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MEASURING MONETARY POLICY*

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We develop a model-based, VAR methodology for measuring innovations in monetary policy and their macroeconomic effects. Using this framework, we are able to compare existing approaches to measuring monetary policy shocks and derive a new measure of policy innovations based directly on (possibly time-varying) estimates of the central bank's operating procedures. We also propose a new measure of the overall stance of policy (including the endogenous or systematic component) that is consistent with our approach.

I. INTRODUCTION

Accurate measurement of the effects of changes in monetary policy on the economy is essential, both for good policy-making and for choosing among alternative macroeconomic theories. Unfortunately, attempts to quantify the links between central bank actions and the economy quickly run into a major roadblock: there is no consensus on how to measure the size and direction of changes in monetary policy. The traditional approach, which identifies changes in monetary policy with changes in the stock of money, is not adequate, since in practice the growth rates of monetary aggregates depend on a variety of nonpolicy influences. For example, because the Federal Reserve System's operating procedures have typically involved some smoothing of short-term interest rates, and hence accommodation of money demand shocks, observed money growth rates in the United States reflect changes in money demand as well as changes in money supply.[1] Secular changes in velocity brought about by financial innovation, deregulation, and other factors are a further barrier to using money growth rates alone as a measure of the direction of policy.

As the deficiencies of money stock growth as a measure of the stance of monetary policy have become widely recognized, many researchers have tried to find alternative indicators. Recent attempts have largely fallen into two general categories. First, following the example of Friedman and Schwartz [1963], Romer

1. The fact that innovations in the money stock reflect demand as well as supply influences helps explain the "liquidity puzzle," the finding that innovations in money are not reliably followed by declines in interest rates; see Reichenstein [1987], Leeper and Gordon [1992], and Strongin [1995] for discussions.

and Romer [1989] reintroduced the "narrative approach" to the study of monetary policy. Based on a reading of the minutes of the Federal Open Market Committee, Romer and Romer determined a set of dates at which policy-makers appeared to shift to a more anti-inflationary stance. An appealing aspect of the Romers' approach is that it uses additional information—specifically, policy-makers' statements of their own intentions—to try to disentangle money supply from money demand shocks. A disadvantage of this approach, besides inherent problems of reproducibility and subjectivity, is that it does not clearly distinguish between the endogenous and exogenous components of policy change, which is necessary for identifying the effects of monetary policy on the economy [Dotsey and Reid 1992; Leeper 1993; Shapiro 1994; Hoover and Perez 1994; Sims and Zha 1995; Bernanke, Gertler, and Watson 1997]. The Romers' methodology also yields a rather limited amount of information: they give dates only for contractionary changes in policy, not expansionary shifts, and their method provides no indication of the severity or duration of each episode. Building on the Romers' work, Boschen and Mills [1991] used FOMC documents to rate monetary policy in each month as "very tight," "tight," "neutral," "easy," or "very easy," depending on the relative weights the policy-makers assigned to reducing unemployment and reducing inflation. Although Boschen and Mills provide a more continuous and possibly more informative measure of policy than do Romer and Romer, their indicator likely also suffers relatively more severe problems of subjectivity and commingling of endogenous and exogenous policy changes.

The second general strategy for measuring monetary policy stance—which is the focus of the present article—is to use prior information about central bank operating procedures, in conjunction with vector autoregression (VAR) estimation techniques, to develop data-based indexes of policy. For example, Bernanke and Blinder [1992] argued that over much of the past 30 years the Fed has implemented policy changes through changes in the federal funds rate (the overnight rate in the market for commercial bank reserves). They concluded that the funds rate may therefore be used as an indicator of policy stance (see also Laurent [1988] and Bernanke [1990]); in particular, they interpreted VAR innovations to the funds rate as innovations to the Fed's policy. In a similar vein, Sims [1992] used short-term rates as monetary indicators in a multicountry study. However, not all researchers working in the VAR-based literature have adopted short-term interest rates as

their preferred indicator of policy. Following a suggestion of Thornton [1988b], Christiano and Eichenbaum [1992] have made the case for using the quantity of nonborrowed reserves as the primary measure of monetary policy (also see Eichenbaum [1992]). Strongin [1995] proposed as a policy indicator the portion of nonborrowed reserve growth that is orthogonal to total reserve growth. He motivated this measure by arguing that the Fed is constrained to meet total reserve demand in the short run but can effectively tighten policy by reducing nonborrowed reserves and forcing banks to borrow more from the discount window. Cosimano and Sheehan [1994] characterized Fed policy after 1984 as borrowed-reserves targeting, which suggests that borrowed reserves might be a useful indicator for the more recent period.

Both the narrative and VAR-based methods for measuring monetary policy have been widely used in applied work.[2] Unfortunately, there is evidently little agreement on which of the various measures most accurately captures the stance of policy, leading many authors to hedge by using a variety of indicators. Eichenbaum and Evans' [1995] study of the effect of monetary policy on exchange rates is fairly typical in employing three alternative policy measures: in their case, Strongin's measure, innovations to the federal funds rate, and the Romer dates. However, although using alternative measures allows the researcher to claim robustness when the results for each indicator are similar, this strategy provides no guidance for cases when the results for different indicators are inconsistent. (Indeed, we show below that alternative indicators can lead to quite different inferences.) Moreover, simply using a variety of alternative measures of monetary policy cannot guarantee that some more accurate indicator has not been excluded; that the best indicator is not perhaps some combination of the various "pure" indicators; or that the best indicator is the same for all countries or for all periods. Thus, it would be quite useful to have a systematic method of comparing alternative candidate indicators of policy.

Eichenbaum [1992, p. 1010] has stressed the importance of

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2. There are dozens of examples. A sampling of better known studies includes Kashyap, Stein, and Wilcox [1993]; Friedman and Kuttner [1993]; Ramey [1993]; Gertler and Gilchrist [1994]; Cochrane [1994]; Eichenbaum and Evans [1995]; Cecchetti [1995]; and Burnside, Eichenbaum, and Rebelo [1995]. Applications to countries other than the United States, which use the VAR methodology almost exclusively, include Sims [1992]; Cushman and Zha [1994]; Gerlach and Smets [1995]; Kim and Roubini [1995]; Tsatsaronis [1995]; Barran, Coudert, and Mojon [1996]; and Clarida and Gertler [1997], among others.

finding a means of choosing among indicators, noting that in his particular application "inference depends very sensitively on which of the two candidate measures [short-term interest rates or nonborrowed reserves] we work with." He also suggests that "further progress on these issues can be made only by carefully studying the institutional details of how monetary policy is actually carried out in the different countries. . . ." Following Eichenbaum's suggestion, in this article we develop and implement a general, VAR-based methodology in which the indicator of monetary policy stance is not assumed but rather is derived from an estimated model of the central bank's operating procedure. More specifically, we employ a "semi-structural" VAR model that leaves the relationships among macroeconomic variables in the system unrestricted but imposes contemporaneous identification restrictions on a set of variables relevant to the market for commercial bank reserves.

Our method has several advantages over previous approaches. First, because our specification nests the best known quantitative indicators of monetary policy used recently in VAR modeling, including all those mentioned above, we are able to perform explicit statistical comparisons of these and other potential measures, including hybrid measures that combine the basic indicators. Second, our analysis leads directly to estimates of a new policy indicator that is optimal, in the sense of being most consistent with the estimated parameters describing the central bank's operating procedure and the market for bank reserves. Third, by estimating the model over different sample periods, we are able to allow for changes in the structure of the economy and in operating procedures, while imposing a minimal set of identifying assumptions. Finally, although we consider only the post-1965 U. S. case in this paper, our method is applicable to other countries and periods, and to alternative institutional setups.[3]

A frequently heard criticism of the VAR-based approach is that it focuses on monetary policy innovations rather than on the arguably more important systematic or endogenous component of policy. We believe this criticism to be misplaced. The emphasis of the VAR-based approach on policy innovations arises not because shocks to policy are intrinsically important, but because (as we discuss further below) tracing the dynamic response of the

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3. Bernanke and Mihov [1997] apply these methods to a study of German monetary policy.

economy to a monetary policy innovation provides a means of observing the effects of policy changes under minimal identifying assumptions.[4] However, although we disagree with the view that analysis of policy shocks is uninteresting, we also recognize that it would be useful to have an indicator of the overall stance of monetary policy, including the endogenous component. An additional contribution of this article is to propose just such an indicator, one that is both consistent with our underlying approach and, we believe, intuitively appealing.

The rest of the article proceeds as follows. Section II briefly describes our general methodology for identifying innovations to monetary policy. Section III lays out a standard model of the market for bank reserves that nests some common alternative descriptions of Fed operating procedures. Estimation of this model by GMM (Section IV) allows us both to evaluate the leading candidate indicators of policy innovations and to develop an alternative measure. Section V discusses how the choice of policy measure affects our conclusions about the impact of monetary policy on the economy. Section VI introduces our total policy measure, inclusive of both the systematic and random components of policy, and compares it with narrative measures of monetary policy. Section VII concludes.

II. METHODOLOGY

Bernanke and Blinder [1992] proposed the following strategy for measuring the dynamic effects of monetary policy shocks. Suppose that the "true" economic structure is

$$(1) \qquad \mathbf{Y}_t = \sum_{i=0}^{k} \mathbf{B}_i \mathbf{Y}_{t-i} + \sum_{i=0}^{k} \mathbf{C}_i p_{t-i} + \mathbf{A}^y \mathbf{v}_t^y$$

$$(2) \qquad p_t = \sum_{i=0}^{k} \mathbf{D}_i \mathbf{Y}_{t-i} + \sum_{i=1}^{k} \mathbf{g}_i p_{t-i} + v_t^p.$$

Equations (1) and (2) define an unrestricted linear dynamic model that allows both contemporaneous values and up to k lags of any variable to appear in any equation.[5] Boldface letters are used to

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4. Bernanke, Gertler, and Watson [1997] provide a VAR-based method for estimating the effects of systematic or endogenous policy changes; see also Sims and Zha [1995].

5. Expectations variables are not explicitly included in (1)–(2), but these can be accommodated by replacing expected future values of variables occurring in the

indicator vectors or matrices of variables or coefficients. In particular, $\mathbf{Y}$ is a vector of macroeconomic variables, and p is a variable indicating the stance of policy. Note that for the moment p is taken to be a scalar measure, e.g., the federal funds rate. Equation (2) predicts current policy stance given current and lagged values of macroeconomic variables and lagged policy variables, while equation (1) describes a set of structural relationships in the rest of the economy. The vector $\mathbf{v}^y$ and the scalar v^p are mutually uncorrelated "primitive" or "structural" error terms. As in Bernanke [1986], the structural error terms in equation (1) are premultiplied by a general matrix $\mathbf{A}^y$, so that shocks may enter into more than one equation: hence the assumption that the elements of $\mathbf{v}^y$ are uncorrelated imposes no restriction. The assumption that the policy shock v^p is uncorrelated with the elements of $\mathbf{v}^y$ is also not restrictive, in our view; we think of independence from contemporaneous economic conditions as part of the definition of an exogenous policy shock.[6]

The system (1)–(2) is not econometrically identified in general. Bernanke and Blinder point out that to identify the dynamic effects of exogenous policy shocks on the various macro variables $\mathbf{Y}$, *without* necessarily having to identify the entire model structure, it is sufficient to assume that policy shocks do not affect the given macro variables within the current period; i.e., $\mathbf{C}_0 = 0$.[7] Under this assumption the system (1)–(2) can be written in VAR format by projecting the vector of dependent variables on k lags of itself. Estimation of the resulting system by standard VAR methods, followed by a Choleski decomposition of the covariance matrix (with the policy variable ordered last) yields an estimated series for the exogenous policy shock v^p (see Bernanke and Blinder). Impulse response functions for all variables in the system with respect to the policy shock can then be calculated and can be interpreted as the true structural responses to policy shocks.

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model by the linear projections of these expectations on current and lagged variables in the system. Alternatively, think of some of the component equations of (1)–(2) as reduced-form decision rules relating choice variables to observable state variables.

6. The idea of an exogenous policy shock has been criticized as implying that the Fed randomizes its policy decisions. Although the Fed does not explicitly randomize, it seems reasonable to assert that, for a given objective state of the economy, many random factors affect policy decisions. Such factors include the personalities and intellectual predilections of the policy-makers, politics, data errors and revisions, and various technical problems.

7. This assumption is not plausible for all macro variables, notably for various types of asset prices. See footnote 18 below.

The Bernanke-Blinder method assumes that a good scalar measure of policy is available. However, it may be the case that we have only a *vector* of policy indicators, $\mathbf{P}$, which contain information about the stance of policy but are affected by other forces as well. For example, if the Fed's operating procedure is neither pure interest-rate targeting nor pure reserves targeting, then both interest rates and reserves will contain information about monetary policy; but in that case, both variables may also be affected by shocks to the demand for reserves and other factors. In this more general case the structural macroeconomic model (1)–(2) may be written as

$$(3) \qquad \mathbf{Y}_t = \sum_{i=0}^{k} \mathbf{B}_i \mathbf{Y}_{t-i} + \sum_{i=1}^{k} \mathbf{C}_i \mathbf{P}_{t-i} + \mathbf{A}^y \mathbf{v}_t^y$$

$$(4) \qquad \mathbf{P}_t = \sum_{i=0}^{k} \mathbf{D}_i \mathbf{Y}_{t-i} + \sum_{i=0}^{k} \mathbf{G}_i \mathbf{P}_{t-i} + \mathbf{A}^p \mathbf{v}_t^p.$$

Equation (4) states that the set of policy indicators $\mathbf{P}$ depend on current and lagged values of $\mathbf{Y}$ and $\mathbf{P}$, and on a set of disturbances $\mathbf{v}^p$. We assume that one element of the vector $\mathbf{v}^p$ is a money supply shock or policy disturbance v^s; the other elements of $\mathbf{v}^p$ may include shocks to money demand or whatever disturbances affect the policy indicators. Equation (3) allows the nonpolicy variables $\mathbf{Y}$ to depend on current and lagged values of $\mathbf{Y}$ and on lagged values (only) of $\mathbf{P}$; allowing the nonpolicy variables to depend only on lagged values of policy variables ($\mathbf{C}_0 = 0$) is analogous to the identifying assumption made above in the scalar case.

As in the case of a scalar policy indicator, we would like to find a way to measure the dynamic responses of variables in the system to a policy shock v^s. As before, we can rewrite the system (3)–(4) in VAR form (with only lagged variables on the right-hand side) and estimate by standard methods. Let $\mathbf{u}_t^p$ be the portion of the VAR residuals in the policy block that are orthogonal to the VAR residuals in the nonpolicy block. Then straightforward calculation shows that $\mathbf{u}_t^p$ satisfies

$$(5) \qquad \mathbf{u}_t^p = (\mathbf{I} - \mathbf{G}_0)^{-1} \mathbf{A}^p \mathbf{v}_t^p,$$

where the variables on the right-hand side of (5) are as defined in (4). Alternatively, dropping subscripts and superscripts, we can

rewrite (5) as

(6) $\mathbf{u} = \mathbf{Gu} + \mathbf{Av}.$

Equation (6) is a standard structural VAR (SVAR) system, which relates observable VAR-based residuals $\mathbf{u}$ to unobserved structural shocks $\mathbf{v}$, one of which is the policy shock v^s. This system can be identified and estimated by conventional methods, allowing recovery of the structural shocks, including v^s. The policy shock v^s is analogous to the innovation to the federal funds rate in the scalar case analyzed by Bernanke and Blinder [1992]. As in the scalar case, the structural responses of all variables in the system to a policy shock can be measured by the associated impulse response functions. Further, the historical sequence of policy shocks can be recovered from the VAR residuals by means of (6). In the remainder of this article we apply this approach to the measurement of U. S. monetary policy stance since 1965.

III. MONETARY POLICY AND THE MARKET FOR BANK RESERVES

To implement the methodology described in Section II, we need a specific model relating the VAR residuals and the structural shocks in the policy block. We employ a standard model of the market for commercial bank reserves and Federal Reserve operating procedures.[8] Although simple, this model is rich enough to nest all the VAR-based policy indicators mentioned in the introduction, as well as other plausible measures.

Continuing to use u to indicate an (observable) VAR residual and v to indicate an (unobservable) structural disturbance, we assume that the market for bank reserves is described by the following set of equations:

(8) $u_{TR} = -\alpha u_{FFR} + v^d$

(9) $u_{BR} = \beta(u_{FFR} - u_{DISC}) + v^b$

(10) $u_{NBR} = \phi^d v^d + \phi^b v^b + v^s.$

Equation (8) is the banks' total demand for reserves, expressed in innovation form. It states that the innovation in the demand for total reserves u_{TR} depends (negatively) on the innovation in the federal funds rate u_{FFR} (the price of reserves) and on a demand

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8. Similar models are estimated by Brunner [1994] and Gordon and Leeper [1994], among many others. A stochastic general equilibrium model of the federal funds market is provided by Coleman, Gilles, and Labadie [1993].

disturbance v^d. Equation (9) determines the portion of reserves that banks choose to borrow at the discount window. As is conventional, the demand for borrowed reserves (in innovation form), u_{BR}, is taken to depend positively on the innovation in the federal funds rate u_{FFR} (the rate at which borrowed reserves can be relent) and negatively on the discount rate u_{DISC} (the cost of borrowed reserves); v^b is a disturbance to the borrowing function.[9] The innovation in the demand for nonborrowed reserves, the difference between total and borrowed reserves, is $u_{TR} - u_{BR}$.

Equation (10) describes the behavior of the Federal Reserve. We assume that the Fed observes and responds to shocks to the total demand for reserves and to the demand for borrowed reserves within the period, with the strength of the response given by the coefficients ϕ^d and ϕ^b. That the Fed observes reserve demand shocks within the period is reasonable, since it monitors total reserves (except vault cash) and borrowings continuously. However, the case in which the Fed does not observe (or does not respond to) one or the other of these disturbances can be accommodated by setting the relevant coefficients to zero. The disturbance term v^s is the shock to policy that we are interested in identifying. Note that the system (8)–(10) is in the form of equation (6).

It will also be useful to write the reduced-form relationship between the VAR residuals **u** and the structural disturbances **v**, as in equation (5). To do so, we first make the simplifying assumption that the innovation to the discount rate u_{DISC} is zero.[10] To solve the model, we impose the condition that the supply of nonborrowed reserves plus borrowings must equal the total demand for reserves. Solving in terms of innovations to total reserves, nonborrowed reserves, and the federal funds rate, we

9. Various sanctions and restrictions imposed by the Fed on banks' use of the discount window make the true cost of borrowing greater than the discount rate; hence, banks do not attempt to borrow infinite quantities when the funds rate exceeds the discount rate. A borrowing function of the form of (9) is used in standard Federal Reserve models of money markets (e.g., Tinsley et al. [1982]). Goodfriend [1983], Peristiani [1991], and Clouse [1994] explore the empirical robustness of the borrowing function along various dimensions.

10. We make this assumption to conform with the previous studies being examined, all of which ignore the discount rate. The discount rate, which is an infrequently changed administered rate, may also not be well modeled by the linear VAR framework. An alternative to assuming that the innovation to the discount rate is zero, but which has essentially the same effect, is to treat the discount rate innovation as part of the innovation to the borrowings function. For estimates of the model with a nonzero discount rate innovation, see Bernanke and Mihov [1995] or Bernanke and Mihov [1997] (for Germany); their results are quite consistent with those reported here.

have

(11) $$\mathbf{u} = (\mathbf{I} - \mathbf{G})^{-1}\mathbf{Av},$$

where

$$\mathbf{u}' = [u_{TR} \quad u_{NBR} \quad u_{FFR}] \qquad \mathbf{v}' = [v^d \quad v^s \quad v^b]$$

and

$$(\mathbf{I} - \mathbf{G})^{-1}\mathbf{A} = \begin{bmatrix} -\left(\dfrac{\alpha}{\alpha + \beta}\right)(1 - \phi^d) + 1 & \dfrac{\alpha}{\alpha + \beta} & \left(\dfrac{\alpha}{\alpha + \beta}\right)(1 + \phi^b) \\ \phi^d & 1 & \phi^b \\ \left(\dfrac{1}{\alpha + \beta}\right)(1 - \phi^d) & -\dfrac{1}{\alpha + \beta} & -\left(\dfrac{1}{\alpha + \beta}\right)(1 + \phi^b) \end{bmatrix}.$$

One can also invert the relationship (11) to determine how the monetary policy shock v^s depends on the VAR residuals:

(12) $\quad v^s = -(\phi^d + \phi^b)u_{TR} + (1 + \phi^b)u_{NBR} - (\alpha\phi^d - \beta\phi^b)u_{FFR}.$

The model described by equation (11) has seven unknown parameters (including the variances of the three structural shocks) to be estimated from six covariances; hence it is underidentified by one restriction. However, as was noted earlier, this model nests some previous attempts to measure policy innovations, each of which implies additional parameter restrictions. We consider five alternative identifications of our unrestricted model, corresponding to four indicators of policy proposed in the literature (each of which implies overidentification) and one just-identified variant. These are as follows.

Model FFR (federal funds rate). The Bernanke-Blinder assumption that the Fed targets the federal funds rate corresponds to the parametric assumptions $\phi^d = 1$, $\phi^b = -1$; i.e., the Fed fully offsets shocks to total reserves demand and borrowing demand. From (12) we see that the monetary policy shock implied by these restrictions is $v^s = -(\alpha + \beta)u_{FFR}$; i.e., the policy shock is proportional to the innovation to the federal funds rate, as expected.

Model NBR (nonborrowed reserves). Christiano and Eichenbaum's assumption is that nonborrowed reserves respond only to policy shocks. In our context this assumption implies the restrictions $\phi^d = 0$, $\phi^b = 0$ in (10). With these restrictions the policy shock becomes $v^s = u_{NBR}$.

Model NBR/TR ("orthogonalized" nonborrowed reserves). Strongin's key assumption is that shocks to total reserves are purely demand shocks, which the Fed has no choice in the short run but to accommodate (either through open-market operations or the discount window). His specification also ignores the possibility that the Fed responds to borrowing shocks. Hence the parametric restrictions imposed by Strongin's model are $\alpha = 0$, $\phi^b = 0$. For Strongin's model innovations to monetary policy are given by $v^s = -\phi^d u_{TR} + u_{NBR}$.

Model BR. Several authors have provided evidence for borrowed-reserves targeting by the Fed during certain periods (see, e.g., Cosimano and Sheehan [1994]), implying that the quantity of borrowed reserves is another potential indicator of policy. It is straightforward to see that borrowed-reserves targeting corresponds to the restrictions $\phi^d = 1$, $\phi^b = \alpha/\beta$. The implied policy shock is $v^s = -(1 + \alpha/\beta)(u_{TR} - u_{NBR})$, which is proportional to the negative of the innovation to borrowed reserves.

Model JI ($\alpha = 0$, just-identification). Each of the four models above imposes two restrictions and hence is overidentified by one restriction (recall that the base model is underidentified by one restriction). Tests of these models thus take the form of a test of the overidentifying restriction. An alternative strategy is to estimate a just-identified model and check how well the parameter estimates correspond to the predictions of the alternative models. Strongin makes plausible institutional arguments for his identifying assumption that the demand for total reserves is inelastic in the short run ($\alpha = 0$). Hence we also consider as a separate case the just-identified model that imposes only that restriction.[11]

IV. DATA, ESTIMATION, AND RESULTS

An important practical issue in measuring policy stance is that the preferred indicator of monetary policy may change over time, as operating procedures or other factors change. A useful feature of our approach is that it can accommodate such changes, by allowing for changes in the values of parameters (e.g., those describing the Fed's behavior). In the estimates presented in this

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11. Alternatively, the model could be identified by imposing a "long-run" restriction, e.g., that monetary policy shocks have only price-level effects in the long run; see, e.g., Bernanke and Mihov [forthcoming].

section, we take two alternative approaches to dealing with possible regime shifts. First, we present estimates for both the entire 1965–1996 sample and various subsamples, with sample break dates chosen using a combination of historical and statistical evidence. Second, we estimate a Hamilton [1989]-style regime-switching model, which bases inference about the dates at which Fed behavior changed entirely on the data. As we show, the results from these two approaches are qualitatively consistent.

Because our identifying assumption is that there is no feedback from policy variables to the economy within the period,[12] the length of "the period" is potentially important. For our first approach, with fixed sample break dates, we report results based on monthly and biweekly data (to conserve space, results from the regime-switching model, below, are reported for monthly data only).[13] Estimates using quarterly data generated qualitatively similar conclusions, but it is more difficult to defend the identification assumption of no feedback from policy to the economy at the quarterly frequency.[14]

As we discussed in Section II, our procedure accommodates the inclusion of both policy variables and nonpolicy variables in the VARs. At both frequencies the policy variables we use are total bank reserves, nonborrowed reserves,[15] and the federal funds rate. At the monthly frequency the nonpolicy variables used were real GDP, the GDP deflator, and the Dow-Jones index of spot commodity prices. Real GDP and the GDP deflator were chosen because presumably they are better indicators of broad macroeconomic conditions than are more conventional monthly indicators

12. As Bernanke and Blinder [1992] discuss, there are actually two alternative timing assumptions that can be used for identifying the effects of policy, which may be appropriate under different circumstances: either that policy-makers have contemporaneous information about the nonpolicy variables (implying that the policy variables should be ordered last in the VAR), or that policy-makers know only lagged values of the nonpolicy variables (implying that the policy variables should be ordered first).

13. Weekly data are available prior to the change in reserve accounting procedures in 1984, but subsequently only biweekly data are available. For comparability we report only biweekly results for the whole sample period.

14. However, in related work using only reserves-market data, Geweke and Runkle [1995] find that time aggregation from biweekly to quarterly intervals is not a problem for the identification of monetary policy.

15. We use nonborrowed reserves plus extended credit, in order to eliminate the effects of a bulge of borrowings associated with the Continental Illinois episode in 1984. To induce stationarity, we normalize total bank reserves and nonborrowed reserves by a long (36-month) moving average of total reserves. This normalization is preferable to taking logs because the model is specified in levels. Strongin [1995] normalized by a short moving average of total reserves. However, we found that this procedure creates "jerky" impulse response functions and does not cleanly separate the dynamics of total reserves and nonborrowed reserves.

like industrial production and the CPI. Monthly data for real GDP and the GDP deflator were constructed by state space methods, using a list of monthly interpolator variables and assuming that the interpolation error is describable as an AR(1) process (see Bernanke, Gertler, and Watson [1997] for details).[16] At the biweekly frequency the nonpolicy variables included the *Business Week* production index and the index of spot commodity prices (broader weekly price indices are unavailable).[17]

The index of commodity prices was included as a nonpolicy variable in order to capture additional information available to the Fed about the future course of inflation. As is now well-known, exclusion of the commodity price index tends to lead to the "price puzzle," the finding that monetary tightening leads to a rising rather than falling price level.[18] If the commodity price index is included to capture the Fed's information about future inflation, then a parallel argument suggests putting an indicator of future output movements into the system as well. For this reason, in our initial estimation we included the index of leading indicators (short horizon) in the quarterly and monthly systems. However, unlike the case of the commodity price index, inclusion of the index of leading indicators had little effect on model estimates or implied impulse response functions. For comparability with earlier results, therefore, we excluded that variable when deriving the estimates presented here.

For estimation of the model with fixed break dates, we used a two-step efficient GMM procedure (maximum likelihood esti-

16. James Stock pointed out to us that the moving average interpolation error created by the interpolation procedure could in principle invalidate our identifying assumption, that policy shocks do not feed back to the economy within the period. As a check for robustness, we repeated our monthly estimates using industrial production in place of real GDP and the CPI (less shelter) in place of the GDP deflator. The resulting parameter estimates were virtually identical to those reported here. Because the results using real GDP and the GDP deflator yield impulse response functions that are more easily interpretable and useful for policy-making, we continue to focus on the estimates using the interpolated variables.

17. The nonpolicy variables are measured Saturday to Saturday and the policy variables are measured Wednesday to Wednesday. We use values of the nonpolicy variables corresponding to the week lying in the middle of the two-week reserve accounting period.

18. For further discussion see Sims [1992] and Christiano, Eichenbaum, and Evans [1994a, 1994b]. In particular, the latter show that the price puzzle is largely an artifact of not controlling for oil supply shocks. Sims and Zha [1995] argue that treating commodity prices as predetermined for monetary policy shocks is inappropriate, since these prices may well respond within the period to monetary surprises. As a check for robustness, we reestimated the just-identified model allowing commodity prices to be determined simultaneously with policy innovations; this change did not significantly affect our conclusions.

mates were similar). The first step of the procedure was equation-by-equation OLS estimation of the coefficients of the VAR system.[19] The second step involved matching the second moments implied by the particular theoretical model being estimated to the covariance matrix of the "policy sector" VAR residuals. We performed two types of tests of the various models: (1) tests of overidentifying restrictions based on the minimized value of the sample criterion function (Hansen's J test); and (2) tests of hypotheses on the estimates of the structural parameters.

    Estimates of the model based on monthly data are given in Table I, estimates from biweekly data in Table II. Each table reports parameter estimates, with standard errors in parentheses, for the five models introduced in Section III. Parameter restrictions associated with each model are indicated in boldface. The final two columns of Tables I and II show, for each of the four overidentified models, (1) a p-value corresponding to the test of the single overidentifying restriction (OIR); and (2) a p-value for the two parameter restrictions of the model, conditional on maintaining the just-identified model (and hence assuming $\alpha = 0$). P-values greater than 0.05 are shown in boldface, indicating that the particular model cannot be rejected at the 5 percent level of significance. As indicated above, to allow for possible regime switches, we present estimates for the entire sample period (1965:1–1996:12) and for selected subperiods, including 1965:1–1979:9, 1979:10–1996:12, 1984:2–1996:12, and 1988:9–1996:12.[20] The subperiods were chosen as follows. First, we made a list of candidate subperiods corresponding broadly to periods identified by Federal Reserve insiders and observers as possibly distinct operating regimes (see, e.g., Strongin [1995]). For example, 1979: 10, when Chairman Volcker announced dramatic changes in the operating procedure, is a conventional break date; 1984:2 reflects both the end of the Volcker experiment and the beginning of contemporaneous reserve accounting; and 1988:9 roughly marks

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    19. To determine the number of lags in the VAR, for each sample we began with fifteen lags and kept eliminating the last lag as long as it was statistically insignificant. This procedure led to the use of thirteen lags in the full sample, eleven lags in the 1965–1979 subsample, twelve lags in the 1979–1996 subsample, and seven lags in the 1984–1996 subsample. For the short 1988–1996 sample we also tested lags other than the final one, and settled on using lags 1 to 6, 8, 10, and 11. A similar procedure was followed in the biweekly data, starting with a maximum of 29 lags. Results based on a fixed lag structure of twelve or thirteen lags were very similar to what we report here.
    20. Both the reduced-form VAR and the structural model parameters are reestimated within each subsample.

TABLE I

PARAMETER ESTIMATES FOR ALL MODELS (MONTHLY)

Sample	Model	α	β	ϕ^d	ϕ^b	Tests	
						For OIR	Restrictions under JI model
1965:1–1996:12	FFR	−0.004 (0.001)	0.012 (0.001)	1	−1	**0.119**	0.004
	NBR	0.031 (0.010)	0.014 (0.001)	0	0	0.000	0.000
	NBR/TR	0	0.049 (0.012)	0.828 (0.061)	0	0.033	0.020
	BR	−0.004 (0.001)	0.041 (0.008)	1	α/β	**0.119**	0.000
	JI	0	0.020 (0.006)	0.809 (0.058)	−0.636 (0.274)	—	—
1965:1–1979:9	FFR	−0.005 (0.002)	0.015 (0.003)	1	−1	**0.148**	0.048
	NBR	0.014 (0.004)	0.056 (0.005)	0	0	0.000	0.000
	NBR/TR	0	0.077 (0.012)	0.776 (0.106)	0	**0.078**	0.049
	BR	−0.005 (0.002)	0.067 (0.010)	1	α/β	**0.148**	0.000
	JI	0	0.028 (0.011)	0.749 (0.102)	−0.620 (0.315)	—	—
1979:10–1996:12	FFR	−0.002 (0.001)	0.013 (0.001)	1	−1	0.013	0.000
	NBR	0.029 (0.008)	0.014 (0.001)	0	0	0.001	0.000
	NBR/TR	0	0.036 (0.009)	0.725 (0.076)	0	**0.778**	**0.753**
	BR	−0.002 (0.001)	0.024 (0.004)	1	α/β	0.013	0.001
	JI	0	0.041 (0.021)	0.730 (0.079)	0.040 (0.128)	—	—
1984:2–1996:12	FFR	−0.007 (0.005)	0.005 (0.002)	1	−1	0.041	0.031
	NBR	−0.498 (0.593)	0.005 (0.002)	0	0	0.007	0.000
	NBR/TR	0	0.254 (0.126)	0.812 (0.090)	0	**0.161**	**0.202**
	BR	−0.007 (0.005)	0.117 (0.073)	1	α/β	0.041	0.002
	JI	0	0.043 (0.027)	0.810 (0.078)	−0.402 (0.315)	—	—
1988:9–1996:12	FFR	−0.021 (0.005)	−0.001 (0.002)	1	−1	**0.466**	**0.636**
	NBR	−0.131 (0.029)	−0.005 (0.002)	0		0.048	0.000
	NBR/TR	0	0.141 (0.117)	0.904 (0.035)	0	0.001	0.000
	BR	−0.021 (0.005)	−0.462 (1.374)	1	α/β	**0.466**	0.000
	JI	0	0.002 (0.005)	0.984 (0.033)	−0.980 (0.071)	—	—

The estimates come from a six-variable monthly VAR (see text for explanations). The next-to-the-last column presents p-values from tests of overidentifying restrictions based on the minimized value of the criterion function. The last column gives p-values from tests of the implied restrictions under the just-identified model ($\alpha = 0$). In the last two columns the values in boldface indicate that the restrictions implied by the particular model cannot be rejected at the 5 percent level of significance. The figures in parentheses are standard errors.

TABLE II
PARAMETER ESTIMATES FOR ALL MODELS (BIWEEKLY)

Sample	Model	α	β	ϕ^d	ϕ^b	Tests	
						For OIR	Restrictions under JI model
1967:1–1996:12	FFR	-0.003 (0.001)	0.013 (0.002)	1	-1	**0.185**	**0.110**
	NBR	0.063 (0.018)	0.015 (0.002)	0	0	0.000	0.000
	NBR/TR	0	0.095 (0.022)	0.912 (0.047)	0	**0.147**	**0.117**
	BR	-0.003 (0.001)	0.087 (0.019)	1	α/β	**0.185**	0.001
	JI	0	0.030 (0.016)	0.907 (0.045)	-0.618 (0.394)	—	—
1967:1–1979:9	FFR	-0.005 (0.002)	0.017 (0.003)	1	-1	**0.361**	**0.212**
	NBR	0.012 (0.004)	0.071 (0.006)	0	0	0.000	0.000
	NBR/TR	0	0.132 (0.020)	0.870 (0.081)	0	**0.087**	**0.069**
	BR	-0.005 (0.002)	0.130 (0.019)	1	α/β	**0.361**	0.000
	JI	0	0.034 (0.021)	0.865 (0.082)	-0.727 (0.400)	—	—
1979:10–1996:12	FFR	-0.002 (0.002)	0.013 (0.002)	1	-1	0.021	0.000
	NBR	0.071 (0.029)	0.012 (0.003)	0	0	0.000	0.000
	NBR/TR	0	0.084 (0.029)	0.850 (0.061)	0	**0.944**	**0.940**
	BR	-0.002 (0.002)	0.047 (0.014)	1	α/β	0.021	0.048
	JI	0	0.090 (0.109)	0.850 (0.062)	0.011 (0.143)	—	—
1979:10–1982:10	FFR	-0.002 (0.001)	0.011 (0.003)	1	-1	**0.102**	0.013
	NBR	-0.000 (0.001)	0.061 (0.023)	0	0	0.627	**0.905**
	NBR/TR	0	0.058 (0.022)	0.139 (0.390)	0	0.657	**0.722**
	BR	-0.002 (0.001)	0.047 (0.016)	1	α/β	**0.102**	**0.075**
	JI	0	0.039 (0.031)	0.119 (0.398)	-0.144 (0.405)	—	—

1984:2–1996:12	FFR	−0.003 (0.005)	0.006 (0.003)	1	−1	**0.097**	**0.059**
	NBR	0.502 (0.387)	0.010 (0.002)	0	0	0.003	0.000
	NBR/TR	0	1.333 (2.420)	0.898 (0.063)	0	**0.596**	**0.737**
	BR	−0.003 (0.005)	0.261 (0.191)	1	α/β	**0.097**	**0.215**
	JI	0	0.123 (0.215)	0.894 (0.063)	−0.151 (0.450)	—	—
1988:9–1996:12	FFR	0.007 (0.007)	0.011 (0.002)	1	−1	**0.226**	**0.172**
	NBR	−0.019 (0.030)	−0.073 (0.050)	0	0	0.000	0.000
	NBR/TR	0	0.079 (0.013)	1.037 (0.031)	0	**0.300**	**0.198**
	BR	0.007 (0.007)	0.076 (0.013)	1	α/β	**0.226**	**0.226**
	JI	0	0.027 (0.015)	1.048 (0.026)	−0.556 (0.432)	—	0.019

The estimates come from a five-variable biweekly VAR (see text for explanations). The next-to-the-last column presents p-values from tests of overidentifying restrictions based on the minimized value of the criterion function. The last column gives p-values from tests of the implied restrictions under the just-identified model ($\alpha = 0$). In the last two columns the values in boldface indicate that the restrictions implied by the particular model cannot be rejected at the 5 percent level of significance. The figures in parentheses are standard errors.

the beginning of the Greenspan regime, excluding the stock market crash and its aftermath. We then checked whether instability of the structural parameters could be statistically rejected for adjacent candidate subperiods (on this basis we chose not to report results separately for 1965–1972 and 1972–1979, two subsamples suggested by Strongin). Finally, using the test for structural change with unknown break dates proposed by Andrews [1993], we confirmed that our imposed break dates are not seriously inconsistent with the data (in particular, the Andrews test found evidence for a change in the structural parameters in 1980 and in 1988 or 1989, corresponding closely to our imposed breaks in 1979 and 1988).

A useful starting point for reviewing Tables I and II is the estimates of ϕ^d, the coefficient that describes the Fed's propensity to accommodate reserves demand shocks (see equation (10)). Estimates of this coefficient are obtained under two of the five identifications, namely NBR/TR and JI. In monthly data this coefficient is always estimated to lie between 0.725 and 1.00, with high statistical significance, implying something close to full accommodation of reserves demand shocks ($\phi^d = 1$). This result is inconsistent with the nonborrowed reserves (NBR) model, which assumes that $\phi^d = 0$; consequently, the NBR model is strongly rejected in all sample periods in the monthly data. The biweekly data (Table II) generally show an even greater degree of accommodation of demand shocks (values of ϕ^d between 0.85 and 1.05), and hence rejection of the NBR model. The important exception to this finding occurs in the subperiod 1979:10–1982:10, during which accommodation of reserves demand shocks is estimated to be quite low, around 0.1, and the restrictions of the NBR model are far from being rejected. This last result is quite interesting, since 1979–1982 was the only period in which the Fed indicated publicly that it was using a nonborrowed-reserves targeting procedure. We take this correspondence of our results to the conventional wisdom as support for our approach.[21]

A tendency by the Fed to offset shocks in the reserves market

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21. An alternative, and perhaps sharper test of the NBR model can be obtained under the identification $\phi^d = -\phi^b$, which nests the FFR model ($\phi^d = 1$), the NBR model ($\phi^d = 0$), and combination policies in which the Fed puts weight on both interest-rate smoothing and nonborrowed-reserves smoothing objectives ($0 < \phi^d < 1$). Estimation under this identification generally finds values of ϕ^d much closer to 1 than to 0, especially in the pre-1979 and post-1988 subsamples, consistent with the view that those were periods in which the Fed smoothed interest rates. Estimates for 1979–1982 using weekly data, in contrast, find the value of ϕ^d close to zero, again confirming the relevance of the NBR model to the

is also indicated by the fact that the Fed's response to borrowing shocks, ϕ^b, is in almost all cases estimated to be negative (e.g., the full-sample estimates under the JI model are -0.64 and -0.62 in monthly and biweekly data, respectively). However, in general, the absolute values of the estimates of ϕ^b are smaller, and the standard errors larger, than for ϕ^d. As we will see momentarily, this makes it difficult in some cases to choose the preferred model from among FFR, NBR/TR, and BR, which are distinguished primarily by their predictions for the value of ϕ^b.

Estimates for the slope coefficients α and β are available for all models in the latter case, and for all models except NBR/TR and JI in the former. Although the magnitudes of estimates of these coefficients differ across models, they seem reasonably consistent across subsamples and data frequencies for any given model. Estimates of α are often found to be negative (the wrong sign), although very small in magnitude; this provides some support for the identifying assumption $\alpha = 0$ made by the NBR/TR and JI models. The sign of the estimates of β is almost always as predicted (positive), except for a few models in the most recent subsample. As a check on the reasonableness of the estimated magnitudes of these parameters, note that the liquidity effect (the effect of a 1 percent exogenous increase in nonborrowed reserves on the funds rate) in our encompassing model is $1/(\alpha + \beta)$. So, for example, in biweekly data for the full sample, the FFR model implies a liquidity effect of about 100 basis points. Hamilton [1997, p. 94] notes that this number is "remarkably consistent" with his estimate of the short-run liquidity effect, obtained by different methods. Admittedly, however, other identifications often give smaller liquidity effects, and the size of this effect is not always sharply identified statistically. Estimation of the liquidity effect is discussed further in the next section.

What do the results in Tables I and II indicate about which model of the Fed's operating procedure, and hence which indicator of policy innovations, is to be preferred? The strongest message is that the Fed's procedures appear to have changed over time, and hence no single model is optimal for the 1965–1996 time period. The FFR model is found to do well for the pre-1979 period, as argued by Bernanke and Blinder [1992], and it does exceptionally well for the Greenspan era, post-1988, which most Fed watchers

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early Volcker period. We thank an anonymous referee for suggesting this alternative identification.

would not find surprising. The FFR model also appears to be (marginally) the best choice for the sample period as a whole, based on both the monthly and biweekly results. As noted above, the NBR model does well for the brief period of the Volcker experiment, 1979–1982, but is otherwise strongly rejected.

Evaluation of the borrowed reserves (BR) model is more difficult. It is well-known that operating procedures based on targeting the funds rate and on targeting borrowed reserves are quite similar in practice (see, e.g., Thornton [1988a]), and indeed, the tests of overidentifying restrictions (OIR) give essentially identical results, subperiod by subperiod, for the FFR and BR models. Formally, the models differ only in that the BR model predicts $\phi^b = \alpha/\beta$, while the FFR model predicts $\phi^b = -1$; and as we have noted, ϕ^b is not always well identified in our data. Much stronger differentiation between the two models is found under the just-identified model, however, in which we impose Strongin's assumption of inelastic reserves demand ($\alpha = 0$). Under the JI restrictions the FFR model generally outperforms the BR model for the full sample, the pre-1979 period, and (particularly) in the post-1988 period. However, it is interesting that the BR model does noticeably better than FFR in the sample of biweekly data beginning in 1984. This last result is consistent with the common assertion that borrowed-reserves targeting was part of the transition from the nonborrowed-reserves targeting of the early Volcker era to the federal-funds-rate targeting of the Greenspan period (see Cosimano and Sheehan [1994]). Comparison of the biweekly and monthly results, however, suggests that even in the period beginning in 1984, borrowed-reserves targeting was relevant only at higher frequencies.

What of the NBR/TR model, proposed by Strongin [1995]? This model has the "advantage" that, unlike the other identifications, it treats the degree to which the Fed accommodates reserves demand shocks (ϕ^d) as a free, rather than imposed, parameter. Also, like our JI model, the NBR/TR model assume that $\alpha = 0$. This flexibility is probably the reason why the NBR/TR model has by far the highest p-values for the 1979–1996 subsample, a period that appears to mix several different operating procedures. The NBR/TR model is also not rejected for the 1965–1979 period in monthly data (although it is rejected for the sample as a whole and for the post-1988 subsample), and it is not rejected for any sample or subsample in the biweekly data—a good performance.

Overall, our methodology seems to do a plausible job of

identifying the Fed's operating regime in different periods. There is considerable evidence that this regime has changed over time. It is a strength of our approach that these changes can be identified and (at least potentially) accommodated in the construction of innovations to monetary policy, in contrast to most VAR-based approaches currently in the literature.

In the estimates reported in Tables I and II, we imposed sample break points using prior knowledge of recent monetary history. An alternative approach is to allow the sample breaks to be determined entirely by statistical procedures. To do this, we began by looking for evidence for breaks in the reduced-form parameters of the VAR system. Next, we applied Hamilton's [1989] regime-switching model to the estimation of the structural VAR model of the bank reserves market, focusing particularly on possible switches in the parameters describing Fed behavior.

Conceptually, the appropriate test for stability of the reduced-form VAR system should allow for an arbitrary break point or points, as in Andrews [1993]. However, such a test applied to our six-variable, thirteen-lag system as a whole would no doubt have minimal power, given the large number of estimated parameters. To increase power, and to focus attention on qualitative changes in the dynamics, we tested for breaks in the coefficients on all lags of each variable in each equation; e.g., we tested all thirteen lags of GDP in the nonborrowed reserves equation simultaneously, allowing arbitrary break points.

Since there are six equations in the system, each with six sets of lagged variables, our procedure involved 36 separate tests. We used the LM variant of the test proposed by Andrews; this involves calculating the LM statistic for every possible break point and then comparing the highest value in the sequence against the tabulated critical values. Of the 36 tests conducted, only one was significant at the 5 percent level, and then only marginally so. We did not consider this to be very strong evidence against stability of the reduced-form VAR system, and so for the purposes of this exercise we proceeded under the assumption of no breaks in the reduced-from coefficients of the VAR over the 1965–1996 sample.[22]

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22. We also conducted approximate Andrews-type tests for each complete equation in the VAR, finding no evidence against stability. Stability of the reduced form is also accepted for shorter lag lengths (e.g., three or four lags). Bernanke and Mihov [forthcoming] also fail to reject full-sample stability in a similar VAR system.

Given the residuals from a VAR estimated for the whole sample period, our second step was to apply Hamilton's regime-switching approach to the (just-identified) structural VAR model of the market for bank reserves. We focused on the two parameters describing the Fed's response to shocks in the reserves market, ϕ^d and ϕ^b. We assumed that there are two possible states, each characterized by a different (freely estimated) combination of the two reaction parameters.[23] Note that, although we key regime switches to changes in the reaction parameters, since the model is just-identified, we must allow for the possibility that other structural parameters change when the state changes. Estimates were obtained using the EM algorithm, derived for this approach by Hamilton [1990].

Figure I shows the results of this exercise graphically. The solid line in the figure shows the probability that the operating regime is in the first of the two states at each date (the probabilities are smoothed in the sense that full-sample information is used in inferring the state probabilities at each date). The results are striking: there is extremely strong evidence of there having been precisely two regime switches in the sample period—one in late 1979, the other during 1982. These switches correspond closely to the period of the "Volcker experiment" with nonborrowed-reserves targeting (the conventional beginning and end dates of the experiment, 1979:10 and 1982:10, are indicated by the vertical dashed lines in Figure I). The estimated values of the two reaction parameters are consistent with this interpretation. During the 1979–1982 regime the estimated values are $\phi^d = 0.183$, $\phi^b = -0.216$, suggesting a nonborrowed-reserves targeting regime with just a bit of concern for interest-rate smoothing. Outside of the 1979–1982 window, the parameter estimates ($\phi^d = 0.863$, $\phi^b = -0.778$) suggest just the opposite approach by the Fed, an interest-rate-focused operating regime with minor attention to smoothing reserves or other aggregates. These results (using

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23. This specification is the simplest possible. In a previous version of the paper, we reported results in which we allowed ϕ^d and ϕ^b to switch independently, for a total of four possible states. We have also considered a specification in which the two parameters must switch together but there are three, rather than two, possible states. Both of these variants gave results very similar to what we report here. In particular, using the model with independent switches, we found that the switches occurred at about the same time, suggesting that switches in the two parameters are linked; and estimates of the three-state model yielded two states with similar parameter estimates, suggesting that a two-state model is adequate to capture Fed behavior over this period.

$\phi^d(1) = 0.863$

$\phi^b(1) = -0.778$

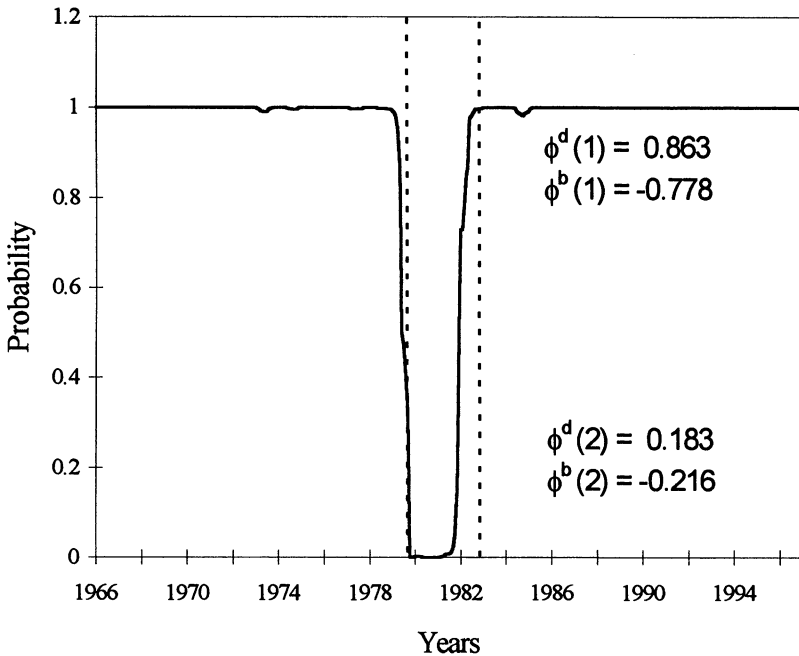$\phi^d(2) = 0.183$

$\phi^b(2) = -0.216$

FIGURE I
Structural Parameter Estimates and Smoothed Probabilities from a
Regime-Switching Model of Federal Reserve Operating Procedures

monthly data) conform closely to the estimates using biweekly
data reported in Table II.

Summarizing the results of this section, we conclude first that
the common practice of using only one policy indicator for the
entire 1965–1996 period in the United States is not justified. In
particular, there is considerable evidence that the Fed did indeed
switch to targeting nonborrowed reserves during the 1979–1982
period, as many have claimed. During the rest of the sample, in
contrast, the Fed was largely accommodating shocks to the
demand for reserves. Treating the federal funds rate as the policy
indicator for this greater portion of the sample is therefore
probably a reasonable approximation, although as we have seen
the Strongin model is also useful, particularly for the post-1979
period.

The comparisons we have drawn in this section thus far
presuppose a choice among the existing simple indicators. How-
ever, as discussed in Section II, our analysis also suggests an

alternative strategy, which is to use the just-identified model to calculate the policy shock in each period which is most consistent with our estimates of the Fed's operating procedure in that period (see equation (12)). This strategy makes it possible to analyze the effects of monetary policy, despite differences or changes in regime, in a unified framework. In addition, this approach can accommodate "hybrid" as well as "pure" operating procedures. In the next section we show that using our framework may substantially affect the inferences one draws about monetary policy.

V. IMPLICATIONS FOR ESTIMATED RESPONSES TO MONETARY POLICY SHOCKS

As the Introduction discussed, our reason for estimating the parameters of the model for bank reserves is not our interest in that market, or in the Fed's operating procedures, per se. Rather, the goal is to isolate relatively "clean" measures of monetary policy shocks. Given these shocks, standard impulse response functions can be used to provide a quantitative measure of the dynamic effects of policy changes on the economy. As we have noted, many recent studies have used this approach; and methods of this type have been used as an input to monetary policy-making both in the United States and other countries.

Figure II shows estimated dynamic responses of output, the price level, and the federal funds rate to a monetary policy shock, as derived from the alternative models we have been considering. For comparability, in each case we consider an expansionary shock with an impact effect on the funds rate of -25 basis points. The left column shows the impulse responses, with 95 percent confidence bands, implied by our just-identified model.[24] The right panel of Figure II shows impulse responses as implied by the four overidentified models (FFR, NBR, NBR/TR, and BR). Standard error bands are omitted from the right-hand panels for legibility.

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24. Conditional on chosen values for the monetary policy shock, the impulse responses for the JI model depend only on the reduced-form VAR parameters and the estimated value of β. They do not depend on the parameters describing the Fed's operating procedure. (Estimates of the latter parameters are, of course, needed to calculate policy shocks from measured VAR residuals; see equation (12).) Since we cannot reject stability of the reduced-form coefficients for the 1965–1996 sample, and there is little evidence of a shift in β, we consider it reasonable to base the impulse responses in Figure II on the full-sample estimates of the JI model (Table I). Similar results are obtained if we use parameter estimates from either regime of the regime-switching estimation (see Figure I). Bernanke and Mihov [1995, Figure 9] show impulse responses for the JI and alternative models for the key subsamples.
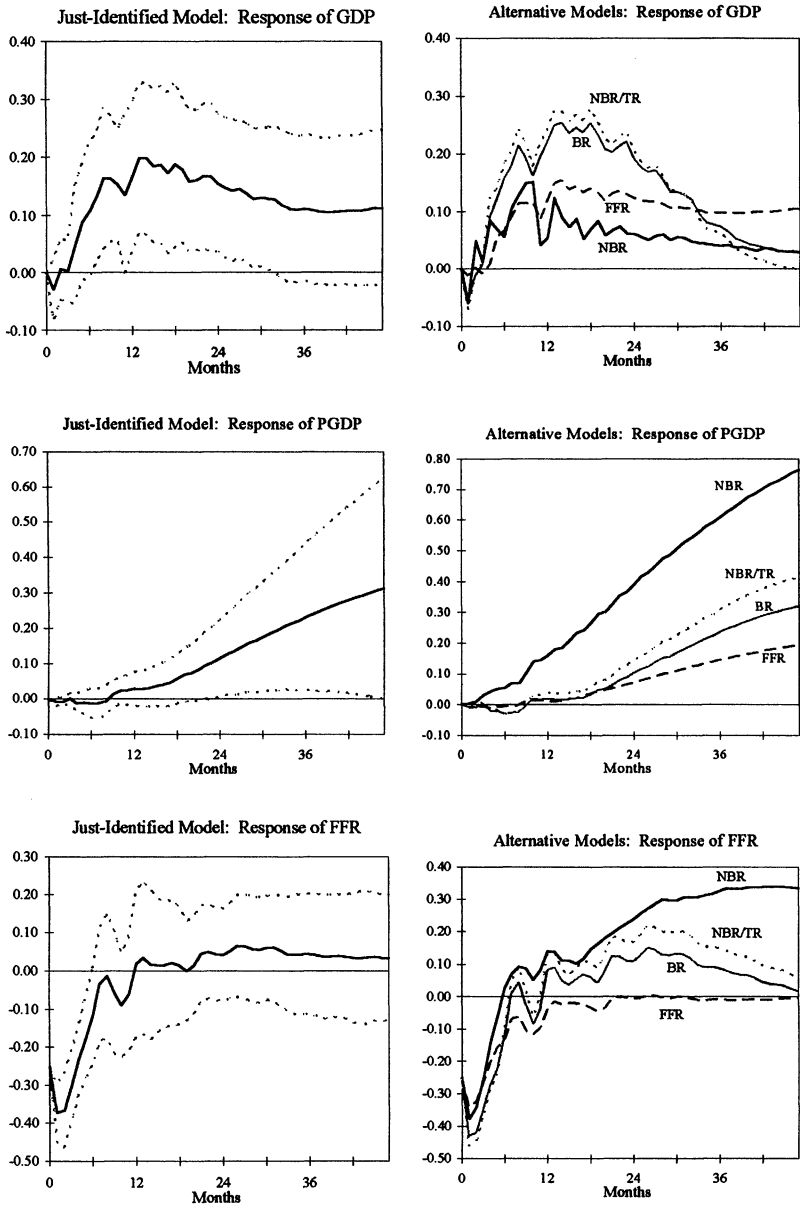
Just-Identified Model: Response of GDP

Alternative Models: Response of GDP

Just-Identified Model: Response of PGDP

Alternative Models: Response of PGDP

Just-Identified Model: Response of FFR

Alternative Models: Response of FFR

FIGURE II

Responses of Output, Prices, and the Federal Funds Rate to Monetary Policy
Shock in Alternative Models (1965:1–1996:12)

Qualitatively, the results from all five identifications are reasonable, in the sense of conforming to the predictions of standard models and conventional wisdom. In each case, an expansionary monetary policy shock increases output relatively rapidly (the peak effect is typically at twelve–eighteen months), and raises the price level more slowly (with relatively little impact in the first year) but more persistently. However, quantitatively, the results differ noticeably according to the method of identifying policy shocks. For example, under the NBR/TR identification the cumulative response of output to a policy shock of a given size is estimated to be more than twice as large as found under the NBR identification. Similarly, at four years the response of prices under the NBR identification is found to be four times greater than under the FFR identification. Further, these alternative measured responses often differ significantly (in both the economic and statistical senses) from those implied by the benchmark JI model. Similar or greater divergences are found when the responses implied by the various models are compared for shorter sample periods (omitted here to conserve space). Overall, these differences are certainly large enough to have important effects on the inferences one draws about the effects of monetary policy; and thus, they underscore the need to choose a model of the Fed's operating procedure that is as nearly correct as possible.

How is it that alternative indicators of monetary policy innovations can have such different implications for the calculated impulse responses? Our analysis provides a simple framework for understanding these differences in results. Let z_{FFR}, z_{NBR}, $z_{NBR/TR}$, and z_{BR} be the policy shocks implied by the four overidentified models, with signs chosen so that a positive innovation corresponds to an expansionary policy shock. Suppose that in fact our just-identified model (with $\alpha = 0$) is true. Then, using equation (11), we can write the putative measures of monetary policy shocks implied by the overidentified models in terms of the "true" structural shocks, as follows:

$$(13) \qquad z_{FFR} = -\beta u_{FF} = -(1 - \phi^d)v^d + v^s + (1 + \phi^b)v^b$$

$$(14) \qquad\qquad z_{NBR} = u_{NBR} = \phi^d v^d + v^s + \phi^b v^b$$

$$(15) \qquad\qquad z_{NBR/TR} = u_{NBR} - \phi^d u_{TR} = v^s + \phi^b v^b$$

$$(16) \quad z_{BR} = -u_{BR} = -(u_{TR} - u_{NBR}) = -(1 - \phi^d)v^d + v^s + \phi^b v^b.$$

Note that the alternative policy-shock measures of (13)–(16) are

the same (or in the case of z_{FFR}, proportional to) the policy innovations that would be constructed from more conventional identifications based on the Choleski decomposition (see, e.g., Bernanke and Blinder [1992], Christiano, Eichenbaum, and Evans [1994b], or Strongin [1995]).

From (13)–(16) we see that—if the JI model of this paper is correct—each of the putative policy indicators z is contaminated, in the sense of placing some weight on reserves demand or borrowings shocks. The degree of contamination depends, of course, on the values of the parameters. For example, if ϕ^d is close to one (as we have found to be the case, except during the early Volcker period), then the nonborrowed reserves indicator z_{NBR} puts considerably more weight on reserves demand shocks relative to policy shocks than does the federal funds indicator z_{FFR} (compare equations (13) and (14)). To the extent that z_{NBR} reflects shocks to reserves demand rather than to policy, the associated impulse responses will not be reliable guides to the effects of policy innovations. Similarly, the Strongin measure $z_{NBR/TR}$ will be a good measure of monetary policy shocks only to the degree that (or during periods in which) the Fed's response to borrowing shocks, ϕ^b, is small (see equation (15)). Equations (13)–(16) also imply that the variances of the structural shocks play an important role, e.g., if the variance of the borrowings shock v^b is sufficiently small, the Strongin measure may be a robust indicator of policy shocks even if ϕ^b is not close to zero. Of course, conditional on our estimated model, the least contaminated impulse responses are those shown in the left panel of Figure II.

As another application of our approach to the analysis of empirical impulse responses, consider the recent debate on the "vanishing liquidity effect." A number of economists, using nonborrowed reserves as an indicator of policy, have found that the liquidity effect—the impact of a given increase in nonborrowed reserves on the interest rate—has become much smaller or even disappeared since 1982 [Pagan and Robertson 1995; Christiano 1995]. If correct, this finding has important practical implications for policy-making. However, our approach suggests that this result is largely due to the bias associated with using nonborrowed reserves as the policy indicator.

To understand this bias in more detail, note first that, according to the model developed in the present paper, the magnitude of the liquidity effect is given by the (3,2)-element in the matrix $(\mathbf{I} - \mathbf{G})^{-1}\mathbf{A}$, which is $-1/(\alpha + \beta)$ (see equation (11)).

However, if nonborrowed reserves are used as the policy indicator, and the interest rate (say, the federal funds rate) follows immediately in the ordering, then the liquidity effect will be measured as the projection (regression coefficient) of the funds rate on nonborrowed reserves. Calculating this projection, we find that

(17)   Estimated liquidity effect

$$= -\frac{1}{\alpha + \beta}\left[1 + \frac{\phi^b\sigma_b^2 - \phi^d\sigma_d^2}{(\phi^d)^2\sigma_d^2 + \sigma_s^2 + (\phi^b)^2\sigma_b^2}\right],$$

where the σ_i^2 are the variances of the structural shocks.

If $\phi^d = \phi^b = 0$, so that the Fed is targeting nonborrowed reserves, then the bias term (the second term in the brackets in equation (17)) is zero. However, if $\phi^d > 0$ and $\phi^b < 0$, as our estimates nearly always imply, the bias term is negative, i.e., the magnitude of the liquidity effect is understated by using the nonborrowed reserves indicator. Indeed, using the parameter estimates for the just-identified model for 1984–1996 (Table I), we evaluate the bias term to be -1.035! Hence, if the true liquidity effect for that period is of reasonable magnitude and the correct sign, the estimated liquidity effect for the 1984–1996 period using the nonborrowed reserves indicator will be small and of the wrong sign. We note that 1984–1996 is the only period we have examined for which the calculated bias exceeds one in absolute value (e.g., the bias for the full sample is -0.777; for 1988–1996 it is -0.904). Thus, although the magnitude of the liquidity effect is always seriously understated by the NBR model (assuming that the JI model is true), only in the 1984–1996 period is its sign actually reversed. We conclude that the "vanishing liquidity effect" may well be the result of using a biased indicator of monetary policy, rather than of a change in the economy.

The empirical analyses of this section are meant only to be illustrative. Nevertheless, we believe that they demonstrate the potential of our method to clarify important debates about the quantitative effects of changes in monetary policy.

VI. A MEASURE OF THE OVERALL STANCE OF MONETARY POLICY

Our focus thus far has been on modeling innovations to the stance of monetary policy, as opposed to the anticipated or endogenous part of policy (the "policy rule"). As we have discussed, the advantage of studying shocks to policy is that it allows us to

gauge (at least roughly) the effects of monetary policy on the economy, with minimal identifying assumptions. In contrast, empirical analysis of the effects of different monetary rules on the economy is much more difficult; such an analysis requires either observations on a large number of monetary regimes, or else a structural model identified by strong prior restrictions.

Nevertheless, it would be interesting to have an indicator of monetary policy stance that includes the endogenous as well as the exogenous (unforecastable) component of policy. Such an indicator might be useful in characterizing the overall behavior of the Fed—e.g., the degree to which it accommodates various types of shocks—and in providing a general measure of current monetary conditions. Indeed, central banks in a number of countries currently use "monetary conditions indices," intended to provide assessments of overall tightness or ease, in their day-to-day policy-making (see, e.g., Freedman [1994]).

It is not difficult to devise a monetary conditions index, or measure of overall policy stance, consistent with the framework of this paper. For example, using the framework and notation of Section II, consider the vector of variables $\mathbf{A}^{-1}(\mathbf{I} - \mathbf{G})\mathbf{P}$. This vector, which is observable given estimates of the structural VAR system, is a full-rank linear combination of the policy indicators $\mathbf{P}$, with the property that the orthogonalized VAR innovations of its elements correspond to the structural disturbances $\mathbf{v}$. In particular, one element of this vector, call it p, has the property that its VAR innovations correspond to the monetary policy shocks derived by our approach.

In analogy to the scalar case, in which there is a single observable variable (e.g., the funds rate) whose innovations correspond to the policy shock, one might consider using p as an overall measure of policy. Indeed, under the FFR model's restrictions, $\phi^d = 1$ and $\phi^b = -1$, p equals the funds rate; similarly, under the NBR model's restrictions, p equals nonborrowed reserves, etc. Bernanke and Mihov [1995] show that a measure constructed in this way correlates well (in the full sample and in subsamples) with other candidate indicators of policy, such as the Boschen-Mills [1991] index discussed in the Introduction.

However, as a measure of overall policy stance, p has some shortcomings: first, this indicator is not even approximately continuous over changes in regime (e.g., the funds rate and nonborrowed reserves growth are not in comparable units, so that a switch, say, from targeting the former to targeting the latter

would show up as a discontinuity in the indicator). Second, this measure does not provide a natural metric for thinking about whether policy at a given time is "tight" or "easy" (a similar problem affects simple indicators like the level of the funds rate of the growth rate of nominal nonborrowed reserves). However, we have found that a simple transformation of this variable seems to correct both problems. Analogous to the normalization applied to the reserves aggregates in the estimation, to construct a final total policy measure we normalize p at each date by subtracting from it a 36-month moving average of its own past values. This has the effects of greatly moderating the incommensurable units problem, as well as defining zero as the benchmark for "normal" monetary policy (normal, at least, in terms of recent experience).

Historical values for our suggested measure are shown in Figure III.[25] Also shown in the figure, for comparison, are the two narrative-based measures of monetary policy, the Romer-Romer [1989] dates and the Boschen–Mills [1991] index (scaled to have the same mean and variance as our proposed measure). Examination of Figure III suggests that our measure conforms well with the Boschen-Mills index (the monthly correlation is 0.71), as well as with other historical accounts of U. S. monetary policy. However, contractionary turns in our indicator appear to lead rather than to coincide with the Romer dates; by our measure, Romer dates look more like points of maximum tightness in monetary policy, rather than points at which policy changed from expansionary to contractionary.

Various exercises can be conducted using this indicator, in conjunction with the basic VAR estimated in this paper. For example, an impulse response analysis can be used to characterize the dynamic responses of monetary policy to various types of macroeconomic shocks, as identified in the nonpolicy block. We leave this and other analyses using our measure of monetary conditions to future research.

VII. CONCLUSION

We have used a "semi-structural VAR" approach to evaluate and develop measures of monetary policy based on reserve market indicators. A principal conclusion is that no simple measure of

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25. We use the parameter estimates from the switching-regime estimation of Section IV, weighted at each date by the probability of each regime, in constructing this series.
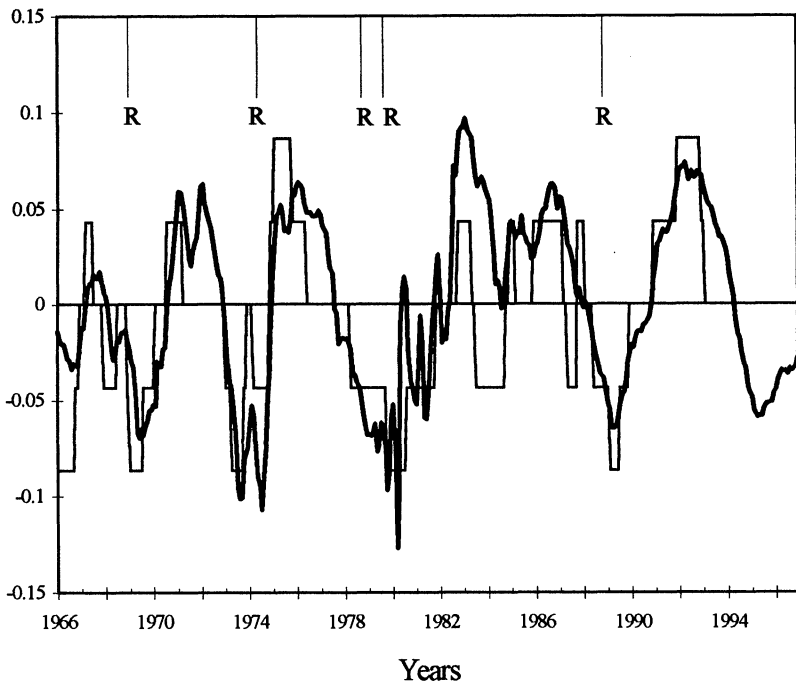
FIGURE III
An Indicator of Overall Monetary Policy Stance

policy is appropriate for the entire 1965–1996 period; changes in operating procedure, such as those that occurred during the 1979–1982 Volcker experiment, imply changes in the preferred indicator. For practitioners looking for a simple indicator of policy stance, our results suggest that using the federal funds rate prior to 1979; nonborrowed reserves from 1979 to 1982; and either the funds rate or Strongin's measure in the more recent period, will give reasonable results. However, a more general and only slightly more complicated alternative is to base the policy measure on an estimated model of the market for bank reserves, along the lines of our just-identified model.[26] The latter approach has the advan-

26. A RATS procedure that estimates the model, constructs the resulting policy indicators, and calculates impulse response functions (with standard errors) for arbitrary sets of nonpolicy variables is obtainable from the authors.

tage of being able to incorporate the effects of possible changes in reserve-market structure and in the Fed's operating procedures. Unlike the simpler indicators, our method can also be generalized to other countries or periods. Finally, associated (and consistent) with our approach is a measure of overall monetary conditions, which we believe could prove useful in both the analysis and conduct of monetary policy.

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