say that the effort has been very productive.



Oil Price Volatility and U.S. Macroeconomic Activity

Hui Guo and Kevin L. Kliesen

Oil shocks exert influence on macroeconomic activity through various channels, many of which imply a symmetric effect. However, the effect can also be asymmetric. In particular, sharp oil price changes—either increases or decreases—may reduce aggregate output temporarily because they delay business investment by raising uncertainty or induce costly sectoral resource reallocation. Consistent with these asymmetric-effect hypotheses, the authors find that a volatility measure constructed using daily crude oil futures prices has a negative and significant effect on future gross domestic product (GDP) growth over the period 1984-2004. Moreover, the effect becomes more significant after oil price changes are also included in the regression to control for the symmetric effect. The evidence here provides economic rationales for Hamilton's (2003) nonlinear oil shock measure: It captures *overall* effects, both symmetric and asymmetric, of oil price shocks on output.

Federal Reserve Bank of St. Louis *Review*, November/December 2005, 87(6), pp. 669-83.

Monetary policymakers are concerned with large price movements in the crude oil market, and Figure 1 explains why.¹ It shows that most U.S. post-World War II recessions, including the most recent 2001 recession, were preceded by sharp increases in crude oil prices. One conventional explanation is that oil price increases lower future GDP growth by raising production costs. Alternatively and complementarily, large oil price changes—either increases or decreases—may affect aggregate output adversely because they delay business investment by raising uncertainty or they induce costly sectoral resource reallocation.

In this paper, we explicitly distinguish between these two channels through which changes in oil prices affect aggregate economic

activity: (i) the change in the dollar price of crude oil (relative price change) and (ii) the increase in uncertainty about future prices (volatility). It should be noted that, as we discuss in the next section, although the former channel implies a symmetric effect of oil shocks, the latter implies an asymmetric effect. Therefore, a joint consideration of both channels sheds light on Hamilton's (2003) nonlinear oil shock measure: It captures overall effects, both symmetric and asymmetric, of oil price shocks.

We constructed an oil price volatility measure using daily prices of crude oil futures traded on the New York Mercantile Exchange (NYMEX) over the period 1984-2004. Our main results can be summarized as follows. First, as hypothesized, oil price volatility has a negative and significant effect on future GDP growth. Second, this volatility effect becomes more significant after we control for the oil price change, indicating that both channels are potentially important. Third, both the oil price change and its volatility lose their

¹ Unless otherwise indicated in our empirical analysis, we use an updated version of Hamilton's (2003) data of both raw and transformed oil prices. We thank James Hamilton for providing the data.

Hui Guo is a senior economist and Kevin L. Kliesen is an economist at the Federal Reserve Bank of St. Louis. The authors thank James Hamilton for comments and data. Jason Higbee provided research assistance.

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significance after we control for Hamilton's (2003) nonlinear oil shock measure. This result confirms that Hamilton's measure captures the overall effects of oil shocks on aggregate output and, therefore, cannot be entirely attributed to data mining, as suggested by Hooker (1996a,b).

It is also important to note that, consistent with work by Hamilton (1983 and 1985) and others, a vast majority of the largest daily oil futures price changes in our data are associated with exogenous events such as wars or political instability in the Middle East. Moreover, the dynamic of the oil price volatility measure cannot be explained by standard macroeconomic variables. This evidence is consistent with a causal interpretation of the macroeconomic effect of oil shocks.²

As a robustness check, we measured volatility also using squared quarterly oil price changes over a longer sample, 1947-2004, and obtained very similar results. For example, oil price volatility has a negative and (marginally) significant effect on future GDP growth when combined with oil price changes, which are statistically significant as well. Again, both variables lose their predictive power after we control for Hamilton's (2003) nonlinear oil shock measure.

The remainder of the paper is organized as follows. After providing a brief summary of the relation between oil prices and output, we discuss our measure of realized variance of oil futures prices. We then investigate the relation between realized variance of oil futures prices and various measures of macroeconomic activity.

RELATED LITERATURE

Hamilton (1983), among many others, has documented a negative and significant relation between oil price changes and future GDP growth. This result, however, breaks down in data after 1986 (e.g., Hooker, 1996a). The unstable relation possibly reflects that Hamilton has implicitly assumed a symmetric effect of oil shocks in his linear specification: An increase (decrease) in oil

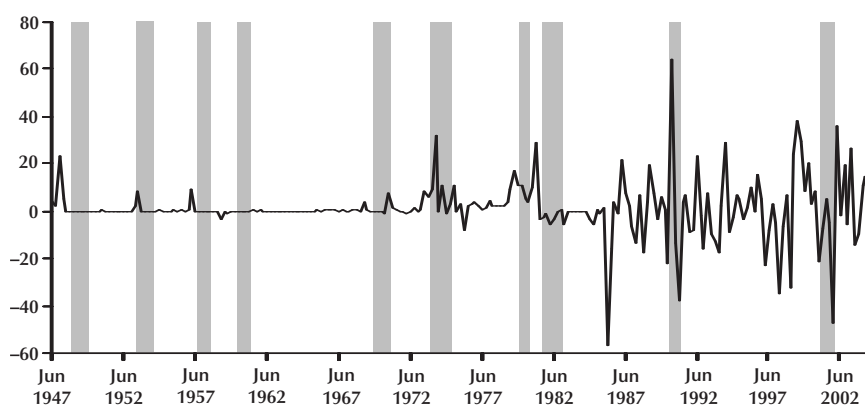
prices reduces (increases) future GDP growth. This specification is consistent with some transmission channels (e.g., Rasche and Tatom, 1977a,b; Baily, 1981; and Wei, 2003) through which oil shocks exert influence on macroeconomic activity.³ However, the effect can be also *asymmetric*: An oil price decrease may actually lower future GDP growth through other channels. In particular, as we investigate in this paper, a sharp oil price change—either increase or decrease—affects the macroeconomy adversely for at least two reasons. First, it raises uncertainty about future oil prices and thus causes delays in business investment (e.g., Bernanke, 1983, and Pindyck, 1991). Second, it induces resource reallocation, for example, from more adversely influenced sectors to less adversely influenced sectors, and such reallocation is costly (e.g., Lilien, 1982, and Hamilton, 1988). Overall, whereas an oil price increase has a negative effect on future GDP growth, the effect of an oil price decrease is ambiguous. That is, given that both the oil price change and volatility are related to future GDP growth, Hamilton's (1983) specification suffers from an omitted variables problem.

As shown in Figure 1, this explanation of the omitted variables problem is plausible. Most oil price changes are positive before 1986; in contrast, oil prices exhibit larger swings in both directions afterward. As a result, although Hamilton's (1983) linear specification is a good approximation before 1986, it is not after 1986 because of the increased importance of nonlinearity induced by large negative oil price changes.

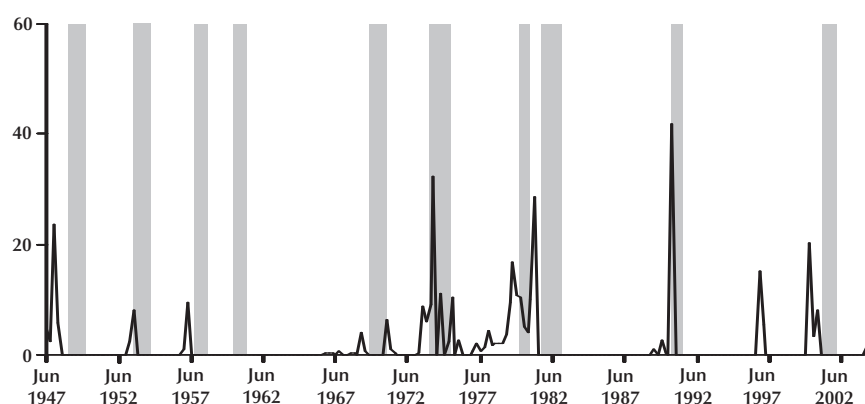
To take into account the asymmetric effect, Hamilton (1996 and 2003) proposed a transformation of raw oil prices. In particular, an oil shock is equal to the difference between the current oil price and the maximum price in the past 4 or 12 quarters if the difference is positive and is equal to zero otherwise. Hamilton found that the transformed oil shock measure exhibits a negative and stable relation with future GDP growth. Figure 2 illustrates his results by showing that a positive oil shock measured using a 12-quarter

² Barsky and Kilian (2004), however, have argued that causality runs from macroeconomic variables to oil prices.

³ Also see Jones, Leiby, and Paik (2004) for discussion on various transmission channels of oil price shocks.

Figure 1**Percentage Change in Quarterly Crude Oil Prices**

NOTE: Shaded bars indicate National Bureau of Economic Research–dated recessions.

Figure 2**Hamilton's Oil Shocks Measured Using a 12-Quarter Horizon**

horizon proceeds almost all the recessions in the post-World War II sample. Nevertheless, it is important to verify that Hamilton's measure of oil shocks indeed captures the nonlinear relation between oil prices and real GDP growth; otherwise, it is vulnerable to the criticism of data mining (Hooker, 1996b). That is, if the change and volatility of crude oil prices have distinct effects on the macroeconomy, these effects should be related

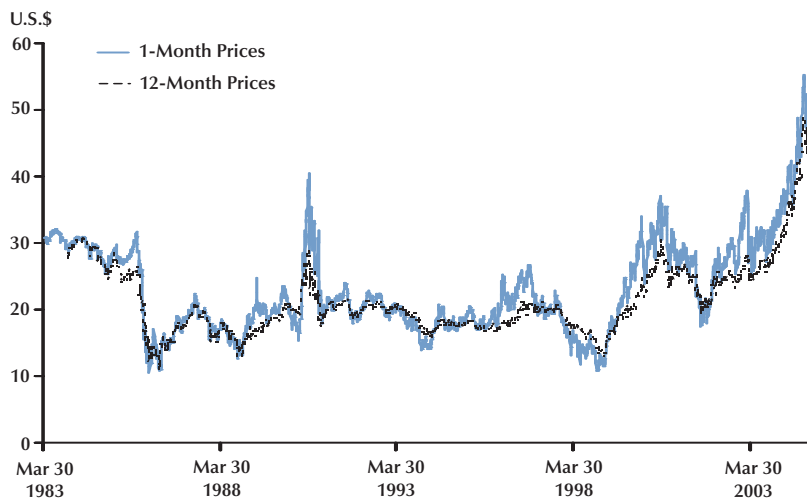
to or even subsumed by Hamilton's modified oil shock measure. This is the main focus of our paper.

REALIZED OIL PRICE VARIANCE

We measured uncertainty about oil prices using a realized oil price variance series constructed from daily crude oil futures prices obtained from the NYMEX. In particular, as in Merton (1980) and

Figure 3

Daily 1-Month and 12-Month Crude Oil Prices



Andersen et al. (2003), among others, quarterly realized oil price variance, RV_O , is the sum of squared daily price changes in a quarter:

$$(1) \quad RV_O = \sum_{d=1}^{D_t} (RET_{-O_d})^2,$$

where RET_{-O_d} is the change in daily futures prices in day d of quarter t .⁴ Realized variance of other frequencies (e.g., a month) is defined in a similar manner.

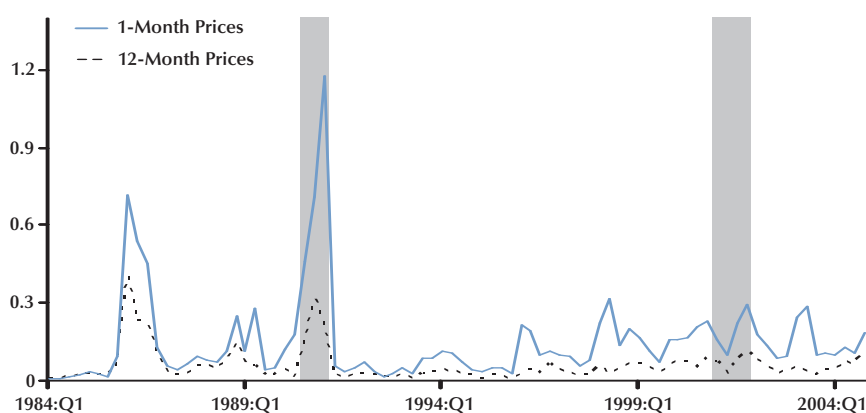
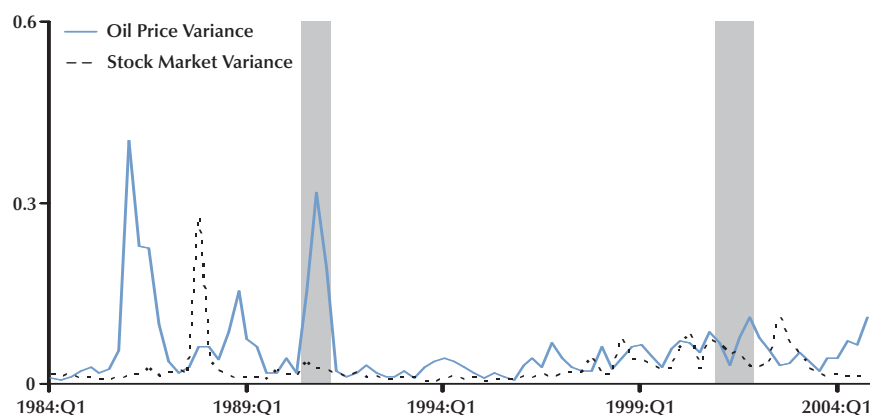
Figure 3 plots daily prices of 1-month (solid line) and 12-month (dashed line) futures contracts of West Texas Intermediate traded on the NYMEX. The data span from April 1983 to December 2004 for the 1-month futures contracts and from December 1983 to December 2004 for the 12-month futures contracts.⁵ As seen in the figure, although the two series move similarly, the 1-month futures contracts appear to be considerably

more volatile than the 12-month futures contracts. Figure 4 plots realized variance of 1-month (solid line) and 12-month (dashed line) futures contracts from 1984 to 2004 (quarterly). Increased volatility in the prices of 1-month futures contracts probably reflects that the market is more vulnerable to temporary disruptions in supply stemming from strikes, refinery shut-downs, or unexpected changes in inventories. These high-frequency shocks mainly reflect transitory noises, which are unlikely to have any significant effect on investors' perceptions about the uncertainty of future oil prices. Therefore, we focused on the volatility measure using 12-month futures contracts in our empirical analysis; nevertheless, we found qualitatively the same results using futures contracts of different maturities.

Figure 4 also shows that oil price volatility increased dramatically in 1986 and 1990, with the former episode reflecting a steep decline in oil prices and the latter a sharp increase because of the first Gulf War (see Figure 1). However, volatility stays at a relatively low level after the first Gulf War, although oil prices continue to exhibit large swings (see Figure 1). We did not observe any large spikes in realized volatility after 1990, even during the second Gulf War in

⁴ The volatility measure defined in equation (1) seems to be plausible because changes in daily crude oil futures prices have a sample average close to zero and negligible serial correlation. We found very similar results using various alternative specifications—for example, using the average daily return in a quarter as a proxy for the conditional return or controlling for serial correlation.

⁵ The 12-month contract is the contract with the most distant maturity and for which daily prices are reliably available since 1984.

Figure 4**Realized Oil Price Volatilities: 1-Month and 12-Month Futures Contracts****Figure 5****Quarterly Realized Oil Price Variance and Stock Market Variance**

2003 and its aftermath. Moreover, oil price volatility seems to have an upward linear trend after 1990, but we were unable to make any formal inference because of the small number of observations.

Many authors (e.g., Guo, 2002) have shown that stock market volatility also has an adverse effects on aggregate output. Given that stock market prices are equal to discounted future cash flows, oil price volatility might be closely related to stock market volatility. To investigate whether

these two volatility measures have similar forecasting power for GDP growth, we also constructed quarterly realized stock market variance, RV_S , using daily stock return data (obtained from Kenneth French at Dartmouth College)⁶:

$$(2) \quad RV_S = \sum_{d=1}^{D_t} (RET_S)_d^2,$$

⁶ We downloaded the data from his homepage: <http://mba.tuck.dartmouth.edu/pages/faculty/ken.french>.

where RET_{S_d} is the change in stock market prices in day d of quarter t . Figure 5 plots realized variance of 12-month crude oil futures prices (solid line) and stock market prices (dashed line). Interestingly, oil price volatility is at least as high as stock market volatility, but the timing of the spikes generally do not coincide. The correlation coefficient between the two volatility measures is a modest 7 percent.

WHAT EXPLAINS OIL PRICE VOLATILITY?

Unanticipated economic developments could, in principle, roil crude oil markets and increase volatility. Recent examples include the unexpected surge in energy demand from China and India, which helped to draw down worldwide buffer stocks, and the decline in the trade-weighted value of the U.S. dollar. According to the International Monetary Fund's April 2004 *World Economic Outlook*,

This decline in commercial stocks and concerns about low U.S. gasoline inventories resulted in a noticeable increase in the volatility of oil prices and the average price of crude oil. A build up of large long speculative positions in futures markets also contributed to the increase in spot prices. (pp. 54-55)

Another cause of increased uncertainty could reflect exogenous events that are noneconomic in nature.⁷ Hamilton (1985) shows that several of the principal causes of increases in crude oil prices from 1947 to 1981 were labor strikes, political disturbances such as the Iranian revolution or the Suez Canal crisis, and wars. In practice, there are two methods that can be used to test whether economic developments or noneconomic developments are the principle cause of increased oil price volatility. Table 1 reports the first method,

⁷ These exogenous events, of course, could precipitate an economic policy response that might increase uncertainty in the oil markets: (i) a more restrictive response by the Federal Reserve in 1979 to combat rising inflationary pressures and heightened inflation expectations in the aftermath of the 1979 Iranian revolution and (ii) the Fed's more accommodative monetary policy in the immediate aftermath of the 2001 terrorist attacks in the United States.

a narrative approach that relates *Wall Street Journal* news accounts with the 10 largest daily price movements of the 12-month futures contracts over the period April 1983–December 2004.⁸ Most of the events associated with the largest percentage changes are related to developments among the Organization of the Petroleum Exporting Countries (OPEC) or political instabilities in the Middle East. Interestingly, among the 10 largest price changes, half occurred during 1986, when crude oil prices plunged. We also found similar results using the next 40 largest price movements (which are available upon request). We confirmed Hamilton's results using higher-frequency data.

The second method relies on formal statistical tests. Table 2 measures whether standard macrovariables forecast one-quarter-ahead realized oil futures variance. The predictive variables include past realized oil variance, RV_O ; the oil price change, RET_O ; realized stock market variance, RV_S ; stock market return, RET_S ; the default premium, DEF ; the term premium, $TERM$; and the growth rate of real GDP, D_GDP . The default premium is the difference between the yield on Baa- and Aaa-rated corporate bonds, and the term premium is the difference between the yield on 10-year Treasury notes and 3-month Treasury bills. A sizable literature suggests that yield spreads like these contain valuable information about current and prospective business conditions (e.g., see Dueker, 1997, and the references therein).

In Table 2 (row 1 of Panel A), realized oil price variance is strongly autocorrelated; the size of the coefficient is 0.565. This result is consistent with those obtained from the other financial markets, such as the stock market, where volatility tends to persist at a high level after it rises (e.g., see Guo, 2002, and references therein). Interestingly, real GDP growth (D_GDP) is negatively—and significantly—related to realized oil price variance. However, as shown in Panel B of Table 2, it loses its predictive power after we add the lagged dependent variable to the regression. The other macrovariables, however, are not related to oil

⁸ We find similar results using futures contracts of different maturities.

Table 1**The Ten Largest Changes in 12-Month Crude Oil Futures Prices**

| | Date | Price change | Wall Street Journal Description |
|----|-------------|---------------------|---|
| 1 | 1/17/91 | -0.13 | U.S. attacks Iraq. NYMEX opens with price controls on crude oil (first move of \$7.50 halts trading for 1 hr, the second in the same direction locks in a price floor or ceiling). Feb. contract falls \$10.56. Spot rose by \$5 then dropped \$15. |
| 2 | 8/5/86 | 0.11 | Oil prices soar on OPEC pact to cut output. Jump to \$15-a-barrel mark was prompted by news of two-month accord. |
| 3 | 4/8/86 | -0.098 | White House appears likely to endorse repeal of "windfall profits" tax on crude oil and moved to quell oil market jitters that U.S. support for free-market oil prices could change if prices drop too much. Chevron chairman criticizes Bush's remarks to Saudis. World oil prices plummeted on news that the Soviet Union has begun selling oil in Europe through netback transactions that could be adding more than a million barrels a day to overburdened world supply. |
| 4 | 2/24/86 | 0.095 | A Bermuda-based trading firm accused four major oil companies of conspiring to force crude oil prices lower to maximize refining profits and minimize tax payments. Saudi Arabia launched a campaign to deny responsibility for the oil price collapse while continuing to expand its world oil market share in ways certain to keep downward pressure on prices. |
| 5 | 09/24/01 | -0.088 | The Organization of Petroleum Exporting Countries plans to leave output quotas unchanged because of uncertainty over the global economy. Crude-oil prices tumbled 17% since Sept. 10, futures hit a 22-month low. |
| 6 | 10/22/90 | -0.084 | Crude oil futures for November delivery, which expired at yesterday's close, skidded \$5.41 a barrel to \$28.38. December futures were down the \$3 a barrel daily limit. Crude oil falls below \$30 as sentiment shifts after statements in Middle East. |
| 7 | 10/25/90 | 0.082 | Oil prices surge again on new Middle East fears. Traders in the slippery oil market bet that recent slide won't last. |
| 8 | 8/4/86 | 0.082 | OPEC considers oil-production quotas as Saudis' voluntary-cut plan stalls. |
| 9 | 2/4/86 | -0.082 | Oil contracts plunge as doubt grows. OPEC can stabilize petroleum prices. |
| 10 | 8/27/90 | -0.078 | OPEC meets as oil picture deteriorates; some to seek "blessing" to raise their output. A sense that Middle East tensions are easing. |

NOTE: This table reports the ten largest daily price movements (percent) in 12-month crude oil futures and the associated *Wall Street Journal* reports.

Table 2
Forecasting One-Quarter-Ahead Realized Oil Price Variance

| | <i>RV_O</i> | <i>RET_O</i> | <i>RV_S</i> | <i>RET_S</i> | <i>DEF</i> | <i>TERM</i> | <i>D_GDP</i> | ARSQ |
|---|---------------------|-------------------|-------------------|-------------------|------------------|-------------------|---------------------|--------|
| A. Without controlling for the lagged dependent variable | | | | | | | | |
| 1 | 0.565*** (0.050) | | | | | | | 0.310 |
| 2 | | -0.001 (0.069) | | | | | | -0.012 |
| 3 | | | 0.154 (0.251) | | | | | -0.009 |
| 4 | | | | -0.030 (0.121) | | | | -0.011 |
| 5 | | | | | 0.059 (0.042) | | | 0.055 |
| 6 | | | | | | -0.008 (0.005) | | 0.004 |
| 7 | | | | | | | -2.946** (1.397) | 0.044 |
| B. Controlling for the lagged dependent variable | | | | | | | | |
| 8 | 0.614*** (0.057) | 0.062 (0.047) | | | | | | 0.330 |
| 9 | 0.565*** (0.048) | | 0.029 (0.153) | | | | | 0.302 |
| 10 | 0.567*** (0.049) | | | -0.042 (0.104) | | | | 0.305 |
| 11 | 0.539*** (0.071) | | | | 0.019 (0.029) | | | 0.308 |
| 12 | 0.560*** (0.047) | | | | | -0.002 (0.004) | | 0.303 |
| 13 | 0.541*** (0.043) | | | | | | -1.066 (0.863) | 0.309 |
| 14 | 0.537*** (0.063) | 0.058 (0.043) | -0.136 (0.213) | -0.023 (0.106) | 0.029 (0.033) | -0.004 (0.005) | -1.064 (0.871) | 0.304 |

NOTE: The table reports the results of the forecasting regression for realized oil price variance over the period 1984:Q2–2004:Q4. Newey-West standard errors are reported in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively. ARSQ: Adjusted R².

The independent variables are *RV_O*, past realized oil variance; *RET_O*, the oil price change; *RV_S*, realized stock market variance; *RET_S*, stock market return; *DEF*, the default premium; *TERM*, the term premium; *D_GDP*, the growth rate of real GDP.

Table 3
Forecasting One-Quarter-Ahead GDP Growth, with Control for Macrovariables

| | <i>RV_O</i> | <i>RV_S</i> | <i>RET_S</i> | <i>DEF</i> | <i>TERM</i> | <i>D_GDP</i> | <i>ARSQ</i> |
|---|----------------------|------------------|--------------------|------------------|------------------|--------------------|-------------|
| 1 | -0.018** (0.007) | | | | | 0.218* (0.113) | 0.111 |
| 2 | -0.018*** (0.007) | 0.011* (0.07) | | | | 0.215* (0.110) | 0.138 |
| 3 | -0.017*** (0.006) | | -0.045* (0.024) | | | 0.207** (0.103) | 0.140 |
| 4 | -0.019*** (0.007) | | | 0.001 (0.001) | | 0.212* (0.113) | 0.102 |
| 5 | -0.017** (0.007) | | | | 0.000 (0.000) | 0.201* (0.114) | 0.110 |

NOTE: See the note for Table 2.

price volatility in either panel.⁹ Therefore, our results are consistent with the evidence in Table 1 that oil price volatility originates mainly from exogenous shocks to the U.S. economy rather than endogenous responses to these shocks.

OIL PRICE VOLATILITY AND GDP GROWTH

As the previous discussion makes clear, increases in the relative price of crude oil tend to have negative effects on output and employment, because the increases act as a tax on consumption. Moreover, because firms also face higher costs, increases in oil prices also tend to increase inflation.¹⁰ In this section we test whether oil price volatility also has negative effects on output and, in particular, whether uncertainty causes a delay in business investment, as mentioned previously. We addressed this issue by investigating whether realized oil price variance (*R_VO*) forecasts one-quarter-ahead real GDP growth; our results are reported in Table 3.

⁹ The growth of fixed nonresidential business investment also forecasts oil price volatility but loses the predictive power after we control for the lagged dependent variable. Moreover, changes in the federal funds rate target, a measure of monetary policy, have negligible forecasting power for realized oil price variance. These results are available upon request.

¹⁰ See recent speeches by Greenspan (2004) and Bernanke (2004).

Row 1 of Table 3 shows that oil price variance does have a significantly negative effect, even after we controlled for past GDP growth. Stock and Watson (2003), among many others, show that many macroeconomic variables help forecast real GDP growth. To address this issue, we investigated the possibility that realized oil price variance forecasts real GDP growth merely because of its co-movement with the macroeconomic variables used in Table 2. We found that, although stock market returns (row 3) and volatility (row 2) are marginally significant, they do not significantly diminish the usefulness of realized oil price variance to forecast real GDP growth. Similarly, the default premium (row 4) and the term spread (row 5) do not reduce the significance of realized oil price variance to help forecast one-quarter-ahead real GDP growth.

GRANGER CAUSALITY TESTS

To formally address whether oil price uncertainty has a significant effect on output, we also conducted Granger causality tests (as in Hamilton, 1983, 1996, and 2003) and report the results in Table 4. In particular, we regressed real GDP growth on its own lags and lagged realized oil price variances as well as the other variables. If realized oil price variance has no effect on output,

Table 4
Granger Causality Tests Using Realized Oil Price Variance

| Lags | <i>RV_O</i> | <i>RET_O</i> | <i>MAX_RET_O</i> | <i>D_GDP</i> | ARSQ |
|---|----------------------|--------------------|----------------------|---------------------|-------|
| A. Quarterly data without oil prices | | | | | |
| 1 | -0.020** (0.009) | | | 0.133 (0.106) | 0.167 |
| 2 | 0.012 (0.010) | | | 0.302*** (0.089) | |
| $\chi^2(2)$ | 5.579 [0.061] | | | 18.063 [0.000] | |
| B. Quarterly data with <i>RET_O</i> | | | | | |
| 1 | -0.023*** (0.009) | -0.004 (0.003) | | 0.127 (0.100) | 0.167 |
| 2 | 0.013 (0.009) | 0.001 (0.002) | | 0.319*** (0.091) | |
| $\chi^2(2)$ | 9.249 [0.010] | 2.229 [0.328] | | 18.437 [0.000] | |
| C. Quarterly data with <i>MAX_RET_O</i> oil prices | | | | | |
| 1 | -0.011 (0.007) | -0.002 (0.003) | -0.012 (0.011) | 0.081 (0.091) | |
| 2 | 0.010 (0.007) | 0.003 (0.003) | -0.027*** (0.009) | 0.292*** (0.078) | 0.214 |
| $\chi^2(2)$ | 3.276 [0.194] | 2.785 [0.248] | 10.485 [0.005] | 16.203 [0.000] | |
| D. Monthly data with industrial production | | | | | |
| 1 | -0.014*** (0.004) | 0.008** (0.004) | | -0.013 (0.073) | 0.157 |
| 2 | 0.001 (0.008) | -0.003 (0.005) | | 0.190*** (0.065) | |
| 3 | -0.009** (0.005) | -0.009* (0.005) | | 0.226*** (0.064) | |
| 4 | 0.012*** (0.005) | -0.005 (0.005) | | 0.098 (0.063) | |
| $\chi^2(4)$ | 27.940 [0.000] | 7.234 [0.124] | | 26.108 [0.000] | |

NOTE: The table reports the results of the forecasting regression for growth of real GDP (Panels A to C) and industrial production (Panel D) using realized oil price variance over the period 1984 to 2004. Newey-West standard errors are in parentheses; ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively. The number of lags is determined by the Akaike information criterion. The last row of each panel reports the Wald test statistics (with the null hypothesis that lags of each variable are jointly insignificant), which has a χ^2 distribution with the degrees of freedom equal to the number of lags; *p*-values for these Wald statistics are in brackets; ARSQ is adjusted R^2 ; see variable descriptions in the note for Table 2.

we should expect that its lags jointly have no explanatory power for real GDP growth. We tested this hypothesis using the Wald test, which has a χ^2 distribution with the degrees of freedom equal to the number of lags. Unless otherwise indicated, we chose the number of lags (which is two in our sample) using the Akaike information criterion (AIC); however, we found qualitatively the same results using four lags, as in Hamilton (1983, 1996, and 2003).

In Panel A of Table 4, we included two lags of the realized oil price variance and the lagged dependent variable in the forecasting equation. Consistent with the results reported in Table 3, the one-quarter-lagged realized oil price variance is significantly negative; however, the two-quarter-lagged realized variance is actually positive, although statistically insignificant. Overall, the Wald test indicates that realized oil price variance has a marginally significant effect, with a p-value of 6 percent.

We also included raw oil price changes (*RET_O*) in the forecasting equation and report the results in Panel B of Table 4. In this specification, we explicitly considered two distinct effects of oil price changes on output and expect that both *RV_O* and *RET_O* have negative effects. Interestingly, the Wald test indicates that the overall effect of realized oil price variance becomes significant at the 1 percent level. The sum of coefficients of lagged *RET_O* is also negative, as expected; however, it is not statistically significant. Our results indicate that both channels might be important, because we uncovered more significant results when including both the oil price and its variance in the forecasting equation. As we show below, the coefficients on *RET_O* are not by themselves statistically significant, possibly because of the relatively small number of observations.

In Panel C of Table 4, we also include Hamilton's (2003) transformed oil price measure, *MAX_RET_O*, with a 12-quarter horizon. It is negative and significant at the 1 percent level; moreover, it subsumes the information content of both the oil price change (*RET_O*) and its volatility (*RV_O*). This result provides support that Hamilton's specification captures overall

effects of oil prices on aggregate output and, therefore, its forecasting abilities cannot be entirely attributed to data mining.

To check for robustness, we also analyzed monthly data for industrial production growth and report the results in Panel D of Table 4. Consistent with quarterly data, realized oil price variance is highly significant but the oil price change is not.¹¹

With only 20 years of observations, we were concerned that the results might be sample specific. To address this issue, we also used a longer sample, originally analyzed by Hamilton (2003), and updated the data through 2004. We used the squared oil price change as a proxy for oil price volatility and report the results in Table 5. Oil price variance by itself is not significant (χ^2 test statistic in Panel A) at the 10 percent level; however, it becomes marginally significant when combined with the change in oil prices, *RET_O*, which itself is highly significant (Panel B). Therefore, over the longer sample, we found that both channels through which oil prices affect the macroeconomy are important. Again, as shown in Panel C, both variables lose their forecasting power after we control for *MAX_RET_O*, which itself is highly significant.

OIL PRICE VOLATILITY, INVESTMENT, AND EMPLOYMENT

As discussed previously, the delay hypothesis suggests that oil price volatility can affect output mainly because it deters business investment in capital goods, especially those with longer-service lives.¹² Moreover, since employment growth tends to be highly dependent on output growth, a corollary to this hypothesis is that increases in oil price volatility decrease employment growth and increase the unemployment rate. Our results in Table 6 are generally consistent with this hypothesis.

¹¹ This finding is consistent with the results by Federer (1996), who found that oil price volatility improves forecasts of industrial production at a monthly frequency.

¹² More formally, if an investment is irreversible, increased uncertainty raises the option value of waiting to invest. See Bernanke (1983), Pindyck (1991), and Hubbard (1998).

Table 5
Granger Causality Tests: 1947:Q2 to 2004:Q4

| Lags | <i>RV_O</i> | <i>RET_O</i> | <i>MAX_RET_O</i> | <i>D_GDP</i> | <i>ARSQ</i> |
|--|----------------------|----------------------|----------------------|---------------------|-------------|
| A. Without oil prices | | | | | |
| 1 | -0.020** (0.010) | | | 0.285*** (0.062) | 0.123 |
| 2 | -0.006 (0.101) | | | 0.133* (0.078) | |
| 3 | -0.008 (0.107) | | | -0.086 (0.064) | |
| 4 | -0.014 (0.008) | | | -0.121 (0.074) | |
| $\chi^2(4)$ | 6.410 [0.171] | | | 34.449 [0.000] | |
| B. With <i>RET_O</i> | | | | | |
| 1 | -0.024*** (0.009) | -0.002 (0.005) | | 0.271*** (0.061) | 0.140 |
| 2 | -0.002 (0.007) | -0.007 (0.004) | | 0.126 (0.077) | |
| 3 | -0.011 (0.009) | -0.002 (0.004) | | -0.083 (0.062) | |
| 4 | -0.010 (0.014) | -0.016*** (0.004) | | -0.129* (0.071) | |
| $\chi^2(4)$ | 8.742 [0.068] | 17.491 [0.002] | | 33.122 [0.000] | |
| C. With <i>RET_O</i> and <i>MAX_RET_O</i> | | | | | |
| 1 | -0.013 (0.009) | 0.000 (0.004) | -0.018 (0.019) | 0.224*** (0.062) | 0.173 |
| 2 | 0.006 (0.010) | -0.001 (0.005) | -0.019 (0.019) | 0.110 (0.075) | |
| 3 | -0.007 (0.011) | 0.004 (0.005) | -0.017 (0.013) | -0.097 (0.060) | |
| 4 | 0.008 (0.014) | -0.002 (0.004) | -0.042*** (0.013) | -0.157** (0.070) | |
| $\chi^2(4)$ | 3.034 [0.552] | 1.116 [0.892] | 28.237 [0.000] | 24.588 [0.000] | |

NOTE: The table reports the results of the forecasting regression for GDP growth using realized oil price variance over the period 1984-2004. Newey-West standard errors are in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively. The last row of each panel reports the Wald test statistics, which determine whether the lags of each variable are jointly insignificant. These statistics have a χ^2 distribution with the degrees of freedom equal to the number of lags; *p*-values for these Wald statistics are in brackets. See variable descriptions in the note for Table 2.

Table 6
Forecasting GDP Components and Labor Market Variables

| Lags | <i>RV_O</i> | <i>RET_O</i> | Lagged dependent variable | ARSQ |
|---|-------------------|------------------|---------------------------|-------|
| A. Nonresidential business fixed investment | | | | |
| 1 | -0.072*** (0.025) | 0.003 (0.008) | 0.254** (0.100) | 0.329 |
| 2 | 0.010 (0.029) | 0.006 (0.008) | 0.305*** (0.109) | |
| $\chi^2(2)$ | 10.378 [0.006] | 0.593 [0.743] | 17.749 [0.000] | |
| B. Structures investment | | | | |
| 1 | -0.157*** (0.050) | 0.028* (0.017) | 0.044 (0.099) | 0.215 |
| 2 | 0.041 (0.054) | 0.018 (0.015) | 0.262** (0.108) | |
| $\chi^2(2)$ | 13.855 [0.000] | 6.737 [0.034] | 5.949 [0.051] | |
| C. Equipment and software investment | | | | |
| 1 | -0.034 (0.049) | -0.008 (0.008) | 0.239** (0.104) | 0.173 |
| 2 | -0.036 (0.029) | -0.000 (0.013) | 0.232** (0.104) | |
| $\chi^2(2)$ | 6.565 [0.038] | 0.918 [0.632] | 10.999 [0.004] | |
| D. Personal consumption expenditures, durable goods | | | | |
| 1 | -0.035 (0.053) | -0.017 (0.015) | -0.284*** (0.108) | 0.033 |
| 2 | -0.036 (0.062) | -0.027 (0.020) | -0.166 (0.166) | |
| $\chi^2(2)$ | 0.847 [0.655] | 2.638 [0.267] | 6.914 [0.032] | |
| E. Personal consumption expenditures, nondurable goods | | | | |
| 1 | -0.012 (0.016) | -0.008* (0.004) | -0.079 (0.112) | 0.016 |
| 2 | -0.005 (0.012) | 0.003 (0.003) | 0.157 (0.118) | |
| $\chi^2(2)$ | 5.414 [0.067] | 3.904 [0.142] | 3.312 [0.191] | |
| F. Personal consumption expenditures, services | | | | |
| 1 | -0.021*** (0.007) | -0.005** (0.002) | 0.229** (0.101) | 0.176 |
| 2 | 0.017*** (0.005) | -0.000 (0.002) | 0.254*** (0.097) | |
| $\chi^2(2)$ | 13.577 [0.001] | 6.340 [0.042] | 26.584 [0.000] | |
| G. Nonfarm payroll employment | | | | |
| 1 | -0.003*** (0.001) | -0.000 (0.000) | 0.813*** (0.123) | 0.748 |
| 2 | 0.003** (0.001) | -0.001** (0.000) | 0.042 (0.111) | |
| $\chi^2(2)$ | 19.193 [0.001] | 6.862 [0.032] | 338.159 [0.000] | |
| H. Civilian unemployment rate | | | | |
| 1 | 0.795*** (0.189) | 0.147 (0.096) | 1.507*** (0.089) | 0.973 |
| 2 | -0.744*** (0.267) | -0.116 (0.081) | -0.537*** (0.089) | |
| $\chi^2(2)$ | 20.150 [0.000] | 3.897 [0.142] | 2,693.344 [0.000] | |

NOTE: The table reports the results of the one-quarter-ahead forecasting regression for GDP components, payroll employment, and the unemployment rate, using realized oil price variance over the period 1984-2004. Newey-West standard errors are reported in parentheses. ***, **, and * denote significance at the 1, 5, and 10 percent levels, respectively. The last row of each panel reports the Wald test statistics, which determine whether the lags of each variable are jointly insignificant. These statistics have a χ^2 distribution with the degrees of freedom equal to the number of lags; *p*-values for these Wald statistics are in brackets. The independent variables are *RV_O*, past realized oil variance, and *RET_O*, the oil price change.

As seen by the Wald statistics in Panel A of Table 6, forecasts for real business (nonresidential) fixed investment (BFI) growth one quarter ahead improve with the use of the volatility (RV_O) but not the level of oil prices (RET_O).¹³ Moreover, the sum of the coefficients of the lagged values of RV_O are negative, meaning that increases in oil price volatility predict weaker growth of BFI in the following quarter. We reestimated the regression using the two components of BFI: structures (Panel B) and equipment and software (Panel C). In the former case, lagged oil prices and lagged oil price volatility are statistically significant predictors, but in the latter case, only volatility matters (the signs were also correct).

In the next three panels, we report results for forecasts of consumption of real durable goods (Panel D), real nondurable goods (Panel E), and real services (Panel F). Both the level and volatility of oil prices appear to have little effect on the growth of consumption of durable goods and nondurable goods, which may be surprising to some because the conventional wisdom is that higher oil prices act as a consumption tax. However, they are both significant in the forecast of real services, perhaps because they include expenditures on such items as utilities and transportation services, which are energy sensitive.

The final two panels report results for one-quarter-ahead forecasts of nonfarm employment (Panel G) and the unemployment rate (Panel H). Both the level and volatility of oil prices are significant in forecasting employment, whereas only volatility matters for forecasting the unemployment rate.

CONCLUSION

The results of this paper are consistent with much of the previous research that suggests that oil matters. In particular, using a measure of volatility constructed from daily crude oil futures prices traded on the NYMEX, we find that over the period 1984-2004 oil price volatility has had a significant and adverse effect on various key

measures of the U.S. macroeconomy such as fixed investment, consumption, employment, and the unemployment rate. This finding, which is consistent with the nonlinear effect documented by Hamilton (1996 and 2003), means that an increase in the price of crude oil from, say, \$40 to \$50 per barrel generally matters less than increased uncertainty about the future direction of prices (increased volatility).

We also find that standard macroeconomic variables do not forecast realized oil price volatility, which suggests that changes in the supply and demand for crude oil that raise the variance of future crude oil prices tend to reflect stochastic disturbances. This finding implies that crude oil price volatility is mainly driven by exogenous (random) events such as significant terrorist attacks and military conflicts in the Middle East.

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¹³ Because of the relatively small sample size, in Table 6 we set the number of lags to 2.

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An Analysis of Recent Studies of the Effect of Foreign Exchange Intervention

Christopher J. Neely

Two recent strands of research have contributed to our understanding of the effects of foreign exchange intervention: (i) the use of high-frequency data and (ii) the use of event studies to evaluate the effects of intervention. This article surveys recent empirical studies of the effect of foreign exchange intervention and analyzes the implicit assumptions and limitations of such work. After explicitly detailing such drawbacks, the paper suggests ways to better investigate the effects of intervention.

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Foreign exchange intervention is the practice of monetary authorities buying and selling currency in the foreign exchange market to influence exchange rates. Researchers have studied whether intervention is successful in influencing exchange rate movements and how it affects volatility. Secondly, they have asked how the type of intervention affects these results and through which channels it might operate.

Intervention has several characteristics that complicate one's ability to study it. It is conducted sporadically, with several interventions over the course of a few days or weeks. Thus, it has an unusual distribution. Intervention policy is rarely stable for long periods. Finally, because intervention quickly reacts to exchange rate movements and other variables, exchange rates and intervention are determined simultaneously. These problems have made it difficult to show that central bank intervention has reduced exchange rate volatility or moved the exchange rate in the desired direction. Yet, every central banker surveyed in Neely (2000)—those who actually conduct intervention—remains con-

vinced that intervention is effective in changing the exchange rate.¹

Recently two phenomena have advanced our understanding of intervention. The first is the use of *event studies* to evaluate the effects of intervention. Generically, an event study is an examination of asset price behavior associated with some event, such as a merger, announcement, or intervention. Event studies are used to assess the market's reaction to the event, how the event influenced prices, and whether the market priced the event efficiently. The second advance is the use of high-frequency data—both exchange rates and intervention—to better understand the behavior of exchange rates immediately around intervention.

Despite these advances, inferring the effects of central bank intervention remains difficult. Although describing the data is a worthy and necessary goal, explaining the nature of the process by which exchange rates and intervention

¹ Neely (2000) received responses from the central banks of Belgium, Brazil, Canada, Chile, the Czech Republic, Denmark, France, Germany, Hong Kong, Indonesia, Ireland, Italy, Japan, Mexico, New Zealand, Poland, South Korea, Spain, Sweden, Switzerland, Taiwan, and the United States.

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are jointly determined requires strong assumptions, which are rarely explicitly stated. While many intervention researchers are doubtless cognizant of such issues, those less familiar with the literature are probably not well aware of them. The purpose of this article is to selectively review the recent literature on the effects of intervention and to analyze the assumptions and limitations of such exercises.² Identifying the assumptions and limitations of the intervention literature is not to condemn those procedures. Rather such recognition enables the limitations to be better understood and overcome. This paper does not expend much effort describing the disparate conclusions of the literature. The appendix summarizes such conclusions and specific methods for interested readers.

This article first discusses central bank intervention practices and explains how researchers typically study intervention. Selected intervention studies are then discussed. The fourth section considers the assumptions behind intervention studies, with a special emphasis on the often implicit assumptions behind the new event-study methodologies. In its conclusion, the article discusses the strengths and weaknesses of the methods of studying the effects of intervention and suggests avenues for future research.

CENTRAL BANK INTERVENTION

After the breakdown of the Bretton Woods system of fixed exchange rates in 1973, the Articles of the International Monetary Fund (IMF) were amended to provide that members “would collaborate with the Fund and other members to assure orderly exchange arrangements and to promote a stable system of exchange rates.” IMF members could choose their own exchange rate arrangements subject to the proviso that they avoid exchange rate manipulation and foster orderly economic growth. Many countries choose to float their exchange rates and conduct occasional

foreign exchange intervention to influence the value of their currencies.³

Central banks choose to intervene for different reasons. The Foreign Currency Directive of the Federal Reserve System, for example, directs intervention to “counter disorderly market conditions,” which has been interpreted differently at different times.⁴ Often, excessive exchange rate volatility or deviations from long-run equilibrium exchange rates have prompted intervention. Multiple central banks often *coordinate* intervention, intervening in the same direction on the same day.

The response rule of central bank intervention to economic conditions is known as the central bank’s *intervention reaction function*. Neely (2002) estimates a typical reaction function for U.S. intervention with a *friction model*. A friction model permits the dependent variable—intervention—to be insensitive to its determinants over a range of values (Rosett, 1959). This is appropriate for a variable such as intervention that takes the value zero for a large proportion of observations. The study confirms previous findings that U.S. intervention “leans against the wind” and is conducted to counter misalignment. Leaning-against-the-wind intervention is conducted to oppose strong short-term trends. For example, if the U.S. dollar (USD) has been depreciating, a USD purchase would constitute leaning against the wind. Misalignment means that the exchange rate deviates from what the monetary authorities might regard as long-run fundamentals, such as those implied by a purchasing power parity relation.

Figure 1 shows U.S. intervention in the Deutsche mark (DEM) market, as well as the exchange rate, compared with a purchasing power

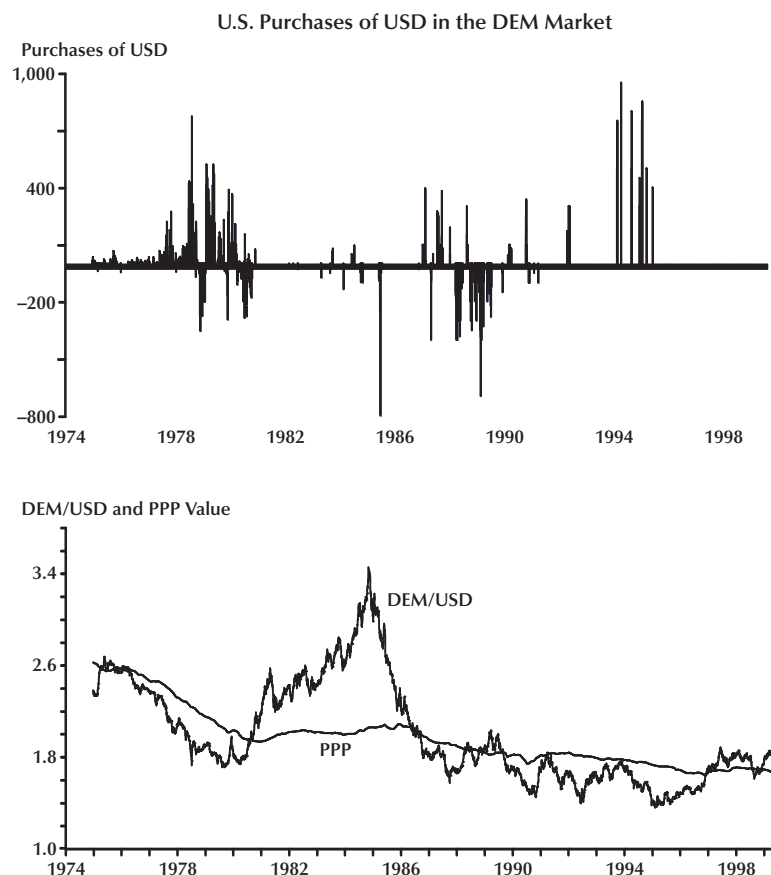
² This paper is a fairly narrow and selective survey of the intervention literature. Edison (1993), Sarno and Taylor (2001), and Humpage (2004) provide more wide-ranging treatments of intervention studies. The Bank for International Settlements (BIS) (2005) provides a range of views on intervention in emerging markets.

³ In the United States, for example, the Federal Reserve and the U.S. Treasury generally collaborate on foreign exchange intervention decisions, and the Federal Reserve Bank of New York conducts operations on behalf of both. Humpage (1994) and Cross (1998) describe the institutional aspects of U.S. intervention, whereas Edison (1993) reviews the extensive literature on central bank intervention.

⁴ The directive mandates intervention in cooperation with foreign central banks, consistent with International Monetary Fund Article IV, Section 1, that forbids attempts to remedy balance-of-payments problems by manipulating exchange rates. “The Foreign Currency Directive” is published annually in the *Federal Reserve Bulletin* with the minutes of the first Federal Open Market Committee meeting of each year.

Figure 1

Intervention, Exchange Rates, and Purchasing Power Parity Fundamentals



parity-based fundamental value. Statistical analysis confirms the impression that U.S. authorities tend to purchase USD when the USD is relatively undervalued and sell USD in the reverse circumstance. Leahy (1995) and Neely (1998) find that U.S. authorities make substantial profits as a result of this intervention strategy.⁵

When a central bank buys (sells) its own currency in exchange for a foreign currency, it decreases (increases) the amount of its currency in circulation, lowering (raising) its domestic money supply. By itself, this transaction would

influence exchange rates in the same way as ordinary domestic open market operations; however, most central banks routinely “sterilize” their foreign exchange operations; that is, they buy and sell domestic bonds to reverse the effect of the foreign exchange operation on the domestic money supply (Edison, 1993).⁶ For example, if the Federal Reserve Bank of New York bought \$100 million worth of euros (EUR) in a foreign exchange intervention, the U.S. monetary base would increase by \$100 million in the absence of sterilization. Other things equal, interest rates and prices would also change. To prevent changes

⁵ Although U.S. authorities—as with those of many other countries—have profited from their foreign exchange intervention activities, this does not mean that profit is the goal of those trades; it is merely a side benefit.

⁶ Conducting monetary policy by way of a short-term interest rate target automatically sterilizes intervention.

to domestic interest rates and prices, the Federal Reserve Bank of New York would sterilize the intervention—sell \$100 million worth of government securities—and absorb the liquidity. Complete sterilization would also require that the foreign central bank—the European Central Bank (ECB) in the case of the EUR—automatically reverse the effect of the intervention on the foreign money market by increasing the supply of foreign currency through open market operations. The net effect would be to increase the relative supply of U.S. government securities versus foreign securities but to leave domestic and foreign money supplies unchanged.

Because fully sterilized intervention doesn't affect either prices or interest rates, it doesn't influence the exchange rate directly. But official intervention might affect the foreign exchange market indirectly through the *portfolio balance* channel and/or the *signaling* channel.

The portfolio balance theory recognizes that sterilized intervention changes the relative supplies of bonds denominated in different currencies. If bonds in different currencies are imperfect substitutes, investors must be compensated with a higher expected return to hold the relatively more numerous bonds. The higher return must result from a change in either the price of the bonds or the exchange rate.

The signaling channel suggests that official intervention communicates, or signals to the market, information about future monetary policy or the long-run equilibrium value of the exchange rate. Complicating a belief in the signaling channel is the fact that central banks often conduct intervention secretly. In fact, 77 percent of central banks report that they sometimes or always conduct intervention secretly to maximize market impact (Neely, 2000).

ESTIMATING THE EFFECTS OF INTERVENTION

The most important questions confronting researchers on intervention are as follows: What effect does intervention have on the level and volatility of exchange rates? To what conditions do central banks respond? Secondarily, how do

factors such as coordination, direction, secrecy, and the amount of intervention affect the answers to those questions?

Researchers have used at least three types of studies to investigate these questions: By far the most common type of study has been a *time-series event study*. More recently, researchers have pursued a different type of event study in which interventions are grouped into clusters and the effect of the cluster is considered as one event. These will be termed *other event studies*. Both types of event studies examine the behavior of exchange rates around intervention, without making explicit assumptions about the data-generating process. The third—and least common—type of study is an explicitly identified structural analysis of the effects of intervention. We briefly describe each of these procedures before proceeding to a literature review.

Time-Series Event Studies

Time-series event studies have a long history: Humpage (1984) and Dominguez and Frankel (1993) are two early efforts. Such studies typically investigate the effect of intervention on returns using a single equation in which intervention (I_t) and a limited set of regressors explain the change in the exchange rate (ΔS_t):

$$(1) \quad \Delta S_t = c_r + \beta I_t + Ax_{1t} + e_t,$$

where $\{c_r, \beta, A\}$ is the coefficient vector, S_t is units of foreign currency per unit of domestic currency, and the set of regressors x_{1t} might include interest rate differentials or macroeconomic news or other variables that might influence the exchange rate. How the data are timed is important in such regressions. Variables are usually defined so that intervention at time t would occur during the exchange rate change of the same date. In other words, if exchange rates are collected at the end of the business day, $\Delta S_t = \ln S_t / S_{t-1}$, then intervention explains contemporaneous exchange rate changes. Such studies interpret the coefficient β as the effect of intervention on exchange rate changes.

Recently, researchers have begun studying the effect of intervention on option-implied volatility

(IV), implied skewness, kurtosis, and even correlations by using a regression setup similar to (1) (Campa and Chang, 1998).⁷ Such studies have most of the strengths and limitations of studies of the effect of intervention on returns.

The other common way to study the effect of intervention on volatility is with a GARCH (1,1) model (Bollerslev, 1986) in which intervention and other variables can influence exchange rate conditional variance (h_t) contemporaneously, as follows:

$$(2) \quad h_t = \omega + \beta h_{t-1} + \alpha e_{t-1}^2 + b_I I_t.$$

Such specifications frequently also include lagged values of intervention and/or indicator variables for weekends and holidays as explanatory variables in the GARCH model. Again, studies interpret the coefficient on intervention (b_I) as the effect of intervention on volatility.

Although it is not the subject of this paper, it is worth noting that there is also a large—and usually unconnected—literature estimating intervention reaction functions. Such studies usually describe intervention as a function of contemporaneous and past exchange rate changes and volatility, lagged intervention, and deviations from some exchange rate target:

$$(3) \quad I_t = \sum_{i=0}^P a_i \Delta S_{t-i} + \sum_{i=1}^P b_i I_{t-i} + c(S_t - \bar{S}_t) + d + v_t.$$

Limited dependent variable frameworks, such as the friction model of Rosett (1959) or Tobin's (1958) Tobit model, are often applied to intervention because of its unusual distribution.

Other Event Studies

The second class of event study typically uses data only from around periods of intervention, ignoring the behavior of exchange rates when there is no intervention. Such studies provide a seemingly natural way to model the sporadic nature of intervention. As Fatum and Hutchison note,

An event study framework is better suited to the study of sporadic and intense periods of official intervention, juxtaposed with continuously changing exchange rates, than standard time-series studies. Focusing on daily Bundesbank and US official intervention operations, we identify separate intervention “episodes” and analyse the subsequent effect on the exchange rate. (Fatum and Hutchison, 2003b, p. 390)

To conduct an event study, one must define the events, a window around the event, a success criterion, and a method of evaluating the success criterion. Events might be defined as a single intervention or a series of interventions in the same direction within a short time. Windows are typically chosen to be from 1 to 30 days. The exchange rate behavior in the pre-event window is compared with exchange rate behavior in the post-event window.

Choosing an event window requires one to make trade-offs. Longer event windows permit researchers to judge the overall effect of related interventions. On the other hand, longer windows increase the danger of omitting important variables that influence exchange rates. Perhaps more seriously, monetary authorities might intervene until the exchange rate moves in the desired direction. Even if intervention has no influence on exchange rates, if the authority keeps intervening until it observes the desired outcome, then intervention appears to be successful. Longer event windows increase this danger.

Researchers have considered various success criteria. The most commonly used are the *direction criterion* and the *smoothing criterion* (Humpage, 2000). The *direction criterion* defines intervention as successful if the purchased currency appreciates after an intervention. That is, a USD purchase would be successful if the dollar appreciated in the post-event window. But, mindful that most intervention is “against the wind”—that is, the authorities are buying the currency that is depreciating—one might also consider an official purchase to be successful if the purchased currency depreciates less in the post-event window than in the pre-event window. The standard that the intervention should moderate the pre-event trend

⁷ Option-implied volatility, implied skewness, and implied kurtosis are measures of the second, third, and fourth moments of the distribution of the underlying asset that are obtained from options prices. Neely (2005c) discusses implied volatility in some detail.

in the exchange rate is known as the *smoothing criterion*.

Once the success criterion is defined, one needs some method to evaluate whether it has been achieved. In strictly narrative studies, the researcher might simply graph the data or compute simple summary statistics, such as the percentage of successes or mean change in the exchange rate, to informally judge whether intervention has been successful. Otherwise, one formally tests whether differences between pre- and post-event behavior are statistically significant.

Humpage (1999 and 2000), for example, examines whether one can reject that the observed number of exchange rate changes of a given type (e.g., depreciations) come from a null distribution.⁸ In other words, for example, does the Japanese yen (JPY) depreciate more often than one would expect when the Fed sells JPY for USD? The number of successes under the null of no effect is distributed as a hypergeometric random variable. Humpage goes on to test whether successful interventions are related to factors such as amount, coordination, and secrecy by regressing success indicators on those factors in a probit framework.

Fatum and Hutchison (2003a) similarly test whether the number of “successful” interventions is greater than one would expect if intervention were ineffective. And they use a “matched sample” *t*-test to ask whether the mean post-intervention exchange rate change is statistically significantly different from the mean pre-intervention change.

STUDIES OF THE EFFECTS OF INTERVENTION

Event studies have recently gained greater popularity, particularly those that consider a cluster of interventions as one event and/or use nonparametric methods to evaluate the success of those interventions. The difficulties of applying traditional structural econometric techniques—simultaneity, identification, the unusual distri-

bution of intervention—have doubtless played a significant role in the rise of such studies. This section first enumerates some recent event studies before considering some explicitly identified investigations of intervention.

Event Studies with Daily Data

Many papers using daily intervention and exchange rate data describe themselves as event studies: Fatum and Hutchison (2003a,b) and Edison, Cashin, and Liang (2003). Other papers can reasonably be described as event studies—even though they do not use that term—because they characterize the behavior of exchange rates around periods of intervention, without explicitly identifying a structural relation: Humpage (1999, 2000), Aguilar and Nydalh (2000), Kim, Kortian, and Sheen (2000), Ito (2002), and Chaboud and Humpage (2005). Such studies provide mixed support for the hypothesis that intervention influences exchange rates in the desired direction and also mixed conclusions as to its effect on volatility. Coordinated interventions were usually found to be more successful than unilateral interventions.

Intraday Event Studies

More recently, a third group of papers have used intraday data to evaluate the behavior of exchange rates at very high frequencies around the times of intervention. Fischer and Zurlinden (1999), Payne and Vitale (2003), and Pasquariello (2002) have exploited the fact that the Swiss National Bank has released data on the exact times of intervention, not just the day and amount. Fischer and Zurlinden (1999) look at irregularly timed observations at times of intervention to examine the effects of intervention. Payne and Vitale (2003) use exchange rate data sampled at 15-minute intervals to quantify the effects of intervention operations on the U.S. dollar/Swiss franc (USD/CHF) rate. Pasquariello (2002) looks at a wider variety of exchange rate behavior—including spreads—in a similar exercise. Beattie and Fillion (1999) use confidential timed intervention data from the Bank of Canada to similarly investigate the intraday effects of Canadian inter-

⁸ Humpage's studies can be interpreted as time-series event studies in which the dependent variable is an indicator variable that depends on the behavior of exchange rates, conditional on intervention. Such tests use all the exchange rate data, not just data around interventions.

vention. Fatum and King (2005) compare the effects of Canadian intervention on high-frequency data over periods with both rule-based and discretionary intervention. They find that intervention does systematically affect the Canadian dollar/U.S. dollar (CAD/USD) rate and might be associated with reduced volatility. Finally, Dominguez (2003a,b) regresses 5-minute exchange rate returns and 5-minute volatility on leads and lags of news announcement and intervention news dummies—taken from Reuters reports, collected by Olsen and Associates—during days of U.S. intervention from 1987 to 1993. Dominguez interprets the coefficients on intervention and news dummies as showing the impact of those events on exchange rate behavior at that horizon. The consensus of these papers has been that interventions successfully move exchange rates, at least in the very short term.

Identified Studies of Intervention

Not all studies of intervention can be classified as event studies. Some explicitly model structural economic relations to identify the effect of intervention on exchange rate behavior.⁹ Three such studies are those of Kim (2003), Kearns and Rigobon (2005), and Neely (2005b).

Kim (2003), for example, estimates a structural vector autoregression (VAR) adapted from the monetary policy literature to examine the effects of intervention and monetary policy on a trade-weighted exchange rate. The monthly data span 1974:01–1996:12 and include the ratio of foreign exchange intervention to a quadratic trend in the monetary base, the federal funds rate, monetary aggregates, the consumer price index, industrial production, the trade-weighted exchange rate, and commodity prices. The specification permits two-way contemporaneous interaction between intervention and exchange rates, the federal funds rate and the monetary aggregates, and the federal funds rate and commodity prices. The inclusion of

monetary policy measures and macro variables might mitigate the problem of omitted variables bias: If some independent variables are omitted from a relation, then one will generally not get consistent estimates of coefficients on correlated regressors. Unfortunately, the low-frequency monthly macro data will miss the important high-frequency interactions and complicates the task of sorting out the interaction between intervention and exchange rates.¹⁰ Thus, Kim (2003) estimates a rich set of macroeconomic relations and policy interactions, at the price of greater simultaneity bias and possibly noisier parameter estimates from lower-frequency data.

Neely (2005a) shows, however, that some parameters are not identified in Kim's (2003) study. Identification requires that one have at least as many estimable moments from the reduced form as there are structural parameters. But that is only a necessary condition (i.e., the order condition) to identify all the parameters; it is not sufficient. Unfortunately, the system fails the rank condition (Hamilton, 1994). A subset of the parameters—including those governing the cross-reactions of exchange rates and intervention—appear to be unidentified. This calls the estimated impulse responses into question, though they might be interpreted as a set (not unique) of impulse responses that are consistent with the data.

Kearns and Rigobon (2005) perform an innovative study that takes advantage of structural breaks in the Japanese and Australian authorities' reaction functions to estimate a nonlinear model of intervention.¹¹ The first equation describes the reaction of the exchange rate return to intervention, I_t , exogenous variables, z_t , and an exchange rate shock, ε_t :

$$(4) \quad \Delta S_t = \beta I_t + \gamma z_t + \varepsilon_t.$$

The second equation is a central bank reaction function that describes the “shadow” intervention level, I_t^* , as a function of exchange rate changes (ΔS_t) and exogenous variables:

⁹ In models derived from a microeconomic optimization problem, the parameters are often termed “deep” structural parameters and will be invariant to changes in the economic environment. This paper uses the more traditional meaning of what are sometimes called pseudo-structural parameters—they have economic interpretations.

¹⁰ Kim (2003) does make an effort to capture the higher-frequency interaction with a separate exercise.

¹¹ This simplified version of the model suppresses constants and lags to facilitate the explanation of the identification scheme.

$$(5) \quad I_t^* = \delta \Delta S_t + z_t + \eta_t.$$

The third equation models the binary decision to intervene if shadow intervention exceeds some threshold. $\text{Ind}(\cdot)$ is an indicator function that equals 1 if its argument is true and 0 otherwise:

$$(6) \quad I_t = \text{Ind}(|I_t^*| > \bar{I}) \cdot I_t^*.$$

This simplified model has seven parameters of interest $\{\beta, \gamma, \delta, \bar{I}, \sigma_\varepsilon, \sigma_\eta, \sigma_z\}$, but there are only five moments of the data: the probability of intervention, the variance of the exchange rate when there is no intervention, and the three elements of the covariance matrix when an intervention has taken place. Clearly, one cannot estimate seven independent structural parameters with five moments.

But, if one allows for the break in the threshold of intervention, then the threshold takes the low value, \bar{I}_l , prior to the break in the reaction function at \hat{t} and the high value, \bar{I}_h , after the break. The intervention decision can be expressed as follows:

$$(7) \quad I_t = \begin{cases} \text{Ind}(|I_t^*| > \bar{I}_l) \cdot I_t^* & t < \hat{t} \\ \text{Ind}(|I_t^*| > \bar{I}_h) \cdot I_t^* & t > \hat{t} \end{cases}$$

Allowing for the break in the reaction function and assuming that other structural parameters do not change after the break, the model has one more structural parameter— $\{\bar{I}_l, \bar{I}_h\}$ instead of $\{\bar{I}\}$ —but one can compute 10 moments from the data: 5 from the pre-break period and 5 from the post-break period. The system now can be identified.¹²

Kearns and Rigobon (2005) estimate the model by the simulated method of moments and interpret their estimates of β as indicating that intervention has a large effect on the Australian dollar/U.S. dollar (AUD/USD) exchange rate and a smaller effect on the JPY/USD rate. The baseline model estimates that a sale of \$100 million is associated with a 1.81 percent AUD appreciation but just a 0.2 percent JPY appreciation. Kearns and Rigobon (2005) go on to calculate impulse response functions for more elaborate models, emphasizing the importance of estimating dynamic responses.

¹² Of course, even if one has more estimable moments from the data than parameters to estimate, that does not guarantee identification, but a lack of sufficient moments does preclude it.

Neely (2005b) identifies the cross-effects of intervention with the level and volatility of exchange rates using the likely timing of intervention, macroeconomic announcements as instruments, and the nonlinear structure of the U.S. intervention reaction function. Proper identification of the effects of intervention indicates that it is moderately effective in changing the levels of exchange rates but has no significant effect on volatility. The paper also illustrates that such inference depends on seemingly innocuous identification assumptions.

ASSUMPTIONS BEHIND EVENT AND STRUCTURAL STUDIES OF INTERVENTION

An important goal in studying intervention and exchange rate behavior is to ascertain the effect of intervention on exchange rates. An event study, by definition, looks at the behavior of an asset price (e.g., exchange rates) around periods of intervention. This does not necessarily mean, however, that intervention causes the exchange rate behavior. To determine the effect of intervention on exchange rates, one must consider how all the variables that influence exchange rates and intervention interact.

A System of Exchange Rates and Intervention

Consider a simple but general case (equation (8)) in which exchange rate returns and intervention potentially depend on one lag of returns and intervention and the exogenous variables x_{1t} and x_{2t} :

$$(8) \quad \begin{bmatrix} 1 & -\beta \\ -\delta & 1 \end{bmatrix} \begin{bmatrix} \Delta S_t \\ I_t \end{bmatrix} = \begin{bmatrix} a_{11} & a_{12} \\ a_{21} & a_{22} \end{bmatrix} \begin{bmatrix} \Delta S_{t-1} \\ I_{t-1} \end{bmatrix} + \begin{bmatrix} b_{11} & b_{12} \\ b_{21} & b_{22} \end{bmatrix} \begin{bmatrix} x_{1t} \\ x_{2t} \end{bmatrix} + \begin{bmatrix} c_r \\ c_I \end{bmatrix} + \begin{bmatrix} u_{rt} \\ u_{It} \end{bmatrix},$$

where $E[uu'] = \Omega$.¹³ In this system, β governs the contemporaneous reaction of exchange rate

¹³ This equation ignores the nonlinear nature of intervention. This feature of the data will be discussed later.

returns to intervention and δ governs the reaction of intervention to exchange rate returns.

The simultaneous determination of exchange rate returns and intervention will generate the most immediate problem—acknowledged by most researchers—in inferring the effects of intervention on exchange rates: A simple regression of exchange rate returns on contemporaneous intervention will produce inconsistent estimates of β because intervention will be correlated with the estimated error:

[T]he issue of endogeneity arises in our study (and every intervention study) since the central bank usually takes its cue to intervene on the basis of observed exchange rate movements. (Fatun and Hutchison, 2003b, p. 392)

The most common method to deal with this simultaneity is to use an instrumental variables procedure. To estimate β consistently, one would find an instrument for intervention—a variable correlated with intervention but uncorrelated with the shock to exchange rates—and perform a two-stage least squares or a similar instrumental variables procedure.

A slightly subtler problem is that equation (8) contains more structural parameters— β , δ , a_{11} , a_{12} , a_{21} , a_{22} , b_{11} , b_{12} , b_{21} , b_{22} , c_r , c_I , Ω_{11} , Ω_{12} , Ω_{22} , (15)—than moments estimable from the reduced form (13):

$$(9) \quad \begin{bmatrix} \Delta S_t \\ I_t \end{bmatrix} = \frac{1}{1 - \beta\delta} \begin{bmatrix} \begin{bmatrix} a_{11} + a_{21}\beta & a_{12} + a_{22}\beta \\ \delta a_{11} + a_{21} & a_{12}\delta + a_{22} \end{bmatrix} \begin{bmatrix} \Delta S_{t-1} \\ I_{t-1} \end{bmatrix} \\ + \begin{bmatrix} b_{11} + b_{21}\beta & b_{12} + b_{22}\beta \\ \delta b_{11} + b_{21} & b_{12}\delta + b_{22} \end{bmatrix} \begin{bmatrix} x_{1t} \\ x_{2t} \end{bmatrix} \\ + \begin{bmatrix} c_r + \beta c_I \\ \delta c_r + c_I \end{bmatrix} + \begin{bmatrix} u_{rt} + \beta u_{It} \\ \delta u_{rt} + u_{It} \end{bmatrix} \end{bmatrix}.^{14}$$

The fact that there are more parameters in the structural system than moments estimable from the reduced form means that some of the structural parameters are not *identified*.

Simultaneity can contribute to a failure of

identification. For example, if β were equal to zero—intervention does not cause returns—then all the structural parameters of the returns equation would be identified, but one would still not be able to uniquely identify δ and the structural shocks' covariance (Ω_{12}). Simultaneity requires specific estimation methods to obtain consistent estimates of parameters; identification means that some estimation methods can consistently estimate the parameters.

Structural Breaks as a Source of Identification

The usual way to achieve identification is to restrict the structural parameters in a way that allows one to uniquely solve for those parameters from the estimable moments. For example, one might assume that certain regressors don't appear in some equations in a system—which restricts their structural coefficient to be zero—or that certain endogenous variables do not affect each other contemporaneously.

Identification can also be achieved in less traditional ways, however. Changes in the structure of the economy can also offer sources of information that help to identify structural parameters.¹⁵ For example, Kearns and Rigobon (2005) take advantage of the fact that both the Japanese and Australian authorities changed their intervention procedures to make intervention larger but less frequent. In other words, thresholds for intervention increased.

Can Instability Be Exploited To Achieve Identification?

The use of structural breaks to identify structural parameters is potentially dangerous, however, because such exercises might be subject to the Lucas critique.¹⁶ Lucas (1976) argued that evaluating alternative policies using reduced-

¹⁴ If intervention is modeled as a limited dependent variable that takes non-zero values only when its determinants exceed some threshold, the reduced form actually depends on whether intervention takes the value zero or not. The reduced form in (9) is true for non-zero values of intervention.

¹⁵ Other sorts of variation in the data can also be used to identify models. Rigobon and Sack (2003), for example, take advantage of heteroskedasticity in stock market returns to measure the reaction of monetary policy to the stock market.

¹⁶ Noting that the Kearns and Rigobon (2005) study is potentially subject to the Lucas critique is not a particularly damning criticism of their work. Even studies, such as that of Kearns and Rigobon (2005), that pay careful attention to identification must make simplifying assumptions about the structure of the economy. Such assumptions are almost always subject to some criticism.

form econometric models would often produce misleading results because such policies would produce different expectations and different behavior. That is, reduced-form models are not stable when the rules of the economy change.

In the present context, central bank intervention functions are notoriously unstable over time, meaning that the structural parameters of an econometric model—e.g., (8)—might not be stable when the economic environment changes. Estimation of an intervention model will provide results that are specific to the size of the market and intervention and the nature of the reaction function, including the purpose of intervention. Intuitively, the signaling channel depends on intervention signaling future monetary policy or coordinating expectations. If intervention is instead conducted randomly, then it will contain no information and will not influence exchange rates.

Although Kearns and Rigobon (2005) exploit the instability of Japanese and Australian reaction functions to identify their model, instability is an unacknowledged problem for many studies of central bank intervention. If the rules for how intervention is conducted change—as they frequently do—the structural and reduced-form parameters will generally change too.

Event Studies vs. the Structural System

An event study is essentially a single-equation model that looks at the contemporaneous interaction of intervention and exchange rates. One can use single-equation methods to examine the effects of intervention on exchange rates, but this doesn't rescue the econometrician from making assumptions about the structure of the economy—though it often hides those assumptions. For example, suppose that one investigated the effect of intervention on exchange rate returns by estimating the following single-equation regression by ordinary least squares (OLS):

$$(1) \quad \Delta S_t = c_r + \beta I_t + Ax_{1t} + e_t,$$

where ΔS_t , x_{1t} , and I_t are defined as before. When does an event study correctly estimate the structural impact of intervention on returns?

Daily Event Studies and Simultaneity

The first problem to note is that OLS estimates of β would suffer from simultaneous equations bias, unless exchange rate returns did not affect intervention contemporaneously ($\delta = 0$) and the structural errors were uncorrelated. Such assumptions might be tenable in the very-high-frequency (intraday) event studies of Fischer and Zurlinden (1999), Beattie and Fillion (1999), Payne and Vitale (2003), Pasquariello (2002), and Dominguez (2003a,b). But they are certainly not tenable with the daily data needed to determine longer-term responses.

To correct for simultaneous equations bias, some researchers would use an instrumental-variables procedure, such as two-stage least squares (TSLS), to estimate (1). But this would require instruments that are reliably correlated with I_t but not with ΔS_t . Such instruments are difficult to find because foreign exchange intervention policy is determined by factors that could well affect ΔS_t . And using such instruments to estimate the effect of I_t on ΔS_t implicitly constitute identification restrictions because one must exclude the instruments from the structural form of the ΔS_t equation. If the instrument could not be excluded from the ΔS_t equation, then the estimated TSLS coefficient would be an inconsistent estimate of β . Unfortunately, the identifying restrictions used in single-equation models are very rarely explicitly thought out or discussed, leaving it to the reader to determine what they are and whether they are appropriate.

Finding good instruments is important. The literature on instrumental variables has shown that weak instruments—those not strongly predictive of the regressor—will provide very poor estimates of the coefficients. To summarize the long literature on choosing instrument sets, one would like a parsimonious instrument set that strongly predicts the regressor. For good distributional results, Stock, Wright, and Yogo (2002) provide a function that specifies desired F-statistics as a function of the number of instruments. For one instrument, they recommend an F-statistic of 10.

Some researchers have tried to avoid the simultaneity bias by using the lagged value of intervention as the regressor (Huang and Neun, 2004). This practice will not provide the right answer, however. In the simplest case, with no simultaneity and no other regressors, it would have to be the case that

$$(10) \quad E(r_t I_t) = E(r_t I_{t-1})$$

for lagged intervention to provide the correct coefficient for the contemporaneous effect. If one assumes a simple linear model for intervention ($I_t = \rho I_{t-1} + \varepsilon_t$), then (10) can hold only if intervention is a martingale ($\rho = 1$), but this conflicts with the mild positive autocorrelation in intervention.¹⁷ Further, if intervention were an integrated process ($\rho = 1$), then a regression of exchange rate returns ($I(0)$) on intervention ($I(1)$) would be inappropriate because the residuals could not be stationary. Using lagged intervention as the regressor can only properly estimate the response to past intervention, not contemporaneous intervention. It does not resolve simultaneity.

It is worth noting that event studies that eschew regression analysis do not avoid the simultaneity problem, as acknowledged by Fatum and Hutchison (2003b). Whether one actually estimates a regression or uses a nonparametric technique such as the matched sample test or Humpage's discrete distribution methods, if intervention and exchange rates influence each other within the day, then one cannot estimate the impact of intervention on exchange rates (β) consistently, unless one uses appropriate assumptions and estimation methods.

To see such bias, consider a system in which intervention and exchange rates are determined simultaneously and the errors are jointly normal—for tractability. For simplicity, assume that the intervening authority leans against the wind but responds to nothing else:

$$(11) \quad \begin{bmatrix} 1 & -\beta \\ -\delta & 1 \end{bmatrix} \begin{bmatrix} \Delta S_t \\ I_t \end{bmatrix} = \begin{bmatrix} u_{rt} \\ u_{it} \end{bmatrix}.$$

¹⁷ A martingale process is one whose conditional expectation at $t+1$ is the value of the variable at t : $E_t(Z(t+1)) = Z(t)$.

The reduced form for this relation will be

$$(12) \quad \begin{bmatrix} \Delta S_t \\ I_t \end{bmatrix} = \frac{1}{1-\beta\delta} \begin{bmatrix} u_{rt} + \beta u_{it} \\ \delta u_{rt} + u_{it} \end{bmatrix}.$$

And, under the null that intervention has no effect on exchange rates ($\beta = 0$) and that the structural shocks are uncorrelated ($\sigma_{rI} = 0$), $(\Delta S_t, I_t)$ are jointly normal with correlation

$$\rho = \frac{\delta\sigma_r^2}{\sqrt{\sigma_r^2(\delta^2\sigma_r^2 + \sigma_I^2)}} = \frac{\delta\sigma_r}{\sqrt{(\delta^2\sigma_r^2 + \sigma_I^2)}}$$

and

$$\begin{bmatrix} \Delta S_t \\ I_t \end{bmatrix} \sim N \left(\begin{bmatrix} 0 \\ 0 \end{bmatrix}, \begin{bmatrix} \sigma_r^2 & \delta\sigma_r^2 \\ \delta\sigma_r^2 & \delta^2\sigma_r^2 + \sigma_I^2 \end{bmatrix} \right).$$

The conditional expectation of $\Delta S_t | I_t$ is a well-known property of the bivariate normal distribution:

$$(13) \quad E[\Delta S_t | I_t] = \rho \frac{\sigma_r}{\sigma_I} I_t.$$

Humpage's procedure measures the probability of dollar depreciation, conditional on dollar sales (i.e., $P(\Delta S_t < 0 | I_t < 0)$). If δ is less than zero, as is likely if authorities lean against the wind, then the correlation between intervention and exchange rate returns will be negative ($\rho < 0$). This means that the conditional expectation of the exchange rate return will be positive when the authorities sell dollars ($E_{t-1}(\Delta S_t | I_t < 0) > 0$), although intervention has no effect on exchange rates. Because the conditional expectation of ΔS_t is positive and the normal distribution is symmetric, the probability of observing a dollar depreciation ($\Delta S_t < 0$) when the authorities sell dollars is less than 50 percent ($P(\Delta S_t < 0 | I_t < 0) < 0.5$, despite the fact that intervention has no effect on exchange rates in this model ($\beta = 0$). An econometrician estimating this probability will find that intervention has fewer successes than one would expect under independence between exchange rates and intervention. This occurs because the intervention reaction function depends on exchange rate changes—the authorities lean against the wind. This example illustrates that simultaneity will bias the estimates of the effect of intervention in

all event studies, whether they are explicitly regressions or not.¹⁸

Intraday Studies and Simultaneity

An advantage of intraday studies is that one can avoid simultaneity under two assumptions: (i) the timing of intervention is measured precisely enough and (ii) the decision interval of the monetary authority is less than the data frequency used. In other words, if one uses 5-minute data, the monetary authority takes at least 5 minutes to react to market developments and intervene. Under these assumptions, there is no contemporaneous impact of exchange rates on intervention and no simultaneity—changes after the intervention are the result of the intervention and not vice versa. This advantage comes at a price, however. If intervention timing is not correctly known, then the effect of intervention will not be estimated correctly. For example, if one assumes that intervention happens before it actually does, then the effect of intervention will appear to be the conditions that prompt intervention. If intervention is thought to occur later than it actually does, then the study will estimate the lagged effect of intervention, which will probably be smaller than the immediate effect. Fischer (2005) implicitly criticizes the reliance on Reuters' reports used by Dominguez (2003a,b) by showing that such reports were fairly inaccurate for Swiss intervention, whose exact times are known.

Although intraday studies of intervention have been tremendously valuable in understanding the immediate impact of intervention for these data, several potential problems remain, aside from timing issues. First, the paucity of periods/countries for which exact intervention timing is publicly available—only Switzerland over one nine-year period—means that any conclusions from these studies cannot be cross-checked in other samples. Inference could be dangerously fragile. Second, the very short-run effect of intervention might be dominated by transitory effects such as portfolio rebalancing. One cannot rule out

¹⁸ One could argue, of course, that the presence of leaning-against-the-wind intervention biases tests of the effect of intervention toward finding a perverse effect or no effect and that such tests are therefore conservative. But the important point is that such tests are unreliable for finding the correct answer.

the idea that intervention has its full effects over days or weeks. About 40 percent of central bankers surveyed by Neely (2000) believed that intervention takes at least a few days to have its full effect.¹⁹ Therefore, intraday event studies do not answer the question: What is the dynamic response of the exchange rate to intervention? The next section expands on what is required to correctly answer this question.

Dynamic Impacts

A correctly estimated regression coefficient describes the static impact of one variable on another. But one would prefer to estimate the dynamic impact on exchange rates of a shock to intervention. That is, a shock to the intervention process will impact exchange rates, which in turn might affect future exchange rates and intervention. In a VAR, the moving average representation summarizes the dynamic impacts of shocks on the variables in the system.

When does an event study estimate the dynamic impact correctly? Correctly estimating the dynamic impact of a shock to intervention on exchange rates requires even more stringent assumptions than correctly estimating the static impact (β). All the equations for the endogenous variables in the system (at least exchange rates and intervention) must be correctly estimated, which means identifying all the structural parameters and constructing the dynamic impact of a shock to intervention. The nonlinearity of intervention complicates such an exercise, however.

A friction model can characterize intervention's reaction to explanatory variables such as contemporaneous and past returns and volatility (Rosett, 1959). Such a model permits the dependent variable—intervention—to be insensitive to the independent variables over a range of values.²⁰

¹⁹ The conclusion that intervention takes hours or days to achieve its full effect contrasts with the finding in the announcement literature that markets fully adjust to announcements within minutes. The secrecy with which intervention is conducted, however, might delay the adjustment.

²⁰ Rosett (1959) describes the friction model as an extension of the Tobit model (Tobin, 1958). Maddala (1986) provides a very readable introduction to limited dependent-variable models, such as the friction and Tobit models. Almekinders and Eijffinger (1996) used a friction model to study central bank reaction functions.

This is appropriate for a variable like intervention that takes the value zero for a large proportion of observations. The following is a friction model:

$$(14) \quad \begin{aligned} I_t &= \delta \Delta S_t + A_2 X_{2t} + c_I^- + u_{It} & \text{if } I_t < 0 \\ I_t &= 0 & \text{if } I_t = 0 \\ I_t &= \delta \Delta S_t + A_2 X_{2t} - c_I^+ + u_{It} & \text{if } I_t > 0, \end{aligned}$$

where x_{2t} is a vector of all structural explanatory variables and lags of endogenous variables excluding the constant.

Note that in a friction model the value of the intercept term depends on the sign of intervention. This complicates estimation of the intervention equation and the construction of dynamic impulse responses. The structural model when intervention is positive is as follows:

$$(15) \quad \begin{bmatrix} 1 & -\beta \\ -\delta & 1 \end{bmatrix} \begin{bmatrix} \Delta S_t \\ I_t \end{bmatrix} = \begin{bmatrix} A_1 x_{1t} \\ A_2 x_{2t} \end{bmatrix} + \begin{bmatrix} c_r \\ -c_I^+ \end{bmatrix} + \begin{bmatrix} u_{rt} \\ u_{It} \end{bmatrix}.$$

And the model when intervention is negative is the same, except that $-c_I^+$ is replaced by c_I^- .

The reduced form when intervention is positive is as follows:

$$(16) \quad \begin{bmatrix} \Delta S_t \\ I_t \end{bmatrix} = \frac{1}{1 - \beta\delta} \begin{bmatrix} A_1 x_{1t} + \beta A_2 x_{2t} \\ \delta A_1 x_{1t} + A_2 x_{2t} \\ c_r - \beta c_I^+ \\ \delta c_r - c_I^+ \end{bmatrix} + \begin{bmatrix} u_{rt} + \beta u_{It} \\ \delta u_{rt} + u_{It}^+ \end{bmatrix}.$$

When intervention equals zero, however, the first structural and reduced-form equations coincide as follows:

$$(17) \quad \Delta S_t = A_1 x_{1t} + c_r + u_{rt}.$$

And the reduced form for I_t implies the following:

$$(18) \quad \begin{aligned} -\delta A_1 x_{1t} - A_2 x_{2t} - \delta c_r - c_I^- &< \delta u_{rt} - u_{It} \\ < -\delta A_1 x_{1t} - A_2 x_{2t} - \delta c_r + c_I^+ \end{aligned}$$

Because the reduced form for the exchange rate depends on the value of intervention, the nonlinearity can aid in identification of the structural parameters. Neely (2005b) develops an argument of Sickles and Schmidt (1978) to show that the parameters of the structural exchange rate equation are identifiable without instruments or restricting the structural covariance matrix.

CONCLUSIONS

This paper selectively reviews and analyzes the recent literature on intervention to suggest areas for further progress. The examination was spurred by two recent trends that have contributed to the study of central bank intervention: (i) the use of high-frequency data and (ii) the event-study methodology. The event-study technique has been motivated by the argument that it is better suited to study the sporadic, clustered intervention process. And high-frequency data seem to mitigate the simultaneity bias plaguing daily studies.

In the context of a selective review of the literature on the effects of interventions, this paper has argued that even nonparametric event studies are still subject to all the econometric problems that beset more conventional econometric procedures. An examination of simultaneity in a nonparametric event study illustrated this point. Event studies will correctly infer the structural effects of intervention only under fairly strong conditions. Recognition of the assumptions explicit in and analysis of the limitations of the procedures are not criticisms of intervention studies. Rather, explicit identification of drawbacks enables researchers to assess results more realistically and improve their procedures.

With respect to structural studies, this paper shows that the effects of intervention in Kim's (2003) rich macroeconomic model are not identified and cautions that the innovative work of Kearns and Rigobon (2005) is potentially subject to the Lucas critique.

Finally, the paper also argues that the nonlinearity of intervention—which has largely been ignored in the literature on the effects of intervention—could be helpful in identifying the effects of intervention and overcoming simultaneity.

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APPENDIX

Table A1

| Paper | Authority(s) | Period | FX rates | Frequency |
|--|--|-------------------------------------|--|--|
| Aguilar, Javiera and Nydalh, Stefan. "Central Bank Intervention and Exchange Rates: The Case of Sweden." <i>Journal of International Financial Markets, Institutions and Money</i> , September-December 2000, 10(3-4), pp. 303-22 | Sweden | 1993-96 | SEK/USD SEK/DEM | Daily |
| Beattie, Neil and Fillion, Jean-François. "An Intraday Analysis of the Effectiveness of Foreign Exchange Intervention." Working Paper 99-4, Bank of Canada, 1999 | Canada | April 12, 1995– January 30, 1998 | CAN/USD | 10-minute |
| Beine, Michel and Laurent, Sebastien. "Central Bank Interventions and Jumps in Double Long Memory Models of Daily Exchange Rates." <i>Journal of Empirical Finance</i> , 2003, 10(5), pp. 641-60 | Germany Japan United States | 1980-96 | USD/DEM USD/YEN USD/FRF USD/GBP | Daily |
| Beine, Michel; Bos, Charles S. and Laurent, Sebastien. "The Impact of Central Bank FX Interventions on Currency Components." Unpublished manuscript, March 2005 | ECB Germany Japan United States | 1989-2003 | EUR/USD JPY/USD GBP/USD | Hourly returns aggregated to daily returns |
| Beine, Michel; Laurent, Sebastien and Palm, Franz C. "Central Bank Forex Interventions Assessed Using Realized Moments." Unpublished manuscript, January 2005 | ECB Germany Japan United States | 1989-2001 | DEM/USD JPY/USD | Hourly returns aggregated to daily returns |
| Brandner, Peter; Grech, Harald and Stix, Helmut. "The Effectiveness of Central Bank Intervention in the EMS: The Post 1993 Experience." Working Paper 55, Oesterreichische Nationalbank, December 2001 | Belgium Denmark Spain France Ireland Portugal | August 1993– April 1998 | BEF/DEM DKK/DEM ESP/DEM FRF/DEM IEP/DEM PTE/DEM | Daily |

| Type of study | Excerpt from abstract or conclusion |
|-------------------|--|
| Time-series event | Some support is found for the idea that interventions affect the exchange-rate level during certain sub periods but, overall, the results are weak. Furthermore, in line with the findings for other countries, little empirical support is found for the hypothesis that central bank intervention systematically decreases exchange rate volatility. |
| Time-series event | [T]he stabilizing effect of expected intervention came into play as the Canadian dollar approached the upper or lower limits of the band. When the dollar exceeded the band, actual intervention did not have any direct impact because it was expected. Moreover, the results show that discretionary (or unexpected) intervention might have been effective in stabilizing the Canadian dollar, although the impact of an intervention sequence diminished as it increased beyond a few days. |
| Time-series event | Introducing a time-varying jump probability associated to central bank interventions, we find that the central bank interventions, conducted in either a coordinated or unilateral way, induce a jump in the process and tend to increase exchange rate volatility. |
| Time-series event | We identify the currency components of the mean and the volatility processes of exchange rates using the recent Bayesian framework developed by Bos and Shephard (2004). Our results show that in general, the concerted interventions tend to affect the dynamics of both currency components of the exchange rate. In contrast, unilateral interventions are found to primarily affect the currency of the central bank present in the market. Our findings also emphasise some role for interventions conducted by these central banks on other related FOREX markets. |
| Time-series event | The analysis confirms previous empirical findings of an increase of volatility after a co-ordinated CBI. It highlights new findings on the timing and the persistence of co-ordinated interventions on exchange rate volatility, on important volatility spillovers, on the impact on exchange rate covariances and correlations and on skewness coefficients. The empirical findings are partly in line with the predictions of a theoretical model for central bank interventions developed by Vitale (1999). |
| Time-series event | The results from the EGARCH models show that interventions influenced the conditional mean in only one case. Both volatility increasing and decreasing effects are found for the conditional variance. In the MS-ARCH model more effects on the mean are found. If significant, intervention tends to affect the level of the six ERM I exchange rates only in periods of low and medium volatility. For the conditional variance more volatility decreasing than increasing effects are found. Overall, given our approaches (EGARCH and MS-ARCH), the results show that even in the same institutional framework, intervention does not seem to affect the means and variances in a consistent and predictable manner. |

Table A1, cont'd

| Paper | Authority(s) | Period | FX rates | Frequency |
|---|--|--|-------------------------------|---------------------|
| Castrén, Olli. "Do Options Implied Rnd Functions on G3 Currencies Move Around the Times of Interventions on the JPY/USD Exchange Rate?" Working Paper 410, European Central Bank, 2004 | Japan United States | April 2, 1992– December 2003 | JPY/USD USD/EUR JPY/USD | Daily |
| Chaboud, Alain P. and Humpage, Owen F. "An Assessment of the Impact of Japanese Foreign Exchange Intervention: 1991-2004." International Finance Discussion Paper No. 824, Board of Governors of the Federal Reserve System, January 2005 | Japan | 1991-2002 | USD/JPY | Daily |
| Disyatat, Piti and Galati, Gabriele. "The Effectiveness of Foreign Exchange Intervention in Emerging Market Countries: Evidence from the Czech Koruna." Working Paper 172, Bank for International Settlements, March 2005 | Czech Republic | September 2001– October 2002 | Czech Koruna/ EUR | Daily |
| Dominguez, Kathryn. "Foreign Exchange Intervention: Did It Work in the 1990s?" Prepared for "The Dollar" Conference Sponsored by the Institute for International Economics in Washington, DC, September 24, 2002 | ECB Germany Japan United States | 1990-2002 | JPY/USD DEM/USD | Four-hour/ daily |
| Dominguez, Kathryn. "The Market Microstructure of Central Bank Intervention." <i>Journal of International Economics</i> , 2003a, 59(1), pp. 25-45 | United States Germany Japan | 1987-95 | JPY/USD DEM/USD | 5-minute |
| Dominguez, Kathryn. "When Do Central Bank Interventions Influence Intra-Daily and Longer-Term Exchange Rate Movements?" NBER Working Paper 9875, National Bureau of Economic Research, July 2003b | United States Germany Japan | 69 days for USD/DEM and 66 days for JPY/USD over August 1989– August 1995 | USD/DEM JPY/USD | Tick-by-tick |

| Type of study | Excerpt from abstract or conclusion |
|-----------------------------------|--|
| Time-series event | [I]nterventions on the JPY/USD exchange rate coincide with systematic changes in all moments of the estimated risk-neutral density functions (RNDs) on the JPY/USD currency pair, and in several of the moments of the estimated RNDs on the JPY/EUR and USD/EUR currency pairs. In particular, the operations where Japanese yen is sold coincide with a movement in the mean of the RND towards a weaker yen both against the US dollar and the euro, as well as with an increase in implied standard deviations. Prior to the interventions, the RNDs tend to move into opposite directions suggesting, on the average, increasingly unfavourable market conditions and leaning-against-the wind by the Japanese authorities. |
| Other event | The effectiveness of Japanese interventions over the past decade depended in large part on the frequency and size of the transactions. Prior to June 1995, Japanese interventions only had value as a forecast that the previous day's yen appreciation or depreciation would moderate during the current day. After June 1995, Japanese purchases of dollars had value as a forecast that the yen would depreciate. Probit analysis confirms that large, infrequent interventions, which characterized the later period, had a higher likelihood of success than small, frequent interventions. |
| Time-series event | We find that central bank intervention had some (weakly) statistically significant impact on the spot rate and the risk reversal but that this impact was small. We do not find evidence that intervention had an influence on short-term exchange rate volatility. We also find that, in our sample period, Czech authorities appeared to intervene mainly in response to an acceleration of the speed of koruna appreciation. |
| Time-series event/ other event | This study examines the intervention operations of the G3 countries (the United States, Japan and Germany) over the period 1990 through 2002. I analyze the very short-term (four-hour) effects of G3 intervention operations on dollar exchange rates, as well as the longer-term correlations between episodes of intervention and subsequent currency movements. The more recent G3 intervention data suggest that intervention policy is both alive and well—G3 central banks continue to intervene to influence currency values—and these interventions were often successful in influencing short- and longer-term exchange rate movements. |
| Time-series event | [S]ome traders typically know that the Fed is intervening at least one hour prior to the public release of the information in newswire reports. Also, the evidence suggests that the timing of intervention operations matters—interventions that occur during heavy trading volume and that are closely timed to scheduled macro announcements are the most likely to have large effects. Finally, results indicate that interventions that are coordinated with another central bank are more likely to be effective than are unilateral interventions. |
| Time-series event | Using intra-daily and daily exchange rate and intervention data, the paper analyzes the influence of interventions on exchange rate volatility, finding evidence of both within day and daily impact effects, but little evidence that interventions increase longer-term volatility. |

Table A1, cont'd

| Paper | Authority(s) | Period | FX rates | Frequency |
|--|--------------------------|---|-----------------|------------------|
| Edison, Hali J.; Cashin, Paul A. and Liang, Hong. "Foreign Exchange Intervention and the Australian Dollar: Has It Mattered?" Working Paper No. 03/99, International Monetary Fund, May 2003 | Australia | January 1984– December 2001 | AUD/USD | Daily |
| Fatum, Rasmus. "Post-Plaza Intervention in the DEM/USD Exchange Rate." <i>Canadian Journal of Economics</i> , 2002, 35(3), pp. 556-67 | Germany United States | September 1, 1985– December 31, 1995 | DEM/USD | Daily |
| Fatum, Rasmus. "Daily Effects of Foreign Exchange Intervention: Evidence from Official Bank of Canada Data." Working Paper No. 05-12, Santa Cruz Center for International Economics, March 2005 | Canada | January 1995– September 1998 | CAD/USD | Daily |
| Fatum, Rasmus and Hutchison, Michael M. "Effectiveness of Official Daily Foreign Exchange Market Intervention Operations in Japan." NBER Working Paper 9648, National Bureau of Economic Research, 2003a (forthcoming in <i>Journal of International Money and Finance</i>) | Japan | April 1, 1991– December 31, 2000 | USD/JPY | Daily |
| Fatum, Rasmus and Hutchison, Michael M. "Is Sterilised Foreign Exchange Intervention Effective After All? An Event Study Approach." <i>Economic Journal</i> , April 2003b, 113(487), pp. 390-411 | Germany United States | September 1985-95 | DEM/USD | Daily |
| Fatum, Rasmus and King, Michael R. "Rules versus Discretion in Foreign Exchange Intervention: Evidence from Official Bank of Canada High-Frequency Data." Working Paper No. 04-24, Santa Cruz Center for International Economics, May 12, 2005 | Canada | January 1995– September 1998 | CAD/USD | 5-minute |

| Type of study | Excerpt from abstract or conclusion |
|-----------------------------------|---|
| Time-series event/ other event | [O]ver the period 1997-2001, the RBA has had some success in its intervention operations, by moderating the depreciating tendency of the Australian dollar. Second, we investigate the effects of RBA intervention policies on exchange rate <i>volatility</i> over the floating rate period. Our results indicate that intervention operations tend to be associated with an increase in exchange rate volatility, which suggests that official intervention may have added to market uncertainty. Overall, the effects of RBA intervention are quite modest on both the level and the volatility of the Australian dollar exchange rate. |
| Other event | The results suggest that central banks can, in fact, improve the likelihood of success primarily through coordination and that unilateral intervention conducted by the Bundesbank appears to have been destabilizing. Furthermore, it is shown that relatively infrequent intervention has a higher likelihood of success. |
| Other event | Bank of Canada intervention was systematically associated with both a change in the direction and a smoothing of the CAD/USD exchange rate. Bank of Canada intervention did not, however, succeed in reducing the volatility of the CAD/USD exchange rate. Additionally, the paper introduces the issue of currency co-movements to the intervention literature. It is shown that the effects of intervention are weakened when adjusting for general currency co-movements against the USD, suggesting that currency co-movements should be taken into account when addressing the effects of central bank intervention aimed at managing a minor currency vis-à-vis a major currency. |
| Other event | [W]e find strong evidence that sterilized intervention systemically affects the exchange rate in the short-run (less than one month). This result holds even when intervention is not associated with (simultaneous) interest rate changes, whether or not intervention is “secret” (in the sense of no official reports or rumors of intervention reported over the newswires), and against other robustness checks. Large-scale (amounts over \$1 billion) intervention, coordinated with the Bank of Japan and the Federal Reserve working in unison, give the highest success rate. |
| Other event | Focusing on daily Bundesbank and US official intervention operations, we identify separate intervention ‘episodes’ and analyse the subsequent effect on the exchange rate. Using the non-parametric sign test and matched-sample test, we find strong evidence that sterilised intervention systemically affects the exchange rate in the short run. This result is robust to changes in event window definitions over the short run and to controlling for central bank interest rate changes during events. |
| Other event | This paper analyzes official, high-frequency Bank of Canada intervention and exchange rate data (the latter quoted at the end of every 5-minute interval over every 24-hour period) over the January 1995 to September 1998 time-period. The data is of particular interest as it spans over two distinctly different intervention regimes—one characterized by purely rules-based (“mechanistic”) intervention versus one characterized by both rules-based and discretionary intervention. This unique feature of the data allows for both a comparison of the effects of rules-based versus discretionary intervention and a general investigation of intraday effects of intervention. Employing an event-study methodology and three different criteria for success, the study presents strong evidence showing that intervention systematically affects movements in the CAD/USD and in the desired direction along with some evidence that intervention is associated with a reduction of exchange rate volatility. Interestingly, there is no indication that discretionary intervention is more effective than rules-based intervention. |

Table A1, cont'd

| Paper | Authority(s) | Period | FX rates | Frequency |
|---|--|---------------------------------|----------------------|---------------------------|
| Fischer, Andreas M. and Zurlinden, Mathias. "Exchange Rate Effects of Central Bank Interventions: An Analysis of Transaction Prices." <i>Economic Journal</i> , 1999, 109(458), pp. 662-76 | Switzerland | 1986-94 | CHF/USD | Irregular at intervention |
| Fratzscher, Marcel. "On the Long-Term Effectiveness of Exchange Rate Communication and Interventions." Unpublished manuscript, February 2005 | ECB Germany Japan United States | 1990-2003 | EUR/USD JPY/USD | Daily |
| Frenkel, Michael; Pierdzioch, Christian and Stadtmann, Georg. "The Effects of Japanese Foreign Exchange Market Interventions on the Yen/U.S. Dollar Exchange Rate Volatility." Kiel Working Paper No. 1165, 2003 | Bank of Japan | 1993–2000 | USD/JPY | Daily |
| Galati, Gabriele; Melick, William and Micu, Marian. "Foreign Exchange Intervention and Expectations: An Empirical Study of the Dollar/Yen Exchange Rate." Unpublished manuscript, Board of Governors of the Federal Reserve System, International Finance Division, August 2002 | Japan United States | 1993-96, 1997-2000 | USD/JPY | Daily |
| Herrera, Ana Maria and Ozbay, Pinar. "A Dynamic Model of Central Bank Intervention: Evidence from Turkey." Unpublished manuscript, Michigan State University, 2005 | Turkey | November 1993– December 2003 | Turkish lira/ USD | Daily |

| Type of study | Excerpt from abstract or conclusion |
|-------------------|--|
| Other event | The paper extends results from earlier studies by using the actual prices of interventions. Based on the fact that all Swiss National Bank interventions are announced, our test exploits the informational differences between interventions and customer transactions. A key finding is that only initial interventions matter; customer transactions and subsequent interventions have no influence. |
| Time-series event | The paper assesses the strategies and the long-term effectiveness of communication as well as actual interventions. The empirical results for the G3 economies indicate that communication has not only exhibited a significant contemporaneous effect on exchange rates, but also has moved forward exchange rates up to a horizon of six months in the desired direction. Moreover, communication is found to reduce exchange rate volatility and uncertainty whereas actual interventions tend to raise it. Overall this underlines a key difference between these two policy tools and suggests that communication tends to be a fairly effective policy tool over the medium-term. |
| Time-series event | [T]he interventions of the BoJ increased the volatility of the yen/U.S. dollar exchange rate. We find that the interventions of the BoJ, in particular those interventions not reported in the financial press, were positively correlated with exchange rate volatility. |
| Time-series event | We estimate probability density functions (PDFs) from option data to describe market expectations. We find that, between 1993 and 1996, Japanese authorities tended to respond mainly to deviations of the exchange rate from some implicit target levels and to a rise in market uncertainty. Between 1997 and 2000, the Bank of Japan mainly reacted in response to higher uncertainty. On the other hand, the Federal Reserve mainly intervened in cooperation with the Bank of Japan. We find that intervention had no statistically significant systematic effect on the mean of dollar/yen expectations. Consistently, we detect no evidence that intervention systematically altered market participants' bias between a stronger and a weaker dollar with respect to the forward rate. Contrary to most findings of the literature, we failed to find evidence that intervention was associated on average with higher exchange rate variability. Finally, we find that intervention was not followed by an increase in the tails of the distribution of exchange rate expectations. |
| Time-series event | We examine central bank intervention in foreign exchange markets using a dynamic censored regression model. We allow the amount of purchase and sale interventions to depend nonlinearly upon lagged values of intervention and on measures of disorderly foreign exchange markets. Using data for the CBRT, we find persistence in interventions, which suggests the presence of political costs and/or a signal of future monetary policy. We find strong evidence of nonnormality and heteroskedasticity in the Tobit model of the reaction function. Estimation results using Powell's LAD, a robust estimator, reveal the importance of considering these specification issues when modeling central bank intervention. |

Table A1, cont'd

| Paper | Authority(s) | Period | FX rates | Frequency |
|--|--------------------------|---------------------------------------|--------------------|------------------|
| Hillebrand, Eric and Schnabl, Gunther. "The Effects of Japanese Foreign Exchange Intervention. GARCH Estimation and Change Point Detection." Unpublished manuscript, November 2003 | Japan | 1991-2002 | JPY/USD | Daily |
| Huang, Zhaodan and Neun, Stephen. "The Effectiveness of FED Intervention in the USD/GM Foreign Exchange Market." Unpublished manuscript, 2004 | Germany United States | 1978-95 | USD/DEM | Daily |
| Humpage, Owen F. "U.S. Intervention: Assessing the Probability of Success." <i>Journal of Money, Credit, and Banking</i> , 1999, 31(4), pp. 731-47 | United States | February 1987– February 1990 | DEM/USD JPY/USD | Daily |
| Humpage, Owen F. "The United States as an Informed Foreign-Exchange Speculator." <i>Journal of International Financial Markets, Institutions, and Money</i> , 2000, 10(3-4), pp. 287-302 | United States | September 23 and December 31, 1985 | DEM/USD JPY/USD | Daily |
| Ito, Takatoshi. "Is Foreign Exchange Intervention Effective? The Japanese Experience in the 1990's." NBER Working Paper 8914, National Bureau of Economic Research, 2002 | Japan | April 1991– March 2001 | JPY/USD | Daily |

| Type of study | Excerpt from abstract or conclusion |
|-------------------|---|
| Time-series event | Using newly released daily intervention data, we show that the success of interventions varies over time. Measured on the total sample between 1991 and 2002, interventions had the desired effect on the exchange rate at the cost of higher volatility. From 1991 to 1998 interventions were unsuccessful and coincided with increased exchange rate volatility. Since 1999 interventions yield the intended effect while volatility is lower. This provides evidence for successful intervention in Japan's liquidity trap where the distinction between sterilized and unsterilized intervention becomes blurred. |
| Other event | Tests based on the daily intervention data of the FED and Bundesbank show that the FED's interventions indeed systematically change the course of the exchange in the short run, and that the direction of the movement is consistent with the central bank's intention. Further, the paper tests the endogeneity problem and argues that it does not jeopardize the conclusions. These findings are important to understand why central banks continue to use intervention as a policy instrument from time to time. |
| Other event | Results from a logit model suggest that coordinated intervention has a higher probability of success than unilateral intervention. The probability of success also increases with the dollar amount of an intervention. Other conditioning variables are not significant. The paper presents a reaction function, with adjustments for the incidentally truncated nature of intervention data. Predicted values serve as instruments for intervention in the logit models. |
| Other event | US exchange-market interventions have no direct effect on market fundamentals, but they may influence expectations. If intervention has value as a forecast of exchange-rate movements, knowledge that the United States is trading will cause dealers to alter their prior estimates of the distribution of exchange-rate changes. This paper finds that US intervention has had value only as a forecast that recent exchange-rate movements would moderate. Less than half of the interventions, however, seemed successful, and the favorable results were generally confined to two short periods that were characterized by uncertainty about future Federal Reserve policies. |
| Time-series event | The Japanese monetary authorities, by buying the dollar low and selling it high, have produced large profits, in terms of realized capital gains, unrealized capital gains, and carrying (interest rate differential) profits, from interventions during the ten years. Profits amounted to 9 trillion yen (2% of GDP) in ten years. Interventions are found to be effective in the second half of the 1990s, when daily yen/dollar exchange rate changes were regressed on various factors including interventions. The US interventions in the 1990s were always accompanied by the Japanese interventions. The joint interventions were found to be 20-50 times more effective than the Japanese unilateral interventions. Japanese interventions were found to be prompted by rapid changes in the yen/dollar rate and the deviation from the long-run mean (say, 125 yen). The interventions in the second half were less predictable than the first half. |

Table A1, cont'd

| Paper | Authority(s) | Period | FX rates | Frequency |
|--|---------------------|--|-------------------------------|------------------|
| Kearns, Jonathan and Rigobon, Roberto. "Identifying the Efficacy of Central Bank Interventions: The Australian Case." <i>Journal of International Economics</i> , 2005, 66(1), pp. 31-48 | Australia Japan | Australia: July 1986– November 1993; Japan: May 1991– June 2002 | AUD/USD JPY/USD JPY/EUR | Daily |
| Kim, Soyoung. "Monetary Policy, Foreign Exchange Intervention, and the Exchange Rate in a Unifying Framework." <i>Journal of International Economics</i> , 2003, 60(2), pp. 355-86 | United States | 1974–96 | Trade-weighted monthly | Monthly |
| Kim, Suk-Joong; Kortian, Tro and Sheen, Jeffrey. "Central Bank Intervention and Exchange Rate Volatility—Australian Evidence." <i>Journal of International Financial Markets Institutions and Money</i> , 2000, 10(3-4), pp. 381-405 | Australia | 1983-97 | AUD/USD | Daily |
| Morel, Christophe and Teiletche, Jerome. "Do Interventions in Foreign Exchange Markets Modify Investors' Expectations? The Experience of Japan Between 1992 and 2003." Working Paper 2005-04, University of Paris Dauphine, January 2005 | Japan | April 1992– October 2003 | JPY/USD | Daily |
| Neely, Christopher J. "Identifying the Effects of Central Bank Intervention." Working Paper 2005-031A, Federal Reserve Bank of St. Louis, 2005b | United States | 1987-90 | DEM/USD JPY/USD | Twice-daily |

| Type of study | Excerpt from abstract or conclusion |
|--|---|
| Structural | There are three main results. Our point estimates suggest that central bank intervention potentially has an economically and statistically significant contemporaneous effect. For Australia we find a \$US100m purchase of the domestic currency will appreciate the exchange rate by 1.3 to 1.8 per cent. This estimate is similar to that from Dominguez and Frankel (1993c), but larger than previous empirical findings. Our point estimate for Japan is smaller with a \$US100m purchase appreciating the yen by just 0.2 per cent, but interpretation must consider the substantially larger size of interventions conducted by the Bank of Japan. Secondly, the vast majority of the effect of an intervention on the exchange rate is found to occur during the day in which it is conducted, with only a smaller impact on subsequent days. Finally, we confirm that central bank intervention policy can typically be characterized as leaning against the wind. |
| Structural VAR | The structural VAR model is developed to jointly analyze the effects of foreign exchange intervention and (money or interest rate setting) conventional monetary policy on the exchange rate, the two types of policy reactions to the exchange rate, and interactions between the two types of policies. First, many interactions among the two types of policies and the exchange rate are found, which suggests that a joint analysis is important. Second, foreign exchange intervention has substantial effects on the exchange rate, reacts to the exchange rate significantly (to stabilize the exchange rate), and signals future conventional monetary policy stance changes (to back up the intervention). |
| Time-series event | We find contemporaneous positive correlation between the direction of intervention and the conditional mean and variance of the exchange rate returns. We show that sustained and large interventions have a stabilising influence in the foreign exchange market in terms of direction and volatility. Without these interventions, the market would have moved further and exhibited more volatility. |
| Time-series event | The purpose of this paper is to analyze the impact of the Bank of Japan's official interventions on the JPY/USD parity during the period 1992-2003. The novelty of our approach is to combine two recent advances of the empirical literature on foreign exchange interventions: (i) drawing on over-the-counter option prices to characterize more precisely the distribution of market expectations; (ii) redefining interventions in terms of events as they tend to come in clusters. Moreover, in order to deal with the features of the data (small sample size, non-standard distribution), we use bootstrap tests. |
| Structural VAR and structural nonlinear system | Most intervention studies have been silent on the assumed structure of the economic system—implicitly imposing implausible assumptions—despite the fact that inference depends crucially on such issues. This paper proposes to identify the cross-effects of intervention with the level and volatility of exchange rates using the likely timing of intervention, macroeconomic announcements as instruments and the nonlinear structure of the intervention reaction function. Proper identification of the effects of intervention indicates that it is moderately effective in changing the levels of exchange rates but has no significant effect on volatility. The paper also illustrates that such inference depends on paying careful attention to seemingly innocuous identification assumptions. |

Table A1, cont'd

| Paper | Authority(s) | Period | FX rates | Frequency |
|---|---|---------------|-----------------|------------------|
| Pasquariello, Paolo. "Informative Trading or Just Noise? An Analysis of Currency Returns, Market Liquidity, and Transactions Costs in Proximity of Central Bank Interventions." Unpublished manuscript, New York University, November 3, 2002 | Switzerland | 1986-98 | CHF/USD | 5-minute |
| Payne, Richard and Vitale, Paolo. "A Transaction Level Study of the Effects of Central Bank Intervention on Exchange Rates." <i>Journal of International Economics</i> , 2003, 61(2), pp. 331-52 | Switzerland | 1986-95 | CHF/USD | 15-minute |
| Pierdzioch, Christian and Stadtmann, Georg. "The Effectiveness of the Interventions of the Swiss National Bank—An Event-Study Analysis." <i>Swiss Journal of Economics and Statistics</i> , 2004, 140(2), pp. 229-44 | Switzerland | 1986-95 | CHF/USD | Daily |
| Ramaswamy, Ramana and Samiei, Hossein. "The Yen-Dollar Rate: Have Interventions Mattered?" Working Paper No. 00-95, International Monetary Fund, 2000 | Reuters, <i>Financial Times</i> , and <i>Wall Street Journal</i> reports of Bank of Japan and U.S. intervention | 1995-99 | JPY/USD | Daily |
| Reitz, Stefan. "Nonlinear Impact of Central Bank Intervention on Exchange Rates?" Unpublished manuscript, Deutsche Bundesbank, September 2005 | Germany United States | 1980 and 1992 | USD/DEM | Daily |

| Type of study | Excerpt from abstract or conclusion |
|-------------------|--|
| Time-series event | <p>We find that the effectiveness of these trades is crucially related to their perceived information content, rather than to imperfect substitutability or inventory considerations. Indeed, regardless of their size, only SNB interventions (especially when unexpected or inconsistent with market momentum) had significant and persistent effects on daily CHF/USD returns, although they often failed to smooth currency fluctuations. Unsuccessful transactions instead induced the greatest misinformation and heterogeneity of beliefs among market participants and reduced market liquidity. These changes always translated into higher, economically significant transaction costs borne by the population of investors.</p> |
| Other event | <p>Using an event study approach we find that intervention has important short-run effects on exchange rate returns. In particular, among various results, we find that i) intervention has a stronger impact when the SNB moves <i>with-the-market</i> and when its activity is <i>concerted</i> with that of other central banks and ii) exchange rate returns move in the 15 min interval prior to interventions.</p> |
| Other event | <p>We find some evidence that the interventions of the SNB had an impact on exchange rate dynamics. The significance of this effect, however, depends on the direction of intervention. In general, the evidence suggests that the interventions of the SNB to strengthen the Swiss franc were more effective than its interventions to weaken the Swiss franc. We also find that the results of the tests for the effectiveness of the interventions of the SNB depend upon the length of the pre- and post-event window analyzed.</p> |
| Time-series event | <p>Using daily data for 1995-99, this paper estimates a simple forward looking model of the exchange rate to show that foreign exchange interventions have, on the whole, had small but persistent effects on the yen-dollar rate. Contrary to conventional wisdom, sterilized interventions have mattered. Consistent with conventional wisdom, coordinated interventions have a higher probability of success and move the yen-dollar rate by a larger margin than unilateral interventions. A probit model indicates that both an excessive appreciation and depreciation of the yen provoke interventions, and that interventions occur in clusters—if there is one today, there will likely be another tomorrow.</p> |
| Time-series event | <p>In this paper we study a relatively new route of effectiveness of central bank intervention as proposed by Sarno and Taylor (2001). According to their argumentation strong and persistent misalignments of the exchange rate are due to a weakening of stabilizing speculation. Of course, the more the exchange rate deviates from purchasing power parity (ppp) the larger the cumulative losses associated with speculation based on ppp so that stabilizing speculators tend to leave the market. In such circumstances, intervention operations of central banks may encourage their re-entry into the market. Applying daily Federal Reserve intervention data from 1980 to 1992 we find that the dollar/mark exchange rate's reversion to ppp depends nonlinearly on the amount of intervention operations and the degree of misalignment. The empirical results suggest that the FED's interventions have been effective by increasing speculators' confidence in the validity of ppp.</p> |

Table A1, cont'd

| Paper | Authority(s) | Period | FX rates | Frequency |
|--|--------------------------|-------------------------------|--------------------|------------------|
| Rogers, Jeff M. and Siklos, Pierre L. "Foreign Exchange Market Intervention in Two Small Open Economies: The Canadian and Australian Experience." <i>Journal of International Money and Finance</i> , 2003, 22(3), pp. 393-416 | Canada Australia | 1989-98 | AUD/USD CAD/USD | Daily |
| Smith, Michael; McLennan, Michael and Sheen, Jeffrey. "Joint Estimation of an Endogenous Model of Central Bank Intervention and Foreign Exchange Volatility with Application to Australia, 1983 to 2003." Working Paper ECMT2004-3, University of Sydney School of Economics and Political Science, July 2004 | Australia | 1983-2003 | AUD/USD | Daily |
| Taylor, Mark P. "Is Official Exchange Rate Intervention Effective?" <i>Economica</i> , 2004, 71, pp. 1-11 | Germany United States | August 1985– December 1998 | USD/DEM | Monthly |

| Type of study | Excerpt from abstract or conclusion |
|-------------------|---|
| Time-series event | Both central banks intervene in response to excessive exchange rate volatility and uncertainty. Volatility is the implied volatility of foreign currency futures options. Uncertainty is the kurtosis of the implied risk-neutral probability density functions. We also examine the impact of inflation targets. Unlike other studies we also consider commodity futures prices. These turn out to help explain the effectiveness of intervention. Central bank intervention was largely unsuccessful in both countries though volatility and kurtosis were modestly affected. |
| Time-series event | A stochastic volatility model with jumps is employed for the exchange rate, while a threshold model is used for intervention. The jump and latent volatility processes in the stochastic volatility model and latent intervention in the threshold model, are endogenous. To account for this, both models are estimated jointly using Markov chain Monte Carlo (MCMC). The model is applied to the analysis of intervention by the Reserve Bank of Australia (RBA) in the Australian/US dollar exchange rate from 1983 to 2003...The empirical work suggests that RBA intervention is partially precipitated by volatility in the foreign exchange rate. However, RBA intervention appears to have exacerbated contemporaneous volatility between 1983 and 1993, but has since avoided having any effect. Analysis of lagged volatility suggests one reason may be improved targeting of intervention to address contemporaneous volatility, as opposed to volatility occurring on previous trading days. The RBA does not appear to respond to jumps identified in the exchange rate. |
| Time-series event | I examine the effectiveness of exchange rate intervention within the context of a Markov-switching model for the real exchange rate. The probability of switching between stable and unstable regimes depends non-linearly upon the amount of intervention, the degree of misalignment and the duration of the regime. Applying this to dollar-mark data for the period 1985-98, I find that intervention increases the probability of stability when the rate is misaligned, and that its influence grows with the degree of misalignment. Intervention within a small neighbourhood of equilibrium will result in a greater probability of instability. |



Discrete Monetary Policy Changes and Changing Inflation Targets in Estimated Dynamic Stochastic General Equilibrium Models

Anatoliy Belaygorod and Michael J. Dueker

Many estimated macroeconomic models assume interest rate smoothing in the monetary policy equation. In practice, monetary policymakers adjust a target level for the federal funds rate by discrete increments. One often-neglected consequence of using a quarterly average of the daily federal funds rate in empirical work is that any change in the target federal funds rate will affect the quarterly average in the current quarter and the subsequent quarter. Despite this clear source of predictable change in the quarterly average of the federal funds rate, the vast bulk of the literature that estimates policy rules ignores information concerning the timing and magnitude of discrete changes to the target federal funds rate. Consequently, policy equations that include interest rate smoothing inadvertently make the strong and unnecessary assumption that the starting point for interest rate smoothing is last quarter's average level of the federal funds rate. The authors consider, within an estimated general equilibrium model, whether policymakers put weight on the end-of-quarter target level of the federal funds rate when choosing a point at which to smooth the interest rate.

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Macroeconomic models that are linearized reduced forms of dynamic stochastic general equilibrium (DSGE) models with sticky prices are now widely considered to be ready for prime time—in the sense that they can confront the data, yield sensible parameter estimates, and provide useful policy analysis (Christiano, Eichenbaum, and Evans, 2005; Smets and Wouters, 2003 and 2005; McCallum and Nelson, 1999). With specific reference to monetary policy, DSGE models have begun to address two issues: whether policy rules are indeterminate and whether monetary policy rules include interest rate smoothing (Rotemberg and Woodford,

1999; Lubik and Schorfheide, 2004). The promise of estimated DSGE models is that one can take the parameter estimates, plug them into the underlying optimizing model, and perform welfare calculations. In this way, policymakers could get a handle on the welfare implications of key features of alternative monetary policy rules, such as the benefits of interest rate smoothing or the value of avoiding policy indeterminacy.

Prior to attempting such welfare calculations, however, it is worthwhile to refine the estimated monetary policy rule to reduce the scope of the mis-specification. In this article, we highlight ways to sharpen the specification of interest rate smoothing in DSGE models. In particular, we focus on a key issue that affects inferences regarding

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interest rate smoothing: the discreteness of monetary policy changes.

In practice, the Federal Open Market Committee adjusts a target level for the federal funds rate by discrete increments at their regularly scheduled meetings or in conference calls. One often-neglected consequence of using a quarterly average of the daily federal funds rate in empirical work is that any change in the target federal funds rate will affect the quarterly average in two different quarters. For example, if policymakers raise the target by 50 basis points precisely halfway through this quarter, then the current quarter's average will rise by 25 basis points relative to last quarter and next quarter's average will also exceed this quarter's average by 25 basis points, all else equal.

Despite this clear source of predictable change in the quarterly average of the federal funds rate, the vast bulk of the literature that estimates policy rules uses a monthly or quarterly average of the interest rate, yet ignores information concerning the timing and magnitude of discrete changes to the target federal funds rate. As a result, such empirical models end up trying to predict the effect on the quarterly average of known, past policy actions rather than including this piece of data in the forecast information set. Consequently, policy equations that include interest rate smoothing inadvertently make the strong and unnecessary assumption that the starting point for interest rate smoothing is last quarter's average level of the federal funds rate. It seems clear, however, that policymakers would put weight on the end-of-quarter target level of the federal funds when choosing a point at which to smooth the interest rate.

INTEREST RATE SMOOTHING: AN UNSETTLED ISSUE

One cart-versus-horse issue in empirical macroeconomics is whether monetary policymakers adjust the federal funds rate gradually in response to developments in the economy or, alternatively, whether developments in the economy emerge slowly enough to account for the slug-

gish pace of observed changes in the interest rate. Sack (2000) and Clarida, Galí, and Gertler (2000) emphasize interest rate smoothing; Rudebusch (2002) believes that factors omitted from the empirical policy equation account for the apparent sluggishness of interest rate changes; English, Nelson, and Sack (2003) find evidence of both. The question is whether policymakers overtly decide to adjust the federal funds rate gradually. Three reasons have been put forth for rate smoothing and partial adjustment. First, policymakers are uncertain about the true structure of the economy; and this potential source of policy error leads them to act less forcefully than they otherwise would (Sack, 2000). Second, policymakers are similarly hesitant to act on initial data releases that are subject to subsequent revision (Orphanides, 2001).

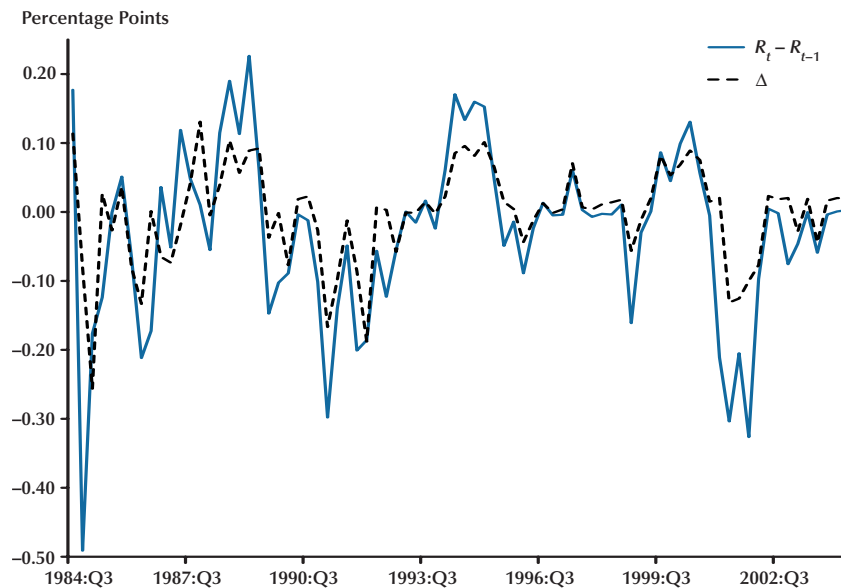
Third, Woodford (2003a,b) suggests that monetary policymakers can influence market expectations if they show a willingness to implement—even through gradual actions—a large interest rate response if it proves necessary. For example, suppose that policymakers indicate that they are willing to raise the federal funds rate by an eventual amount of 120 basis points if a 40-basis-point increase in inflation persists. Policymakers demonstrate this willingness by embarking on a path of raising the interest rate gradually. If the public believes that this gradual path will be implemented for as long as necessary to reduce inflation, market expectations will adjust quickly, with the beneficial effect of reducing inflation without requiring much actual increase in the interest rate. Another way to state the Woodford scenario is to say that interest rate smoothing raises the unconditional variance of the interest rate relative to the variance of inflation, and the latter depends on the expectations of agents. Faced with this policy, the welfare-maximizing response of agents is to minimize the variance of inflation to reduce the realized fluctuations in the nominal and real interest rates.

DISCRETE TARGET CHANGES AND INTEREST RATE SMOOTHING

In the standard setup, interest rate smoothing takes the form

Figure 1

Discreteness Adjustment from Time $t-1$ and Change of Quarterly Average of Federal Funds Rate at Time t



$$(1) \quad R_t = \rho_R R_{t-1} + (1 - \rho_R) \hat{R}_t,$$

where R_t is the quarterly average of the federal funds rate and \hat{R}_t is the desired rate based on current economic conditions, such as the Taylor rule-implied level. As discussed here previously, one shortcoming of equation (1) is that the most recent quarterly average, R_t , is assumed to be the reference point for interest rate smoothing, despite the fact that policymakers are apt to take into consideration the most recent target level of the federal funds rate, denoted R_{t-1}^T . Our empirical specification of an interest rate smoothing policy equation would be

$$(2) \quad R_t = \rho_R [(1 - \delta)R_{t-1} + \delta R_{t-1}^T] + (1 - \rho_R) \hat{R}_t,$$

where δ indicates the weight given to R_{t-1} versus R_{t-1}^T in determining the starting point for interest rate smoothing. An equivalent way to write equation (2) is

$$(3) \quad R_t = \rho_R R_{t-1} + (1 - \rho_R) \hat{R}_t + (\rho_R + D)(R_{t-1}^T - R_{t-1}),$$

where $D = (\delta - 1)\rho_R$. Viewed this way, $R_{t-1}^T - R_{t-1}$ is a discreteness-adjustment term appended to the basic interest rate smoothing equation. Dueker (2002) included such a discreteness-adjustment term in a vector autoregression, and Dueker and Rasche (2004) included it in an estimated Taylor-type policy equation. Note that it is possible to find $D > 0$, in which case $\delta > 1$. The interpretation of this result would be that monetary policymakers do not use either R_{t-1} or R_{t-1}^T as the starting point for interest rate smoothing; instead, they use $R_{t-1}^T + (\delta - 1)(R_{t-1}^T - R_{t-1})$, which implies that they impute some continuation of last period's target change(s) in the same direction into this quarter's baseline rate. That is, past target changes appear to imply some momentum for additional changes in the same direction. We might expect this type of momentum, given the way policymakers make relatively long series of target changes in the same direction. Figure 1 plots the changes in the quarterly average, $R_t - R_{t-1}$, with the discreteness-adjustment term, $R_{t-1}^T - R_{t-1}$, for the federal funds rate. It is clear from the close correspondence that

the discreteness-adjustment term is a predictor of changes in the quarterly average of the federal funds rate, based on target changes that took place in the previous quarter. Consequently, failure to include this term could affect estimated policy rules, especially with regard to interest rate smoothing. We turn next to the issue of policy indeterminacy.

INDETERMINACY IN TAYLOR RULES

A standard Taylor rule (Taylor, 1993) assumes that monetary policy operates through an interest rate rule that responds to expected inflation gaps and output gaps:

$$(4) \quad \hat{R}_t = r^* + \pi_t + (\psi_1 - 1)(\pi_t - \pi^*) + \psi_2 \widehat{GDP}_t + \varepsilon_{R,t},$$

where r^* is the steady-state real rate of interest, π is inflation, π^* is the long-run target rate of inflation, and \widehat{GDP} is the gap between actual output and the level implied by the long-run balanced growth path. The policy rule in a standard DSGE model assumes a constant inflation target and subsumes r^* and $(1 - \psi_1)\pi^*$ together in the constant term:

$$(5) \quad \hat{R}_t = [r^* + (1 - \psi_1)\pi^*] + \psi_1 \pi_t + \psi_2 \widehat{GDP}_t + \varepsilon_{R,t}.$$

In a general equilibrium setting, a determinacy condition for monetary policy essentially states that the coefficient ψ_1 on inflation exceeds 1.0. Equation (5) suggests that indeterminacy results when policymakers are not responsive enough with their interest rate instrument to changes in inflation. Through the lens of equation (4), the determinacy condition requires that monetary policymakers make a positive interest rate response to an increase in the inflation gap. Not all increases in observed inflation would correspond one-to-one with an increase in the inflation gap if the target rate of inflation were not constant. To allow some sluggish interest rate adjustment to be the result of a changing inflation target and not the result of an indeterminate policy—similar to Smets and Wouters (2003), Gavin, Kydland, and Pakko (2005), Gavin,

Keen, and Pakko (2005), and Ireland (2005)—we allow the target rate of inflation, π^T , to vary across time as an autoregressive process with unconditional mean π^* :

$$(6) \quad \begin{aligned} \hat{R}_t &= r^* + \pi_t + (\psi_1 - 1)(\pi_t - \pi_t^T) + \psi_2 \widehat{GDP}_t + \varepsilon_{R,t} \\ \pi_t^T &= \pi^* + \rho_\pi (\pi_{t-1}^T - \pi^*) + \varepsilon_{\pi,t}, \end{aligned}$$

which is equivalent to

$$(7) \quad \hat{R}_t = r^* + \pi_t^T + \psi_1 (\pi_t - \pi_t^T) + \psi_2 \widehat{GDP}_t + \varepsilon_{R,t}.$$

With a stationary autoregressive target rate of inflation, nominal variables have well-defined steady-state levels. Yet, there is an additional reason why the interest rate might be relatively unresponsive in the face of an increase in inflation: The inflation might be due to a temporary but persistent increase in the target level of inflation. With this additional fundamental shock, $\varepsilon_{\pi,t}$, it is possible that the parameter estimates differ enough from those in the restricted model to increase the posterior odds of determinacy. In other words, we might not need policy indeterminacy to help explain the complex interplay between the interest rate and inflation if the target rate of inflation is not assumed to be constant.

THE DSGE MODEL

We log-linearize the New Keynesian monetary DSGE model from Woodford (2003a,b) and express variables as deviations from the steady-state levels:

$$(8) \quad \begin{aligned} \widehat{GDP}_t &= E_t \widehat{GDP}_{t+1} - \tau (\tilde{R}_t - E_t \tilde{\pi}_{t+1}) + g_t \\ \tilde{\pi}_t &= \beta E_t \tilde{\pi}_{t+1} + \kappa (\widehat{GDP}_t - z_t) \\ \tilde{R}_t &= \rho_R \tilde{R}_{t-1} + (\rho_R + D)(R_{t-1}^T - R_{t-1}) \\ &+ (1 - \rho_R) [\tilde{\pi}_t^T + \psi_1 (\tilde{\pi}_t - \tilde{\pi}_t^T) + \psi_2 (\widehat{GDP}_t - z_t)] + \varepsilon_{R,t} \\ \tilde{\pi}_t^T &= \rho_\pi \tilde{\pi}_{t-1}^T + \varepsilon_{\pi,t}, \end{aligned}$$

where z is a technology shock, g is a demand shock, π_t^T is the target rate of inflation, ε_π is the innovation to the target rate of inflation, and ε_R is a monetary policy shock. The formal condition for determinacy in this model is that

$$\psi_1 > 1 - \frac{\beta\psi_2}{\kappa} \left(\frac{1}{\beta} - 1 \right),$$

which essentially means $\psi_1 > 1$, given that β , the time discount factor, is so close to 1 in these models.

In this paper, we estimate a model enriched with discreteness correction, $D \neq -\rho_R$, and a time-varying inflation target, $\varepsilon_\pi \neq 0$, assuming that during the sample period between 1984 and 2004 U.S. monetary policy was determinate—an assumption supported by a number of empirical studies, such as Lubik and Schorfheide (2004).

The discreteness-adjustment term, $R_{t-1}^T - R_{t-1}$, which is the gap between the end-of-quarter target level and the quarterly average of the federal funds rate, is largely a function of the timing of monetary policy meetings within a quarter. Other things equal, the later the meeting at which the target is changed, the larger will be the gap between the quarterly average and the end-of-quarter target. Because the calendar of monetary policy meetings, while important for forecasting the quarterly average of the federal funds rate, is not something we want to determine within the general equilibrium model, we treat the discreteness adjustment as a predetermined variable. In the appendix, we describe how to handle such a predetermined variable in the solution and estimation of the general equilibrium model. Lubik and Schorfheide (2004) derive a condition with which they express forecast errors strictly as a function of structural shocks in a determinate model. We want the predetermined variable, which is dated $t-1$, to affect only *expected values* at time t , not forecast errors, because rational expectations forecast errors are orthogonal to everything in the current information set. The mechanics of imposing this condition are spelled out in the appendix. In fact, these methods for dealing with predetermined variables are used in DSGE models of small open economies (Kollmann, 2001 and 2002), where rest-of-world variables are decomposed into expected and unexpected components. However, the DSGE solution methodology implemented in this literature is based on the older approach attributed to Blanchard and Kahn (1980). In this paper we are using a more recent and superior approach

for solving DSGE models attributed to Sims (2002). It is important to make the following distinction here: While one of the main improvements of Sims (2002) over Blanchard and Kahn (1980) is the handling of the *endogenous* predetermined variables (which are elements of the state vector), our generalization of Sims (2002) comes from adding *exogenous* predetermined regressors. Obviously, by definition of being exogenous, such regressors cannot be elements of the state vector because the dynamics of their evolution are determined exogenously (outside this model's specification).

The quarterly data are gross domestic product (expressed in logs as the Hodrick-Prescott-filtered deviation from trend), inflation (measured as the percentage change in the personal consumption expenditures chain-weighted deflator), the quarterly average of the federal funds rate, and the predetermined discreteness-correction scalar, $\Delta_{t-1} = R_{t-1}^T - R_{t-1}$, where $t = 1984:Q2, \dots, 2004:Q2$. In the appendix, we describe how the DSGE solution procedure of Sims (2002) can be extended to handle predetermined variables, such as the discreteness adjustment, Δ_{t-1} . We chose post-1984 data because we wanted a time period when U.S. monetary policy was unambiguously determinate. Data on a target federal funds rate are available for earlier time periods, although the exact dates and magnitudes of target changes are open to debate.

We use Kalman filter recursions to evaluate the likelihood function of the data. We apply Bayesian and maximum-likelihood estimation to this model. Our objective is not only to find the point estimates of the parameters, but also to plot the entire marginal posterior distribution for parameters of interest. This objective could be accomplished only in a Bayesian framework. Whether the Bayesian Markov chain Monte Carlo (MCMC) posterior densities look like the asymptotic normal distributions implied by maximum-likelihood estimation is an empirical question. Although Bayesian MCMC methods converge faster if supplied with the true maximum-likelihood parameter estimates and a smooth function surface, it is not a matter of necessity for the MCMC methods to work. To the extent that our proposal density is off the mark, our MCMC

Table 1**Parameter Estimates**

| Parameter | Tailored Metropolis-Hastings (M-H) mean | Standard deviation |
|--------------|---|-----------------------|
| τ | 0.31156 | 0.09457 |
| β | 0.98946 | 0.00461 |
| κ | 0.40955 | 0.10878 |
| ψ_1 | 2.22968 | 0.28474 |
| ψ_2 | 0.28733 | 0.09322 |
| ρ_π | 0.77564 | 0.06405 |
| ρ_R | 0.88040 | 0.02368 |
| ρ_g | 0.86345 | 0.02954 |
| ρ_z | 0.71307 | 0.04449 |
| D | 0.19775 | 0.15038 |
| π^* | 2.56057 | 0.30705 |
| r^* | 1.71577 | 0.41087 |
| σ_R | 0.08642 | 0.01096 |
| σ_z | 0.70809 | 0.08030 |
| σ_g | 0.11248 | 0.01739 |
| σ_π | 0.22998 | 0.06885 |
| ρ_{gz} | 0.80050 | 0.06404 |

NOTE: 10,000 M-H iterations including 10 percent burn-in. M-H algorithm elapsed time was 55 seconds.

sampler will be less efficient and will require more draws, but the results will still be valid.

ESTIMATION RESULTS

For this sample period, we restrict the parameter space to the determinacy region. This restriction should not contradict the true distribution of the parameters in the post-1984 sample period. Table 1 presents the Bayesian MCMC parameter estimates for the full model estimated using a tailored Metropolis-Hastings (M-H) algorithm (Chib and Greenberg, 1994).¹ For brevity, we do not report standard errors for the maximum-likelihood estimates because, for all four model specifications reported in Table 2, standard devi-

ations are very close to the corresponding values in Table 1. A key result is that, while both additions to the basic model—the discreteness adjustment and the time-varying inflation target—affect the parameter estimates relative to the basic model, especially in terms of reducing σ_R , the constant inflation target specification essentially does not change any other parameter within the full model. Thus, if one had to choose between dropping either the discreteness adjustment or the time-varying inflation target, the discreteness adjustment would be the one to keep. At the same time, both the discreteness adjustment and the time-varying inflation target yield lower estimates of the interest-sensitivity of output (lower τ) and a steeper Phillips curve (lower κ), relative to the basic model. The discreteness adjustment reduces the estimates of these two parameters below that which the time-varying inflation target would imply alone.

The remainder of our discussion of the parameter estimates focuses on Bayesian MCMC estimates of the full model, for which we report standard deviations in Table 1. We also include plots of the smoothed normalized marginal posteriors (superimposed with the corresponding priors²) for key parameters because such output is much more informative than point estimates and asymptotic standard errors. In addition, when forecasting, or drawing inferences about the latent variables, such as the target rate of inflation, we are not limited to point estimates of the parameters; instead, we can study the entire distribution of parameters and latent variables. We used this approach to estimate the dynamics of the latent inflation expectations and inflation target (see Figure 2).

The posterior means from the MCMC algorithm are close to the maximum-likelihood estimates. The very tight distribution for σ_R shows that the basic model unambiguously forecasts the quarterly average of the federal funds rate worse than the enhanced model does. Thus, it clearly behooves models to take into account the effects of recent target changes when forecasting the

¹ See Chib (1995, 1996, and 2001) for detailed discussion of the M-H theory and implementation.

² We used exactly the same priors as Lubik and Schorfheide (2004) for all common parameters.

Table 2**Maximum Likelihood Parameter Estimates for the Full Model and for Three Reduced Models**

| Parameters | Full model | No discreteness adjustment | Constant inflation target | No discreteness adjustment and constant inflation target |
|--------------|----------------|----------------------------|---------------------------|--|
| τ | 0.25649 | 0.33904 | 0.25155 | 0.47566 |
| β | 0.98987 | 0.99002 | 0.98986 | 0.99000 |
| κ | 0.41184 | 0.57857 | 0.42309 | 0.80306 |
| ψ_1 | 2.23232 | 2.41409 | 2.22892 | 2.37387 |
| ψ_2 | 0.27812 | 0.28805 | 0.25515 | 0.27423 |
| ρ_π | 0.80691 | 0.67133 | 0.00000 | 0.00000 |
| ρ_R | 0.89177 | 0.78397 | 0.90167 | 0.83369 |
| ρ_g | 0.86592 | 0.86562 | 0.86471 | 0.88184 |
| ρ_z | 0.71581 | 0.75238 | 0.71162 | 0.72691 |
| D | 0.21510 | $D = -\rho_R$ | 0.12513 | $D = -\rho_R$ |
| π^* | 2.58790 | 2.42552 | 2.54855 | 2.40189 |
| r^* | 1.68736 | 1.91675 | 1.82423 | 1.93581 |
| σ_R | 0.08070 | 0.08897 | 0.09317 | 0.14872 |
| σ_z | 0.69329 | 0.59847 | 0.69992 | 0.60681 |
| σ_g | 0.10262 | 0.12009 | 0.10212 | 0.11823 |
| σ_π | 0.18525 | 0.35018 | 0.00000 | 0.00000 |
| ρ_{gz} | 0.84009 | 0.89611 | 0.80383 | 0.83375 |

NOTE: Bold highlights the estimates of σ_R across all four models.

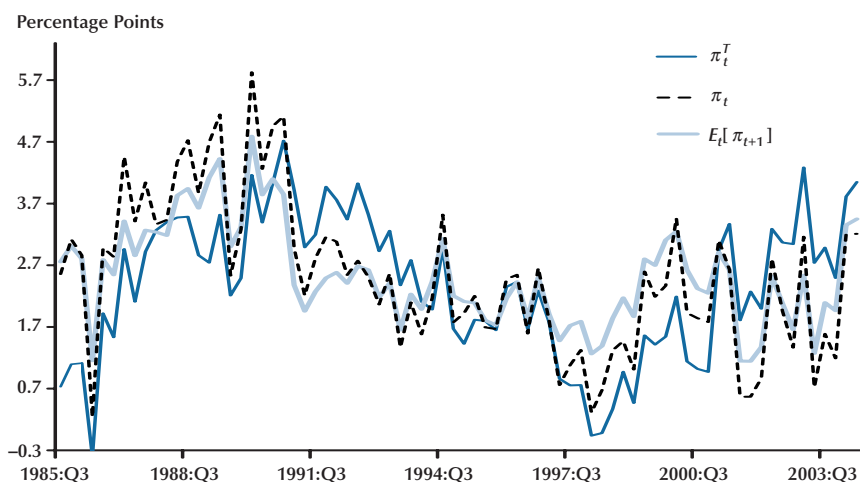
Figure 2**Time-Varying Inflation Target, Expected and Actual Inflation**

Figure 3
Discreteness-Adjustment Coefficient

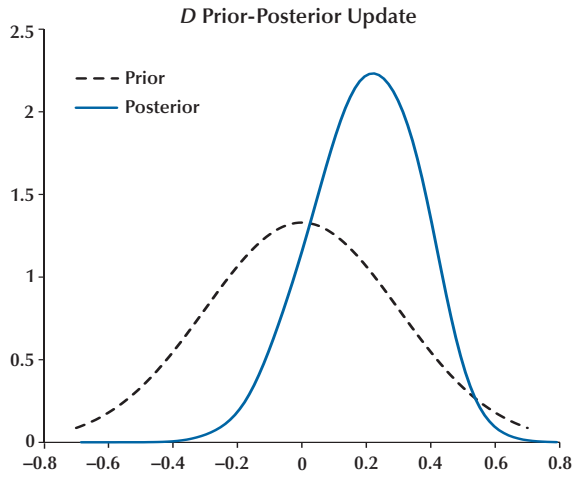


Figure 4
Time-Varying Inflation Target Coefficient

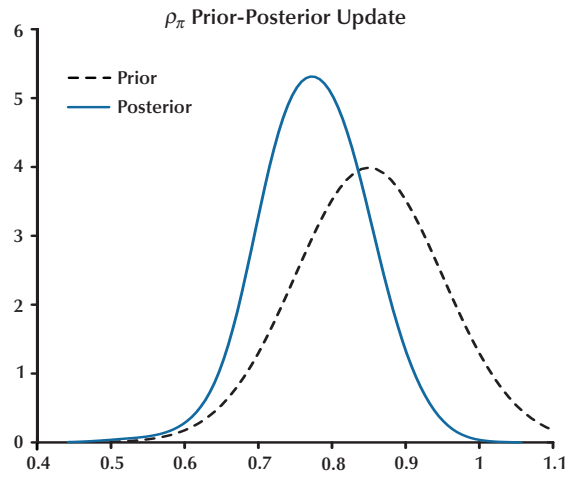


Figure 5
Intertemporal Substitution Elasticity of Consumption

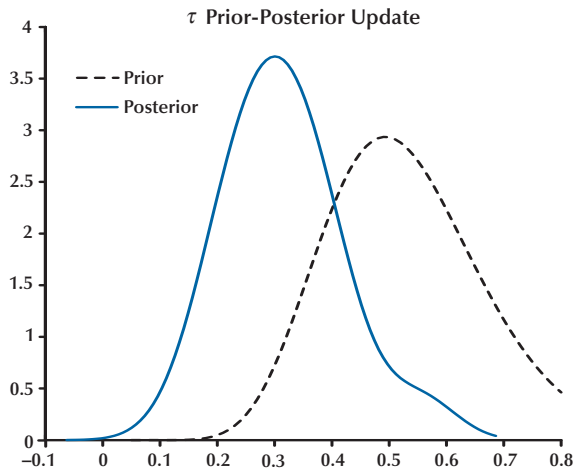


Figure 6
Slope of Phillips Curve

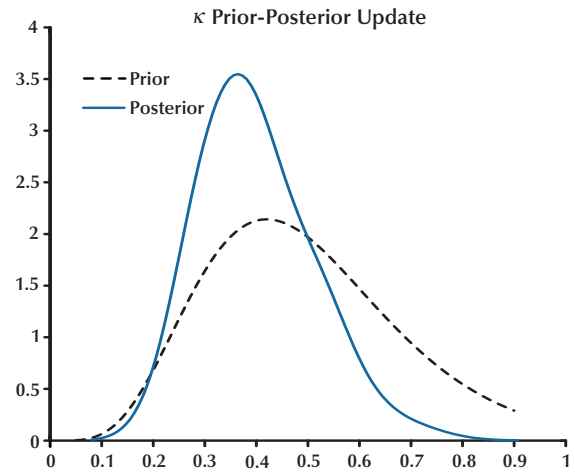
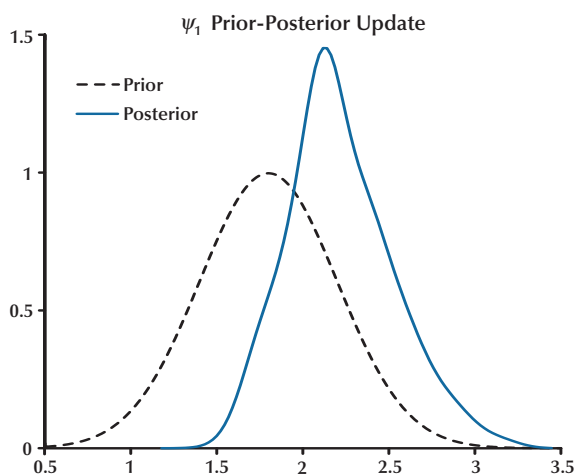
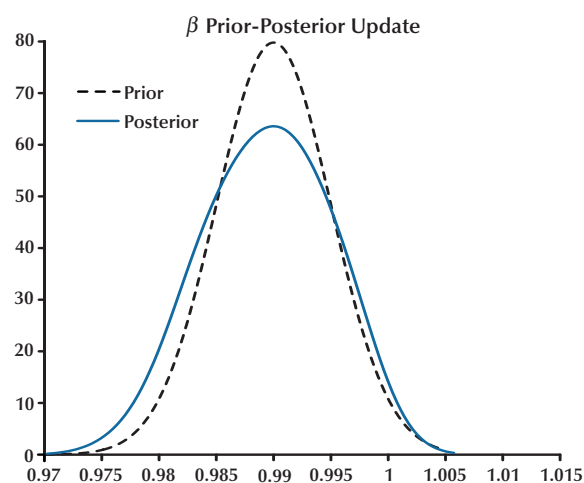


Figure 7**Taylor Rule Coefficient on Inflation Gap**

change in the quarterly average of the federal funds rate. Another key result concerns the estimate of the discreteness-adjustment coefficient, D . The estimate of $D = 0.198$, with a standard deviation of 0.15, matches the estimates of D from a vector autoregression in Dueker (2002) and from a single-equation Taylor rule in Dueker and Rasche (2004). From the posterior histogram in Figure 3 we see that the posterior mass lies above zero and “light years” above $D = -\rho_R$, which is what the standard model without the discreteness adjustment would impose. Figure 3 illustrates that the coefficient on the discreteness adjustment, which has a distribution centered at 0.20, has a posterior distribution that is determined by the data. The prior distribution is centered at zero and is quite diffuse. Nevertheless, the data are strong enough to move the posterior distribution to the right tail of the prior, although not by enough to rule out $D = 0$. Recall that, when $D = 0$, the starting point for interest rate smoothing is the end-of-quarter target level of the federal funds rate. The fact that considerable probability mass lies above zero indicates that one expects some continuation of last period’s target changes.

The posterior plot of the discount factor, β (Figure 8), shows that very little probability mass

Figure 8**Household Discount Factor**

lies above 1.0. Thus, unlike Lubik and Schorfheide (2004) and others, we did not have to tie β , the rate of discounting the time-separable utility, to other steady-state parameters to infer a value below 1.0. The posterior plots for τ and κ , found in Figures 5 and 6, respectively, show that the data shift the posterior to the left of the prior in the presence of the discreteness adjustment. Thus, the discreteness adjustment leads to the conclusion that monetary policy faces a flatter Phillips curve than the basic model would suggest.

From the posterior plot on Figure 7 we see that the feedback parameter from the inflation gap, ψ_1 , shows that the posterior distribution is unambiguously above 1.0. That is, the data strongly support monetary policy determinacy in the post-1984 period.

We turn next to our estimates of the time-varying inflation target. The time-varying inflation target inferred from the data has an unconditional mean of about 2.5 percent, and its deviations are persistent, with an autoregressive coefficient of $\rho_\pi = 0.78$. Figure 2 plots this model-implied inflation target against actual inflation and model-implied inflation expectations. During this sample period, the model-implied inflation target often moved in the same direction as actual inflation.

Target and actual inflation have a positive correlation (0.65), but it is still much lower than the 0.96 correlation between actual and expected inflation.

IMPULSE RESPONSES

Impulse response functions illustrate the different economic implications of the models specified with and without the discreteness adjustment. In order to calculate an impulse response for the model with the discreteness adjustment, it is necessary to make some assumptions concerning the interaction between the target federal funds rate and the effective federal funds rate. The first assumption is that in the simulated quarters the Federal Reserve achieves on average its target for the daily effective rate. The second is that the starting point for the impulse simulation is one where the effective rate equals the target. For the third assumption, we consider two cases: one where the federal funds target is shocked halfway through a quarter (the empirically relevant case) and one where the target is shocked at the very beginning of the quarter. The latter case facilitates a comparison between the coefficients pertaining to models estimated with and without the discreteness adjustment. These timing assumptions pin down the response of the discreteness adjustment, $R^T - R$, to an interest rate shock. With these assumptions, at the time of the interest rate shock, the change in the quarterly average equals $(1 - \lambda)$ times the change in the target, where λ is the portion of the quarter that has elapsed when the target shock occurs. Thus, the change in the discreteness-adjustment term ($R^T - R$) in response to an interest rate shock equals $\lambda/(1 - \lambda)$ times the size of the shock.

When the simulated target change is assumed to take place right at the beginning of the quarter such that $\lambda = 0$, the discreteness-adjustment term does not enter the impulse response and the only difference between the models with and without the discreteness adjustment is that the estimated coefficients differ, depending on whether D is restricted to zero. The impulse responses corresponding to the case where $\lambda = 0$ are shown in the

left-side panel of Figure 6. They show that, even when the simulated target change takes place at the beginning of the quarter, such that no gap is opened between the quarterly average and the target rate, an interest rate shock is estimated to have a larger impact in the model estimated with the discreteness adjustment.

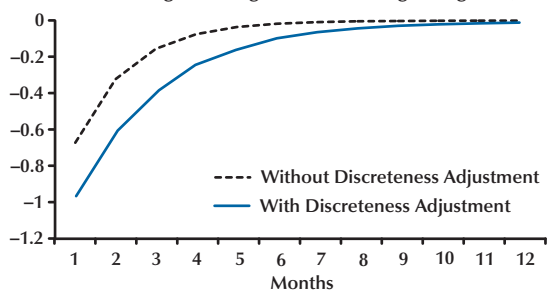
The case where $\lambda = 0.50$ has greater empirical relevance because, from 1984 through 1993, 50.1 percent of the weighted mass of target changes took place in the second half of the quarters, on average, and from 1994 through May 2005 the same measure is 52.2 percent. The impulse responses for these models stem from equation (A3) in the appendix, which shows that when the discreteness adjustment is omitted from the model, then the impulse responses are those of a first-order vector auto regression, VAR(1). The effect of the discreteness adjustment is to change the structure of the model to include a moving average component—a VARMA (1,1). The additional response comes from the discreteness-adjustment term, where the response is proportional to $\lambda/(1 - \lambda)$, as discussed above. Consequently, the impulse responses from the VARMA (1,1) specification will show an extra kink from the moving-average component.

The left panel of Figure 9 shows the discreteness adjustment leads to estimated coefficients that imply stronger impulse responses (in absolute value) to a monetary policy shock, relative to the model without the discreteness adjustment. The right-hand panel of Figure 9 shows that the discreteness adjustment does what it is supposed to do. A 100-basis-point shock to the target federal funds rate eventually has the same effect—whether it is implemented at the beginning or the middle of a quarter—once it is fully reflected in the quarterly average. This equivalence holds because the model with the discreteness adjustment accurately predicts the consequences of a mid-quarter target change for the quarterly average. The model without the discreteness adjustment, in contrast, would treat the change in the quarterly average in the subsequent period as a surprise. In this context, the VAR(1) structure of the DSGE model acts as a limitation because Dueker (2002) showed

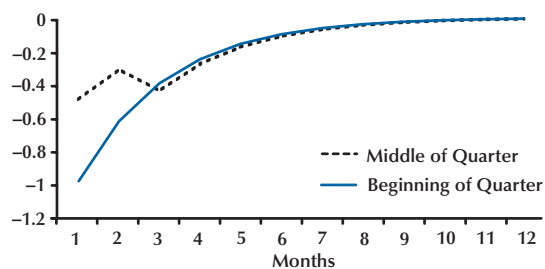
Figure 9

Impulse Response Functions

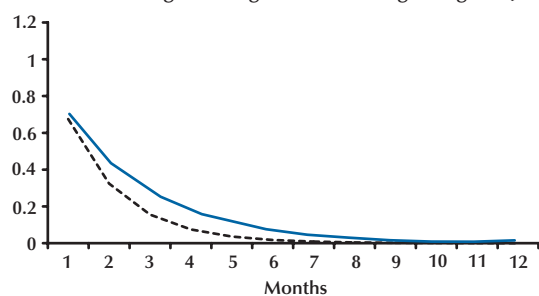
Response of Output to Monetary Policy Shock When 100-Basis-Point Target Change Occurs at Beginning of Quarter



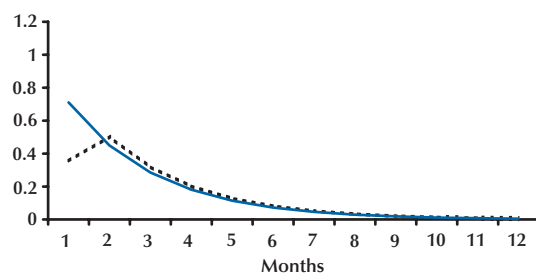
Response of Output to Monetary Policy Shock When 100-Basis-Point Target Change Occurs in Middle of Quarter vs. Beginning of Quarter



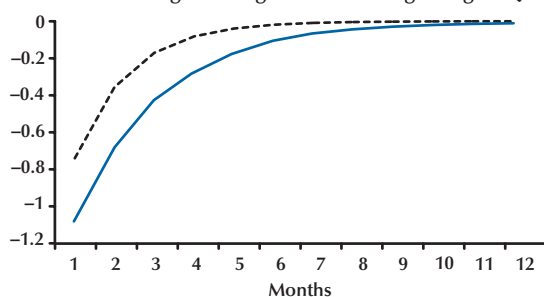
Response of Federal Funds Rate to Own Shock When 100-Basis-Point Target Change Occurs at Beginning of Quarter



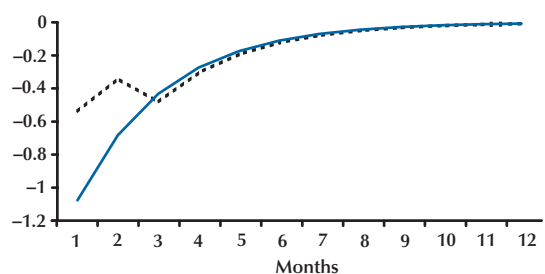
Response of Federal Funds Rate to Own Shock When 100-Basis-Point Target Change Occurs in Middle of Quarter vs. Beginning of Quarter



Response of Inflation to Monetary Policy Shock When 100-Basis-Point Target Change Occurs at Beginning of Quarter



Response of Inflation to Monetary Policy Shock When 100-Basis-Point Target Change Occurs in Middle of Quarter vs. Beginning of Quarter



that the federal funds rate equation in a higher-order VAR can imply the hump-shaped response of the quarterly average to its own shock. The conclusion is that, since many linear DSGE models imply a first-order VAR structure, it is even more important to include the discreteness adjustment than it is in higher-order nonstructural VARs.

CONCLUSIONS

We have made a key enhancement to the Taylor-type monetary policy equation analyzed in estimated DSGE models: We allow the interest rate smoothing to start at a point other than last quarter's average because last quarter's average does not fully reflect the discrete target changes policymakers made in that quarter. Our estimates indicate that the starting point for interest rate smoothing is the end-of-period target federal funds rate plus a small degree of momentum built into the starting point. This enhancement leads to a dramatic fall in the standard error of the interest rate equation on the order of 40 percent. Thus, previous conclusions regarding determinacy and the degree of interest rate smoothing are subject to omitted error bias in the absence of such a discreteness adjustment. We also find that the importance of allowing for a time-varying inflation target is greatly reduced in our post-1984 data set, provided that the discreteness adjustment is included. Without the discreteness adjustment, the time-varying inflation appears to be an indispensable feature of the data.

The discreteness adjustment also leads to lower estimates of the interest sensitivity of output and a flatter estimate of the Phillips curve, relative to the baseline model. On balance, monetary policy would appear to have more influence on the behavior of the real economy when one accounts for the discreteness of monetary policy actions.

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APPENDIX

THE DSGE MODEL SOLUTION WITH PREDETERMINED EXOGENOUS REGRESSORS

Sims (2002) introduces a general method for solving DSGE models. Here we show how to adapt his solution methodology to the same setup enriched by the presence of predetermined regressors. Consider a DSGE model of the following canonical form:

$$(A1) \quad \Gamma_0 s_t = \Gamma_1 s_{t-1} + C + \Psi \varepsilon_t + \Pi \eta_t + D \Delta_{t-1},$$

where, at time t , s_t is the vector of state variables, ε_t is a vector of structural shocks, η_t is a vector of expectational errors, Δ_{t-1} is a vector of predetermined regressors (not present in the model considered by Sims, 2002) and $\Gamma_0, \Gamma_1, C, \Psi, \Pi, D$ are parameter matrices.

Following Sims (2002) we apply a generalized Schur QZ decomposition, $(\Gamma_0, \Gamma_1) = (Q' \Lambda Z', Q' \Omega Z')$; partition the resulting system into non-explosive (denoted by subscript 1) and explosive components (denoted by subscript 2); and use “solution uniqueness” and “stability” conditions worked out in Sims (2002) to write

$$(A2) \quad \begin{pmatrix} I & -\Phi \\ 0 & \Lambda_{22}^{-1} \end{pmatrix} \begin{pmatrix} \Lambda_{11} & \Lambda_{12} \\ 0 & \Lambda_{22} \end{pmatrix} \begin{pmatrix} \omega_1(t) \\ \omega_2(t) \end{pmatrix} = \begin{pmatrix} I & -\Phi \\ 0 & 0 \end{pmatrix} \begin{pmatrix} \Omega_{11} & \Omega_{12} \\ 0 & \Omega_{22} \end{pmatrix} \begin{pmatrix} \omega_{1,t-1} \\ \omega_{2,t-1} \end{pmatrix} + \begin{pmatrix} I & -\Phi \\ 0 & (\Lambda_{22} - \Omega_{22})^{-1} \end{pmatrix} \begin{pmatrix} Q_1 \\ Q_2 \end{pmatrix} C + \begin{pmatrix} I & -\Phi \\ 0 & 0 \end{pmatrix} \begin{pmatrix} Q_1 \\ Q_2 \end{pmatrix} (\Psi \varepsilon_t + D \Delta_{t-1}),$$

where $\omega(t) = Z' s_t$, $\Phi = Q_1 \Pi (Q_2 \Pi)^{-1}$ and $I, 0$ denote identity and zero matrices, respectively, with dimensionality easily deduced from the preceding equation.

Therefore, the solution to the DSGE model in equation (A1) could be written as

$$(A3) \quad s_t = \Theta_1 s_{t-1} + \Theta_c + \Theta_0 \varepsilon_t + D^* \Delta_{t-1},$$

where $\Theta_1, \Theta_c, \Theta_0$ were derived in Sims (2002) and are identical to those variables in our expanded model. The coefficient D^* can be found by focusing on the last term in equation (A2):

$$(A4) \quad \begin{pmatrix} I & -\Phi \\ 0 & \Lambda_{22}^{-1} \end{pmatrix}^{-1} \begin{pmatrix} I & -\Phi \\ 0 & 0 \end{pmatrix} \begin{pmatrix} Q_1 \\ Q_2 \end{pmatrix} (\Psi \varepsilon_t + D \Delta_{t-1}) = \begin{pmatrix} Q_1^{-1} & -\Phi Q_2 \\ 0 & \end{pmatrix} (\Psi \varepsilon_t + D \Delta_{t-1}) = \begin{pmatrix} Q_1 \\ Q_2 \end{pmatrix} (I - \Pi (Q_2 \Pi)^{-1} Q_2) (\Psi \varepsilon_t + D \Delta_{t-1}).$$

If we define $\Pi^* = (\Gamma_0)^{-1} \Pi$, $\Psi^* = (\Gamma_0)^{-1} \Psi$, then we can use equations (A3) and (A4) to find that³

$$(A5) \quad \begin{aligned} \Theta_0 &= \Psi^* - \Pi^* (Q_2 \Pi)^{-1} Q_2 \Psi \\ D^* &= (\Gamma_0)^{-1} D - \Pi^* (Q_2 \Pi)^{-1} Q_2 D. \end{aligned}$$

³ If Γ_0 is not invertible, we can still use its generalized Schur decomposition representation to carry out this analysis using more clumsy notation in terms of partitions of Q, Λ, Z as in Sims (2002).

All this time $\Psi\varepsilon_t$ and $D\Delta_{t-1}$ were treated symmetrically in the derivations just noted, and as a result the final formulas noted previously look the same: The new coefficient equals $(\Gamma_0)^{-1}$ times the old coefficient plus correction for expectational error coming from the “solution existence” condition:

$$(A6) \quad Q_2 D\Delta_{t-1} + Q_2 \Psi\varepsilon_t + Q_2 \Pi\eta_t = 0,$$

which allows us to solve for expectational error as a function of structural shocks, predetermined regressors, and parameters:

$$(A7) \quad \eta_t = -\left((Q_2 \Pi)^{-1} Q_2 D\Delta_{t-1} + (Q_2 \Pi)^{-1} Q_2 \Psi\varepsilon_t \right).$$

However, there is an important distinction between structural shocks and predetermined regressors. We require that expectational errors depend only on the shocks and are independent of predetermined regressors. From the econometric prospective, this requirement amounts to setting the coefficient on Δ in the regression of η on ε and Δ to zero:

$$(A8) \quad (Q_2 \Pi)^{-1} Q_2 D = 0.$$

Then, the equations in (A5) become

$$(A9) \quad \begin{aligned} \Theta_0 &= \Psi^* - \Pi^* (Q_2 \Pi)^{-1} Q_2 \Psi \\ D^* &= (\Gamma_0)^{-1} D. \end{aligned}$$

Revisions to User Costs for the Federal Reserve Bank of St. Louis Monetary Services Indices

Richard G. Anderson and Jason Buol

This analysis discusses recent changes to the user cost figures that are computed as part of the Federal Reserve Bank of St. Louis monetary services indices (MSI). The authors first introduce an alternative splicing procedure, robust to differences in scale between series, for those price subindices which, individually, have a time span shorter than the overall MSI but are spliced to span the entire period. They then correct an error in the calculation of user costs for money market mutual funds that caused these funds' user costs to be based, for a considerable period of time, on the last-reported value for one input data series. Finally, the authors also restore the yield-curve adjustment for composite assets, which they removed from published data during 2004 as they explored the unusual behavior of the user cost data for small-denomination time deposits.

Federal Reserve Bank of St. Louis *Review*, November/December 2005, 87(6), pp. 735-49.

The Federal Reserve Bank of St. Louis has published monetary index numbers (often referred to as Divisia monetary aggregates) since the 1980s. In a set of papers, Anderson, Jones, and Nesmith (1997a,b,c) published a major revision and extension of the Federal Reserve Bank of St. Louis monetary services indices (MSI). A significant feature of that extension was new user costs for the MSI, based on an expanded collection of historical data and updated procedures for building user-cost index numbers.

Here, we discuss two recently implemented revisions to the MSI user costs:

- First, we introduce an alternative index-number splicing procedure. For some monetary assets, data are available to measure user costs only for intervals shorter than the interval of the overall index. In these cases, indices for the individual periods are spliced to create a user-cost measure that spans the quantity data's longer observation interval. In Anderson, Jones, and Nesmith (1997c), a geometric-mean formula (similar to the geometric mean used to create unilateral index numbers) is used to splice these subindices. At that time, the geometric mean formula produced (apparently) acceptable indices. During recent years, however, the scaling (normalization) in that method suggested to some users of the St. Louis MSI that small-denomination time deposits had negative own rates of return. Here, we replace that splicing method with a procedure proposed by Hill and Fox (1997), also based on geometric means. This primarily affects small time deposits.
- Second, we correct a programming error that caused one user cost—for money market

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mutual funds—to be based, for a considerable period of time, on the last-reported value for its own-rate input data series. We also improve, perhaps slightly, the accuracy of the MSI by introducing separate user costs for general-purpose/broker-dealer funds and institutional-type money market mutual funds.

In addition to the above, we also restore the yield curve adjustment to the calculation of user costs for composite assets; see the appendix for details. We removed the yield curve adjustment from the calculation of user costs during 2004 as we explored the causes of unusual behavior in the user costs for small time deposits. At that time, we were concerned that the yield curve adjustment, which assumes a common term premium in yields on Treasury securities and on banks' deposit offering rates, was distorting calculated user costs during periods when spreads between offering rates on short- and long-term deposits were near zero. Further investigation suggested this was not the case.

UNILATERAL INDEX NUMBERS AND SPLICING TIME SERIES

In an ideal world, index numbers would always be built from flawless sets of matching price and quantity data that span the complete desired time interval. In the real world, building index numbers requires methods to handle missing and/or incomplete data; two of the more common techniques used are *unilateral index numbers* and *splicing*.

A *unilateral index number* is an index number constructed from either price or quantity data, but not both—that is, an index number constructed in the absence of one type of data. Because quantity data are more expensive to collect than price data, available price data often are more detailed than corresponding quantity data. In such circumstances, it is desirable to combine the price data into an index that matches, in its level of aggregation, the available quantity data. Such indices are known as unilateral price indices (Diewert, 1995).¹ In empirical studies, unilateral indices

often arise in the case of “low-level” aggregation where the data are repeated observations in a panel-data structure—that is, repeated observations of a single product's price on different dates at, say, a number of retail outlets. Most often, quantity data—such as the quantity sold at each outlet—is not recorded. A common textbook example is the price of toothpaste, which often is collected at a large number of discount and drug stores without corresponding store-by-store sales data.

A distinctly different operation is *splicing* index numbers. Splicing is necessary when no single index number spans, in its date range, the entire desired time interval. (In most cases, length of the desired time interval is a judgment call by the researcher regarding the longest time span for which reasonably consistent indices can be constructed.) For monetary data, this typically happens when one data source or survey ends and a new one begins, perhaps with an overlap of several periods.

For the MSI user costs, Anderson, Jones, and Nesmith (1997c) built unilateral index numbers for a number of assets, including small time deposits, eurodollars, and repurchase agreements. Their discussion did not separate, however, the construction of unilateral price indices when all the components are defined over a common time span from the splicing of shorter, individual price indices. The primary focus of this analysis is to examine circumstance in which this decision matters importantly for interpreting the indices.

Unilateral Index Numbers

As we noted above, a *unilateral index number* is an index constructed from either price or quantity data, but not both. Because these are *economic* index numbers, it is desirable that the index be interpretable within an economic aggregation or demand theory framework. To do so for unilateral price indices, certain assertions must be made regarding the properties of the demand

¹ Readers are cautioned that the term “unilateral” has been used with alternative meanings in other discussions of index numbers. Barnett (2005), for example, uses the term, in a multi-country index number framework, to refer to an approach in which there exists a single representative agent who is indifferent to his country of residence. This is not our context here.

functions for the unobserved quantity data. Unfortunately, because the quantity data are not observed, these assertions are untestable. In applied studies, there are two common alternative assertions: either that the goods have infinite cross-price elasticities (perfect substitutes in demand) or that they have unitary cross-price elasticities (constant expenditure on the goods included in the subaggregate). Anderson, Jones, and Nesmith (1997c) accept an argument advanced by Erwin Diewert (1974) that the latter is more reasonable, albeit less commonly made. An implication of this assumption is that the unilateral price indices should be constructed using a Jevons-style geometric mean method, in which the growth rate of the index equals the growth rate of the ratio of the current period's geometric mean divided by the geometric mean in the previous period.

To be specific, consider a unilateral price index created from own rates of return on two sets of assets. Let $\{r_{1,t}, \dots, r_{M,t}\}$ be a vector of own rates observed on $m = 1, \dots, M$ assets during period t , and let $\{r_{1,t-1}, \dots, r_{S,t-1}\}$ be a vector of own rates observed on $s = 1, \dots, S$ assets during period $t-1$, where M need not be equal to S . The growth rate of the Jevons user-cost subindex for these assets is calculated as

$$(1) \quad \pi_t = \pi_{t-1} \left(\frac{\prod_{m=1}^M (\pi_{m,t})^{(1/M)}}{\prod_{s=1}^S (\pi_{s,t-1})^{(1/S)}} \right).$$

The term $\pi_{m,t}$ is the real user cost of the monetary services received from monetary asset m during period t ,

$$(2) \quad \pi_{m,t} = \frac{R_t - r_{m,t}}{1 + R_t},$$

where $r_{m,t}$ is the holding-period yield between periods t and $t+1$ (interest being received at the end of the period) and R_t is the holding-period yield on the benchmark asset (Barnett, 1978 and 1980). In monetary aggregation theory, the benchmark asset is defined to be an asset that (i) has zero default risk and (ii) furnishes no monetary (liquidity) services during the household's planning period. An asset is assumed to furnish no

monetary services to a household during a specific period if the cost of converting the asset into medium-of-exchange during that period is prohibitive. In empirical studies, the holding-period yield on the benchmark asset often is proxied by the yield-to-maturity on a low-rated but investment-grade corporate bond, such as a Baa bond.² Markets for lower-grade investment bonds tend to be thin and, hence, the transaction cost for speedy sale of a Baa bond likely is so uncertain as to cause the household to rank the bond at the very bottom of its continuum of monetary assets. The assumption that the benchmark asset has no default risk (that is, that the benchmark rate is nonstochastic) may be relaxed; see Barnett, Liu, and Jensen (1997), Barnett and Serletis (2000, Chap. 12), and Barnett and Wu (2005).

It is important to note that equation (1) contains no terms to adjust the two price vectors, $\{r_{1,t}, \dots, r_{M,t}\}$ and $\{r_{1,t-1}, \dots, r_{S,t-1}\}$, for differences in their average levels. It is commonplace to assume when building unilateral price indices that differences in the levels among the component price series are negligible. When they are not, an adjustment for scale is necessary. Such adjustments are commonplace when splicing index numbers, the topic of our next section.

Splicing

Splicing index numbers is necessary when the length (time span) of the individual, component index numbers is shorter than the desired length for the overall, combined index number. The index numbers to be spliced might be of any type, including unilateral indices. This situation most often occurs when a data source or survey ends and a new one begins, perhaps with an overlap of several periods. Care must be exercised when the levels of the two data sources differ. The topic

² A well-known logical conundrum arises when the yield curve is inverted, such that the holding-period yield on a short-term asset that furnishes monetary services is greater than the return on a long-term asset that does not furnish monetary services. In empirical studies—and in the St. Louis MSI—this is resolved by defining the “benchmark asset” to be the asset with the highest holding-period yield, regardless of market liquidity or time to maturity. Such practice sometimes is referred to as the “envelope approach” to defining the benchmark yield; see Barnett, Offenbacher, and Spindt (1981) and Hancock (2005a,b).

of splicing index numbers has been discussed by a number of authors:

If the overlapping parts of the original series differ by only a scalar multiple, then the splicing problem is trivial because the two series can be combined by merely rescaling one of the series. Such an occurrence is unlikely, however, unless the two series overlap by a single observation. (Hill and Fox, 1997, p. 387)

In practice two runs of annual index numbers may overlap by more than one year. There is then a choice: the runs may be spliced together in any one year or over an average of years in the overlap. There is generally no unique result of the application of the splicing technique. The method is empirical and approximate. (Allen, 1975, p. 32; quoted in Hill and Fox, 1997, p. 387)

Hill and Fox (1997) show that only the geometric mean, among the general class of symmetric means, generates a spliced series that is invariant to rebasing/rescaling of either of the original series (when appropriate scale factors are included). Hill and Fox consider splicing two time series, where one series begins in period 1 and ends in period $M + N$, ($N > 1$), and the second series begins in period $M + 1$ and ends in period $M + N + L$.

Specifically, consider two index numbers that share $N > 1$ overlapping periods: x_i , ($i = 1, \dots, M + N$), and y_j , ($j = M + 1, \dots, M + N + L$). Let the spliced index be denoted $(x \sim y)_n$, $n = 1, \dots, M + N + L$. At the first and last overlap points, the relative scales of the two series are (y_{M+1}/x_{M+1}) and (y_{M+N}/x_{M+N}) . Letting the geometric mean of N arguments a_n , $n = 1, \dots, N$ be denoted as

$$M(a_1, \dots, a_N) = \prod_{n=1}^N (a_n)^{1/N},$$

Hill and Fox (1997) define the spliced series as

$$(3) \quad \begin{aligned} (x \sim y)_n &= A_1 x_n, & n = 1, \dots, M \\ &= M(x_n, y_n), & n = M + 1, \dots, M + N \\ &= A_2 y_n, & n = M + N + 1, \dots, M + N + L, \end{aligned}$$

where $A_1 = (1/x_{M+1})M(x_{M+1}, y_{M+1})$ and $A_2 = (1/y_{M+N})M(x_{M+N}, y_{M+N})$.

Essentially, the Hill and Fox method is a scaled version of the Jevons geometric mean method. Rescaling index numbers is a common practice because many index numbers are unique only up to a linear transformation. Spliced index numbers created via equation (3) have this feature. In particular, the spliced index number may be rescaled further, if desired, by dividing all observations by A_1 , A_2 , or a linear combination of A_1 and A_2 , perhaps to preserve the level of either the first or second input series. Hereafter, we will refer to $(x \sim y)$ as the *un-normalized Hill-Fox index* and to

$$(4) \quad \frac{(x \sim y)}{A_2}$$

as the *A_2 -normalized Hill-Fox index*. For comparison, we also discuss the *A_1 -normalized Hill-Fox index*,

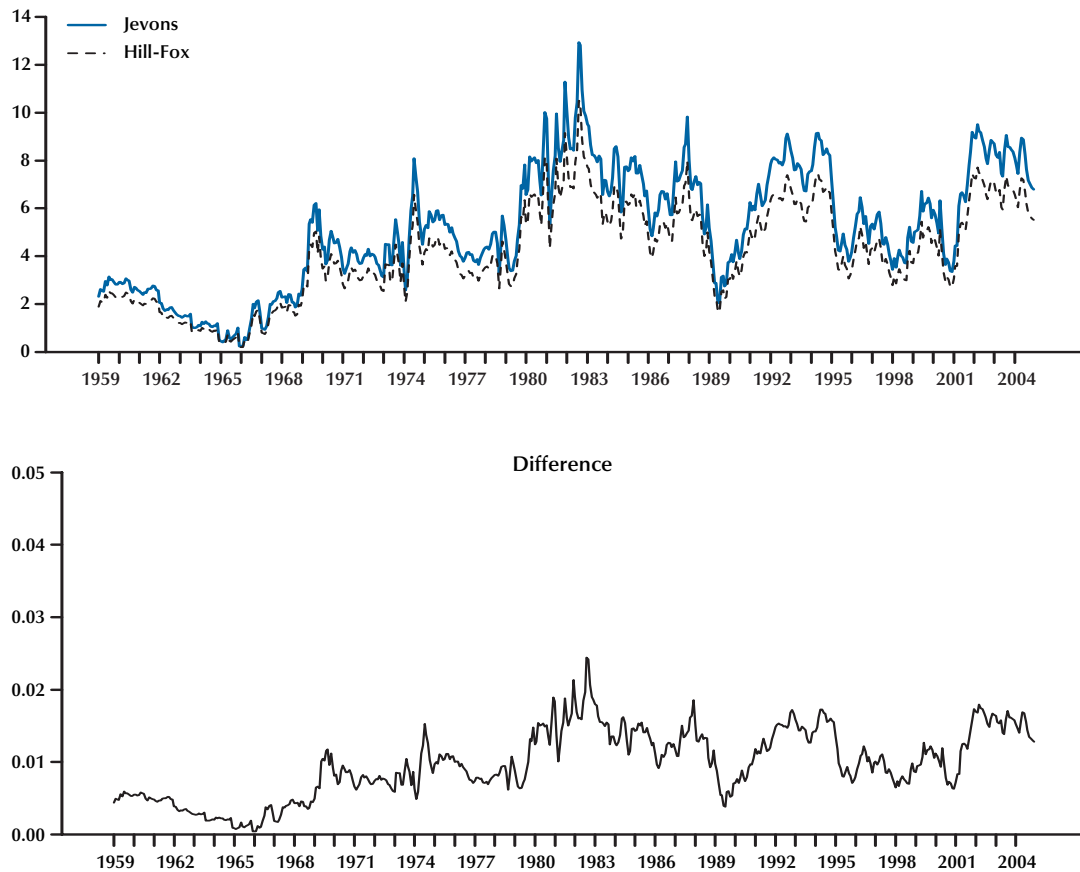
$$(5) \quad \frac{(x \sim y)}{A_1}.$$

When interpreting index numbers, it is important to note that splicing index numbers via the geometric mean method is a mathematically nonlinear and non-invertible transformation. In other words, the original series $\{x\}$ and $\{y\}$ cannot be recovered from the spliced series $(x \sim y)$ even if the ratios A_1 and A_2 are known.

In previous versions of the St. Louis MSI (Anderson, Jones, and Nesmith, 1997c), longer spliced indices for user costs were created from shorter component user cost indices in a two-step method. First, the Jevons geometric mean formula shown in equation (1) was used to splice the two component indices. Second, the spliced index number was divided by A_1 , forcing the spliced index's value to equal the geometric mean of the two component indices for the earliest time period when both of the component indices had valid data. (This formula differs from the one used in the A_1 -normalized Hill-Fox index because the factors A_1 and A_2 are not used in building the index prior to normalization.) When the component indices were of different magnitudes, this practice imparted some undesirable properties to the spliced index number. The most serious

Figure 1

**Small Time Deposit User Costs, Jevons Method vs. Un-Normalized Hill-Fox Method
(commercial bank seasonally adjusted monthly data, January 1959–December 2004)**



problems were for small time deposits, which we examine in the next section.

USER COSTS FOR SMALL TIME DEPOSITS

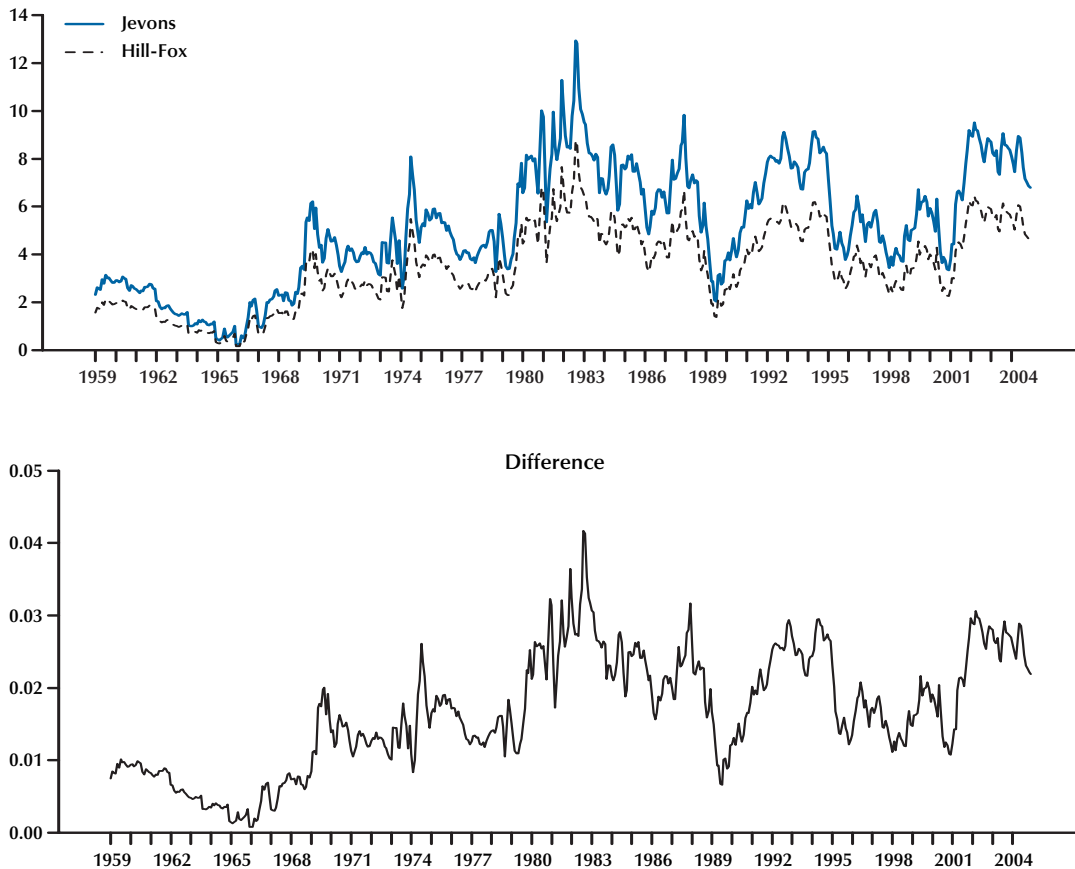
Creating index numbers for small time deposits is troublesome because of the lack of appropriate quantity data. For quantity data, only a single quantity is collected by the Federal Reserve—the total amount of small time deposit liabilities of depository institutions. No data are collected regarding either the original maturity or remaining time to maturity. For deposit offering rates, much

more data are collected, including rates offered on new deposits for five maturities (7 to 91 days, 92 to 182 days, 183 days to 1 year, 1 year to 2.5 years, and 2.5 years or more). But, no data are collected on the distribution of actual rates being paid, and no data on the volume of new deposits issued at each rate. The challenge is to combine these data into accurate maturity-related user cost and quantity indices—an all-but-impossible task given the data limitations.

Data problems for small time deposits are further complicated by breaks in the data. From late 1983 (the demise of Regulation Q) through early 1997, the Board of Governors conducted a monthly survey known as the “Monthly Survey

Figure 2

**Small Time Deposit User Costs, Jevons Method vs. Normalized Hill-Fox Method
(commercial bank seasonally adjusted monthly data, January 1959–December 2004)**



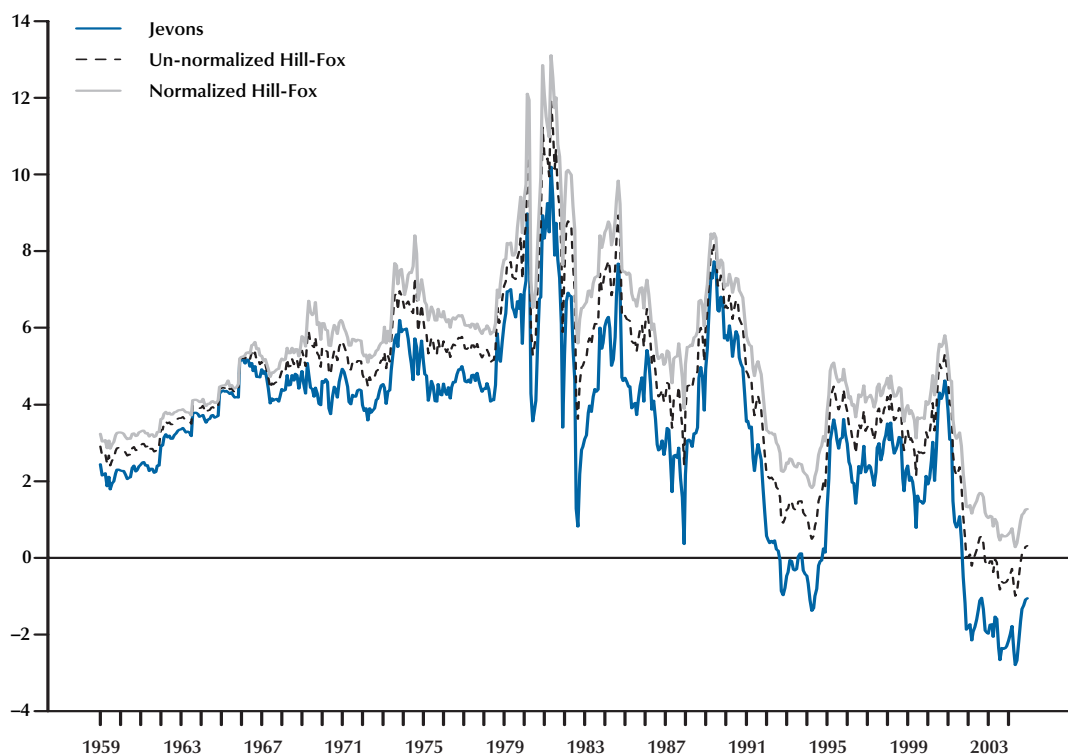
of Selected Deposits,” or the FR2042 survey, to collect from approximately 500 larger banks offering rates on small time deposits. Questions asked on the survey varied somewhat through time. In our judgment, the changes were not so large as to invalidate the survey’s time series for our purposes. The survey was discontinued and replaced in 1997 with survey data purchased from the Bank Rate Monitor Company; these data are available to us beginning in 1987. The Bank Rate Monitor survey includes a larger number of banks than the previous survey and, for the span of years when both are available, differs in level at times by as much as 200 basis points.

We measure the overall user cost of aggregate

small time deposits at each date in each of the two data segments (corresponding to the FR2042 and Bank Rate Monitor surveys) using a Jevons-style geometric mean method for the user costs. The first step in its calculation is to “yield-curve adjust” the offering rates (own rates of return) on the five maturities of small time deposits by subtracting estimated maturity-specific liquidity premiums. (Details of the yield-curve adjustment are discussed in the appendix.) Next, user costs for each maturity and date, within each data segment, are calculated by subtracting the yield-curve-adjusted own rates from the estimated benchmark rate. Finally, the two user-cost segments are spliced using the normalized Hill-Fox

Figure 3

**Small Time Deposits at Banks, Own Rates Implied by Spliced User Costs
(monthly data, January 1959–December 2004)**



method—that is, the final user cost series is measured as equation (4). The two Hill-Fox scale factors, A_1 and A_2 , are shown in Table 1. We construct separate user cost indices for commercial banks and thrift institutions; here, we consider only the data for commercial banks. Data for thrifts are similar.

Figures 1 and 2 compare user costs for small time deposits constructed with three methods: the Jevons method, equation (1); the un-normalized Hill-Fox method, equation (3); and the A_2 -normalized Hill-Fox method, equation (4). The un-normalized Hill-Fox values, shown in Figure 1, are consistently lower than, but quite close to, the values from the Jevons method. The values from the normalized Hill-Fox method, shown in Figure 2, are consistently lower than values from the un-normalized Hill-Fox method. The difference between the normalized and un-normalized

Table 1

Splice Factors for Small Time Deposits in the Hill-Fox Method

| | A_1 | A_2 |
|---------------------|-------|-------|
| Commercial banks | 0.811 | 1.19 |
| Thrift institutions | 0.775 | 1.24 |

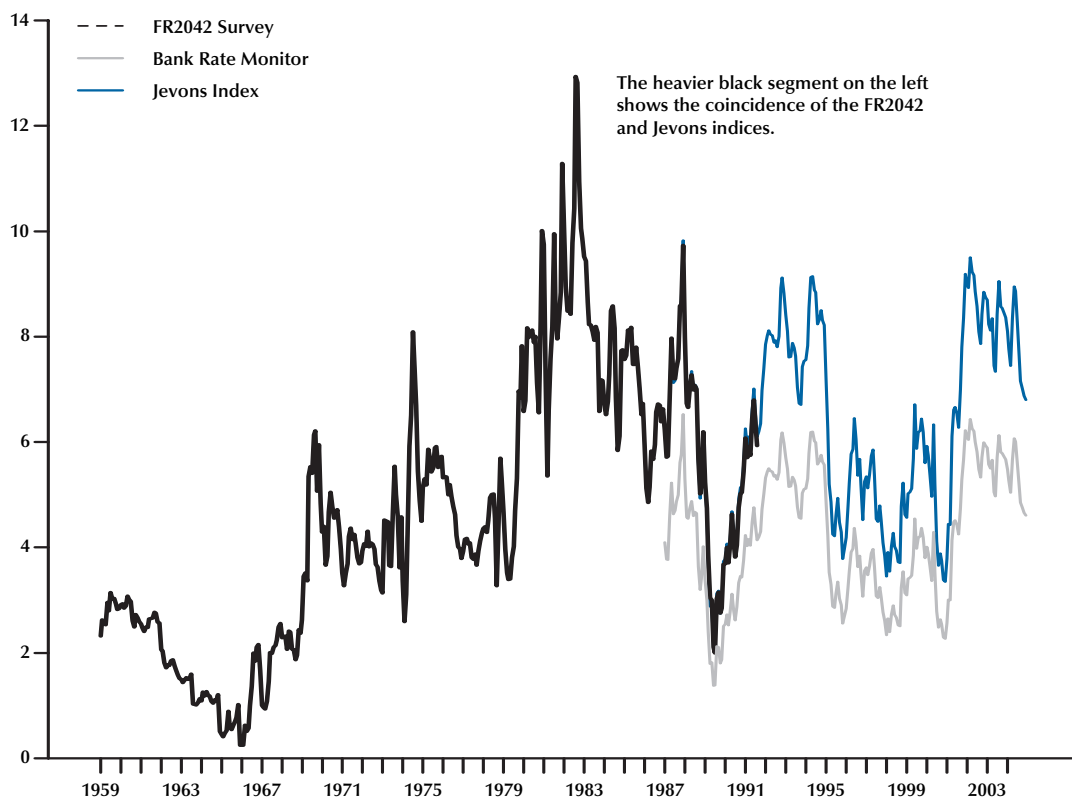
values, algebraically, is due to division by the factor A_2 ; the information content of the two indices is the same.

Our preference for the normalized Hill-Fox index is based on the analysis shown in Figures 3 through 6.

Figure 3 illustrates our previous point that splicing index numbers via geometric means is

Figure 4

Small Time Deposit User Costs, Jevons Method and Components
(commercial bank seasonally adjusted monthly data, January 1959–December 2004)



a nonlinear and non-invertible transformation. In this case, own rates of return for small time deposits cannot be recovered from spliced user costs by the familiar equation

$$(6) \quad \text{own rate} = \text{benchmark rate} - \text{user cost}.$$

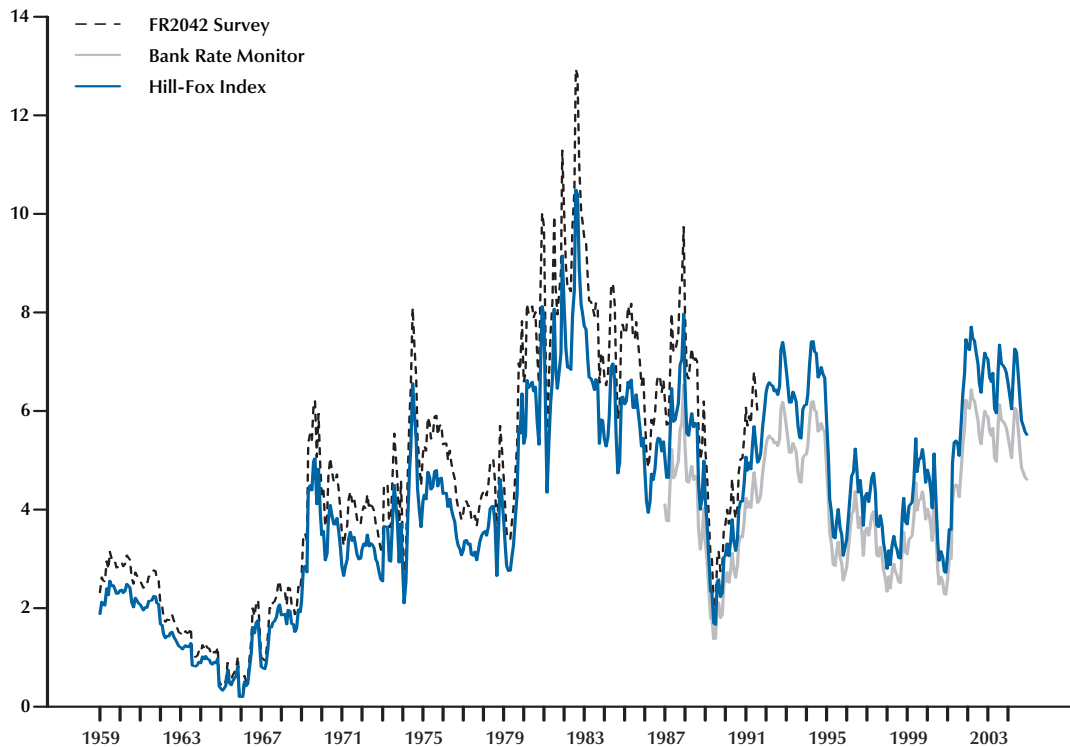
The three lines in the figure correspond to own rates of return calculated with equation (6) from spliced user cost series constructed with three methods: our previous Jevons method; the unnormalized Hill-Fox method (equation (3)); and the A_2 -normalized Hill-Fox method (equation (4)). For the Jevons method, calculated own rates of return are negative during the first half of the 1990s and after 2000. Some users of the MSI have calculated such negative own rates and called them to our attention as an error in the MSI con-

struction; in fact, the negative values are an artifact from use of the Jevons method. For the unnormalized Hill-Fox method (equation (3)), the calculated own rates of return are negative, but less so, during 2003 and 2004. Own rates calculated with the A_2 -normalized Hill-Fox method (equation (4)), are positive, although very close to zero during 2003.

The comparison shown in Figure 3 has disturbed some users of the MSI, who would prefer that own rates and user costs be invariant to the method used to construct the MSI and its components. Unfortunately, this is impossible, as is illustrated in Figures 4, 5, and 6. Each figure displays three index numbers. Two of the index numbers are the same on all three figures: a Jevons subindex built from the various maturity-specific

Figure 5

Small Time Deposit User Costs, Un-Normalized Hill-Fox Method and Components
(commercial bank seasonally adjusted monthly data, January 1959–December 2004)



deposit offering rates collected on the FR2042 survey and a Jevons subindex built from similar data collected in the Bank Rate Monitor survey. The third index number in each figure corresponds to a method of splicing the FR2042 and Bank Rate Monitor index numbers.

- In Figure 4, the spliced index is the A_1 -normalized Hill-Fox index, equation (5). As expected, the index tracks the FR2042 index prior to 1987. Beginning in 1987, the index follows the shape but not the level of the Bank Rate Monitor index.
- In Figure 5, the spliced index is the Hill-Fox (1997) index, equation (3). As expected, the Hill-Fox index lies below the FR2042 data prior to 1987 (the date corresponding to A_1), between the FR2042 and Bank Rate Monitor data from 1987 to 1997 (the date

corresponding to A_2), and above the Bank Rate Monitor data after 1997.

- In Figure 6, the spliced index is the A_2 -normalized Hill-Fox index, equation (4). As expected, the pattern is the opposite of Figure 4, with the normalized Hill-Fox index tracking the Bank Rate Monitor index after the end of the FR2042 index in 1997.

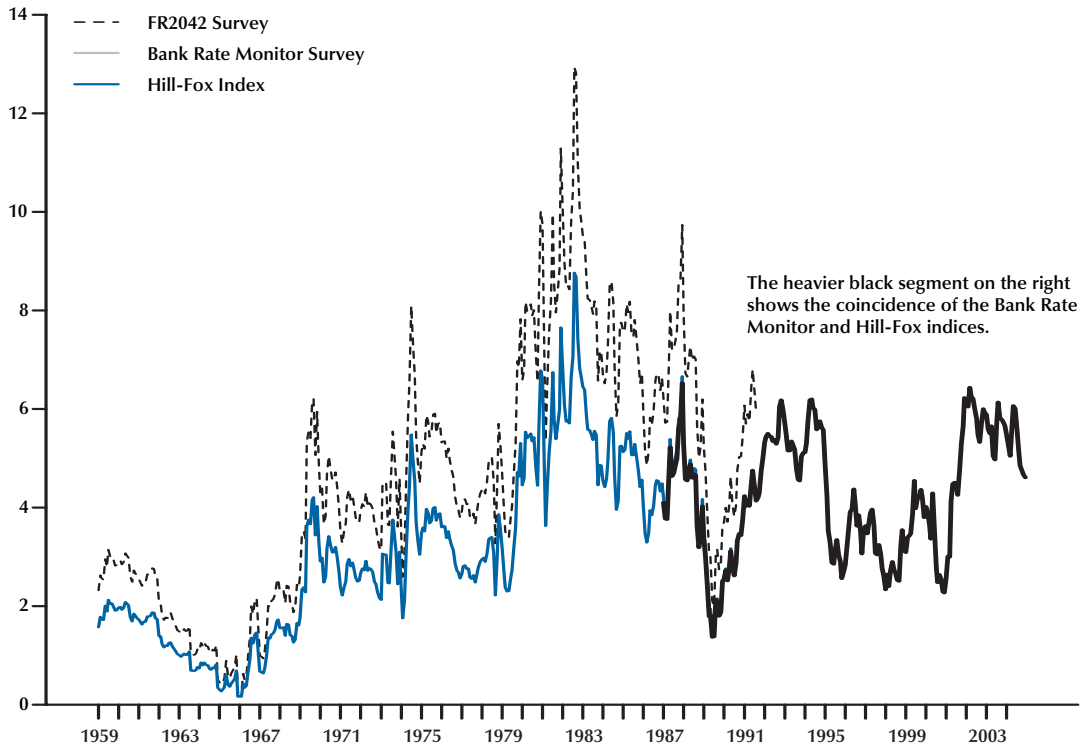
In the published, revised MSI user costs, we follow the method of Figure 6.

MONEY MARKET MUTUAL FUND YIELD

Money market mutual funds are an important asset in the MSI. In this revision, we both correct an error in the calculation of their user cost and introduce an extension. The error was the result

Figure 6

Small Time Deposit User Costs, Normalized Hill-Fox Method and Components
(commercial bank seasonally adjusted monthly data, January 1959–December 2004)



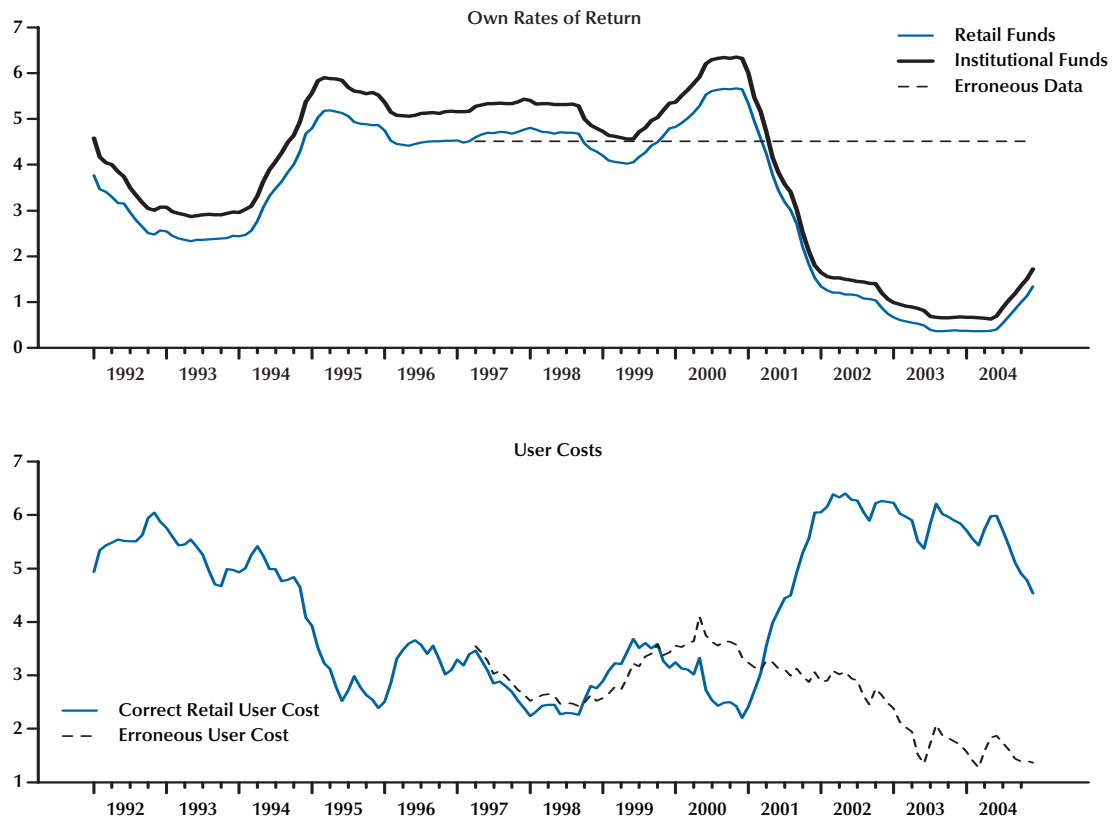
of an attempt gone awry to ensure timely publication even when the arrival of certain data was delayed. The extension improves the indices, beginning with data in 1997, by using separate own rates series for general-purpose/broker-dealer funds and institutionally oriented funds.

In their calculations, Anderson, Jones, and Nesmith (1997c) used unpublished data regarding the yield on money market mutual funds obtained from the Federal Reserve Board. Sometimes, tardy arrival of these data threatened to delay timely publication of the MSI figures even when other data had arrived. To minimize publication delays, when necessary and for one additional period, the last-reported figure was carried forward. This compromise was based on the assumption that any delayed observation would be in place by the following month's production date and, at

that time, the correct observation would replace the temporary extrapolated value.

In April 1997, the data source for money market mutual funds changed. Unfortunately, due to an error, the last-reported figure from the previous database continued to be carried forward by the computer program. A sharp-eyed user of the MSI brought this error to our attention. We have since modified our procedures and programs such that replacement of a missing figure by extrapolation of the previous value cannot continue automatically for more than one additional month. This meets, in large part, the sometimes conflicting goals of producing high-quality data in a timely fashion even when receipt of some needed input figures is delayed.

The correct and incorrect figures for money market mutual fund yields during 1997-2003 are shown in Figure 7. The computer-generated incor-

Figure 7**Money Market Mutual Funds
(percent annual rate, not seasonally adjusted)**

rect series (shown as the dotted line in Figure 7) shows no change after mid-1997, whereas the actual data, of course, have changed dramatically. In early 2004, for example, the average yield on broker-dealer money funds was approximately 1 percent, as opposed to 1997's nearly 5 percent yield. Assuming a benchmark yield of 5 percent, the difference in the user cost would be almost 4 percent (3.8 percent – 0.06 percent).

SUMMARY AND CONCLUSIONS

Creating new index numbers by combining other index numbers is a common occurrence in applied research, and the geometric mean formula has well-known desirable properties for this pur-

pose. In the St. Louis MSI, the geometric mean is used in two places: It is used to create unilateral index numbers for certain aggregate composite assets, and it is used to create certain longer indices by splicing shorter index numbers. In this analysis, we have emphasized that splicing indices differs in certain respects from the more general practice of creating a unilateral index number from a large number of component series. Building general unilateral indices usually entails combining a large number of component indices that exist for all dates and are of similar size. Splicing, however, usually entails creating a longer index from two shorter indices that are not defined over the complete time span but do overlap for a certain number of periods. In addition, the normalization differs. General unilateral indices have

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no natural normalization—in the St. Louis MSI, they are normalized to their first period. Spliced series also have no natural normalization but, because their components often differ in scale, explicit adjustments for scale are included. (Similar to most index numbers, spliced indices may be renormalized to an arbitrary period without loss of information.) In the revised data presented herein, the spliced unilateral user-cost index for small time deposits is normalized to the latest time period in which both component indices are observed.

The use of index number theory to measure the amount of monetary services that consumers receive from their asset portfolio continues to be, after 25 years, an active subject of economic research. The Bank of England recently published revised series (Hancock, 2005a,b), and the European Central Bank is preparing new monetary index numbers for the euro area. For the United States, the only currently published monetary index number data are those of the Federal Reserve Bank of St. Louis. This analysis has introduced two changes to the St. Louis figures so as to improve the measured user costs of small time deposits and money market mutual funds.

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APPENDIX

THE YIELD CURVE ADJUSTMENT FOR USER COSTS OF MONETARY ASSETS

Certain aggregate assets included in the St. Louis MSI, such as small time deposits, are sums of individual components that differ by maturity; see Table A1. For these assets, maturity-specific own rates of return are available for the components, whereas maturity-specific quantities are not. The problem arises, then, regarding how to choose an own rate of return for such aggregated composite assets that is representative of the own rates of return on its components.

Choosing an own rate of return for a composite asset requires an economic assumption regarding its component assets' cross-price elasticities of substitution. In many studies, the components are assumed to be perfect substitutes. Under this assumption, the appropriate measure of the aggregate asset's own rate of return is the maximum of the components' own rates of return. We find this assumption implausible. Instead, the St. Louis MSI assumes that the components are imperfect substitutes for each other and that the entire group is separable in demand from other asset groups such that the household's total expenditure on the monetary services obtained from the asset group is invariant to changes in the relative own rates of return *within* the group. In this case, the appropriate measure of the aggregate asset's own rate is a Jevons-style geometric index number. Before the index can be calculated, however, maturity-related differences in the component assets' own rates of return must be removed by subtracting a *yield curve adjustment*. In the St. Louis MSI, the magnitude of the yield curve adjustment is equal to the slope of the Treasury constant-maturity yield curve between the appropriate maturities, if the slope is positive, or equal to zero, if the slope is negative (in other words, the yield curve is inverted). After subtracting the appropriate adjustment from each component's own rate of return, the composite asset's own rate of return is set equal to the maximum of the components' adjusted own rates of return. (All rates of return are stated as annualized, one-month holding-period yields on a bond interest, or 365-day, basis.)

The yield curve adjustment may be defined algebraically as follows (Anderson, Jones, and Nesmith, 1997c): Let r_n be the own rate of return for a particular monetary asset and let r_n^T be the yield-to-maturity (on a bond-equivalent basis) for a Treasury security, each having n months to maturity. Let r_1^T be the expected annualized one-month yield on Treasury bills, on a bond-equivalent basis. Then the yield curve-adjusted own rate is defined as

$$r_n^{YCA} = r_n - \max(r_n^T - r_1^T, 0).$$

For small time deposits, the effect of the yield curve adjustment on own rates of return for 1-, 2- and 3-year maturities from January 1999 to December 2004 is shown in Figure 8. For earlier discussions of yield curve adjustment in the context of monetary index numbers, see Cockerline and Murray (1981) and Farr and Johnson (1985).

Table A1**Composite Monetary Assets in the MSI and Their Components**

| Composite monetary assets in the MSI and components | Relative importance as of January 2005 (billions of dollars, and share of total assets in MSI aggregate) | Treasury yields used to calculate yield-curve adjustment |
|--|---|---|
| Eurodollars* | | |
| Overnight, 3- and 6-month maturities | \$381 billion (overnight and term); 4 percent of MSI-M3 | 3- and 6-month secondary market Treasury bill rate |
| Commercial paper† | | |
| 3- and 6-month maturities | (Discontinued September 1998) | 3- and 6-month secondary market Treasury bill rate |
| Bankers acceptances‡ | | |
| 3- and 6-month maturities | (Discontinued September 1998) | 3- and 6-month secondary market Treasury bill rate |
| Large-denomination time deposits‡ | | |
| 3- and 6-month maturities | \$1,116 billion (negotiable and nonnegotiable); 11.8 percent of MSI-M3 | 3- and 6-month secondary market Treasury bill rate |
| Small-denomination time deposits§ | | |
| 7 to 91 day, 92 to 182 day, 183 day to 1 year, 1 to 2.5 year, and 2.5 year and longer maturities | \$826 billion; 12.9 percent of MS-M2; 8.7 percent of MSI-M3 | 3- and 6-month secondary market Treasury bill rate; 1-, 2-, and 3-year Treasury constant-maturity yield |

NOTE: * Eurodollars are included in the MSI-M3 index. This category includes overnight and term deposits. Federal Reserve data published through 1995 separated overnight from term deposits; data published thereafter does not. A primary reason for discontinuing the separate categories was that overnight deposits often were held under continuing contracts, thereby resembling term deposits, and term deposits often were withdrawable, thereby resembling overnight deposits. The St. Louis MSI use only total eurodollars.

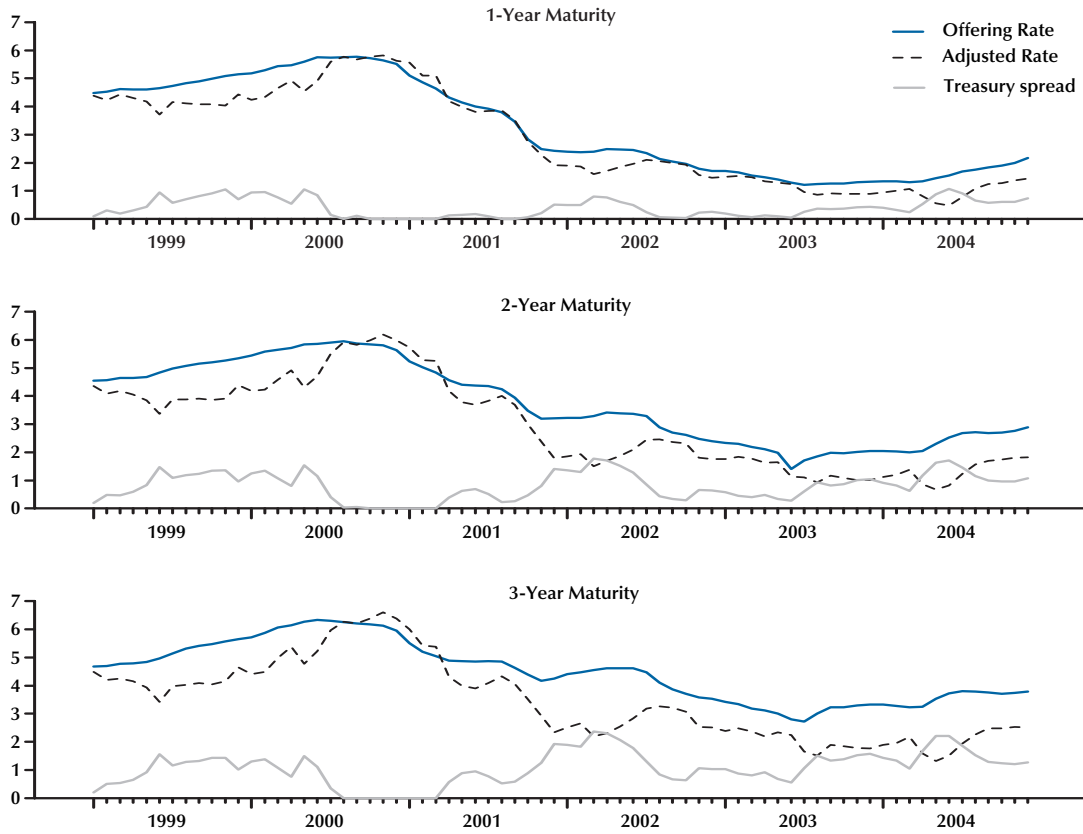
† Commercial paper and bankers acceptances are included in the MSI-L index. Calculation of this index was discontinued in September 1998 when certain required data became unavailable.

‡ Includes negotiable and nonnegotiable CDs. Separate figures for the two categories are not available.

§ Includes "all-savers certificates," with variable ceiling rate and 12-month maturity.

Figure A1

Yield Curve Adjustments for Small Time Deposits
 (adjusted rate = offering rate – Treasury yield spread)





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