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Evaluating Monetary Policy Options

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We present a procedure for evaluating *ex ante* the effects of alternative paths of a monetary policy tool (the federal funds rate in our illustrations) on output and the price level within a variant of a widely used vector autoregressive model of the U.S. economy. This exercise is a supplement to, or even an alternative to, analysis that relies on a particular structural model. Illustrations of the method are provided by evaluating the effects of changes in the funds rate target. Additionally, the Taylor rule is used to generate target funds rates for different target inflation rates, and the effects of these are evaluated.

1. Introduction

One of the critical elements in the formulation of monetary policy is the evaluation of the effects of alternative paths of the policy instrument on the macroeconomy. For example, in Federal Open Market Committee (FOMC) meetings, estimates of the effects of alternative paths of the federal funds rate are presented to policymakers as an input into the policy process; for a discussion, see Meulendyke (1998). The effects of the alternative paths are evaluated within the context of a structural model of the economy; the latest version of the structural model used at the Board of Governors is described in Brayton et al. (1997).

In this article, we present a procedure for evaluating *ex ante* the effects of alternative paths of a monetary policy tool (the federal funds rate in our illustrations) on output and the price level. We demonstrate this procedure employing a variant of a widely used vector autoregressive (VAR) model of the U.S. economy. This exercise can be viewed as a supplement to, or even an alternative to, analysis that relies on a particular structural model. Given the lack of general agreement on the appropriate structural model, evaluation of the effects of changes in the policy instrument within a variety of different types of models is appropriate.

The discussion of the proposed procedure is in the spirit of recent work by Leeper and Sims (1994), who, following earlier work by Sims (1982, 1987), distinguish between normal policymaking and regime changes. For purposes of illustration, we employ a VAR model comprised of the same variables used by Christiano, Eichenbaum, and Evans (hereafter CEE) (1994, 1996) and Bernanke and Mihov (hereafter BM) (1998). We show how to evaluate and compare the current policy path with normal policy alternatives, such as typically sized changes in the federal funds target. We stress that this model is used only for illustrative purposes. Our methodology applies to any generic structural model and thus can easily

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incorporate alternative models and estimation techniques. For instance, the methodology is easily extended to alternative schemes to identify structural shocks such as those proposed by Bernanke (1986) or Blanchard and Quah (1989), including the adoption of prior information into the estimation. Finally, as argued by Sims (1987) and Cooley, LeRoy, and Raymon (1984), the analysis of normal policymaking avoids the difficulties of the Lucas critique.

In section 2, we provide a brief discussion of the VAR model. In section 3, we present a discussion of the econometric technique. In section 4, we present and discuss results that compare the no-change policy with alternatives in which the funds rate target is altered. We conclude in section 5.

2. The VAR Model

The model of CEE and BM comprises output (*Y*), the price level (*P*), a commodity price index (*CP*), and three reserve market variables—total reserves (*TR*), nonborrowed reserves (*NBR*), and the federal funds rate (*FFR*).[1] The analysis uses quarterly data for the period 1959:1–1999:4. Estimation begins in 1961:2 and ends at different points, depending on the policy experiment considered. Eight quarterly lags are employed, and log levels of all variables except *FFR* are used.[2]

In performing the policy experiments, it is assumed that *FFR* is the policy variable. Monetary policy shocks, following CEE (1994, 1996) and Strongin (1995), are identified using a Choleski decomposition with the following ordering: *Y, P, CP, TR, FFR,* and *NBR*. Following Strongin (1995) and BM (1998), we assume that, because the Federal Reserve accommodated the demand for *TR* over much of the sample, shocks to *TR* reflect reserve demand shocks. Ordering *TR* before *FFR* thus purges shocks to *FFR* of any effect of reserve demand shocks. The decomposition implies that monetary policy shocks affect *Y, P, CP,* and *TR* only with a lag but affect *NBR* contemporaneously. It also assumes that monetary policymakers respond in

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[1] Inclusion of the major reserve market variables allows a detailed consideration of how monetary policy actions are implemented. *CP* is included to eliminate the price puzzle often found in VAR models that excludes information about future inflation.

[2] All data (with one exception noted in footnote 16) are from the DRI Basic Economics database, and the database name is enclosed in parentheses after the variable description. *Y* is measured by real *gdp* (*gdpq*, chain-weighted real *gdp*), while *P* is measured by the chain-weighted price index for *gdp* (*gdpdfc*). *CP* is the Commodity Research Bureau's spot market index for all commodities (*psccom*). Both *TR* (*fmrra*) and *NBR* (*fmrnbc*) are adjusted for reserve requirement changes. *NBR* includes extended credit in order to avoid the distortions created by the Continental Illinois crisis of 1984. Recent behavior of the reserves series has been substantially affected by the widespread adoption of retail sweep accounts in which funds from demand deposit accounts are swept into money market deposit accounts, thereby lowering required reserves. To account for this, we estimate two versions of the model. In the first, we add a dummy, which is zero through 1994:1 and one thereafter, corresponding to actual declines in both the reserve aggregates. In the second, we add a dummy variable that is zero through 1992:4 and one thereafter, corresponding to the fall in the growth rate of *TR*. Inclusion of these dummy variables in the model has only a modest effect on the results for *Y* and *P* but does have a larger effect on the other conditioning variables. The results in Figures 2–4 present the results for models including the first dummy. Results are similar for models including the second dummy. The level of *FFR* (*fyff*) is also included in the model.

the current period to shocks to *Y*, *P*, *CP*, and *TR* but respond only with a lag to movements in *NBR*.[3]

Figure 1 presents impulse response functions for the model estimated over 1961:2–1999:4 along with associated 1-SE confidence intervals for a 1-SD positive shock to *FFR*. The patterns of effects are similar to those reported in the literature and are generally consistent with typical views of the operation of monetary policy in an economy with some rigidities. The only troublesome aspect of the results, which also appears in the recent studies of CEE (1994, 1996), BM (1998), and Leeper and Zha (2001), is the puzzling, long-lived negative effect of a transitory shock to *FFR* on *P*, which deserves further investigation.[4] Since we focus on illustrating how to implement our procedure, conditional on a widely used specification, we leave pursuit of model refinements to future research.

3. Methodology

In a precursor to the current analysis, Fackler and Rogers (1995) demonstrated, in the context of a structural VAR, how to use counterfactual analysis to evaluate policy alternatives, terminology also used by Christiano (1998). For present purposes, we adopt a more intuitive terminology used by Sims (1982) and, more recently, Leeper and Sims (1994), who refer to normal policymaking. In a recent article, Leeper and Zha (2001) refer to modest policy interventions rather than normal policymaking. These papers are compared with ours in section 4.

Consider a policy feedback equation that might be embedded in a VAR such as $f = \alpha y + \epsilon$, where f is the proximate objective of policy (the federal funds rate in our exercise), y is a vector of lagged endogenous variables, α is an appropriately dimensioned vector of coefficients, and ϵ is a random structural shock orthogonal to the other shocks in the model.[5] Normal policymaking is an assessment of alternative ϵ paths. In contrast, regime shifts are represented

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[3] The assumption that monetary policy alters *Y* and *P* only with a lag is not controversial. The placement of *CP* before *FFR* reflects a desire to allow the monetary authority to respond contemporaneously to a variable that contains information about future inflation. However, the assumption that monetary policy affects an auction market variable like *CP* only with a lag is more controversial (McCarthy 1995). McCarthy (1995) and Rudebusch (1998) have also questioned the assumption that the Federal Reserve responds to current period movements in *Y* and *P*. They contend the Federal Reserve is likely to have only noisy contemporaneous information about these variables. They further point out that using revised data for *Y* and *P* may have nontrivial effects on the estimates of both structural policy shocks and impulse response functions, depending on the nature of any revisions to the initial estimates. Sims (1998), however, questions the quantitative significance of this particular criticism. Croushore and Evans (1999) estimated CEE-type VARs using real-time and revised data over 1960–1983 and 1968–1998. They found a high correlation between monetary policy shock measures from VARs estimated using real-time and revised data and similar impulse response functions as well.

Although the Strongin-type identification scheme used here has some unappealing as well as appealing features, it is used since it is a well-understood and widely employed method of identifying monetary policy shocks.

[4] In the context of assessing the robustness of alternative identifying restrictions, Faust (1998) shows how to impose long-run restrictions such that the persistent fall in prices is eliminated. The strength of his approach is that it provides an evaluation of whether such long-run restrictions are consistent with available evidence. The example he highlights is whether a given model specification, including various restrictions on impulse responses, is consistent with an empirical regularity that monetary policy shocks make only a small contribution to the variance decomposition of output.

[5] The model in this article is identified using the Choleski decomposition so that the contemporaneous structural model is recursive. In this sense, ϵ is a structural shock. As noted earlier, our technique may also be applied to structural VARs such as the Bernanke (1986) or Blanchard and Quah (1989) approaches, so that referring to ϵ as structural in the current case is suggestive of broader applications. Note that, with these alternative identifying techniques, the vector y in the policy feedback equation can include contemporaneous endogenous variables.

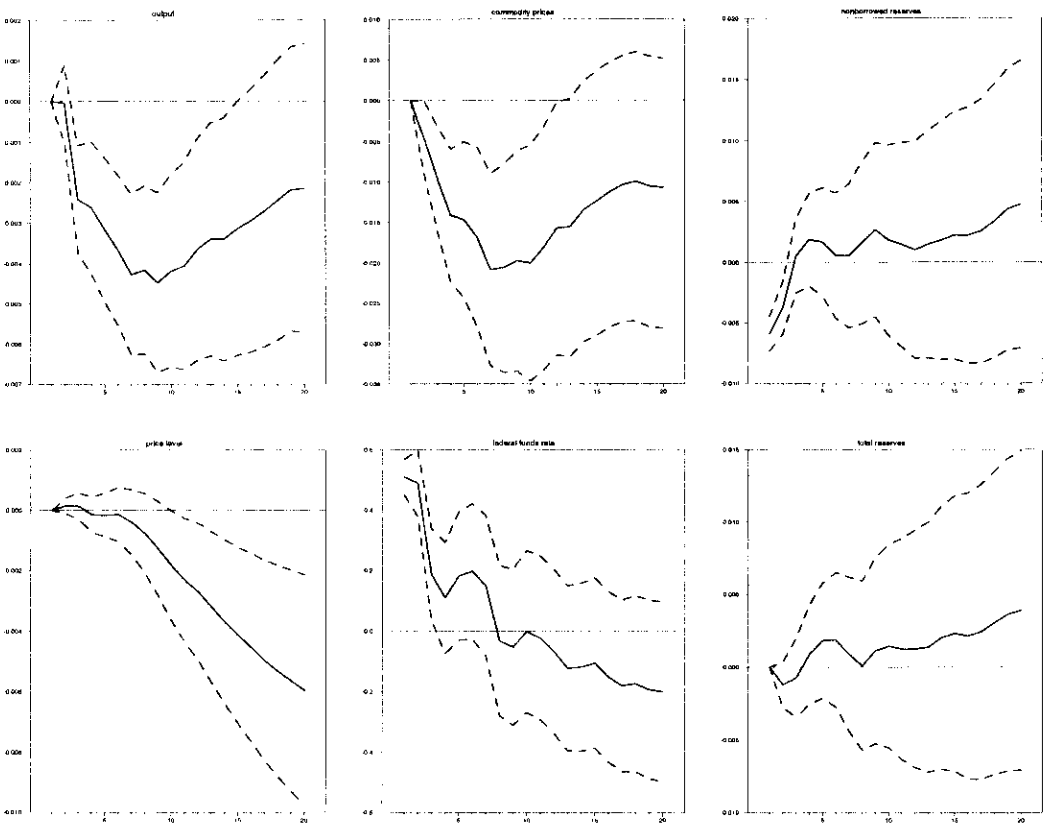

Figure 1. Impulse Response Functions; Sample: 1962:2–1999:4

by changes in one or more of the coefficients of α; shifting to an interest rate peg would be one example.[6]

Our reading of the policy literature, along with assessments in the financial press, suggests that most policy actions represent normal policymaking. Agents are likely aware of continuing debates about optimal policy both inside and outside the monetary authority. While these debates, for purposes of emphasis and clarity, are often presented in terms of regime shifts, few shifts in policy regime seem to occur in practice. Agents may even discount announcements of regime shifts until the authority has pursued the new regime long enough to convince them that a shift has indeed occurred.

Suppose the policymaker wants to evaluate the prospective impact on the economy of lowering the funds rate one-quarter percentage point below the current setting. Using the funds rate equation of the VAR, $f = \alpha y + \epsilon$, in normal policymaking, as suggested by Leeper and Sims (1994, p. 91), "... one would solve for ϵ_f sequences that make the time path of interest rates behave as desired. Because the model implies that there are many potential stochastic influences on interest rates, this

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[6] Sims (1982, 1987) and Cooley, LeRoy, and Raymon (1984) argue that policy interventions implemented by means of altering the ϵ path are not subject to the Lucas critique. They make a further, and stronger, argument: that if agents view the elements of the vector α as random variables rather than literally as parameters, then, in principle, policy regime shifts can be evaluated by optimizing agents in a rational expectations setting as well. In practice, such evaluation would require, among other things, specification of deep parameters such as technology and preference parameters. We will provide some casual evidence on the ϵ path relative to the endogenous component, αy.

kind of projection is generally quite different from simply forecasting conditional on a given time path of the interest rate." As will be derived in Equation 3, the technical expression for the moving average representation of the model in period $t + j$ is

$$y_{t+j} = \sum_{s=0}^{j-1} D_s \epsilon_{t+j-s} + \sum_{s=j}^{\infty} D_s \epsilon_{t+j-s},$$

where D_s is the moving average coefficient matrix associated with the structural shocks in the ϵ vector, the second term on the right-hand side is the dynamic forecast or base projection, and the first term on the right-hand side is the j-period-ahead forecast error. With n variables in the VAR, for Equation i (the policy equation, say), this forecast error is

$$\sum_{s=0}^{j-1} \sum_{h=1}^{n} D_{s,ih} \epsilon_{h,t+j-s}.$$

A conditional forecast, such as a particular interest rate path for several quarters, can be attained in a wide variety of ways by judicious selection of the elements of the ϵ_{t+j-s}, $s = 0, \ldots, j-1$, vectors; in general, there are multiple constraints for which the target path obtains.[7] As noted in the Leeper–Sims quote above, choosing from among these constraints is generally different from selecting the ϵ path as described below.

Our description of normal policymaking begins with the historical decomposition (HD), which quantifies, given the identification of a model, the period-by-period relative importance of the various structural shocks. The HD is derived from a structural model,[8]

$$y_t = A_0 y_t + A_1 y_{t-1} + \ldots + A_p y_{t-p} + \epsilon_t. \tag{1}$$

In Equation 1, the A_i represent the structural coefficients and the ϵ_t are the structural shocks. The elements of ϵ_t are assumed to be mutually orthogonal. Let $e_t = (I - A_0)^{-1}\epsilon_t$ represent the reduced form shocks and Π_i the reduced-form coefficient matrices. Define $\Pi(L) = (I - \Pi_1 L - \ldots - \Pi_p L^p)$. The moving average matrix is given by $C(L) = [\Pi(L)]^{-1}$, with $C_0 = I$. The moving average representation (MAR) of Equation 1 in terms of structural shocks is

$$y_t = \sum_{s=0}^{\infty} D_s \epsilon_{t-s}, \tag{2}$$

where $\epsilon_t = (I - A_0)e_t$ and $D_s = C_s(I - A_0)^{-1}$. For a particular period $t + j$, Equation 2 may be written as

$$y_{t+j} = \sum_{s=0}^{j-1} D_s \epsilon_{t+j-s} + \sum_{s=j}^{\infty} D_s \epsilon_{t+j-s}, \tag{3}$$

which represents the HD.

Equation 3 shows an in-sample accounting identity for a model estimated through period $t + j$. Specifically, the actual data is the sum of two terms. The second term on the right-hand

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[7] For example, a recent version of the RATS manual notes that multiperiod forecasts conditional on future values of endogenous variables depend on "... innovations in *all* variables ..." with there being "... many ways to achieve the particular value. We have only a single constraint on a linear combination of [the] variables" (Doan 1992, p. 8-26).

[8] We stress again that we start with a generic structural model. The discussion in the remainder of this section applies to this general model, not just the particular model used here for illustrative purposes.

side of Equation 3 is the expectation of y_{t+j} given information available at time t, that is, the base projection. The first term on the right-hand side shows the difference between the actual series and the base projection due to the structural innovations in the variables subsequent to period t. This term shows that the gap between an actual series and its base projection is the sum of the (weighted) contributions of the structural innovations to the individual series in the analysis.

An immediate implication of this identity is that we can distinguish between the endogenous and exogenous components of policy. Consider again Equation 3. Using the estimated parameters and residuals, suppose we constrain to zero the structural shocks in the policy equation (the *FFR* equation in our analysis). The accounting identity implies that the constructed (i.e., the counterfactual) path for y is the path the economy would have followed had all shocks except for the exogenous policy shock taken on their actual values; that is, the constructed path includes only the endogenous responses of the policy variable incorporated in the estimated feedback equation. The difference between the actual data and this constructed path represents the impact of the actual, exogenous component of policy. Note that, if the values of the elements of the ϵ path are large relative to the endogenous component of the policy equation, perhaps due to a regime shift, agents may begin to revise their expectations of the policy feedback rule, raising the usual problems associated with the Lucas critique. We will offer two types of casual evidence aimed at exploring whether the elements of the ϵ path associated with a proposed policy alternative raise quantitatively important Lucas critique issues. First, we will investigate the ratio of the exogenous policy component to the value of *FFR*. Second, in the context of a relatively large number of trials using a bootstrapping technique, we will investigate whether the extreme values of the exogenous component lie outside the estimated residuals using the actual data. Note that, if the exogenous policy interventions are small, then we will expect that evaluation of alternative policies like "raise the *FFR* target 25 basis points" to have relatively modest effects on the economy.[9]

Our primary focus is on *ex ante* evaluation of policy alternatives, which uses the accounting identity in the following way. We compute the exogenous component of policy—the ϵ path—required to achieve the policy objective, a computation that presumes the policymaker takes the endogenous component of policy into account; that is, the ϵ path is the size of the policy intervention which, when added to the endogenous response of the policy variable to the economy, achieves the policy objective. When the ϵ path for the policy instrument is combined with structural shocks to the other equations, we compute the path the economy will follow if the values of the policy variable implicit in the ϵ path are implemented. We refer to this as the fundamental property of normal policymaking. Also note that, when the elements of the ϵ path are small relative to the endogenous component, as should be the case with normal policymaking, agents are unlikely to benefit from reassessing the systematic policy rule. This is the empirical analog to the arguments by Sims (1982, 1987) and Cooley, LeRoy, and Ramon (1984) that, with normal policymaking, the Lucas critique is unlikely to be an issue.

We use the fundamental property in out-of-sample policy analysis in the following way. Suppose we want to learn at time t the implications of a particular path for the policy variable T periods into the future; for example, suppose we want the impact of a 25 basis point rise in *FFR*. Assume for a moment that shocks to the other equations are known over the forecast

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[9] See Sims and Zha (1995), Bernanke, Gertler, and Watson (1997), and Leeper and Zha (2001) for additional discussion and analysis of the distinction between endogenous and exogenous policy components.

horizon. Using the coefficients estimated through period t, Equation 3 shows the decomposition for a particular period, $t + j$, in terms of the base projections conditional on information at time t and the contributions of nonpolicy shocks subsequent to t. Consider Equation 3 for $j = 1$:

$$y_{t+1} = D_0\epsilon_{t+1} + \sum_{s=1}^{\infty} D_s\epsilon_{t+1-s} = D_0\epsilon_{t+1} + BP_t.$$

Note that the ith equation in this system, in our example representing the *FFR* equation, is

$$y_{i,t+1} = d_{0,ii}\epsilon_{i,t+1} + \sum_{j\neq i} d_{0,ij}\epsilon_{j,t+1} + BP_{1,i,t},$$

where $BP_{k,i,t}$ is the k-period-ahead base projection for the ith equation at time t and where $d_{k,ij}$ is the (i, j) element of matrix D_k. To find the shock to this equation that will produce a target value for *FFR*, denoted by $y^*_{i,t+1}$, solve the following equation for $\hat{\epsilon}_{i,t+1}$:

$$y^*_{i,t+1} = d_{0,ii}\hat{\epsilon}_{i,t+1} + \sum_{j\neq i} d_{0,ij}\epsilon_{j,t+1} + BP_{1,i,t},$$

the solution for which is

$$\hat{\epsilon}_{i,t+1} = (d_{0,ii})^{-1}\left[y^*_{i,t+1} - BP_{1,i,t} - \sum_{j\neq i} d_{0,ij}\epsilon_{j,t+1}\right].^{10} \tag{4}$$

Proceeding in a similar manner, the structural residual needed to achieve a particular value for $y_{i,t+2}$, denoted by $y^*_{i,t+2}$, is

$$\hat{\epsilon}_{i,t+2} = (d_{0,ii})^{-1}\left[y^*_{i,t+2} - BP_{2,i,t} - \sum_{j\neq i} d_{0,ij}\epsilon_{j,t+2} - \sum_{j\neq i} d_{1,ij}\epsilon_{j,t+1} - d_{1,ii}\hat{\epsilon}_{i,t+1}\right]. \tag{5}$$

Similar iterations produce a path of structural shocks that generate a path for $y_{i,t+j}$ that matches the desired path $y^*_{i,t+j}$ for $j = 1, \ldots, T$, where T is the planning horizon. This path of structural shocks for the policy variable, combined with the values of the shocks to the other variables, then produces an expected path for the system as a whole.[11]

Finally, we drop the assumption that the other equation shocks are known over the forecast horizon. We compute the ϵ path by employing a bootstrap technique that samples with replacement from the estimated residuals for each equation. Hence, we do not impose an arbitrary assumption about the probability density generating the residuals. For each trial, the fundamental property suggests that the computed values for the system variables are those the economy will follow under the assumed ϵ path for the policy equation, given the shocks to the other equations.[12]

Note that the computed value for $\hat{\epsilon}_{i,t+j}$ formalizes the description of policy formulation and revision described by Blinder (1997). In particular, he argues the following:

> First, you must plan an entire hypothetical path for your policy instrument, from now until the end of the planning horizon, even though you know you will activate only the first step of the plan. It is simply illogical

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[10] In our example, D_0 is lower triangular with units on the main diagonal. In other identifying schemes, neither of these need hold.

[11] Note that, if the values for y^* follow the actual data, then the system as a whole follows the actual path of the data.

[12] In addition to the additive uncertainty obtained when we draw from the actual residuals, it is also possible to incorporate multiplicative uncertainty in the spirit of Brainard (1967) by using the computed standard errors of the coefficients. For simplicity, we do not undertake this exercise here.

> to make your current decision in splendid isolation from what you expect to do in subsequent periods. Second, when next period actually comes, you must appraise the new information that has arrived and make an entirely new multiperiod plan. If the surprises were trivial, that is, if the stochastic errors were approximately zero, step one of your new plan will mimic the hypothetical step two of your old plan. But if significant new information has arrived, the new plan will differ notably from the old one. Third, you must repeat this reappraisal process each and every period. (p. 9)

For each trial, we assign each element of a vector of length $t + j$, an integer randomly drawn (with replacement) from the set $(1, 2, \ldots, N)$, where N is the number of observations in the estimation. The first integer selected, corresponding to that particular observation in the estimation period, has an associated set of residuals for the nonpolicy equations. These residuals are used in the computation of the $\hat{\epsilon}_{i,t+1}$ needed to attain the policy objective in period $t + 1$. The second integer is associated with another set of residuals, which proxies for the new information that arrives that period, and so on. For each trial in our bootstrap procedure, this sequence of exogenous shocks to the policy variable along with the shocks to the other variables and the base projections are used to generate paths for the variables in the system. Thus, each trial simulates arriving information to update the exogenous policy component and keep the policy variable at its target level. The average of the constructed paths—the mean path—over these trials then represents the expected impact of the policy.[13] We take this expected path to be the focus of attention for monetary policymakers.

4. Policy Experiments

General Description of the Experiments

In this section, we describe three policy simulations constructed using the methodology described above. Each example is intended to characterize, at least roughly, the type of policy analysis undertaken in anticipation of a change in policy. Specifically, policymakers can be presented with comparison forecasts for the current policy relative to alternative policy options.

The first exercise compares the actual increase in the target for *FFR* from 5.50% to 6.00% in 1995:1 with a no-change policy in which the target is maintained at 5.50%. The second simulation analyzes the cuts in the target for *FFR* from 5.50% to 5.25% on September 29, 1998, to 5.00% on October 15, 1998, and then to 4.75% in November 1998, again comparing the results to a no-change policy. (Of course, alternative policies to the one actually adopted could be considered as well, but presentation of them would only clutter the graphical presentation given here.) Third, we examine policymaking employing the Taylor rule for the 1990–1992 period. We discuss these policy options after some general comments about the experiments.

For each policy experiment, we estimate the model through the period ending in the quarter prior to the start of the policy evaluation period. After estimation, we compare forecasts for the entire system of variables for the no-change and the alternative policies based on 1000 trials,

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[13] Confidence bands for the simulations can also be constructed. However, since the base projections, the endogenous component of monetary policy, and the shocks to the nonpolicy variables are the same for both the policy experiment and the no-change case, it would be surprising if the paths from these experiments differed significantly. Indeed, when confidence intervals are constructed for the no-change policy, the mean path for the policy experiment always lies within these intervals. We argue that what is important for policymakers is the difference between the expected paths for the policy change and the no-change policy.

where, for strict comparability, for a given trial, the nonpolicy shocks are the same for the no-change and alternative policies. Note that the no-change policy forecast is distinct from the standard dynamic forecast, or base projection, in that, over the forecast period, *FFR* is maintained at the target level. In contrast, the base projection, which is conditional on information at time t and hence employs only past realized shocks in generating forecasts, generates a time path for *FFR* that generally differs from the target level.

Estimation for each model begins with 1961:2, with values for 1959:1–1961:1 used as presample data. For the graphical presentation, we focus on the levels (not the logs) of the variables in the model.[14]

Comparison with Earlier Studies

Sims (1982), in addition to discussing normal policymaking conceptually, also presented related empirical work. His empirical work assessed the plausibility of the political administration's forecasts of selected macro indicators by computing alternative combinations of shocks to the equations of the 6-variable VAR model he used that are consistent with the administration's forecasts. The shocks to just the M1 money stock, short-term interest rate, and fiscal policy variables required to generate the real gross national product (GNP) and price deflator forecasts of the administration are also computed, as are the shocks to just the money stock and interest rate required to generate the forecasts. Thus, the orientation of the counterfactual experiment is different from this article. The structure of the model differs as well; in Sims' model, M1 and the three-month T-bill rate comprise the monetary sector, whereas the reserves market is a critical element of the model in this article.

Likewise, Leeper and Sims (1994) presented empirical work as well as a conceptual discussion of normal policymaking. They estimated several models: a 3-variable real business cycle model and neoclassical and sticky-price variants of a 10-variable model. In the latter, a monetary policy equation that is similar to the Taylor rule is included. They evaluated the models for goodness of fit. However, policy experiments of the type we conduct were not reported.

Christiano (1998) presented a counterfactual experiment for the Great Depression that is closer to what we do than the two papers just discussed. However, due to instrument instability, Christiano used a weighted average of his estimated and counterfactual residuals for his policy shocks, along with the historical residuals for the other equations in his counterfactual analysis. Use of the estimated residuals implies that his counterfactual simulations are in-sample. Fackler and Rogers (1995) also conducted their analysis in-sample, along with the historical shocks to the other equations, though they used pure counterfactual residuals for the policy shocks.

Our analysis uses pure counterfactual residuals, is conducted out-of-sample, and draws random samples from the estimated residuals for use as shocks to the nonpolicy equations.

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[14] We compute the forecasted levels taking into account the fact that the exponential of the expected value of the log of the series is not equal to the expectation of the exponential of the log. Specifically, we employ the relationship

$$E[(\exp z_{t+s}) \mid H_t] = \exp[E(z_{t+s} \mid H_t) + 0.5V(z_{t+s} \mid H_t)],$$

where z is the log of the variable, H_t is the history of the variable up to time t, and E and V are the expectation and variance operators.

In addition, we report on the extreme values of our counterfactual policy shocks. As we report below, we do not find evidence of instrument instability in our empirical results.

Leeper and Zha (LZ; 2001) conducted experiments similar to ours in a variety of ways. First, their model included output, consumer prices, commodity prices, the funds rate, the unemployment rate, and M2. Our use of the CEE model substitutes *TR* and *NBR* for unemployment and the money supply. Accordingly, the analyses differ on the relative advantages of a detailed specification of the market for reserves.

Second, LZ evaluated policymaking in two historical episodes, the decline in the *FFR* target beginning in 1990 and the rise in the target in the 1994–1995 period. While we both evaluate the opening years of the 1990s, our discussion of this period focuses on the impact of implementing the Taylor rule about the time it was first introduced, while theirs focuses on normal policymaking using the feedback rule in place. Our analyses are most similar in the discussion of policy alternatives in the middle years of the 1990s. In addition, we also consider a more recent episode, in 1998, that LZ do not. Thus, there is only a modest amount of overlap in the specifics of the policy analysis.

The policy experiments presented here and in LZ begin to build a database for policymakers that explain how model economies respond to exogenous policy shocks. Both papers evaluate cases where the target for the funds rate is rising and falling. In addition, this article demonstrates how to evaluate important policy suggestions such as the Taylor rule.

The 1995 Rise in the Federal Funds Rate Target

Gavin (1996) provides a detailed discussion of Federal Open Market Committee deliberations in 1995. For our purposes, we focus on the documented rise in the *FFR* target to 6% on February 1, 1995, from the target rate of 5.5% that had been in place since late 1994. Since we are using quarterly data, we model the target rate for the first quarter of 1995 as 5.83%, the average of the 5.5% target in January with the 6% target for February and March. We then hold the target at 6% for another 19 quarters in order to observe the economic dynamics for this policy. We do not pretend that the FOMC intended to maintain the rate at 6% indefinitely. Rather, we maintain the new target rate in our simulations in order to observe the system dynamics over a reasonably long time period. Note that our methodology does not require that the target rate be constant, so we could have incorporated the actual cut in the target to 5.75% in July 1995 into the analysis rather than holding the target at 6%. However, we are unaware of any evidence suggesting that the FOMC intended in February to lower the target rate in July. Rather, we generally interpret policy changes during this period as the committee seeing the need for a change in the target rate, making the change, and then observing the effects of the change in the economy at large prior to implementing subsequent policy actions. We also presume that the committee would indeed make projections of the proposed policy change(s) several years into the future and compare these projections with the no-change policy; see Reifschneider, Tetlow, and Williams (1999) as an example.

Figure 2 compares the impact on the nonpolicy variables of the no-change policy with the increase in the *FFR* target actually implemented in early 1995. This comparison assumes that

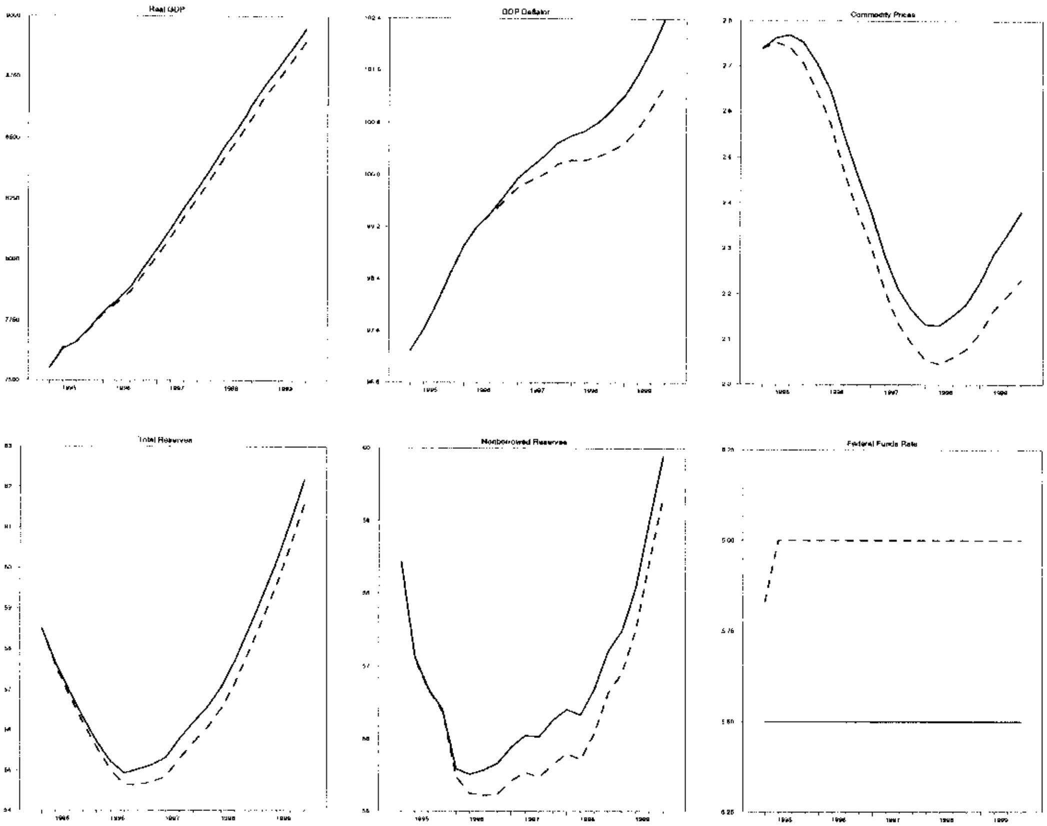

Figure 2. 1995:1 Policy Change: Mean Paths No Change (Solid) and Mean Paths Policy Change (Dashes)

policymakers use data through 1994:4 for estimation.[15] The expected (mean) paths for both the no-change and rate-increase policies for the nonpolicy variables are plotted in Figure 2, as are the no-change path for the federal funds rate and the funds rate path under the policy evaluated. The expected paths in Figure 2 incorporate endogenous and exogenous monetary policy actions, the base projections, and the nonpolicy shocks in the computation of the paths in each trial. Shocks to the nonpolicy variables affect all the variables in the system; for example, these shocks endogenously alter *FFR*. Feedback from the nonpolicy variables to *FFR* is thus captured in the experiments. This feedback, which is explicitly accounted for in Equations 4 and 5,

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[15] We use revised data in our analysis rather than the data that would have been available at the time of estimation. Use of then-current data would not alter the mechanics of our illustration of the technique. It might, however, alter the policy comparisons being plotted in the figures. For example, as noted earlier in footnote 3, Croushore and Evans (1999) found a high correlation between monetary policy shock measures from VARs estimated using real-time and revised data and similar impulse response functions as well. On the other hand, Orphanides (2000) used real-time data to estimate a Taylor-rule equation and compared these estimates with Taylor-rule estimates based on revised data. He found substantial differences in the fitted values of the federal funds rate using different vintages of data. In particular, he found the fitted values of the funds rate from the Taylor-rule equation estimated using real-time data were close to the actual values of the funds rate. In contrast, fitted values from the equation estimated using revised data were systematically different from the actual values in the late 1960s and 1970s. Specifically, the fitted values in this period suggest the inflation of the 1970s could have been avoided if the Taylor rule had been followed. However, this inference is not warranted when the Taylor rule is estimated using real-time data since the fitted values were very similar to the actual *FFR* values. These two studies suggest that using real-time versus revised data may matter more for some applications than others.

affects the size of the exogenous policy shocks. We do not directly address the issue of whether the model produces forecasts that compete favorably with available forecasting alternatives.

The direction of effect of the *FFR* increase for each of the variables is as expected. *Y*, *P*, *CP*, *TR*, and *NBR* all fall relative to the no-change policy. However, we see that these changes are relatively small; for example, the decline in *Y* after 20 quarters is approximately $54 billion for the one-half percentage point increase considered in this experiment. The small impact of the rate-hike policy compared with the no-change policy is not too surprising since the base projections and the shocks to the nonpolicy variables are the same in both cases; the only differences are the exogenous shocks to the policy variable. If the exogenous policy shocks for the two policies are similar in magnitude, then the paths will be also. The small magnitude of effects is in line with simulations from structural models. For example, when investigating a relatively large 100 basis-point change in *FFR*, Reifschneider, Tetlow, and Williams (1999) find small effects on key economic variables in a full-model simulation of the Federal Reserve Board (FRB/US) structural model.

The relatively small magnitude of the impact of the shocks from this technical perspective is consistent with our intuition about the conduct of policy. Specifically, we view the FOMC as operating so as to bring about marginal changes in inflationary pressure. After all, the Fed has available a wide array of policy options, including changes of various sizes in the *FFR* target, large changes in the discount rate, and even changes in reserve requirements. In utilizing any of these tools in varying magnitudes, the Fed in principle weighs the inflation risk against the probability of recession associated with applying each tool in each possible magnitude. Given the high costs associated with either recession or acceleration of inflation, it is not surprising that the Fed often moves cautiously, implying relatively small simulated changes. Note that our technique conditional on the model can be easily extended to examine explicitly these trade-offs.

One final question that remains is whether the policy shocks generated in this experiment are consistent with the actual residuals. For example, if the elements of the ϵ path oscillate in an explosive manner, the issue of instrument instability would arise. Thus, a comparison of the actual residuals with those required to attain the no-change and rate-increase paths is appropriate. The actual estimation produced residuals for the *FFR* equation between -1.57 and 2.94. The average minimum value across the 1000 trials for the no-change policy was -2.41 (with a standard deviation of 0.66, so that the actual minimum was within 1.5 SD of the average of the simulated values) and the average maximum value was 0.92 (with standard deviation of 0.51). Similar figures for the rate-increase policy were -2.28 (standard deviation of 0.66) and 1.02 (standard deviation of 0.50). The average maximum value in the simulations was well below that found in the actual estimation, reflecting the fact that the base projection was well above the target path. Thus, the computed ϵ paths are broadly consistent with the actual structural shocks. Finally, we note that the average ratio of the exogenous policy shock to the target value of the interest rate was only 0.054 for the alternative policy and 0.017 for the no-change policy. As noted earlier, this is expected for normal policymaking.

The 1998 Cut in the Federal Funds Rate

Detailed discussion of Federal Open Market Committee deliberations for 1998 are provided by Wheelock (1999). Our experiment compares a no-change policy of 5.50% with a policy that cut *FFR* to 5.25% on September 29, to 5.00% on October 15, and to 4.75% on November 17

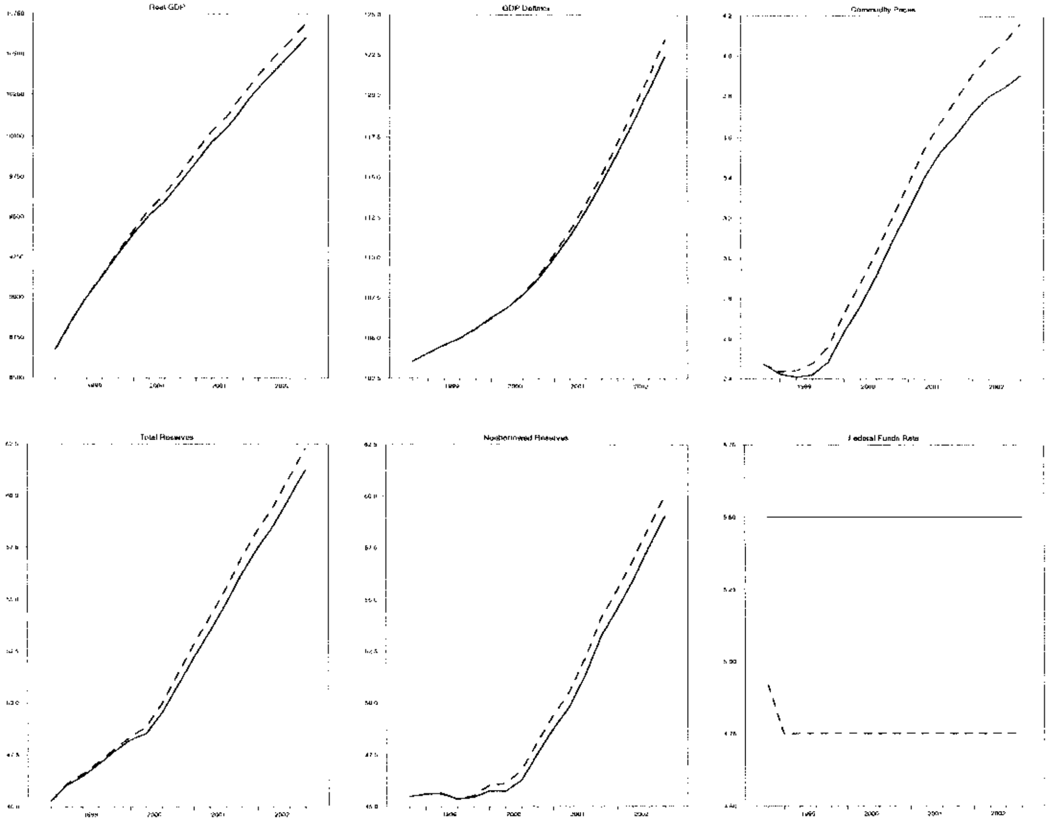

Figure 3. 1998:4 Policy Change: Mean Paths No Change (Solid) and Mean Paths No Change (Solid)

and then kept *FFR* at 4.75% through 2002:4. The *FFR* target for 1998:4 is set at 4.92%, a weighted average of the target rates in this quarter, and is set at 4.75% thereafter. Our target path for *FFR* reflects the recent tendency of the FOMC to gradually change *FFR* in a series of steps and technically highlights the flexibility of our approach to evaluate general target paths, not just constant values for a target variable.

Figure 3 is analogous to Figure 2. We see that the cut in *FFR* leads to an increase in *Y*, *P*, *CP*, and both *TR* and *NBR* relative to the no-change policy, although the effects are relatively small, as before. Although there is little initial effect, *Y* rises by approximately $82 billion by the end of the experiment. The lag in the effect on *P* is longer than for *Y*, as was the case in our previous experiment. The effects on *CP* and *TR* appear quickly, although it takes a few quarters for any substantial effect on *NBR* to appear.

The residuals for the *FFR* equation from the estimation took on a minimum value of -1.61 and a maximum of 3.05. The average minimum value across the 1000 trials for the no-change policy was -2.73 (with standard deviation of 0.51) and the average maximum value was 0.88 (with standard deviation of 0.10). Similar figures for the rate-cut policy were 2.87 (standard deviation of 0.52) and 0.36 (standard deviation of 0.20). Thus, it appears that the technique computes ϵ paths roughly consistent with the shocks actually observed. The average ratio of the exogenous policy shock to the point estimate of the interest rate was only -0.05 for the alternative policy and -0.041 for the no-change policy, again consistent with normal policy-making.

Taylor Rules

Taylor (1993) has proposed a simple policy function in which the *FFR* target responds to deviations of output from its potential and to deviations of inflation from a target specified by the policymaker. Maintaining high output levels along with low inflation are time-honored policy objectives, so this policy function links a key policy instrument to policy goals. Further, as argued recently by Judd and Rudebusch (1998), when the Taylor rule is embedded in a variety of models, output and inflation are reasonably well controlled.

Our implementation of the Taylor rule takes the following form:

$$r = p + 0.5ygap + 0.5(p - p^*) + 2,$$

where r is the nominal funds rate, p is the inflation rate for the previous four-quarter period, p^* is the target inflation rate, and $ygap$ is the deviation between last quarter's actual and potential outputs.[16] Note that, subtracting p from both sides of the equation, we can think of the rule being expressed in terms of the real interest rate. Also note that the real interest rate objective of 2% will be attained when output is at potential and when inflation is at its target rate. We use the Taylor rule to generate alternative paths for *FFR* using last period's actual $ygap$, last period's p, and using alternative values of p^* of 0, 2, 3, and 4.[17] Given the alternative path of *FFR* for, say, $p^* = 2$, for each trial, we compute the ϵ path that generates the desired *FFR* path and then compute the paths for the variables in the system conditional on this ϵ path. As before, we then compute the mean path of the variables in the system across all draws.

Figure 4 shows the effect of alternative objectives for inflation for the period 1990:1–1992:3 on both the nonpolicy variables and the funds rate. This period, approximately the one focused on by Taylor, encompasses the 1990–1991 recession and ends with the last data point for which Taylor computed $ygap$. The solid lines in the figure correspond to the mean path over 1000 draws for the no-change policy, that is, a policy aimed at an *FFR* target of 8.25%, roughly in line with the Fed's actual policy objective at the end of 1989.[18] The dashed lines plot the mean paths for inflation objectives of 0, 2, 3, and 4%. Consistent with intuition, the more tolerant is monetary policy regarding inflation, the higher the path of each of the nonpolicy variables. We see that, for *TR* and *CP*, the mean paths for the inflation alternatives begin to diverge quickly from the no-change path; it takes longer for this to occur for *NBR*, *Y*, and *P*. The effects of the alternative policies cause *Y* to begin to diverge more quickly from the no-change policy than is the case for *P*. This is consistent with earlier results. The paths for the federal funds rate are similar, and, the more tolerant the inflation objective, the lower the funds rate.

As with the previous experiments, we also note the extreme values of the ϵ paths relative to the magnitudes of the residuals in the estimated model. The maximum residual in the interest rate equation for the model estimated over our period of investigation was 2.76; the minimum was -1.45. The average maximum elements over the 1000 draws of the ϵ paths for the no-

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[16] Potential output is computed by the Congressional Budget Office and was downloaded from the Federal Reserve Bank of St. Louis web site (http://www.stls.frb.org/fred/data/gdp/gdppot).

Alternative policy rules like an x-percent rule for money growth without feedback are evaluated in Fackler and McMillin (1998).

[17] Judd and Rudebusch (1998) provide further discussion of the rule if the weights are estimated rather than being arbitrarily assumed to equal both to each other and to 0.5. They also discuss the assumption that the real rate objective is 2%.

[18] The Fed has specified a monitoring range for the funds rate of 6–10%. The rate in 1990:1 was 8.25%, so this is adopted as the no-change policy.

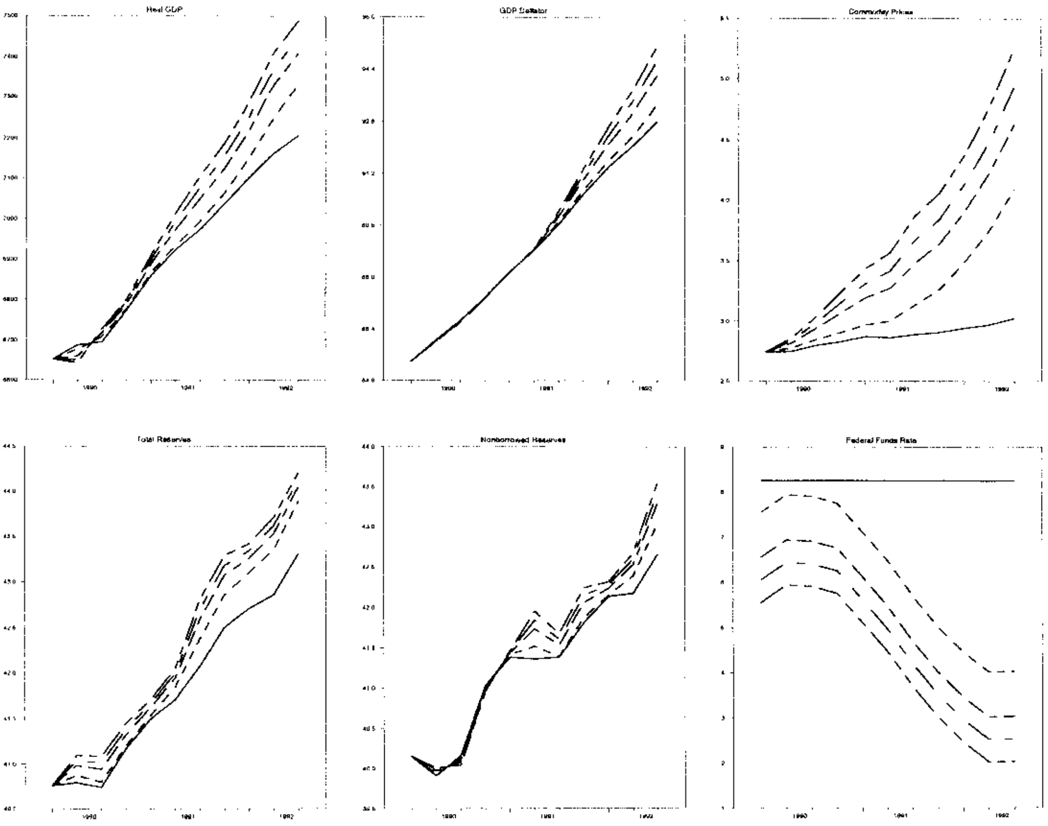

Figure 4. Inflation Targets of 0, 2, 3, and 4%: Mean Paths No Change (Solid) and Taylor Rules (Dashes)

change, 0%, 2%, 3%, and 4% policies were (0.96, 0.46, 0.13, −0.04, −0.19). The average minimum elements, in the same order, were (−1.83, −3.03, −3.30, −3.47, −3.70). The more extreme deviations of the average minimum elements from the estimated minimums suggest that consideration of policy rules that depart substantially from actual policy (as is the case here) is hazardous. We note that the average ratio of the exogenous policy shock to the point estimate of the interest rate was 0.098, 0.086, −0.009, −0.071, and −0.148, respectively, for the no-change, 0%, 2%, 3%, and 4% policies. These results reinforce concern about whether these experiments, especially the ones with higher inflation targets, constitute normal policy-making.

5. Summary and Conclusion

The fundamental purpose of this article is to propose a method for evaluating what Leeper and Sims (1994) refer to as normal policy changes within the context of a VAR model. The procedure developed can be viewed as a supplement to the evaluation of monetary policy changes within the context of a structural model as is currently done in FOMC deliberations. Hence, the technique can provide additional information about the effects of a proposed policy action. The procedure is based on a historical decomposition of the VAR, and the technique developed generates a path of structural policy shocks that keep the policy variable at its target value.

Illustrations of how this method can be implemented within a VAR model are provided by evaluating the effects of several recent changes in the federal funds rate target on output and the price level. Additionally, the Taylor rule is used to generate target funds rates corresponding to different target inflation rates, and the effects of these alternative funds rate targets are evaluated. For each policy action considered, a bootstrapping procedure with 1000 trials is used to generate the expected paths of output and the price level. For each trial, sampling with replacement from the historical shocks is done so that, for each trial, the shocks to the nonpolicy variables are typically nonzero.

The relatively small changes in the federal funds rate for the 1995 and 1998 policy actions suggest relatively small effects on both output and the price level. However, the direction of effect for all policy actions is as expected. After a lag of about a year, modest effects on output emerge, but the effects on the price level are still negligible after eight quarters. The Taylor rule experiment for 1990–1992 suggests a similar lag pattern for output and the price level, and the differences between the policy experiment results and the no-change policy become greater as the inflation target rises.

The empirical results presented here are conditional on a particular VAR model and a particular method of identifying policy shocks. But the technique proposed is easily implemented using other types of VARs, such as those structural VARs that impose short-run or long-run constraints. Consequently, when evaluating monetary policy actions, it may be useful to consider other methods of identifying policy shocks and other VAR models as well as more traditional structural models to get more information on the magnitudes of the effects.

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