BEN S. BERNANKE
Princeton University

MARK GERTLER
New York University

MARK WATSON
Princeton University

Systematic Monetary Policy and the Effects of Oil Price Shocks

THE PRINCIPAL OBJECTIVE of this paper is to increase our understanding of the role of monetary policy in postwar U.S. business cycles. We take as our starting point two common findings in the recent monetary policy literature based on vector autoregressions (VARs).[1] First, identified shocks to monetary policy explain relatively little of the overall variation in output (typically, less than 20 percent). Second, most of the observed movement in the instruments of monetary policy, such as the federal funds rate or nonborrowed reserves, is endogenous; that is, changes in Federal Reserve policy are largely explained by macroeconomic conditions, as one might expect, given the Fed's commitment to macroeconomic stabilization. These two findings obviously do not support the view that erratic and unpredictable fluctuations in Federal Reserve policies are a primary cause of postwar U.S. business cycles; but neither do they rule out the possibility that systematic and predictable monetary policies—the Fed's policy rule—affect the course of the economy in an important way. Put more positively, if one takes the VAR evidence on monetary policy seriously (as we do), then any case for an important role of monetary policy in the business cycle rests on

Thanks to Benjamin Friedman, Christopher Sims, and the Brookings Panel for helpful comments. Expert research assistance was provided by Don Redl and Peter Simon. The financial support of the National Science Foundation is gratefully acknowledged.

1. See, for example, Leeper, Sims, and Zha (1996).

the argument that the choice of the monetary policy rule (the ''reaction function'') has significant macroeconomic effects.

Using time-series evidence to uncover the effects of monetary policy rules on the economy is, however, a daunting task. It is not possible to infer the effects of changes in policy rules from a standard identified VAR system, since this approach typically provides little or no structural interpretation of the coefficients that make up the lag structure of the model. Large-scale econometric models, such as the MIT–Penn–SSRC model, are designed for analyzing alternative policies; but criticisms of the identifying assumptions of these models have been the subject of a number of important papers, notably, by Robert Lucas and Christopher Sims.[2] Particularly relevant to the present paper is Sims's point that the many overidentifying restrictions of large-scale models may be both theoretically and empirically suspect, often implying specifications that do not match the basic time-series properties of the data particularly well. Recent progress in the development of dynamic stochastic general equilibrium models overcomes much of Lucas's objection to the traditional approach, but the ability of these models to fit the time-series data—in particular, the relationships among money, interest rates, output, and prices—seems, if anything, worse than that of traditional large-scale models.

In this paper we take some modest (but, we hope, informative) first steps toward sorting out the effects of systematic monetary policy on the economy, within a framework designed to accommodate the time-series facts about the U.S. economy in a flexible manner. Our strategy involves adding a little bit of structure to an identified VAR. Specifically, we assume that monetary policy works its effects on the economy through the medium of the term structure of open-market interest rates; and that, given the term structure, the policy instrument (in our application, the federal funds rate) has no independent effect on the economy. In combination with the expectations theory of the term structure, this assumption allows one to summarize the effects of alternative expected future monetary policies in terms of their effects on the current short and long interest rates, which, in turn, help to determine the evolution of the economy. By comparing, for example, the historical behavior of the economy with its behavior under an hypothesized alter-

2. Lucas (1976); Sims (1980).

native policy reaction function, we obtain a rough measure of the importance of the systematic component of monetary policy. Our approach is similar in spirit to a methodology due to Sims and Tao Zha; however, these authors do not attempt to sort out the effects of anticipated and partially unanticipated policy changes.[3] While our proposed methodology is crude, and certainly is not invulnerable to the Lucas critique, we believe that it represents a commonsense approach to the problem of measuring the effects of anticipated policy, given currently available tools.

To be able to compare historical and alternative hypothesized responses of monetary policy to economic disturbances, one needs to select some interesting set of macroeconomic shocks to which policy is likely to respond. We focus primarily on oil price shocks, for two reasons.[4] First, periods dominated by oil price shocks are reasonably easy to identify empirically, and the case for exogeneity of at least the major oil price shocks is strong (although, there is also substantial controversy about how these shocks and their economic effects should be modeled). Second, in the view of many economists, oil price shocks are perhaps the leading alternative to monetary policy as the key factor in postwar U.S. recessions: increases in oil prices preceded the recessions of 1973–75, 1980–82, and 1990–91, and James Hamilton presents evidence that increases in oil prices led declines in output before 1972 as well.[5] Further, one of the strongest criticisms of the neomonetarist claim that monetary policy has been a major cause of economic downturns is that it may confound the effects of monetary tightening and previous increases in oil prices.

The rest of the paper is organized as follows. We first document that essentially all the U.S. recessions of the past thirty years have been preceded by both oil price increases and a tightening of monetary policy, which raises the question to what extent the ensuing economic declines can be attributed to each factor. Discussion of this identification problem requires a digression into the parallel VAR-based literature

3. Sims and Zha (1995).

4. Hooker (1996a) also studies the effects of oil price shocks and their interaction with monetary policy in a VAR framework. However, he does not explicitly attempt to decompose the effect of oil price shocks on the economy into a part due to the change in oil prices and a part due to the policy reaction.

5. Hamilton (1983).

on the effects of oil price shocks; one main conclusion is that it is surprisingly difficult to find an indicator of oil price shocks that produces the expected responses of macroeconomic and policy variables in a VAR setting. After comparing alternative indicators, we choose as our principal measure of oil price shocks the "net oil price increase" variable proposed by Hamilton.[6]

We next introduce our identification strategy, which summarizes the effects of an anticipated change in monetary policy in terms of its impact on the current term structure of interest rates (specifically, the three-month and ten-year government rates). We show that this approach provides reasonable results for the analysis of shocks to monetary policy and to oil prices; and, in particular, we find that the endogenous monetary policy response can account for a very substantial portion (in some cases, nearly all) of the depressing effects of oil price shocks on the real economy. This result is reinforced by a more disaggregated analysis, which compares the effects of oil price and monetary policy shocks on components of GDP. Looking more specifically at individual recessionary episodes associated with oil price shocks, we find that both monetary policy and other nonmoney, nonoil disturbances played important roles, but that oil shocks, per se, were not a major cause of these downturns. Overall, these findings help to resolve the long-standing puzzle of the apparently disproportionate effect of oil price increases on the economy. We also show that our method produces reasonable results when applied to the analysis of monetary policy reactions to other types of shocks, such as shocks to output and to commodity prices.

After presenting the basic results, we look in more detail at their robustness and stability. Regarding robustness, we find that the broad conclusion that endogenous monetary policy is an important component of the aggregate impact of oil price shocks holds across a variety of specifications, although the exact proportion of the effect due to monetary policy is sometimes hard to determine statistically. We also find evidence of subsample instability in our estimated system. To some extent, however, this instability helps to strengthen our main conclusions about the role of endogenous monetary policy, in that the total effect of oil price shocks on the economy on output is found to be

6. Hamilton (1996a, 1996b).

strongest during the Volcker era—when the monetary response to inflationary shocks was also the strongest.

Our analysis uses interpolated monthly data on GDP and its components. Appendix A documents the construction of these data, and appendix B describes all of the data that we use.

Is It Monetary Policy or Is It Oil? The Basic Identification Problem

The idea that monetary policy is a major source of real fluctuations in the economy is an old one; much of its lasting appeal reflects the ongoing influence of the seminal work of Milton Friedman and Anna Schwartz.[7] Obtaining credible measurements of monetary policy's contribution to business cycles has proved difficult, however. As discussed above, in recent years numerous authors have addressed the measurement of the effects of monetary policy by means of the VAR methodology, introduced into economics by Sims.[8] Roughly speaking, this approach identifies unanticipated innovations to monetary policy with an unforecasted shock to some policy indicator, such as the federal funds rate or the rate of growth of nonborrowed reserves. Using the estimated VAR system, one can trace out the dynamic responses of output, prices, and other macroeconomic variables to this innovation, thereby obtaining quantitative estimates of how monetary policy innovations affect the economy. As John Cochrane notes, ''this literature has at last produced impulse-response functions that capture common views about monetery policy''; for example, in finding that a positive innovation to monetary policy is followed by increases in output, prices, and money, and by a decline in the short-term nominal interest rate.[9] In addition, despite ongoing debates about precisely how the policy innovation should be identified, the estimated responses of key macroeconomic variables to a policy shock are reasonably similar across a

7. Friedman and Schwartz (1963).

8. Sims (1980); more recently, see Bernanke and Blinder (1992), Christiano and Eichenbaum (1992), Sims (1992), Strongin (1995), Bernanke and Mihov (1995), Sims and Zha (1995), and Leeper, Sims, and Zha (1996).

9. Cochrane (1996, p. 1).

variety of studies and suggest that monetary policy shocks can have significant and persistent real effects.

The VAR literature has focused on unanticipated policy shocks not because they are quantitatively very important—indeed, the conclusion of this literature is that policy shocks are too small to account for much of the overall variation in output and other variables—but because it is argued that cause and effect can be cleanly disentangled only in the case of exogenous, or random, changes in policy. However, looking only at unanticipated policy changes begs the question of how systematic, or endogenous, monetary policy changes affect the economy.[10]

Earlier work on the effects of monetary policy often does not make the distinction between anticipated and unanticipated policy changes.[11] These studies frequently find a very large role for monetary policy in cyclical fluctuations. An important recent example of this genre is an article by Christina Romer and David Romer.[12] Following the narrative approach of Friedman and Schwartz, Romer and Romer use Federal Reserve records to identify a series of dates at which, in response to high inflation, the Fed changed policy in a sharply contractionary direction. Their dates presumably correspond to policy changes with both an unanticipated component (because they were large, or decisive) and an anticipated component (because they were explicit responses to inflation); indeed, Matthew Shapiro shows that these dates are largely forecastable.[13] Romer and Romer find that their dates were typically followed by large declines in real activity and conclude that monetary policy plays an important role in fluctuations.

But as several critiques of Romer and Romer's article and the earlier work on anticipated monetary policy point out, studies that blur the

10. Cochrane (1996) has emphasized that even identification of the effects of unanticipated policy changes may hinge on distinguishing between anticipated and unanticipated changes, since an innovation in policy typically also changes the anticipated future path of policy. The analyst thus faces the conundrum of determining how much of the economy's response to a policy shock is due to the shock, per se, and how much is due to the change in policy anticipations engendered by the shock. The focus of this paper is different from that of Cochrane, in that we emphasize the effects of nonpolicy shocks, such as oil shocks, on anticipated monetary policy; but our methods could also be used to address the specific issue he raises.

11. Nor, for that matter, between changes in the money stock induced by policy and those induced by other factors. See, for example, Andersen and Jordan (1968).

12. Romer and Romer (1989).

13. Shapiro (1994).

distinction between anticipated and unanticipated policies suffer from precisely the identification problem that the VAR literature has attempted to avoid; namely, that it is not obvious how to distinguish the effects of anticipated policies from the effects of the shocks to which the policies are responding. This is not merely methodological carping, but is potentially of great practical importance in the postwar U.S. context, since a number of the most significant tightenings of U.S. monetary policy have followed on the heels of major increases in the price of imported oil.[14]

This point is illustrated in figure 1, which shows the historical behavior of the federal funds rate (here, taken to be an indicator of monetary policy) in the upper panel and the log-level of the nominal price of oil in the lower panel. Recessions, as dated by the National Bureau of Economic Research, are shaded. The upper panel also indicates the five dates identified by Romer and Romer that fall within our sample period. The lower panel shows, in analogy to the Romer dates, seven dates at which there were major disruptions to the oil market, as determined in part by Kevin Hoover and Stephen Perez.[15]

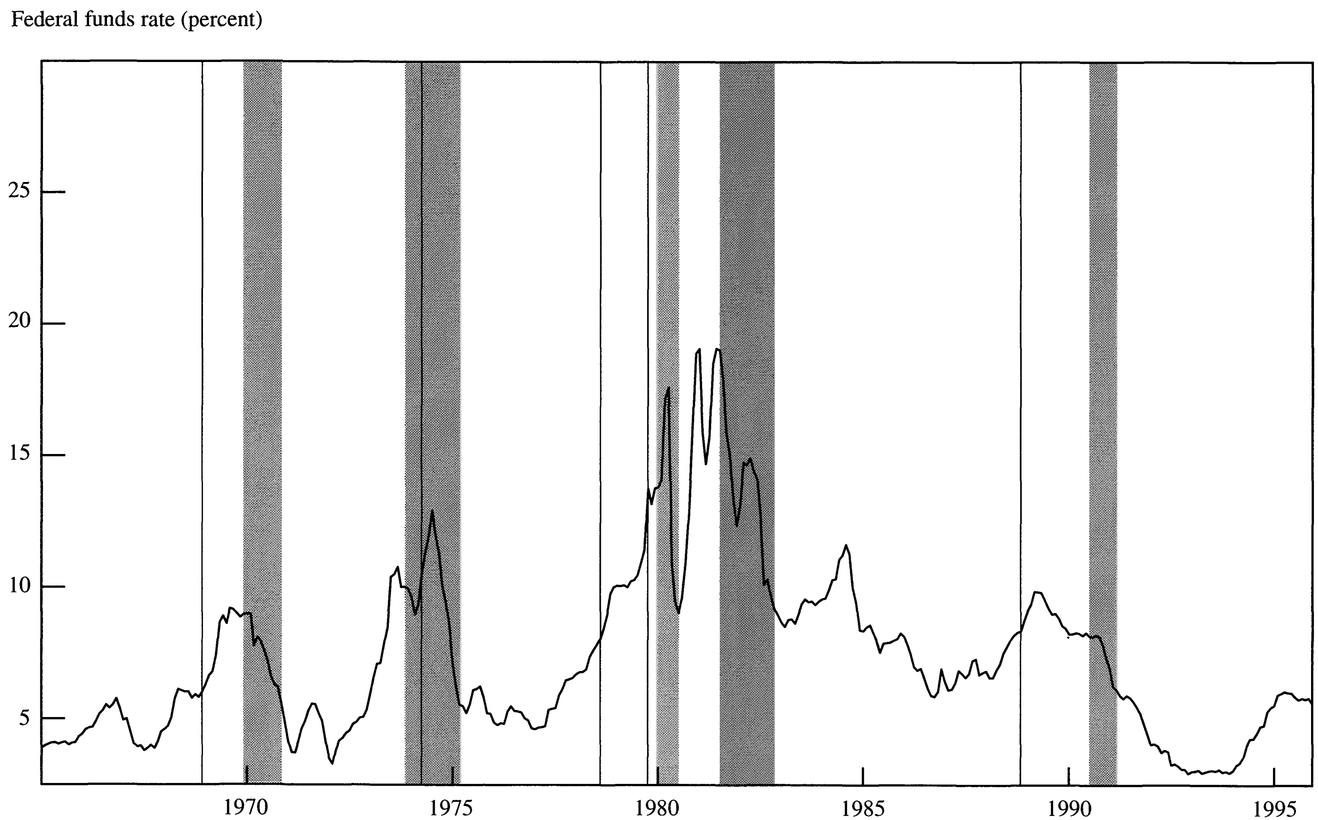
The upper panel of figure 1, taken alone, appears to support the neomonetarist case that tight money is the cause of recessions: each of the first four recessions in the figure was immediately preceded by a sharp increase in the federal funds rate, and the 1990 recession followed a monetary tightening that ended in late 1989. Peaks in the federal funds rate also tend to coincide with the Romer dates. However, the lower panel of figure 1 shows why it would be premature to lay the blame for postwar recessions at the door of the Federal Reserve: as was first emphasized by Hamilton, nearly all of the postwar U.S. recessions have also followed increases in the nominal price of oil, which, in turn, have been associated with monetary tightenings.[16] Further, many of these oil price shocks were arguably exogenous, reflecting a variety of developments both in the Middle East and in the domestic industry, as indicated by the Hoover-Perez dates. Thus the general identification problem is here cast in a specific form: what portion of the last five

14. See Dotsey and Reid (1992) and Hoover and Perez (1994).

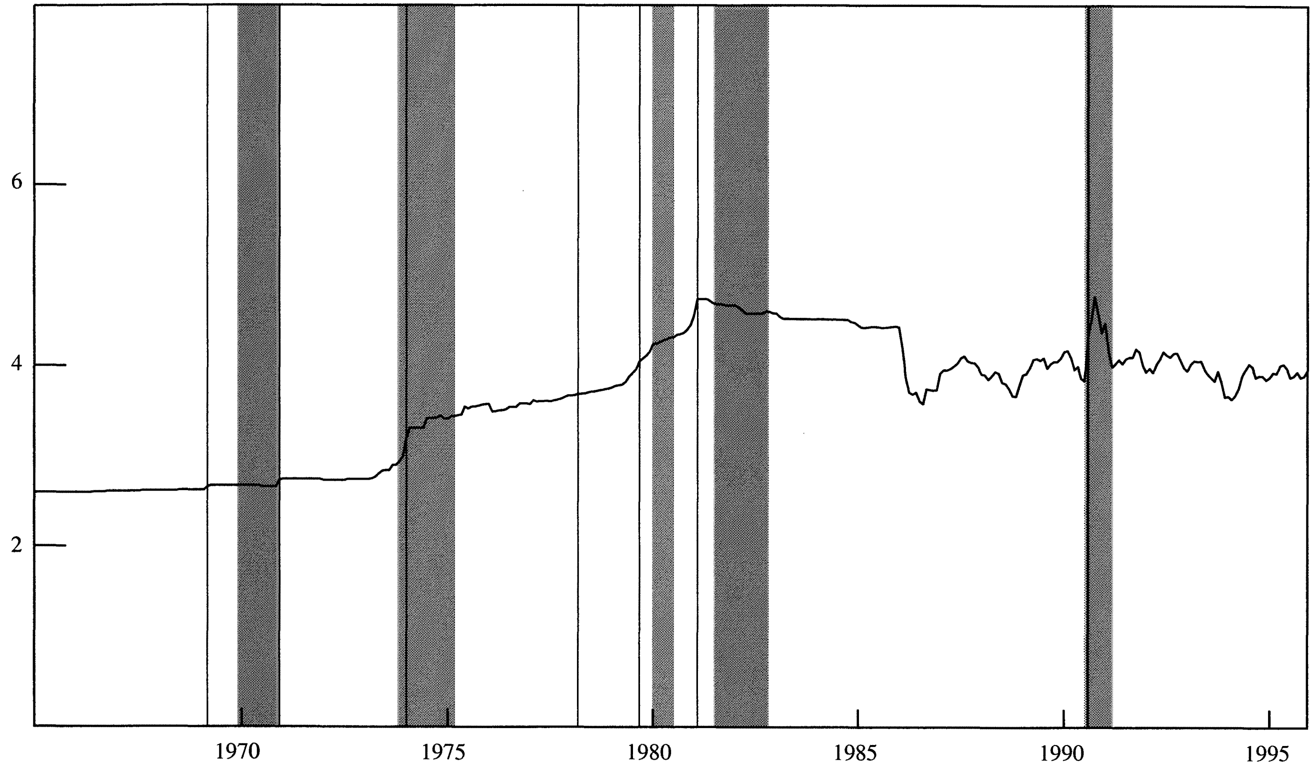
15. Hoover and Perez (1994), in their critique of the Romer and Romer approach, introduce six dates, which are, in turn, based on a chronology due to Hamilton (1983). We have added August 1990, the month when Iraq invaded Kuwait.

16. Hamilton (1983).

Figure 1. Federal Funds Rate, Oil Prices, and NBER Recessions, 1965–95[a]



Log crude oil price index[b]



Source: CITIBASE, series FYFF (federal funds rate) and PW561 (crude oil prices); see appendix B for details of all variables. Also (see note a) Romer and Romer (1989) and Hoover and Perez (1994).

a. Data are monthly; tic marks correspond to January. Shaded bands correspond to recessions as dated by the National Bureau of Economic Research. In the upper panel, vertical lines mark contractionary policy changes by the Federal Reserve, as dated by Romer and Romer. In the lower panel, vertical lines mark oil market disruptions, as dated by Hoover and Perez, plus the month of August 1990, when Iraq invaded Kuwait.

b. Log of index that is constructed so that 1982 = 100.

U.S. recessions, and of aggregate output and price fluctations in general, was due to oil price shocks, per se, and what portion was due to the Federal Reserve's response to those shocks? To answer this question requires a means of measuring the effects of anticipated or systematic monetary policies.[17]

Measuring Oil Price Shocks and their Effects

We propose to identify the importance of the monetary policy feedback rule in a modified VAR framework. In order to do that, however, one needs to find an appropriate indicator of oil price shocks to incorporate into the VAR systems. This is a more difficult task than it may appear at first. The most natural indicator would seem to be changes in the nominal oil price; and indeed, in an article which helped to initiate the literature on the effects of oil price shocks, Hamilton shows that increases in the nominal price of oil Granger-cause downturns in economic activity.[18] However, the arrival of new data has shown this simple measure to have a rather unstable relationship with macroeconomic outcomes, leading subsequent researchers to employ increasingly complicated specifications of the "true" relationship between oil and the economy.[19] In particular, Hamilton argues in his more recent work that the correct measure of oil shocks depends very much upon the precise mechanism by which changes in the price of oil are supposed to affect the economy, a question for which many answers have been proposed but on which there is little agreement.[20] For our purposes, the exact channels through which oil affects the economy are not crucial.

17. In this paper, we take as given that anticipated as well as unanticipated monetary policies influence the real economy, owing to the existence of various nominal rigidities. Our objective is to provide an estimate of the real impact of the systematic component of monetary policy, as opposed to testing the null hypothesis that this component is neutral.

18. Hamilton (1983), to the surprise of many, also demonstrates that there appears to have been a close relationship between oil price increases and recessions even before the major OPEC shocks of the 1970s.

19. See, for example, Mork (1989), Lee, Ni, and Ratti (1995), Hamilton (1996a), and Hooker (1996a, 1996b).

20. Possibilities discussed by Hamilton (1996a) include aggregate supply effects operating through costs of production and the indirect effects of wage rigidity; aggregate demand effects; effects arising from the interaction of uncertainty about future energy prices and the irreversibility of investment; and asymmetric sectoral impacts that force costly reallocations of resources.

What matters is that one can identify an exogenous movement in the price of oil that has a significant and a priori plausible reduced-form impact on the economy.

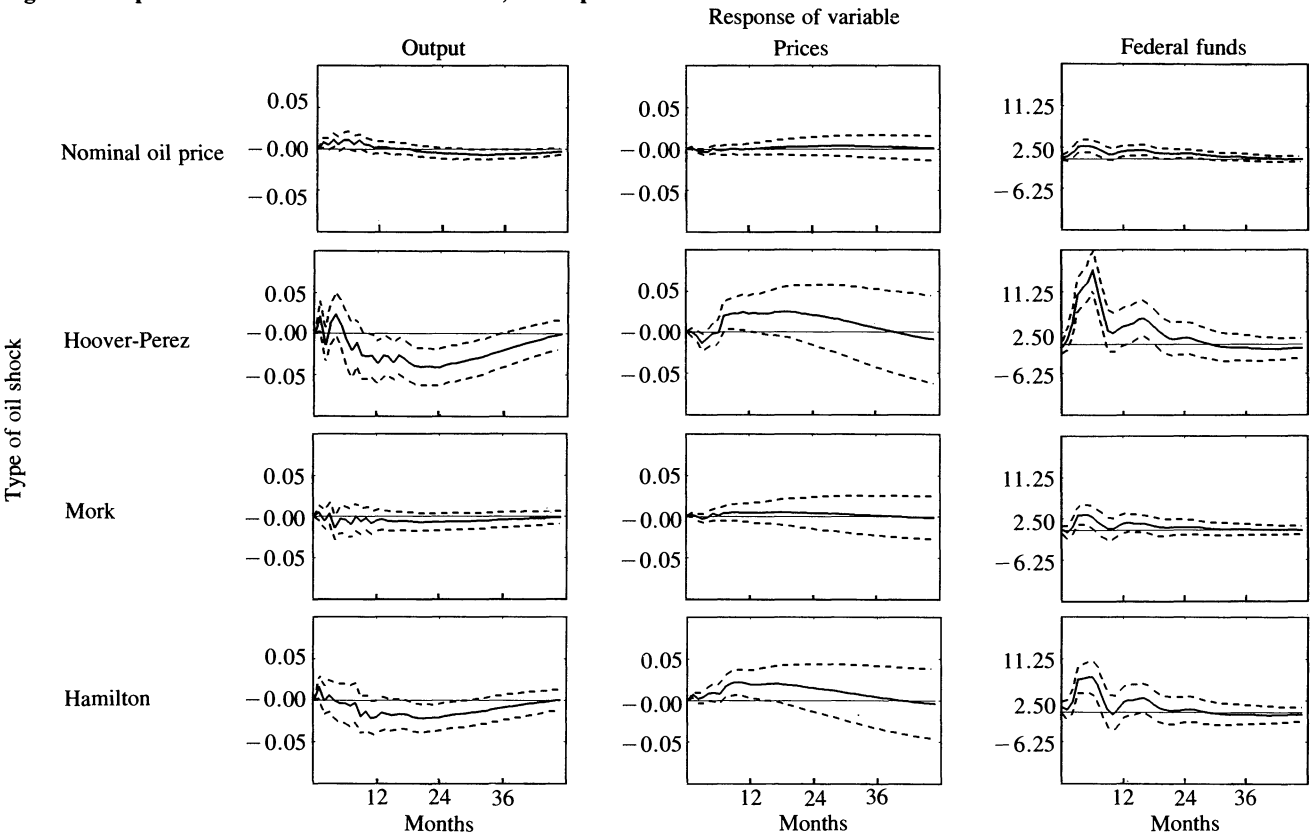
Figure 2 illustrates the effects of some alternative measures of oil price shocks on selected variables, as indicated by estimated impulse response functions (IRFs). Each IRF is based on a five-variable VAR that includes, in this order: (1) the log of real GDP; (2) the log of the GDP deflator; (3) the log of an index of spot commodity prices; (4) an indicator of the state of the oil market; and (5) the level of the federal funds rate. Data are monthly; the VAR is estimated using a constant and seven lags, as determined by the Akaike information criterion (AIC); and the sample period is 1965–95.[21] Only the impulse responses of real GDP, the GDP deflator, and the federal funds rate are shown, in each case over a forty-eight-month horizon and for an oil price shock normalized to correspond to a 1 percent increase in the current nominal oil price. Dashed lines correspond to one standard error bands. As is standard in the VAR literature on the effects of monetary policy, the index of commodity prices is added to the VAR to control for information that the Fed may have about future inflation which is not captured by the other variables in the system.[22] The federal funds rate is included as an indicator of monetary policy.[23] The ordering of the oil indicator after the macroeconomic variables imposes the reasonable

21. Appendix A describes the construction of monthly data for GDP and the GDP deflator. The logarithm of real GDP is detrended with a cubic spline with three equally spaced knot points imposing equality of the levels and first two derivatives at the knot points. The resulting estimated trend component is essentially piecewise linear, with a break in the early 1970s reflecting the productivity slowdown. Other data are from the CITIBASE electronic database, available from Citicorp Database Services (see appendix B). The CITIBASE labels for the series are: FYFF (federal funds rate), PSCCOM (commodity price index), and PW561 (nominal oil price index, Producer Price Index for crude oil and products). We focus here on full sample results; we discuss possible subsample instabilities below.

22. The inclusion of the commodity price index is suggested by Sims (1992) as a way of eliminating the so-called price puzzle in monetary policy VARs. In the present context, it is important to note that, for most of its history, the commodity price index appears to have excluded oil and other energy prices (a little uncertainty remains because of the poor documentation of the series). Since 1987, an oil price has been included in the index. As we report below, however, there is little evidence that its inclusion has any substantive effect on our results.

23. Results from Bernanke and Blinder (1992), Bernanke and Mihov (1995), and Friedman and Kuttner (1996) suggest that it is reasonable to use the funds rate as a policy indicator, except possibly during the 1979–82 reserves-targeting period.

Figure 2. Responses to a 1 Percent Oil Price Shock, Four Specifications[a]



Source: Authors' vector autoregressions (VARs), using data described in appendix B.

a. Graphs show forty-eight month response of variables to each of four oil shock specifications. Vertical axis scales represent percent deviations of variables (basis point deviations for the federal funds rate). Dashed lines represent one-standard-error bands. Sample period is 1965–95.

assumption that oil price shocks do not significantly affect the economy within the month. Similarly, ordering the funds rate last follows the conventional assumption that monetary policy operates with at least a one-month lag. The results are not sensitive to these ordering assumptions, as we document below in the context of a larger system.

In figure 2 we report results for four alternative indicators of the state of the oil market; one is a slight variation of the original Hamilton indicator, the other three are more exotic indicators that have been developed in ongoing attempts to identify a stable relationship between oil price shocks and the economy:

—Log of the nominal Producer Price Index (PPI) for crude oil and products; the nominal oil price, for short. Hamilton employs the log-difference of the nominal oil price, which, given the presence of freely estimated lag parameters, is nearly equivalent to using the log-level. Given the other variables included in the VAR, this indicator is also essentially the same as that used by Julio Rotemberg and Michael Woodford.[24]

—Hoover-Perez. These are the oil shock dates identified by Hoover and Perez plus August 1990, as discussed in regard to figure 1.[25] To scale these dates by relative importance, for each month we multiply the Hoover-Perez dummy variables by the log change in the nominal price of oil over the three months centered on the given month.

—Mork. After the sharp oil price declines of 1985–86 failed to lead to an economic boom, Knut Mork argued that the effects of positive and negative oil price shocks on the economy need not be symmetric.[26] Empirically, he provided evidence that only positive changes in the relative price of oil have important effects on output. Accordingly, in our VARs we employ an indicator that equals the log-difference of the relative price of oil when that change is positive and otherwise is zero.[27]

24. Hamilton (1983); Rotemberg and Woodford (1996).
25. Hoover and Perez (1994).
26. Mork (1989).
27. We measure the relative price of oil as the PPI for crude oil divided by the GDP deflator. Mork (1989) argues that the PPI for crude oil is a distorted measure of the marginal cost of oil during certain periods marked by domestic price controls; he therefore measures oil prices by refiner acquisition cost instead, for the period for which those data are available. We choose to stick with the crude oil PPI for simplicity, and because we feel that there are also problems with the refiner acquisition cost as a measure of the marginal cost of crude.

—Hamilton. In response to the breakdown of the relationship between output and simpler measures of oil price shocks, Hamilton has proposed a more complicated measure of oil price changes: the "net oil price increase."[28] This measure distinguishes between oil price increases that establish new highs relative to recent experience and increases that simply reverse recent decreases. Specifically, in the context of monthly data, Hamilton's measure equals the maximum of (a) zero and (b) the difference between the log-level of the crude oil price for the current month and the maximum value of the logged crude oil price achieved in the previous twelve months. Hamilton provides some evidence for the usefulness of this variable, using semiparametric methods, and Hooker also finds it to perform well, in the sense of having a relatively stable relationship with macroeconomic variables.[29]

The deficiencies of the simplest measure of the state of the oil market, the nominal price of crude oil, are apparent from figure 2. In particular, for our 1965–95 sample period, a shock to the nominal price of oil is followed by a *rise* in output for the first year or so and by a slight short-run *decline* in the price level. Both of these results (which have been verified in the recent literature on oil price shocks) are anomalous, relative to the conventional wisdom about the effects of oil price shocks on the economy. As indicated in note 29, other simple measures, such as the relative price of oil, give similarly unsatisfactory results.

The three more complex indicators (Hoover-Perez, Mork, and Hamilton) produce "better looking" IRFs, in that output falls and prices rise following an oil price shock, although generally neither response is statistically significant. The point estimates of the effect of an oil price shock on output suggest a modest impact from an economic perspective. For example, in the case of the Hamilton indicator, the sum

28. Hamilton (1996a, 1996b).

29. Hamilton (1996b); Hooker (1996a). We also experimented with VARs including the log-difference of the nominal price of oil (the indicator used by Hamilton, 1983); the log of the real price of oil (the nominal oil price divided by the GDP deflator); the log-difference of the real price of oil; and the log of the nominal price of oil weighted by the share of energy costs in GDP (as suggested by William Nordhaus at the Brookings Panel meeting). As the results obtained were very similar to those using the log nominal price of oil, we do not report them here. The literature provides yet additional indicators of oil price shocks. Those proposed by Ferderer (1996) and Lee, Ni, and Ratti (1995), for example, focus on the volatility of oil prices rather than the level. For simplicity, we ignore these second-moment-based measures and concentrate on measures that are functions of the level of oil prices.

of the impulse response coefficients for output over the first forty-eight months is -0.538, implying that a 1 percent (transitory) shock to oil prices leads to a cumulative loss of about 0.5 percent of a month's real GDP, or 0.045 percent of a year's real GDP, over four years. As is touched on below, more economically and statistically significant effects of oil price shocks are estimated (a) when the latter part of the sample, which contains the somewhat anomalous 1990 episode, is omitted; and (b) when the VAR system is augmented with short-term and long-term market interest rates.

Figure 2 also shows that for all four indicators of the oil market, a positive innovation to oil prices is followed by a rise in the funds rate (tighter monetary policy), as expected, and the response is generally statistically significant. This funds rate response illustrates the generic identification problem: without further structure, it is not possible to determine how much of the decline in output is the direct result of the increase in oil prices, as opposed to the ensuing tightening of monetary policy.

This brief exercise demonstrates a main result of the recent literature on the macroeconomic effects of oil prices, that finding a measure of oil price shocks that "works" in a VAR context is not straightforward. It is also true that the estimated impacts of these measures on output and prices can be quite unstable over different samples, as discussed below. For present purposes, however, based on the evidence of the literature and our own analysis (including figure 2), we choose the Hamilton net oil price increase measure of oil price shocks for our basic analyses.[30] As we discuss further below, we have checked the robustness of our exercises to the use of alternative oil market indicators; in general, we find that when a given oil-market indicator yields reasonable results in exercises like those shown in figure 2, our alternative simulations also perform reasonably.

Measuring the Effects of Endogenous Monetary Policy

Figure 2 shows that, at least for some more complex—some might argue, data-mined—indicators of oil prices, an exogenous increase in the price of oil has the expected effects on the economy: output falls,

30. In particular, Hooker (1996a) finds that the Hamilton measure is the most stable across subsamples.

prices rise, and monetary policy tightens (presumably in response to the inflationary pressures from the oil shock). Since James Tobin's Brookings paper, however, it has been argued that oil and energy costs are too small relative to total production costs to account for the entire decline in output that, at least in some episodes, has followed increases in the price of oil.[31] A natural hypothesis, therefore, is that part of the recessionary impact of oil price increases arises from the subsequent monetary contraction.

Sims and Zha attempt to provide rough estimates of the contribution of endogenous monetary policy changes in a VAR context.[32] Their approach is to "shut down" the policy response that would otherwise be implied by the VAR estimates; for example, by setting the federal funds rate (the monetary policy indicator) at its baseline level (the value that it would have taken in the absence of the exogenous nonpolicy shock). The difference between the total effect of the exogenous nonpolicy shock on the system variables and the estimated effect when the policy response is shut down is then interpreted as a measure of the contribution of the endogenous policy response.

As Sims and Zha correctly point out, this procedure is equivalent to combining the initial nonpolicy shock with a series of policy innovations just sufficient to off-set the endogenous policy response. Implicitly, then, in the Sims-Zha exercise, people in the economy are repeatedly "surprised" by the failure of policy to respond to the nonpolicy shock in its accustomed way. The authors argue, not unreasonably, that it would take some time for people to learn that policy was not going to respond in its usual way; so that, for deviations of policy from its historical pattern that are neither too large nor too protracted, their estimates of the policy effects may be acceptable approximations. This justification is similar to the one that Sims uses in earlier articles for conducting policy analyses in a VAR setting, despite the issues raised by the Lucas critique.[33]

31. Tobin (1980). See also Darby (1982), Kim and Loungani (1992), and Rotemberg and Woodford (1996). Rotemberg and Woodford argue that a monopolistically competitive market structure, which leads to changing markups over the business cycle, in principle can explain the strong effect of oil price shocks.

32. Sims and Zha (1995). Counterfactual simulations in a VAR context have also been performed by West (1993) and Kim (1995); neither paper distinguishes anticipated from unanticipated movements in policy.

33. See, for example, Sims (1986).

Rather than ignoring Lucas's argument altogether, however, one might try to accommodate it partially in the VAR context, by acknowledging that it may be more important for some markets than for others. In particular, the evidence for the relevance of the Lucas critique seems much stronger for financial markets—for example, in the determination of the term structure of interest rates—than in labor and product markets, which has led some economic forecasters and policy analysts to propose and estimate models with rational expectations in the financial market only.[34] In that spirit, we modify the Sims-Zha procedure for measuring the effects of endogenous policy by assuming that interest rate expectations are formed rationally (and in particular, that financial markets anticipate alternative policy paths), but that the other equations of the VAR system are invariant to the contemplated policy change. The latter assumption can be rationalized by assuming either that expectations of monetary policy enter the true structural equations for output, prices, and so forth only through the term structure of interest rates; or, if other policy-related expectations enter into those structural equations, that (for policy changes that are not too large) these respond more sluggishly than financial market expectations, as proposed by Sims.[35] Although our method is obviously neither fully structural nor immune to the Lucas critique, it provides an interesting alternative to the Sims-Zha approach.

More specifically, we consider small VAR systems that include standard macroeconomic variables, short-term and long-term interest rates, and the federal funds rate (as an indicator of monetary policy). We make the following assumptions:

—First, that the federal funds rate does not *directly* affect macroeconomic variables such as output and prices; a reasonable assumption, since the funds rate applies to a very limited set of transactions (overnight borrowings of commercial bank reserves). Hence the funds rate is excluded from the equations in the system determining those variables. However, the funds rate is allowed to affect macroeconomic variables indirectly, through its effect on short-term and long-term interest rates, which, in turn, are allowed to enter every equation that deter-

34. See Blanchard (1984) on the comparative relevance of the Lucas critique. See Taylor (1993) for an example of a model with rational expectations limited to the financial market.

35. Sims (1986).

mines a macroeconomic variable. Note that the assumption that monetary policy works strictly through interest rates is conservative, as it ignores other possible channels, such as the exchange rate and the "credit channel." In this sense, our estimates should represent a lower bound on the contribution of endogenous monetary policy.

—Second, following many previous authors, that the macroeconomic variables in the system are Wold-causally prior to all interest rates. That is, in our monthly data, we assume that interest rates respond to contemporaneous developments in the economy, but that changes in interest rates do not affect "slow-moving" variables such as output and prices within the month. This is a plausible assumption, given planning and production lags.[36]

—Third, that the funds rate is Wold-causally prior to the other market interest rates. That is, the covariation between innovations in the funds rate and in other interest rates is caused by the influence of monetary policy changes on interest rates, rather than by the response of the policymakers to market rates within the month. This is a strong assumption, although it appears to give fairly reasonable results in the context of the expectations theory of the term structure. It may be justified if the term premium contains no information about the economy that is not also contained in the other variables seen by the Fed. Below, we briefly discuss an alternative ordering assumption that allows for considerable reaction by the Fed to current market interest rate movements.

Formally, let $\mathbf{Y}_t$ denote a set of macroeconomic variables, including the price of oil, at date t. Similarly, let $\mathbf{R}_t = (R_t^s, R_t^l)$ represent the set of market interest rates; specifically, the three-month Treasury bill rate (the "short rate," R_t^s) and the ten-year Treasury bond rate (the "long rate," R_t^l). Finally, the scalar FF_t is the federal funds rate. Under the assumptions above, the restricted VAR system is written

$$\mathbf{Y}_t = \sum_{i=1}^{p} (\pi_{yy,i}\mathbf{Y}_{t-i} + \pi_{yr,i}\mathbf{R}_{t-i}) + G_{yy}\boldsymbol{\epsilon}_{y,t} \quad (1)$$

36. As Sims points out, however, the assumption is less plausible for the commodity price index, which is included in the nonpolicy block as an information variable; see Leeper, Sims, and Zha (1996).

$$(2)\quad FF_t = \sum_{i=1}^{p} (\boldsymbol{\pi}_{fy,i}\mathbf{Y}_{t-i} + \boldsymbol{\pi}_{fr,i}\mathbf{R}_{t-i} + \pi_{ff,i}FF_{t-i}) + \epsilon_{ff,t} + \mathbf{G}_{fy}\boldsymbol{\epsilon}_{y,t} + \mathbf{G}_{fr}\boldsymbol{\epsilon}_{r,t}$$

$$(3)\quad \mathbf{R}_t = \sum_{i=1}^{p} (\pi_{ry,i}\mathbf{Y}_{t-i} + \pi_{rr,i}\mathbf{R}_{t-i} + \boldsymbol{\pi}_{rf,i}FF_{t-i}) + \boldsymbol{\epsilon}_{r,t} + G_{ry}\boldsymbol{\epsilon}_{y,t} + \mathbf{G}_{rf}\epsilon_{ff,t},$$

where the π and G terms are matrices of coefficients of the appropriate dimensions, the ϵ terms are vectors of orthogonal error terms, and constant terms have been omitted for notational convenience. For equation 1, the exclusion of FF_{t-i} follows from the first assumption above, that the funds rate does not directly affect macroeconomic variables; and the exclusion of $\boldsymbol{\epsilon}_{r,t}$ and $\epsilon_{ff,t}$ is implied by the second assumption, that innovations to interest rates do not affect the nonpolicy variables within the period.

In order to apply the expectations theory to identify a relationship between the funds rate and the market interest rates, and to implement our policy experiments, it is useful to decompose the market rates into two parts: a part reflecting expectations of future values of the nominal funds rate, and a term premium. We define the following variables:

$$(4)\qquad \overline{R}_t^s = E_t\left(\sum_{i=0}^{ns-1} \omega_{s,i}FF_{t+i}\right)$$

$$(5)\qquad \overline{R}_t^l = E_t\left(\sum_{i=0}^{nl-1} \omega_{l,i}FF_{t+i}\right)$$

$$(6)\qquad S_t^s = R_t^s - \overline{R}_t^s$$

$$(7)\qquad S_t^l = R_t^l - \overline{R}_t^l,$$

where $ns = 3$ months and $nl = 120$ months are the terms of the short-term and long-term rates, respectively; the weights, ω, are defined by $\omega_{s,i} = \beta^i \Big/ \sum_{j=0}^{ns-1} \beta^j$ and $\omega_{l,i} = \beta^i \Big/ \sum_{j=0}^{nl-1} \beta^j$; and E is the expectations operator. We set the monthly discount factor, β equal to 0.997, so that β^{12} is equal to 0.96[37]. The $\overline{\mathbf{R}}$ variables defined in equations 4 and 5 are the "expectations components" of the short and long market interest rates, and the residual **S** terms in equations 6 and 7 are time-varying term-cum-risk premiums associated with rates at the two maturities. Note that the time series of the two components of short and long interest rates are easily calculated from current and lagged values of **Y**, *FF*, and **R**, using the estimated π parameters in equations 1–3. In particular, finding the estimated expectations components of short and long rates is purely a forecasting exercise and does not require structural identifying assumptions.

With these definitions, it is useful to rewrite the model of equations 1–3 as

$$(8) \qquad \mathbf{Y}_t = \sum_{i=1}^{p} [\pi_{yy,i}\mathbf{Y}_{t-i} + \pi_{yr,i}(\overline{\mathbf{R}}_{t-i} + S_{t-i})] + G_{yy}\boldsymbol{\epsilon}_{y,t}$$

$$(9) \qquad FF_t = \sum_{i=1}^{p} (\boldsymbol{\pi}_{fy,i}\mathbf{Y}_{t-i} + \boldsymbol{\pi}_{fr,i}\mathbf{R}_{t-i} + \pi_{ff,i}FF_{t-i}) + \epsilon_{ff,t} + \mathbf{G}_{fy}\boldsymbol{\epsilon}_{y,t} + \mathbf{G}_{fs}\boldsymbol{\epsilon}_{s,t}$$

$$(10) \qquad \mathbf{S}_t = \sum_{i=1}^{p} (\lambda_{sy,i}\mathbf{Y}_{t-i} + \lambda_{sr,i}\mathbf{R}_{t-i} + \boldsymbol{\lambda}_{sf,i}FF_{t-i}) + \boldsymbol{\epsilon}_{s,t} + \mathbf{G}_{sy}\boldsymbol{\epsilon}_{y,t} + G_{sf}\epsilon_{ff,t}.$$

Equation 8 is identical to equation 1, except that the two market interest rates have been broken up into their expectations and term premium components. Equations 9 and 10 correspond to equations 2 and 3, with the interest rates, **R**, replaced by the corresponding term premiums, **S**. Since the difference between **R** and **S** is the expectations component of interest rates, which is constructed as a projection on current and lagged values of observable variables, equation 10 are equivalent to equations 2 and 3. In particular, the coefficients in equations 9 and 10 are simply combinations of the coefficients in equation 3 and the projection coefficients of the federal funds rate on current and lagged variables.

37. This weighting function and the value of β are suggested by Shiller, Campbell, and Schoenholz (1983).

We work with the system of equations 8–10 because it simplifies the imposition of some alternative identifying restrictions. Our main identifying assumption, discussed above, is that the federal funds rate is Wold-causally prior to the other interest rates in the model; this corresponds to the assumption that $\mathbf{G}_{fs} = \mathbf{0}$ in equation 9. However, an alternative assumption, which allows for two-way causality between the funds rate and market rates, is that shocks to the federal funds rate affect other interest rates contemporaneously only through their impact on expectations of the future funds rate (that is, funds rate shocks do not affect term premiums contemporaneously); this corresponds to the restriction that $\mathbf{G}_{sf} = \mathbf{0}$ in equation 10. Note that this alternative assumption allows the funds rate to respond to innovations in term premiums. In both cases, we assume that G_{yy} is lower-triangular (with ones on the diagonal), as in conventional VAR analyses employing the Choleski decomposition. In most of our applications, the "macro block" consists of real GDP, the GDP deflator, the commodity price index, and Hamilton's net oil price increase variable, in that order; as we show below, our results are robust to the placement of the oil market indicator.

To illustrate how we carry out policy experiments, consider the scenario of greatest interest in this paper: a shock to the oil price variable. The base case, which incorporates the effects of the endogenous policy response, is calculated in the conventional way, by simulating the effects of an innovation to the oil price variable using the system of equations 8 to 10. Among the results of this exercise are the standard impulse response functions, showing the dynamic impact of an oil price shock on the variables of the system, including the policy variables.

To simulate the effects of an oil price shock under a counterfactual policy regime, we first specify an alternative path for the federal funds rate—more specifically, deviations from the baseline impulse response of the funds rate—in a manner analogous to the approach of Sims and Zha.[38] However, we assume that financial markets understand and anticipate this alternative policy response; by assuming "maximum credibility" of the Fed's announced future policy, we stand in direct contrast to Sims and Zha, who assume that market participants are purely

38. Sims and Zha (1995).

backward-looking. To incorporate this assumption into the simulation, we calculate the expectations component of interest rates, $\overline{\mathbf{R}}_{t+i}$, $i = 0, 1, \ldots$, that is consistent with the proposed future path for the federal funds rate. We then resimulate the effects of the oil shock in the system of equations 8–10, imposing values of $\overline{\mathbf{R}}_t$ consistent with the assumed path of the funds rate, and also choosing values of $\epsilon_{ff,t}$ such that the assumed future path of the funds rate is realized. Note that this method can be used to construct alternative impulse response functions based on full-sample or subsample estimates and to simulate counterfactual economic behavior for specific episodes, such as the major oil price shocks. We use it in both ways below.

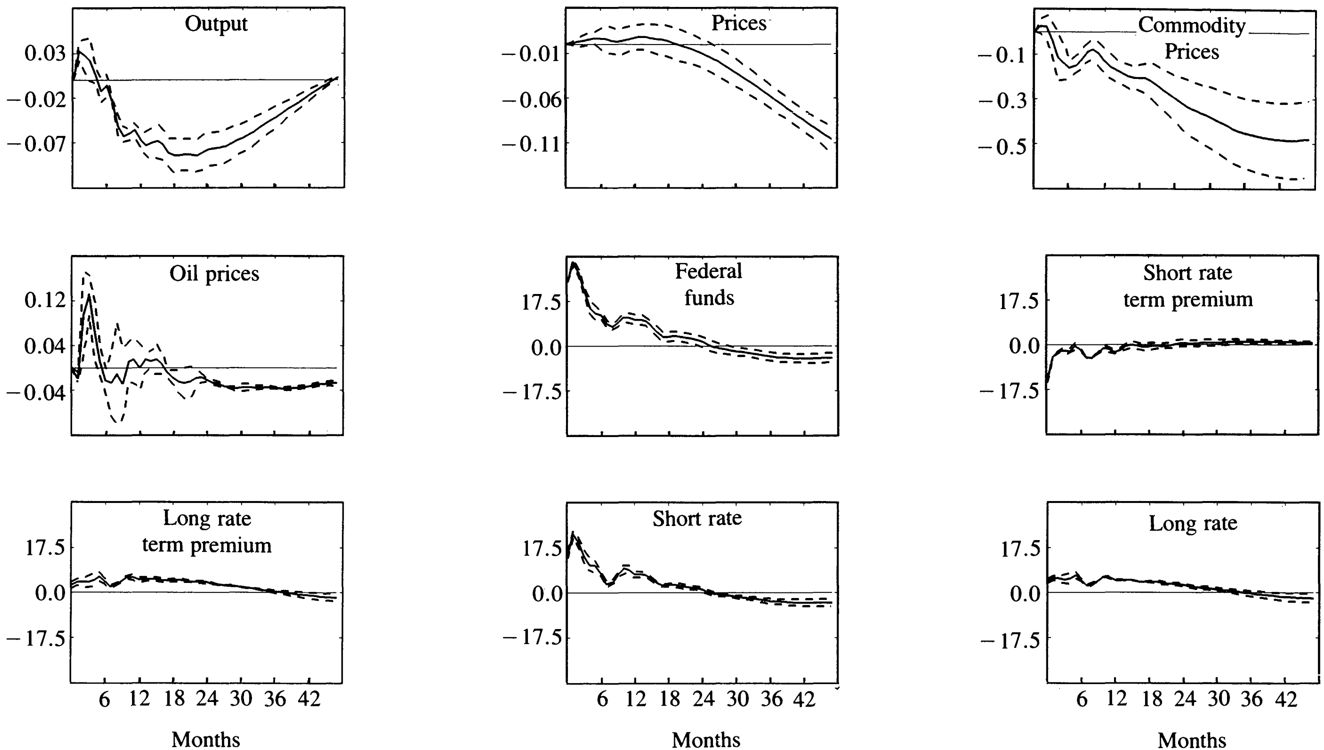
Some Policy Experiments

With the methodology described above, we are able to perform a variety of policy experiments, using estimates from our sample period, January 1965 through December 1995. The VAR is estimated using a constant and seven lags, as determined by AIC.

A Monetary Policy Shock

To check on the reasonableness of the basic estimated system, we begin with the conventional analysis of a monetary policy shock, modeled here as a 25 basis point innovation to the federal funds rate. The effects of an innovation to the federal funds rate are traced out in a seven-variable system that includes output, the price level, the commodity price index, the Hamilton oil measure, the funds rate, and the short and long term premiums. Figure 3 presents the resulting impulse response functions. As described above, the values of the short and long term premiums at each date are calculated by subtracting the expectations component of short and long rates (based on forecasts of future values of the funds rate) from the short and long rates themselves. In this base case analysis, equivalent results are obtained by directly including the short and long rates in the VAR (ordered after the funds rate), and the implied responses for short and long rates are included in figure 3. In the data, there are large low-frequency movements in the term premium of the long rate, with trend increases of about 1 per-

Figure 3. Responses to a Monetary Policy Shock, Seven-Variable System[a]



Source: Authors' VARs, using data described in appendix B.

a. Graphs show forty-eight month response of variables to a 25 basis point innovation in the federal funds rate. Vertical axis scales represent percent deviations of variables (basis point deviations of interest rate variables). Dashed lines represent one-standard-error bands. Sample period is 1965–95.

centage point in both the 1970s and the 1980s. We remove this trend variation with a cubic spline (specified as described in note 21). As we report in the section on robustness below, leaving the long premium undetrended does not significantly affect the results.[39] Impulse response functions to the funds rate innovation in figure 3 are shown with one-standard-error bands.

The results of this exercise will look quite familiar to those who know the recent VAR literature on the effects of monetary policy. The innovation to the funds rate (initially 25 basis points, peaking at about 35 basis points) is largely transitory, mostly dying away in the first nine months. Output declines relatively quickly, reaching a trough at about eighteen to twenty-four months and then gradually recovering. The price level responds sluggishly, but eventually declines, nearly two years after the policy innovation. Commodity prices also decline, and do so much more quickly than does the general price level.

The model's only exclusion restriction, that the funds rate does not belong in the "upper block" (which includes the oil indicator, output, prices, and commodity prices), conditional on the presence of short-term and long-term interest rates in that block, is marginally rejected: the p values for the exclusion of the funds rate from the upper block are, respectively, 0.01 for the output equation, 0.06 for the price level equation, 0.23 for the commodity price equation, and 0.18 for the oil equation. However, the effects of this exclusion do not seem to be economically very significant. For example, if we compare the effects of a funds rate shock on output in the restricted, seven-variable system with the analogous effects in the conventional, unrestricted, five-variable system (excluding the market interest rates), we obtain virtually identical results.

An interesting new feature of the seven-variable system is that it allows one to examine the responses of market interest rates to monetary policy innovations, and in particular, to compare these responses to the predictions of the pure expectations hypothesis. Looking first at short-

39. Fuhrer (1996) shows that the large movements in the long rate can be explained in a way consistent with the expectations hypothesis if the market was making rate forecasts at each date based on a particular set of beliefs about how the Federal Reserve's objective function has varied over time. However, there is nothing in Fuhrer's analysis that connects these hypothesized beliefs with the actual time-series behavior of the funds rate.

term (three-month) rates, a 25 basis point innovation to the funds rate implies about a 15 basis point increase in the short rate, and the two rates then decline synchronously. This seems quantitatively reasonable. To check the consistency of this response with the expectations hypothesis, one can look at the behavior of the short rate term premium, which, by construction, is the difference between the actual short term rate and the short term rate implied by the pure expectations hypothesis. The short rate term premium is significantly negative immediately following a funds rate innovation, implying that in the first month or two after an innovation to the funds rate, the short-term interest rate is estimated to respond less than would be predicted by the expectations hypothesis. However, the short rate term premium quickly becomes statistically and economically insignificant, suggesting that the expectations hypothesis is a reasonable description of the link between the funds rate and the short-term interest rate after the first month.

The long-term interest rate is a different story. As shown in figure 3, the long rate responds by about 5 basis points to the impact of a 25 basis point innovation in the funds rate, and the response remains above zero for some three years, which again does not seem unreasonable. However, comparison of the responses of the long-term interest rate and the long rate term premium reveals that they are very close, the latter being slightly less than the former. The implication is that the expectations theory explains relatively little of the relationship between the funds rate and the ten-year government bond rate. This finding is not so surprising, given the transitory nature of funds rate shocks compared with the duration of these bonds. The estimated behavior of the long term premium thus constitutes some evidence that long rates "overreact" to short rates, a phenomenon that has frequently been documented in the term structure literature (although, we appear to find less overreaction than is typically reported in the literature).[40]

Simulations of the Effects of an Oil Price Shock

Since our expanded model seems to perform reasonably in the case of an innovation to monetary policy, we now turn to the exercise of

40. An alternative explanation for the overreaction of the long rate is that the policy shock is imperfectly identified. Note, for example, the slight "output puzzle"—output increases in the first few months after the policy shock. Possibly a better identification scheme would eliminate the overreaction.

greatest interest, which is to use the model to decompose the effects of an oil price shock into direct and indirect (that is, through endogenous monetary policy) components. Figure 4 shows impulse responses following a shock to Hamilton's net oil price increase measure under three scenarios.

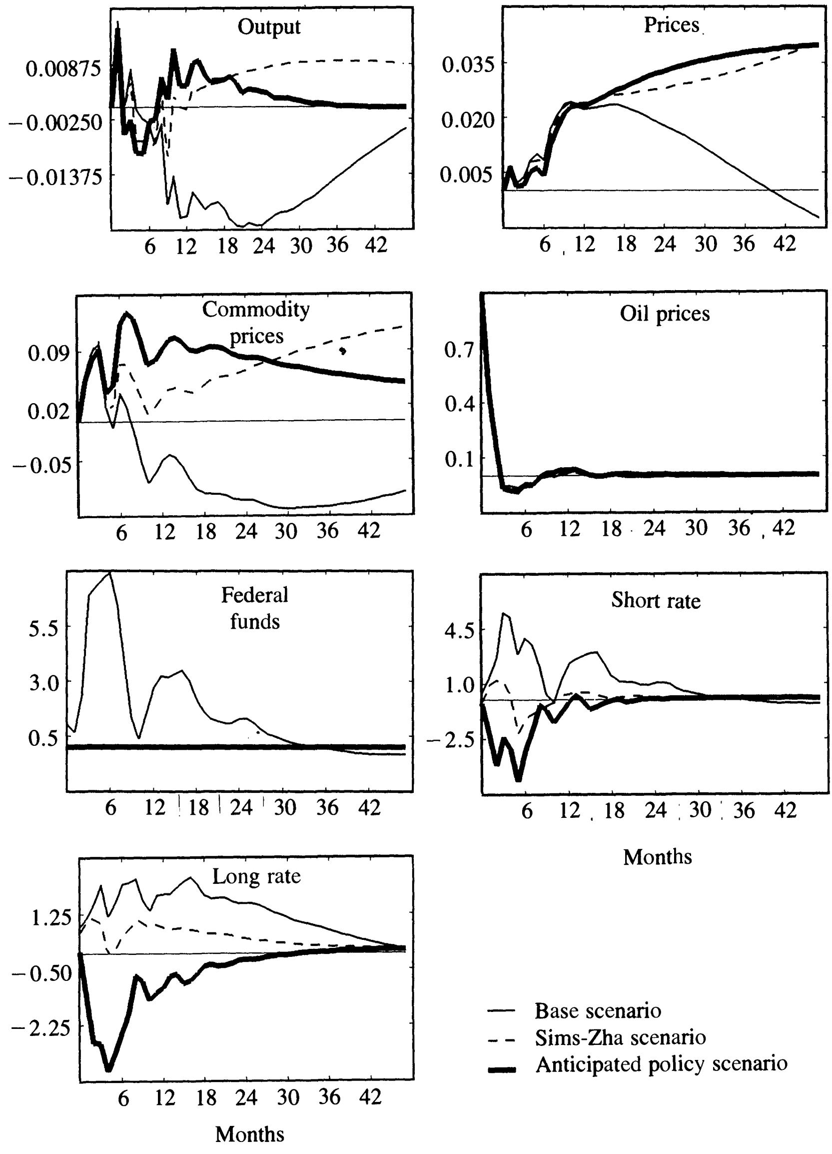
The first scenario, which we label "base," shows the impulse responses of the variables to a 1 percent innovation in the nominal price of oil in the seven-variable system. This is a normal VAR simulation, except that the funds rate does not enter directly into the equations for output, prices, commodity prices, or the oil indicator. This case is intended to show the effects on the economy of an oil price shock, *including* the endogenous response of monetary policy, in contrast with the next two simulations, which involve alternative methods of shutting off the policy response.

The second scenario we label "Sims-Zha" (with some abuse of terminology). In this case we simply fix the funds rate at its base values throughout the simulation, in the manner of Sims and Zha.[41] However, recall that in contrast to the original Sims-Zha exercise, in our system the funds rate does not enter directly into the block of macroeconomic variables. Rather, the funds rate exerts its macroeconomic effects only indirectly, through the short-term and long-term interest rates included in the system. Thus in this exercise, we are effectively allowing the change in the funds rate to act through its unconstrained, reduced-form impact on market interest rates (which are ordered after the funds rate).

The third scenario, which we label "anticipated policy," applies our own methodology, described above. We again set the funds rate equal to its baseline values; that is, we shut off the response of monetary policy to the oil shock and the changes induced by the oil shock in output, prices, and so forth. But in this case, we let the two components of short-term and long-term interest rates be determined separately. The expectations component of both interest rates is set to be consistent with the future path of the funds rate, as assumed in the scenario. The short and long term premiums are allowed to respond as estimated in the base model. (Below, we also consider a case where the term premiums are kept at their baseline values.) For the simple, constant funds rate case being examined here, the Sims-Zha and anticipated policy approaches

41. Sims and Zha (1995).

Figure 4. Responses to a Hamilton Oil Price Shock, Seven-Variable System[a]



Source: Authors' VARs, using data described in appendix B.

a. Graphs show forty-eight month response of variables to a 1 percent Hamilton oil price shock. Sims-Zha and anticipated policy scenarios eliminate the normal response of monetary policy. Vertical axis scales represent percent deviations of variables (basis point deviations of interest rate variables). Sample period is 1965-95.

show roughly similar departures from baseline. Note, however, that the former cannot distinguish between policies that differ only in the expected future values of the funds rate, whereas, in principle, the latter approach can make that distinction.

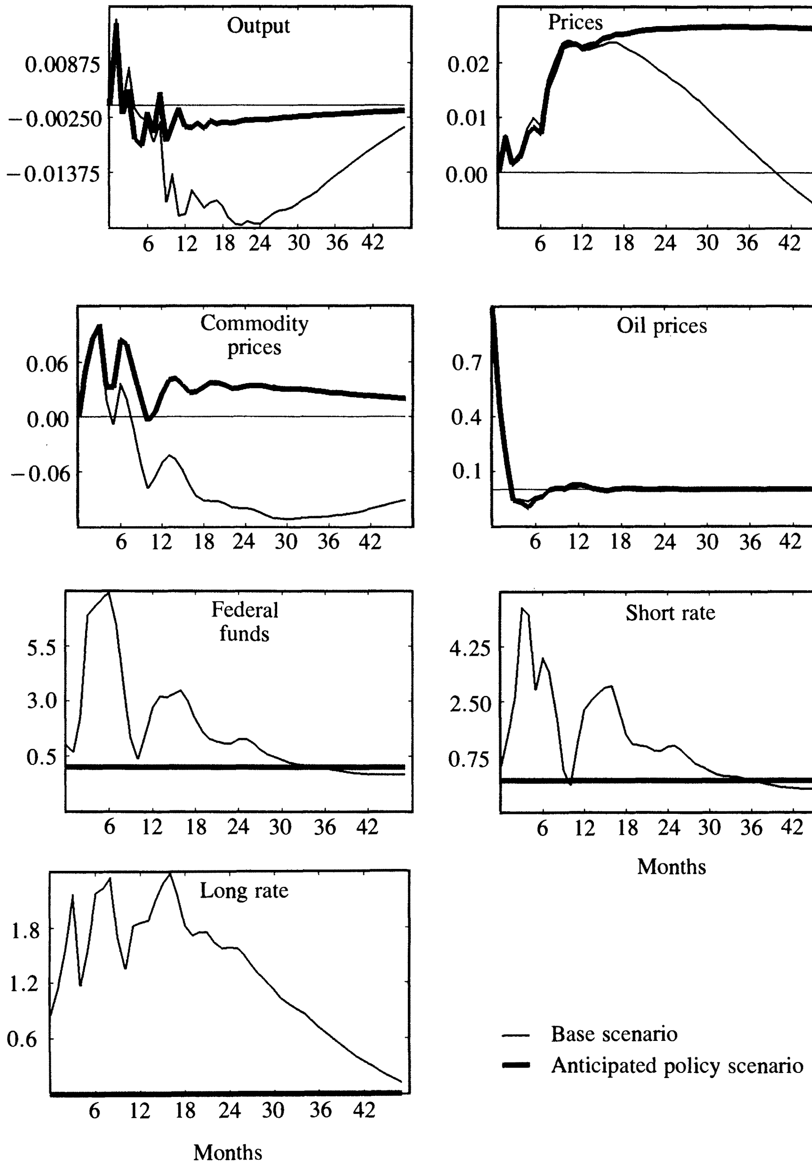
The results of figure 4 are reasonable, with all variables exhibiting their expected qualitative behaviors. In particular, the absence of an endogenously restrictive monetary policy results in higher output and prices, as one would anticipate. Quantitatively, the effects are large, in that a nonresponsive monetary policy suffices to eliminate most of the output effect of an oil price shock, particularly after the first eight to ten months. The conclusion that a substantial part of the real effects of oil price shocks is due to the monetary policy response helps to explain why the effects of these shocks seems larger than can easily be explained in neoclassical (flexible price) models.[42]

The anticipated policy simulation results in modestly higher output and price responses than the Sims-Zha simulation in figure 4. The differences in results occur largely because the anticipated policy simulation involves a negative short-run response in both the short and long term premiums, and thus lower interest rates in the short run. Figure 5 repeats the anticipated policy simulation of figure 4, but with the response of the term premiums shut off; that is, the funds rate is allowed to affect the macroeconomic variables only through its effects on the expectations component of market rates. This alternative simulation attributes somewhat less of the recession that follows an oil shock to the monetary policy response, but endogenous monetary policy still accounts for two-thirds to three-fourths of the total effect of the oil price shock on output.

As another exercise in counterfactual policy simulation, we examine the three major oil price shocks followed by recessions: OPEC 1, OPEC 2, and the Iraqi invasion of Kuwait. Figure 6 shows the results, focusing on the behavior of three key variables (output, the price level, and the funds rate) for the five-year periods surrounding each of these episodes (respectively, 1972–76, 1979–83, and 1988–92). Each panel shows three paths of the given variable. One line depicts the actual historical path of the variable. The line marked "federal funds endog-

42. It should be emphasized that we are not arguing that the policies actually followed by the Fed in the face of oil shocks were necessarily suboptimal; the usual output-inflation trade-off is present in our simulations, and we do not attempt a welfare analysis.

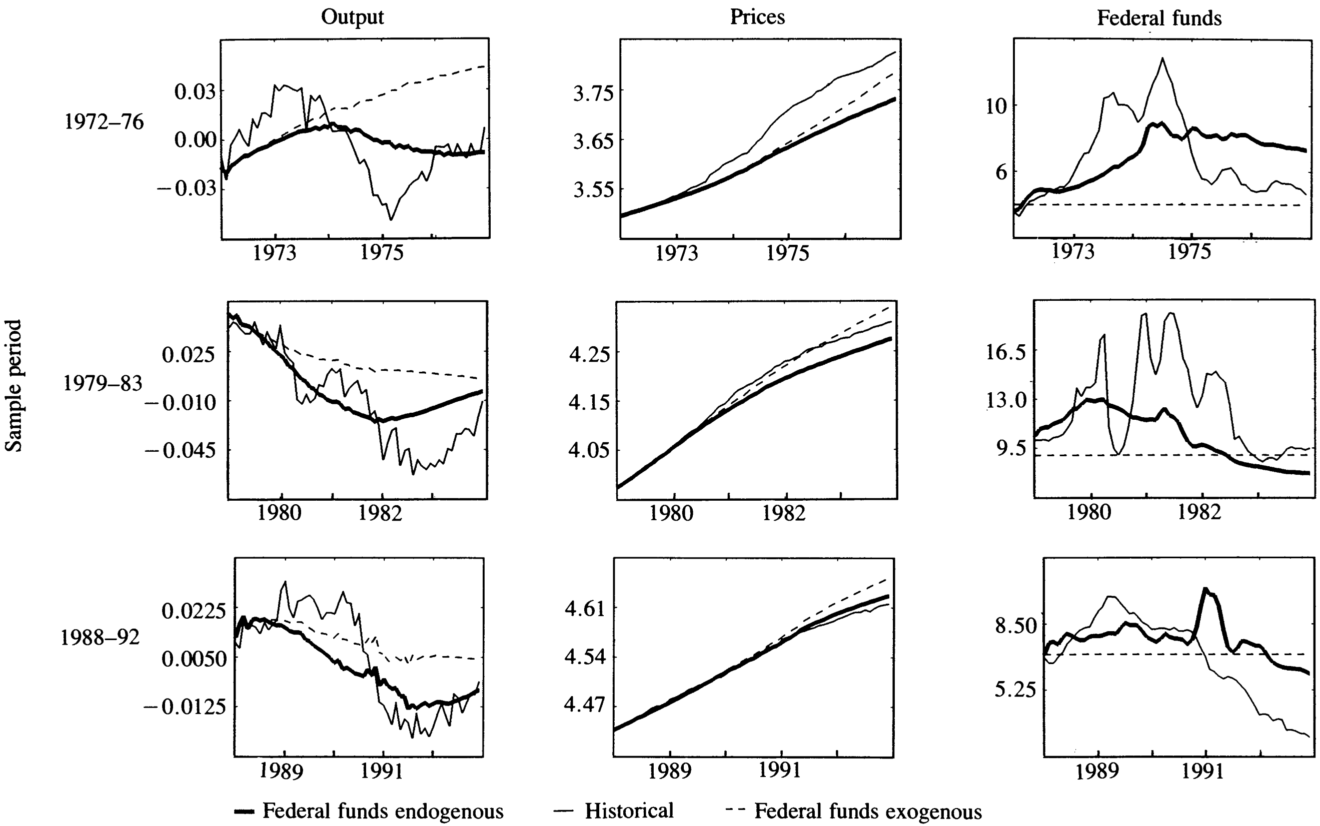
Figure 5. Responses to a Hamilton Oil Price Shock, No Premium Term Response[a]



Source: Authors' VARs, using data described in appendix B.

a. Graphs show forty-eight-month response of variables to a 1 percent Hamilton oil price shock. Scenarios are as those shown in figure 4, except that the responses of term premiums are shut off. Vertical axis scales represent percent deviations of variables (basis point deviations of interest rate variables). Sample period is 1965–95.

Figure 6. Simulating Three Historical Oil Price Shocks[a]



Source: Authors' VARs, using data described in appendix B.

a. For each of three historical episodes, graphs compare actual behavior of variables with their predicted responses under two alternative scenarios, described in the text. Vertical axis scales show log level of (detrended) output, log level of prices, and the federal funds rate in percent.

enous'' shows the behavior of the system when the oil variable is repeatedly shocked, so that it traces out its actual historical path; all other shocks in the system are set to zero; and the funds rate is allowed to respond endogenously to changes in the oil variable and the induced changes in output, prices, and other variables. This scenario is intended to isolate the portion of each recession that results solely from the oil price shocks and the associated monetary policy response. Finally, the line marked ''federal funds exogenous'' describes the results of an exercise in which oil prices equal their historical values, all other shocks are shut off, and the nominal funds rate is arbitrarily fixed at a value close to its initial value in the period. (Term premiums are allowed to respond to the oil price shock.) This last scenario eliminates the policy component of the effect of the oil price shock, leaving only the direct effect of the change in oil prices on the economy.

Several observations can be made from figure 6. First, the 1974–75 decline in output is generally not well explained by the oil price shock. The pattern of shocks reveals, instead, that the major culprit was (non-oil) commodity prices. Commodity prices (not shown) rose very sharply before this recession and stimulated a sharp monetary policy response of their own, as can be seen by comparing the historical path of the funds rate with its path in the federal funds endogenous scenario, in which the commodity price shocks are set to zero. The federal funds exogenous scenario, in which the funds rate responds to neither commodity price nor oil price shocks, exhibits no recession at all, suggesting that endogenous monetary policy (responding to both oil price and commodity price shocks) did, indeed, play an important role in this episode.

The results for 1979–83 generally conform to the conventional wisdom. The decline in output through 1981 is well explained by the 1979 oil price shock and the subsequent response of monetary policy. After the beginning of 1982, the main source of output declines (according to this analysis) was the lagged effect of the autonomous tightening of monetary policy in late 1980 and 1981. Note that if one excludes both the monetary policy reaction to the oil price shocks and the autonomous tightening of monetary policy by Federal Reserve Chairman Paul Volcker (as in the federal funds exogenous scenario), the 1979–83 period exhibits only a modest slowdown, not a serious recession.

The experiment for 1988–92 similarly shows that shutting off the policy response to oil price shocks produces a higher path of output and prices than otherwise; again, compare the paths of the endogenous monetary policy and exogenous monetary policy scenarios. One puzzle that emerges is why the substantial easing of actual policy from late 1990 did not move the actual path of output closer to the alternative policy scenario. It is possible that special factors, such as credit problems, may have been at work.

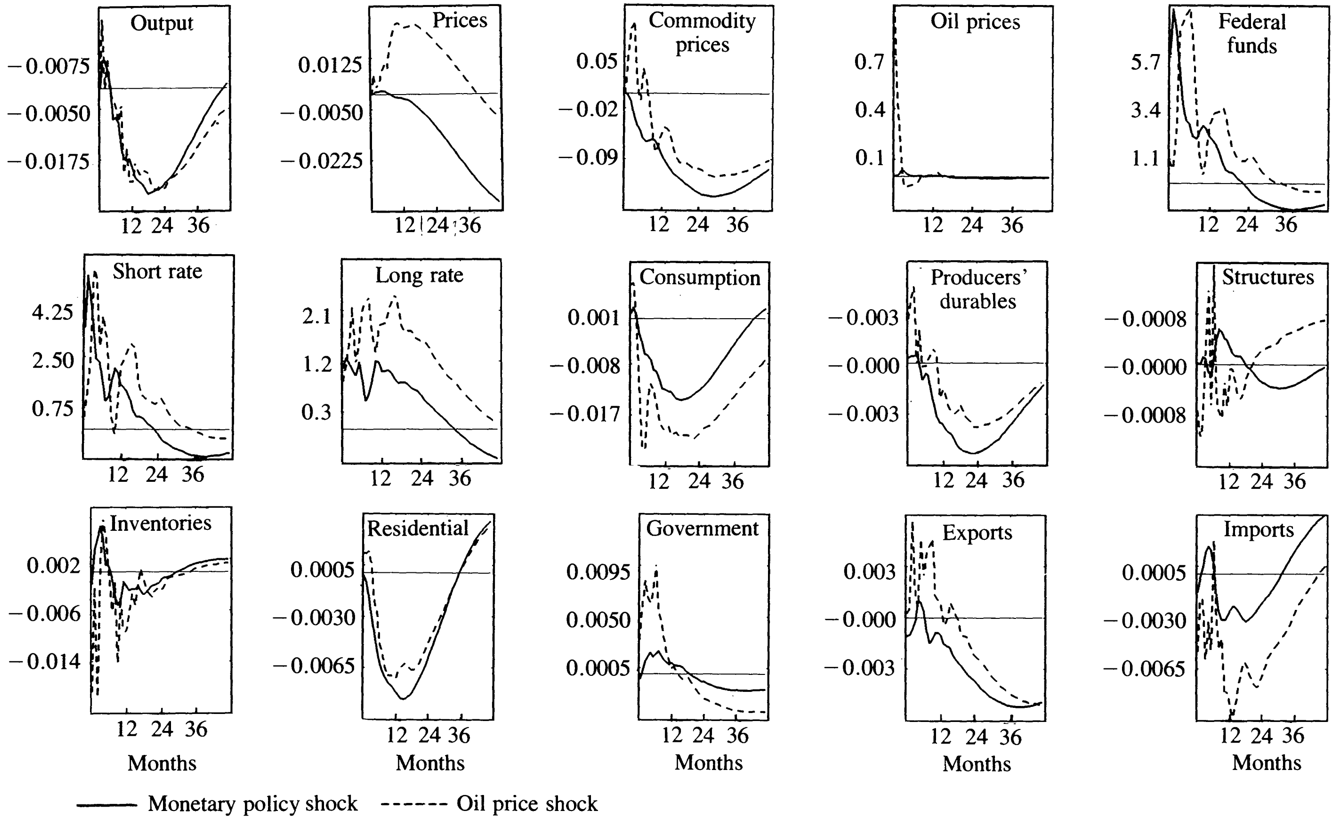
Oil, Money, and the Components of GDP

The application of our method for separating the direct effects of oil price shocks and the indirect effects operating through the monetary policy response leads to a rather strong conclusion: the majority of the impact of an oil price shock on the real economy is attributable to the central bank's response to the inflationary pressures engendered by the shock.

A check on the plausibility of this result, using a different identifying assumption and more disaggregated data, is provided by figure 7. This figure is based on the seven-variable VAR system employed above (real GDP, the GDP deflator, commodity prices, the Hamilton oil market indicator, the funds rate, and short-term and long-term interest rates), with the funds rate excluded from the first four equations. To this system we add, one at a time and without feedback into the main system, eight components of GDP: consumption, producer durables expenditure, structures investment, inventory investment, residential investment, government purchases, exports, and imports.[43] With these systems we conduct two experiments. First, we examine the impulse responses obtained when the Hamilton oil price variable is shocked by 1 percent and the federal funds rate is allowed to respond endogenously (these responses are shown by dashed lines in figure 7). Second, we examine the impulse responses to an exogenous federal funds rate shock of equal maximum value to the endogenous response of the funds rate in the first scenario (shown by solid lines). We think of this exercise as a comparison of the total effect of an oil price shock, including the

43. Except for consumption, which is available at the monthly frequency, monthly data for the GDP components are interpolated by state space methods; see appendix A. Components are measured relative to the exponential of the trend for the logarithm of real GDP, as calculated from the spline regression described in note 21.

Figure 7. Sectoral Responses to Oil Price and Monetary Policy Shocks[a]



Source: Authors' VARs, using data described in appendix B.

a. Graphs show forty-eight month response of variables to a 1 percent Hamilton oil price shock and to a federal funds rate innovation comparable to that resulting from the oil price shock. Vertical axis scales represent percent deviations of variables (basis point deviations of interest rate variables). Sample period is 1965–95.

endogenous monetary response, with the effect of a monetary tightening of similar magnitude but not associated with an oil price shock. To the extent that the two responses are quantitatively similar, it seems reasonable to attribute most of the total effect of the oil price shock to the monetary policy response. Note, however, that we are using a different identification assumption here than above; that is, we implicitly assume that the economy responds in the same way to endogenous and exogenous tighenings of monetary policy.

The results of shown in figure 7 provide substantial support for the view that the monetary policy response is the dominant source of the real effects of an oil price shock. In particular, the response of output is virtually identical in the two scenarios, implying that it matters little for real economic outcomes whether a change in monetary policy of a given magnitude is preceded by an oil price shock or not. Very similar responses across the two experiments are also found at the disaggregated level, especially in equipment investment (producers' durable equipment), inventory investment, and residential investment. Slightly greater effects for the scenario including the oil price shock are found for consumption and structures (although the latter difference is quantitatively small and statistically insignificant). Government purchases responds more strongly in the scenario that includes the oil price shock, for reasons that are not obvious.

The differences between the two scenarios are also instructive. The experiment that includes the initial oil price shock does show a substantial inflationary impact in the short run, which gives some indication as to why the Fed responds so vigorously to such shocks. On the margin, the oil price shock also raises commodity prices and the long-term interest rate (presumably, reflecting an increased risk premium) and it leads to increased real exports and decreased real imports (net of terms-of-trade effects). These responses are as expected.

Some Alternative Experiments

Although we have focused on the role of systematic monetary policy in propagating oil price shocks, our methodology applies equally well to other sorts of driving shocks. As a further check on the plausibility of our method, we briefly consider two alternative cases: a shock to commodity prices and a shock to output.

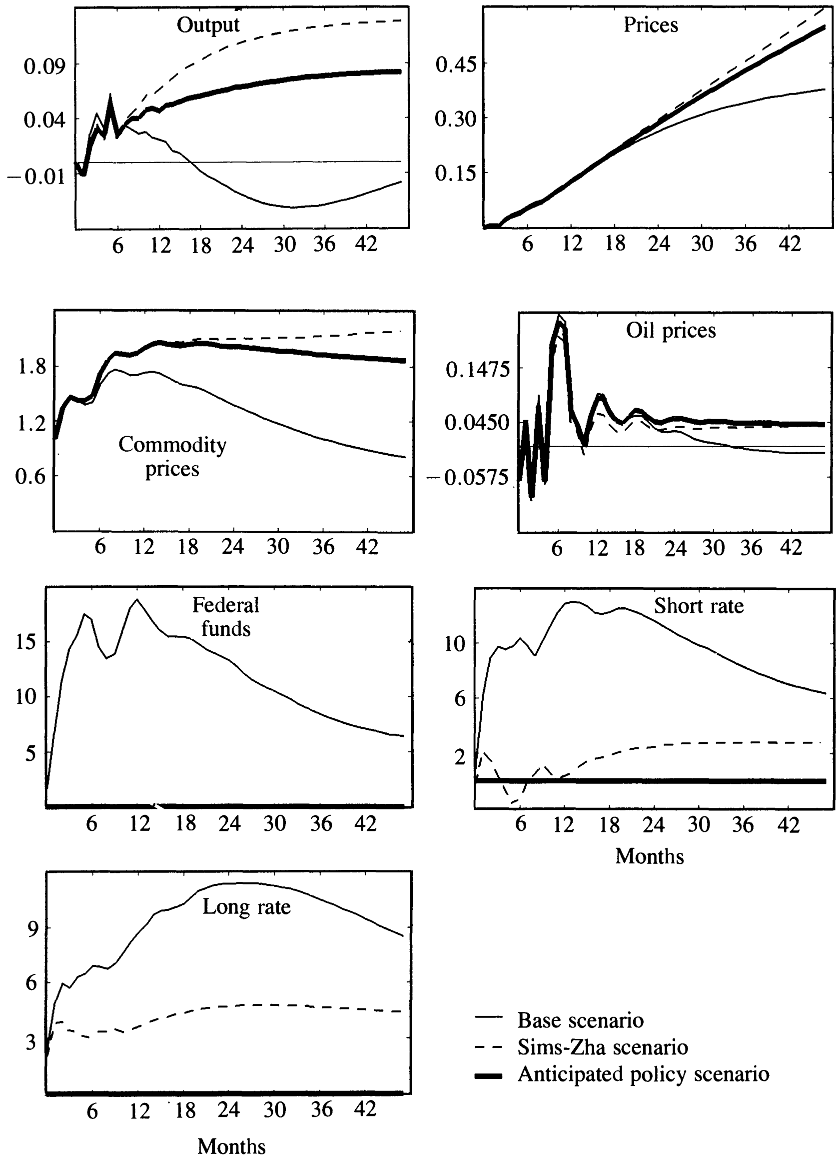
A COMMODITY PRICE SHOCK. Figure 8 looks at the effects of a shock to the commodity price index in our original seven-variable system. As with the oil price shock studied in figures 4 and 5, we consider three scenarios. First, in the base scenario we calculate the impulse responses resulting from a 1 percent innovation in commodity prices, allowing monetary policy (as represented by the federal funds rate) to respond in its normal way. Second, we examine the effects of shutting off the policy response, using the Sims-Zha methodology described above. Finally, we shut off the monetary policy response by means of our anticipated policy approach. For simplicity, in the anticipated policy simulation we set the responses of the term premiums to zero (as in figure 5), so that both short-term and long-term nominal interest rates are effectively assumed not to respond to the shock to commodity prices.

Figure 8 shows that a 1 percent innovation in commodity prices has an ambiguous effect on output: real GDP rises for the first year but declines thereafter. Prices rise unambiguously. One explanation for these results is that what we are labeling a positive shock to commodity prices is, in fact, a mixture of an adverse shock to aggregate supply and an expansionary shock to aggregate demand. The federal funds rate rises sharply in response to an increase in commodity prices, which we interpret as the Fed's response to the inflationary surge; other interest rates also rise. The oil price indicator responds very little in the short run to a commodity price innovation, which is reassuring, in the sense that it confirms that the commodity and oil price variables are not excessively collinear.

Shutting down the monetary policy response to the commodity price shock, by either the Sims-Zha or the anticipated policy method, leads to the expected response. Analogous to the case of oil price shocks, the recessionary impact of a commodity price shock is eliminated and the inflationary impact is magnified. Although it may well be the case that the innovation in commodity prices is not a cleanly identified supply shock, there is no evidence that an increase in commodity prices depresses real activity in the absence of a monetary policy response.

AN OUTPUT SHOCK. Figure 9 shows analogous results when the driving shock is a shock to output. As with the commodity shock, we compute

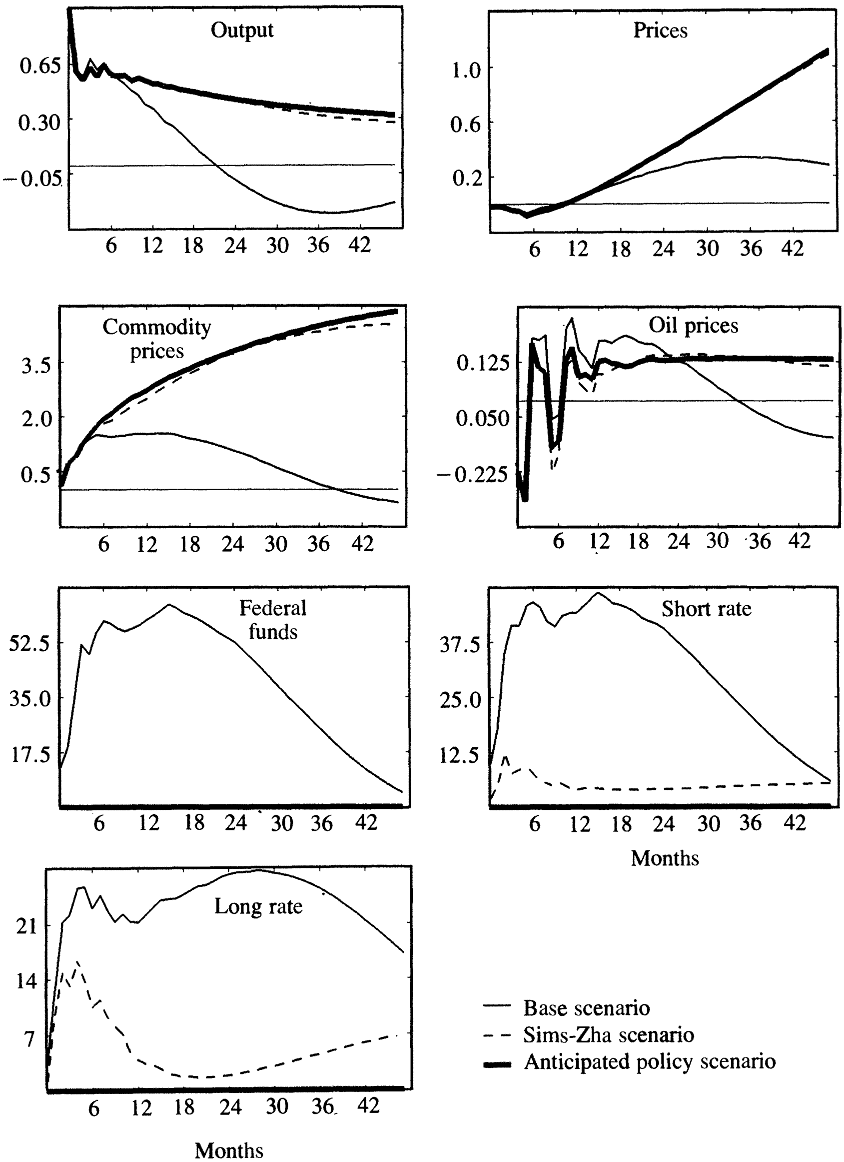
Figure 8. Responses to a Commodity Price Shock, No Term Premium Response[a]



Source: Authors' VARs, using data described in appendix B.

a. Graphs show forty-eight month response of variables to a 1 percent commodity price shock when the responses of term premiums are shut off. Sims-Zha and anticipated policy scenarios eliminate the normal response of monetary policy. Vertical axis scales represent percent deviations of variables (basis point deviations of interest rate variables). Sample period is 1965–95.

Figure 9. Responses to an Output Shock, No Term Premium Response[a]



Source: Authors' VARs, using data described in appendix B.

a. Graphs show forty-eight month response of variables to a 1 percent output shock when the responses of term premiums are shut off. Sims-Zha and anticipated policy scenarios eliminate the normal response of monetary policy. Vertical axis scales represent percent deviations of variables (basis point deviations of interest rate variables). Sample period is 1965–95.

the impulse response functions for three cases: a base case in which monetary policy is allowed to respond in its normal way to the output shock, and cases corresponding to the Sims-Zha and anitcipated policy methods for shutting down the policy response. As before, we assume no response of the term premiums.

Admittedly, like a shock to commodity prices, an output shock does not have a clear a priori economic interpretation; it is an amalgam of various random factors affecting output, holding constant the other variables included in the system. However, based on figure 9 it seems reasonable to interpret output shocks in this system as being dominated by aggregate demand fluctuations: a positive output shock is followed by increases in oil prices, commodity prices, and the general price level, as well as in all three interest rates. Because the historical tendency of monetary policy is to "lean against the wind," when the normal policy response is shut off, the effects of the aggregate demand shock (as we interpret the output shock) are all the greater. Figure 9 shows that in the Sims-Zha and anticipated policy scenarios, the output effect of the shock is much more persistent and prices rise by more than in the base case. Interest rates are lower, reflecting easier monetary policy. Note that in this analysis, the Sims-Zha and anticipated policy approaches give almost identical results.

These experiments demonstrate that our methods for shutting down the response of monetary policy are applicable to, and give reasonable results for, shocks other than oil price shocks. It would be interesting to combine our methodology with identified VAR techniques that could give a sharper structural interpretation to innovations estimated in the macro block of the model.

Robustness and Stability

We return to our main theme, the role of systematic monetary policy in amplifying the real effects of oil price shocks, to consider the robustness and stability of our results.

Robustness of the Results

We perform a variety of checks for robustness, some of which (such as shutting down the term premium response) are alluded to above. To provide more systematic information, table 1 reports some summary statistics from alternative specifications of our VAR system. We consider (a) three alternative oil-market indicators; (b) three alternative orderings of variables within the VAR; and (c) two alternative detrending assumptions. We also calculated results for alternative measures of output (for example, industrial production), alternative measures of the price level (for example, the personal consumption expenditure deflator and the consumer price index), and alternative interest rate maturities; but since none of these variable substitutions have important effects on our findings, they are omitted from the table.

The first row of table 1 reports results for the Hamilton oil indicator (our base specification), whereas the second and third rows substitute the Mork and Hoover-Perez indicators, respectively (see figure 2). The fourth row corresponds to ordering the federal funds rate after, rather than before, the two open market interest rates. The fifth row orders the Hamilton oil market indicator first in the system, and the sixth row orders the oil market indicator third—after output and prices, but before the commodity price index. The seventh row is for a specification in which output and the long rate term premium are not detrended, and the eighth row reports results when all variables in the system are detrended by a cubic spline (as described in note 21).

For each of the eight alternative specifications, table 1 reports the effects on output and prices of a 1 percent oil price shock, under (a) a standard simulation, allowing for the endogenous response of policy to the oil price shock; (b) the Sims-Zha simulation, in which the federal funds rate is fixed at its baseline value; and (c) the anticipated policy simulation. Under the heading ''output,'' we report the sum of the impulse response coefficients for output for the first twenty-four months after the oil price shock, which we employ as a measure of the output loss associated with the shock. Under the heading ''prices,'' we report the twenty-fourth impulse response coefficient for prices, divided by two, which can be interpreted as the increment in the annual average inflation rate over the first two years following the shock. Standard errors, calculated by Monte Carlo methods employing 500 draws per specification, are shown in parentheses. The table also shows the differences between the baseline (endogenous policy) specification and the results obtained under the Sims-Zha and anticipated policy assumptions, again with the associated standard errors.

Table 1. Robustness of Results to Alternative Specifications[a]

	Baseline scenario		*Sims-Zha scenario*		*Anticipated policy scenario*	
Specification	*Output*	*Prices*	*Output*	*Prices*	*Output*	*Prices*
Oil market indicator						
Hamilton	−0.308	0.009	0.133	0.013	0.179	0.016
	(0.334)	(0.014)	(0.361)	(0.015)	(0.565)	(0.022)
Difference from baseline	. . .	. . .	0.440	0.004	0.486	0.007
			(0.156)	(0.006)	(0.460)	(0.018)
Mork	−0.146	0.002	0.047	0.004	0.048	0.006
	(0.237)	(0.010)	(0.245)	(0.010)	(0.507)	(0.027)
Difference from baseline	. . .	. . .	0.193	0.002	0.194	0.004
			(0.065)	(0.003)	(0.449)	(0.026)
Hoover-Perez	−0.590	0.013	0.312	0.025	0.103	0.038
	(0.444)	(0.017)	(0.540)	(0.010)	(1.030)	(0.047)
Difference from baseline	. . .	. . .	0.901	0.012	0.693	0.025
			(0.355)	(0.013)	(0.920)	(0.045)
Ordering						
Federal funds last	−0.304	0.007	−0.079	0.009	0.237	0.015
	(0.356)	(0.015)	(0.371)	(0.015)	(0.682)	(0.024)
Difference from baseline	. . .	. . .	0.225	0.001	0.541	0.008
			(0.116)	(0.004)	(0.560)	(0.020)

Oil price first	−0.430	0.006	−0.111	0.009	0.012	0.011
	(0.391)	(0.015)	(0.407)	(0.015)	(0.463)	(0.017)
Difference from baseline	. . .	. . .	0.319	0.003	0.441	0.006
			(0.111)	(0.004)	(0.249)	(0.008)
Oil price third	−0.335	0.006	0.037	0.010	0.180	0.012
	(0.331)	(0.014)	(0.360)	(0.015)	(0.525)	(0.017)
Difference from baseline	. . .	. . .	0.373	0.004	0.515	0.006
			(0.145)	(0.006)	(0.404)	(0.011)
Detrending						
None	−0.065	0.006	0.195	0.008	0.349	0.008
	(0.360)	(0.015)	(0.368)	(0.015)	(0.571)	(0.023)
Difference from baseline	. . .	. . .	0.260	0.001	0.414	0.002
			(0.076)	(0.003)	(0.439)	(0.018)
All variables	−0.334	0.009	−0.034	0.000	0.330	−0.009
	(0.323)	(0.007)	(0.323)	(0.006)	(0.499)	(0.015)
Difference from baseline	. . .	. . .	0.300	−0.009	0.664	−0.018
			(0.099)	(0.002)	(0.458)	(0.014)

Source: Authors' calculations using data described in appendix B.

a. For eight different specifications (see text) and for the baseline, Sims-Zha, and anticipated policy assumptions regarding the response of monetary policy to an oil shock, the table shows the sum of impulse response coefficients over twenty-four months for output and the annualized inflation rate (the impulse response coefficient on month 24 divided by 2) resulting from a 1 percent shock to oil prices. Standard errors are in parentheses. Also shown are the differences in output and inflation effects from the Sims-Zha and anticipated policy simulations, relative to the baseline simulation, with standard errors. Estimates and standard errors are constructed by Monte Carlo methods, using 500 draws for each simulation.

The point estimates reported in table 1 are consistent with the findings discussed above (in figures 4 and 5, for example). In particular, the baseline simulations show that an oil price shock depresses output and increases inflation, by magnitudes that are reasonably comparable across all specifications. The Sims-Zha method of shutting off the monetary policy response tends to eliminate all or most of the negative effect of the oil price shock and, in almost all cases, increases the inflationary impact, as expected. The anticipated policy method of eliminating the policy response has even larger effects, fully eliminating the recessionary impact of the oil price shock in all cases. The standard errors for most entries in table 1 are quite high, reflecting the fact that the standard error bands on the impulse response functions spread out rather quickly.[44] However, the differences in the output responses between the baseline and alternative simulations are statistically significant in a number of cases, in particular, when the policy response is shut down by the Sims-Zha method.[45]

In general, our results appear to be qualitatively robust, although they are not always precisely estimated. In particular, a view that ascribes most or even all of the real effects of an oil price shock to the endogenous monetary response does not seem inconsistent with the data.

Stability of the Results: The Role of a Changing Policy Response

We take up the issue of subsample stability not only as a qualification of our results, but also because it appears that at least some of the observed instabilities of our system can be given an interesting economic interpretation. Indeed, we show that variations in the Federal Reserve's reaction function have something of the flavor of a natural

44. The standard errors are particularly high for the anticipated policy simulations, apparently reflecting, in part, the uncertainty associated with the long-term interest rate forecasts required by this method.

45. We also considered alternative models estimated with twelve lags, rather than the seven chosen by AIC. In this case, the finding that shutting off the monetary policy response eliminates the effect of the oil shock obtains at short horizons but not at the twenty-four-month horizon. The reason is that with twelve lags, the funds rate is estimated to rise in response to an oil price shock, but then to fall quickly below trend. Our alternative policy, which assumes no response throughout, is thus not effectively easier than the baseline policy over the twenty-four-month horizon.

experiment, which may help to improve the identification of the endogenous policy effect.

Some tests of the stability of the coefficients in our seven-variable base VAR, with lag lengths chosen by the Bayes information criterion, are reported in table 2. For simplicity, the funds rate is allowed to enter all equations. The upper panel, labeled ''Quandt tests,'' gives asymptotic *p* values for the hypothesis that the coefficients of the variable listed in the column heading, together with the regression constant term, are stable over the sample period in the equation given by the row heading. Thus, for example, the Quandt tests show that the hypothesis that the coefficients on the price level in the oil equation are stable over the entire sample can be rejected at the 0.016 confidence level. In a similar format, the Chow split-sample tests reported in the lower panel of table 2 tests each set of coefficients for stability across the two halves of the sample. These tests are included because, unlike the Quandt tests, they are robust to heteroskedasticity.

There is substantial evidence of instability in the VAR system. The equation for the price level is clearly quite unstable, with *p* values near zero for most blocks of coefficients. The Quandt tests also suggest that there is instability in the coefficients relating the funds rate and the short-term and long-term interest rates. Nevertheless, stability of the output equation cannot be rejected.

It appears, however, that at least some of the instability in the link between oil and the macroeconomy may be due to a shift in the policy response. Figure 10 illustrates this point. The figure shows the output, price level, and federal funds rate responses to an oil price shock, as implied by systems estimated over the whole sample and over each of the three decades of the sample (1966–75, 1976–85, and 1986–95).

The full sample estimates of the effects of an oil price shock are as seen above. Note, though, how the responses vary over subsamples (keeping in mind that ten-year subsamples are short for this purpose). The output response across different periods is inversely correlated with the funds rate response. The sharpest decline in output occurs in the period 1976–85, which also exhibits the most aggressive rise in the funds rate. The strong response of monetary policy during this period presumably reflects the Federal Reserve's substantially increased concern with inflation during the Volcker regime. The output response is weakest in the 1986–95 subsample. In this case, there is virtually no

Table 2. Tests for Stability of Coefficients in the VAR[a]

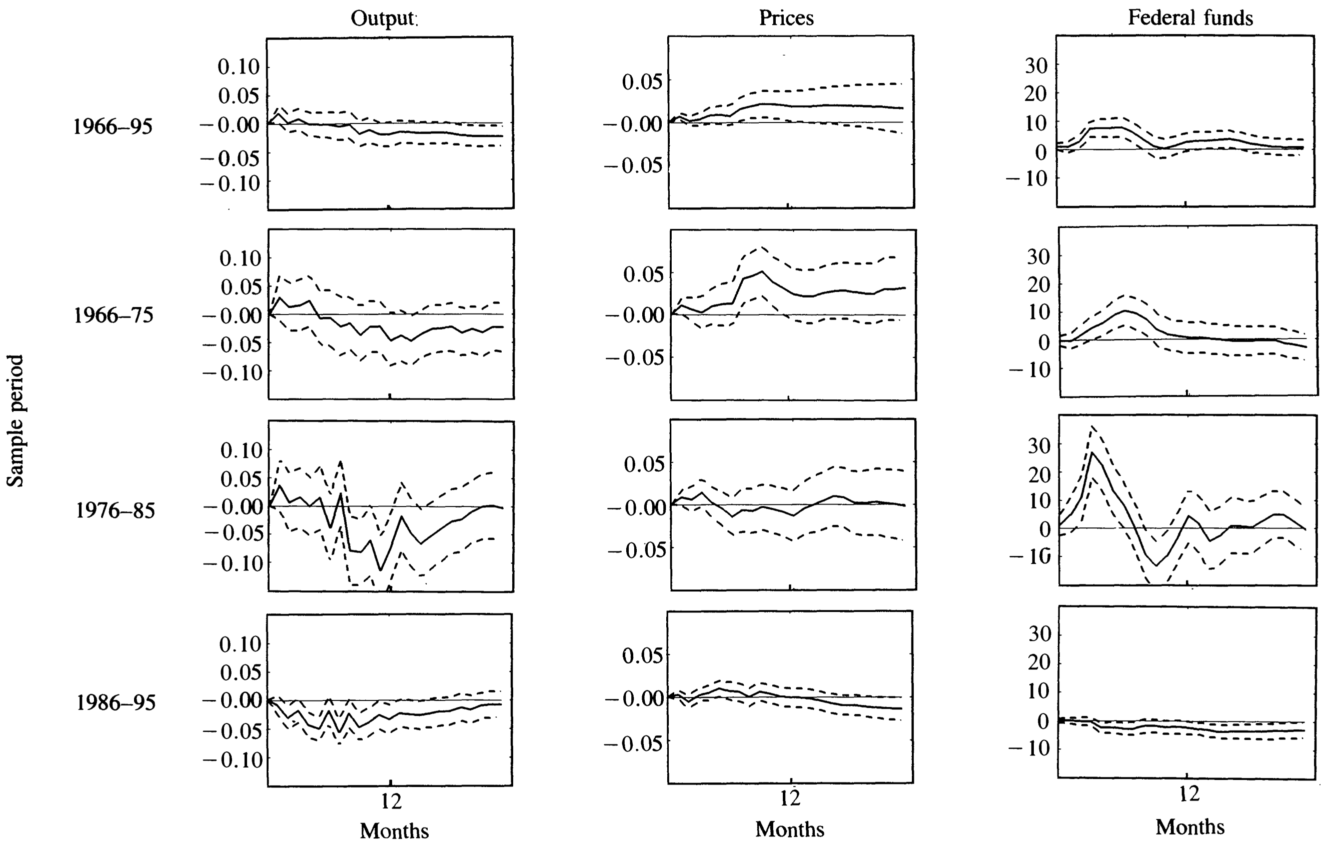
Asymptotic p value

	Regressor						
Equation	*Oil prices*	*Output*	*Prices*	*Commodity prices*	*Federal funds*	*Short rate*	*Long rate*
Quandt tests							
Oil prices	0.000	0.004	0.016	0.004	0.188	0.267	0.461
Output	0.439	0.926	0.699	0.362	0.338	0.607	0.187
Prices	0.000	0.003	0.000	0.000	0.005	0.014	0.002
Commodity prices	0.002	0.177	0.129	0.000	0.001	0.170	0.045
Federal funds	0.012	0.042	0.041	0.483	0.000	0.001	0.000
Short rate	0.152	0.132	0.017	0.092	0.128	0.072	0.000
Long rate	0.644	0.116	0.782	0.459	0.004	0.001	0.609
Chow split-sample tests							
Oil prices	0.882	0.651	0.233	0.422	0.259	0.181	0.667
Output	0.757	0.633	0.591	0.303	0.115	0.919	0.839
Prices	0.000	0.000	0.000	0.000	0.000	0.001	0.000
Commodity prices	0.004	0.131	0.080	0.127	0.109	0.018	0.007
Federal funds	0.814	0.159	0.125	0.123	0.048	0.099	0.030
Short rate	0.809	0.359	0.187	0.335	0.557	0.152	0.031
Long rate	0.254	0.215	0.388	0.658	0.507	0.002	0.581

Source: Authors' calculations using data described in appendix B.

a. The table shows asymptotic p values for tests of the stability of the coefficients of the regressors shown in the column heading, together with the regression constant term, in the equation given by the row heading. The funds rate is allowed to enter all equations. Lag lengths are chosen by the Bayes information criterion. First differences are used for all variables except the oil price indicator, which, by construction, is the difference of various oil prices. The upper panel is based on Wald versions of the Quandt (1960) test over the middle 70 percent of the sample. The p values are computed using the approximation due to Hansen (1997). The lower panel is based on heteroskedasticity-robust Wald tests for breaks at the sample midpoint.

Figure 10. Responses to a 1 Percent Oil Price Shock, Alternative Sample Periods[a]



Source: Authors' VARs, using data described in appendix B.

a. Graphs show twenty-four-month response of variables to a 1 percent shock to the Hamilton oil price measure, estimated over four alternative sample periods. Vertical axis scales represent percent deviations of variables (basis point deviations for the federal funds rate). Dashed lines represent one-standard-error bands.

response in the funds rate. The atypical behavior of the funds rate during this period may reflect the presence of confounding factors, such as the weakness of financial sector balance sheets and the decline in consumer confidence that depressed the economy at the time of the one major oil shock of that subsample, the 1990 increase in prices. In any event, the subsample evidence is highly consistent with the view that the reduced-form impact of oil on the economy depends significantly on the monetary policy reaction function.

Conclusion

This paper offers both methodological and substantive contributions. Methodologically, we show how to modify standard VAR systems to permit simulations of the economy under alternative endogenous policies. Since our focus is on quantifying the economic impact of historical feedback policies, the alternative policy that we consider is very simple; a virtue of our approach is that it would not be difficult to extend the analysis to consider more interesting alternatives, for example, ''Taylor rules.'' It would also be interesting to compare our results with those obtained from alternative (possibly, more structural) methodologies.

Substantively, our results suggest that an important part of the effect of oil price shocks on the economy results not from the change in oil prices, per se, but from the resulting tightening of monetary policy. This finding may help to explain the apparently large effects of oil price changes found by Hamilton and many others.

APPENDIX A

Interpolation of Monthly NIPA Variables

IN THIS PAPER we use interpolated monthly values of GDP, the components of GDP, and the GDP deflator. This appendix describes the interpolation process. The data and additional detailed estimation results are available on a distribution diskette from the authors, upon request.

We designate quarterly series by capital letters and monthly series by lower-case letters. Quarters are indexed by $T = 1, 2, \ldots, N$, and months by $t = 1, 2, \ldots, n$. Let Q_T be an (observed) quarterly variable that is to be interpolated—for example, real GDP—and let S_T be a scaling variable such that $Y_T \equiv Q_T/S_T$ is nontrending. Similarly, let q_t be the (unobserved) monthly series corresponding to Q_T—for example, montly real GDP—and let s_t be a scaling variable such that $y_t \equiv q_t/s_t$ is nontrending. Q_T and q_t are related by the identity

$$Q_T = \frac{1}{3} \sum_{i=0}^{2} q_{3T-i},$$

and hence Y_T and y_t are related by the identity

$$Y_T = \frac{1}{3} \sum_{i=0}^{2} y_{3T-i}(s_{3T-i}/S_T).$$

Interpolation is by state space methods. Suppose that there is a vector of (observable) interpolator variables at the monthly level, $\mathbf{x}_t$; industrial production, for example, is a monthly variable that provides information about within-quarter movements of real GDP. We assume that the unobserved monthly variable y_t is related to the interpolator variables according to the "causal," or "transition," equation

$$y_t = \mathbf{x}_t'\boldsymbol{\beta} + u_t,$$

where

$$u_t = \rho u_{t-1} + \epsilon_t, \qquad \epsilon_t \sim N(0,\sigma^2).$$

In our application, all transition equations include a constant term. When one or more of the interpolators becomes available midsample, all interpolators (including the constant term) are interacted with dummy variables and the possibility of a shift in the value of σ^2 is allowed for.

Let z_t be a monthly "indicator" variable that equals $Y_{t/3}$ in the third month of each quarter and is zero otherwise. Then the indicator, or "measurement," equations are given by

$$z_t = \frac{1}{3} \sum_{i=0}^{2} y_{t-i}(s_{t-i}/S_{3t}),\ t = 3, 6, 9, 12, \ldots, n$$

and

$$z_t = 0 \times y_t, \text{ for all other values of } t.$$

The parameters $\boldsymbol{\beta}$, ρ, and σ^2 are estimated by maximum likelihood, assuming Gaussian errors. Conditional on the estimated parameters, let $y_{t/\tau} = E_\tau y_t$, where E is the expectations operator. The interpolated values, given the full information set, are thus given by

$$q_{t/n} = y_{t/n} s_t.$$

This method is similar to that proposed by Chow and Lin (1971), although it allows for a more general treatment of the serial correlation in u_t.

To estimate the accuracy of the interpolation, one can use R^2 measures of fit. In levels, the measure of fit is

$$R^2_{levels} = \text{var}(y^2_{t/n})/\text{var}(y^2_t),$$

and in differences it is

$$R^2_{diffs} = \text{var}(\Delta y^2_{t/n})/\text{var}(\Delta y^2_t).$$

Table A1 lists the quarterly series that we interpolate, the corresponding monthly interpolators, and the measures of fit (corresponding to the scaled values of the variables). Variables are listed by their CITIBASE mnemonics, which are defined in appendix B. The scale variables used for real flow variables are personal consumption expenditures (GMCQ), at both the quarterly and monthly levels. The personal consumption expenditure deflator (GMDC), monthly and quarterly, is used as the scale variable in the interpolation of the GDP deflator.

Consumption data (disaggregated to durables, nondurables, and services) exist at a monthly frequency and thus do not have to be interpolated. Monthly GDP is calculated as the sum of the monthly GDP components (we ignore the slight deviations from that relationship caused by chain weighting).

The R^2 values suggest that the interpolators explain nearly all of the variability in the levels of the scaled series. With the exceptions of government consumption and the GDP deflator, they also explain nearly all of the implied month-to-month variation in the series.

Table A1. Interpolators and Goodness of Fit

Quarterly series interpolated[a]	*Monthly interpolators*[a]	R^2, *by specification*	
		Levels	*Differences*
GDPD	PWFSA PWFPSA PWIMSA PWCMSA	0.997	0.489
GIPDQ	IPE MSNDF[b] MSMAE[b]	0.999	0.775
GIRQ	IPIC MMCON CONFRC HSF	0.999	0.894
GISQ	IPIC MMCON CONIC CONCC	0.999	0.807
GVQ	Δ IVMFGQ Δ IVRRQ Δ IVWRQ	0.970	0.929
GGEQ	CONQC IPH FBO[b]	0.999	0.633
GEXQ	FSE602 FTE71 FTEF	0.999	0.919
GIMQ	FSM612 FTM333 FTM732	0.998	0.861

Source: Authors' calculations based on data described in appendix B.
a. Series identified by CITIBASE mnemonics; see appendix B.
b. Available beginning in January 1968.

APPENDIX B

Data

THIS APPENDIX describes the data series used in the paper. All data are from the CITIBASE electronic database, available from Citicorp Database Services. Series are identified by their CITIBASE mnemonic codes.

Quarterly series

GDPD GDP deflator, index, 1992 = 100.

GEXQ Exports of goods and services, chained 1992 dollars.

GGEQ Government consumption expenditures and gross investment, chained 1992 dollars.

GIMQ Imports of goods and services, chained 1992 dollars.

GIPDQ Investment, producers' durables, chained 1992 dollars.

GIRQ Investment, residential, chained 1992 dollars.

GISQ Investment, nonresidential structures, chained 1992 dollars.

GVQ Change in business inventories, total, chained 1992 dollars.

Monthly series

CONCC Construction put in place, commercial, seasonally adjusted, 1987 dollars.

CONFRC Construction put in place, private residential building, seasonally adjusted, 1987 dollars.

CONIC Construction put in place, industrial building, seasonally adjusted, 1987 dollars.

CONQC Construction put in place, public, seasonally adjusted, 1987 dollars.

FBO Federal budget, net outlay, not seasonally adjusted; deflated by interpolated government purchases deflator (GDFGEC), seasonally adjusted by the authors by means of a regression on monthly dummies.

FSE602 Exports, excluding military aid shipments, seasonally adjusted; deflated by the PPI for finished goods (PWF).

FSM612 General imports, seasonally adjusted; deflated by the PPI for finished goods (PWF).

FTE71 U.S. merchandise exports, nonelectrical machinery, seasonally adjusted; deflated by the PPI for machinery and equipment (PWME).

FTEF U.S. merchandise exports, agricultural products, seasonally adjusted; deflated by the PPI for farm products, processed foods, and feeds (PWFPF).

FTM333 U.S. merchandise imports, petroleum, and petroleum products, seasonally adjusted; deflated by the PPI for crude petroleum (PW561).

FTM732 U.S. merchandise imports, automobiles and parts, seasonally adjusted; deflated by the PPI for motor vehicles and equipment (PWAUTO).

FYFF Federal funds rate, percent.

FYGM3 Interest rate, three-month Treasury bills from the secondary market, percent.

FYGT5 Interest rate, five-year Treasury bonds, constant maturity, from the secondary market, percent.

FYGT10 Interest rate, ten-year Treasury bonds, constant maturity, from the secondary market, percent.

GMCQ Personal consumption expenditures, seasonally adjusted, chained 1992 dollars.

GMCDQ Personal consumption expenditures, durables, seasonally adjusted, chained 1992 dollars.

GMCNQ Personal consumption expenditures, nondurables, seasonally adjusted, chained 1992 dollars.

GMCSQ Personal consumption expenditures, services, seasonally adjusted, chained 1992 dollars.

GMDC Implicit price deflator, personal consumption expenditures, index, 1987 = 100.

HSF Housing starts, new private housing units, seasonally adjusted.

IP Industrial production index, total, seasonally adjusted, 1987 = 100.

IPE Industrial production index, business equipment, seasonally adjusted, 1987 = 100.

IPH Industrial production index, defense and space equipment, seasonally adjusted, 1987 = 100.

IPIC Industrial production index, construction supplies, seasonally adjusted, 1987 = 100.

IVMFGQ Inventories, manufacturing, seasonally adjusted, chained 1992 dollars.

IVRRQ Manufacturing and trade inventories, retail trade, seasonally adjusted, chained 1992 dollars.

IVWRQ Manufacturing and trade inventories, merchant wholesalers, seasonally adjusted, chained 1992 dollars.

MMCON Manufacturing shipments, construction materials and supplies, seasonally adjusted; deflated by the PPI for materials and components for manufacturing (PWIMSM).

MSMAE Manufacturing shipments, machinery and equipment, seasonally adjusted; deflated by the PPI for machinery and equipment (PWME).

MSNDF Manufacturing shipments, nondefense capital goods industries, seasonally adjusted; deflated by the PPI for capital equipment (PWFP).

PSCCOM Spot market price index, all commodities, from Commodity Research Bureau, not seasonally adjusted, 1967 = 100.

PUNEW CPI-U, all items, seasonally adjusted, 1982–84 = 100.

PW561 PPI, crude petroleum, not seasonally adjusted, 1982 = 100.

PWFPSA PPI, capital equipment, seasonally adjusted, 1982 = 100.

PWFSA PPI, finished goods, seasonally adjusted, 1982 = 100.

PWIMSA PPI, intermediate materials, supplies, and components, seasonally adjusted, 1982 = 100.

PWCMSA PPI, crude materials, seasonally adjusted, 1982 = 100.

Comments and Discussion

Christopher A. Sims: The broad aim of this paper is to go beyond the result, now widely confirmed in the empirical time-series literature on monetary policy, that surprise changes in monetary policy are a minor source of economic fluctuations. The nature of systematic reactions of monetary policy to the state of the economy could be a major determinant of the character of fluctuations, even though erratic disturbances to monetary policy are not. The paper concludes that the evidence is consistent with a major role for monetary policy; so large that, for example, most of the observed output effects of oil price shocks would disappear with a different monetary policy.

I agree with the main conclusion of the paper, but only because the authors have been so careful in stating it. I would emphasize more than they do how much uncertainty remains about the size of the real effects of monetary policy. It remains possible for a skeptic to maintain the view that the effects of both systematic and random shifts in monetary policy are negligibly small. My comments therefore emphasize the reasons to doubt that the effects of systematic monetary policy are large, despite the paper's evidence to the contrary.

The authors pursue their aim by focusing attention primarily on the reaction of the economy to surprise changes in oil prices. On the face of it, this focus is appealing, because most economists believe that they know roughly when large surprise changes in oil prices have occurred and have little doubt that these changes were distinct from surprise changes in monetary policy. Identification—separation of the interpretable disturbance from other sources of variation in the data—therefore promises to be easier than it would be with other types of private sector disturbances. This idea, it seems to me, has not turned out as well as one might have hoped.

In the first place, the intuition that historical oil price ''shocks'' are well understood and easily identified is incorrect. Although Hamilton's original work did not require elaborate filtering of the data, it appears that to extend it to the current time does require such filtering. In the present paper, four different measures of oil price shocks are shown in figure 2 to deliver four quite distinct estimated effects on the economy. The authors choose to proceed with Hamilton's filtration of the oil price data to generate their oil price shocks.

As the paper notes, the estimated effects of the oil price shock are small: a 1 percent oil price shock—which, by the definition of the variable, is expected to lead to a fairly persistent change in the actual level of oil prices—leads, in figure 4, only to a 0.02 percent response of the price level and a 0.025 percent output response at the peak of the responses. This is the size of the pure supply-side effect on GDP that one would expect if oil-related energy inputs had a 2 percent factor share, and most economists would expect estimated reduced-form effects of oil price increases to be larger than that. (This assumes that domestic oil is treated correctly as a primary input and that imports of foreign oil are treated correctly as intermediate inputs in GDP accounting, a perhaps dubious assumption.) It would be useful in assessing these results to know both the response of the oil price level, as opposed to the filtered variable, to this shock and the size of a one standard deviation shock to the filtered oil price measure.

Furthermore, though taken from different models, both the first row of table 1 and the error bands in the bottom row of figure 2 show that the responses of the variables to an oil shock could easily be zero and yet still consistent with the data; one-standard-error bands about the responses barely clear zero. It is true that table 1 shows that the *difference* in the response of the economy in the case where monetary policy responds according to historical norms and the case where it pegs the interest rate is fairly sharply defined by the data and is in the direction expected by the authors. But since the oil shock itself has turned out to be something of a will-o'-the-wisp, the idea that economists' intuitive knowledge of the size and nature of oil shocks would help with identification ends up not having contributed much.

The paper also shows some results for "output" and "commodity price" shocks. These are derived from the statistical model and are harder to interpret than oil shocks. The model gives them no interpretation, except that they are different from and independent of monetary policy shocks. But while these model-based shocks probably mix conceptually distinct non–monetary policy influences on the economy, they do have the advantage of having large effects and accounting for much of the observed variance in the data. It is encouraging to see in figures 8 and 9 that the effects of systematic monetary policy as measured with the oil shocks seem to be confirmed with the output and price shocks, but it is disappointing that all of the careful analysis of robustness and statistical strength centers on the less sharply defined oil price shocks.

The authors point out that previous experiments with analyzing the effects of systematic changes in monetary policy in identified VAR models have stuck to replacing the estimated policy rule in the model with something else. This kind of exercise implicitly assumes that in forming expectations of future policy actions, private agents treat all deviations of policy variables from their historical patterns of behavior as unsystematic deviations from the historical policy rule. The Lucas critique warns that this can lead to error.

My own view of the Lucas critique is that it explains that it is always a mistake to imagine that one can implement changes in policy that have probability zero according to the model of policy underlying private sector behavior. The implication is that if one can contemplate changing the coefficients of the "rule," or "reaction function," those coefficients should have been modeled as stochastic in the first place. There is an internal contradiction in pretending that one can change the coefficients, even though the public is modeled as absolutely certain that they can never change.

While this point is correct in principle, it is difficult to implement in practice. Especially for policy changes quite different from any that have been observed historically, estimation of an appropriate stochastic model that allows for such changes will be difficult and may need to rely heavily on guesswork and a priori knowledge. It is therefore a good idea, where possible, to focus attention on policy changes that are not too dramatic, which can reasonably be modeled as sequences of random disturbances to the policy behavior that is explicit in the model. This applies even when one is generating variations in policy by changing coefficients that in the model are treated as nonstochastic. The changes in coefficients are best chosen so as to correspond to not too dramatic sequences of shocks to the model's original policy rule.

The type of rule change studied in this paper—a shift to an exogenously fixed funds rate from a historical policy that, by contrast, made the funds rate react very sharply to inflationary disturbances—is dramatic. As is made clear in the recent literature on the interaction of monetary and fiscal policy, in particular, the seminal paper by Eric Leeper, a fixed interest rate as policy rule (contrary to some discussions elsewhere in the literature) is consistent with a uniquely determined price level.[1] However, this is true only if the fixed interest rate rule is accompanied by an appropriate fiscal policy, and the appropriate fiscal policy in this case is quite different from that consistent with a determinate price level in the context of an "anti-inflationary" monetary policy. Since in this authors' model fiscal policy has to be thought of as wrapped into the "non–monetary policy" sector, one would expect to find that changing the monetary policy rule alone to a fixed interest rate form would imply unsustainably explosive behavior of prices; and indeed, figures 4, 5, 8, and 9 show that this is exactly what emerges. Private agents are likely to recognize that such a shift in the monetary policy rule is unsustainable and therefore to expect it to end, or to be followed by a shift in fiscal policy. This makes interpreting the effects of the authors' exercise rather difficult. Their paper in places reads as if a different monetary policy might actually have eliminated the output effects of oil price or even output shocks. But since the alternate monetary policy considered is not sustainable, this interpretation does not seem to me correct. The simulations suggest instead only that by delaying or dampening an interest rate response to inflationary pressures, the monetary authority can trade delay or dampening of the output effects for increased inflationary effects. It would also have been interesting to see an analysis of effects of less extreme shifts in the policy rule that would have been sustainable; for example, smaller or slower, rather than zero, interest rate responses.

The authors attempt to respond to the Lucas critique by building into the model one particular form of endogenous adjustment of private sector expectations to the change in policy rule. They impose the the-

1. Leeper (1991).

oretical term structure relationships between the federal funds rate, another short rate, and a long rate. Then they attribute to those private agents doing interest rate arbitrage perfect foresight of the new policy fixing the federal funds rate. It is apparent from the figures that this modification of the model does nothing to correct the fundamental problem that the change in policy rule is unsustainable. Indeed, one might think that the sector most likely to realize that fixing the federal funds rate is not a sustainable policy, in the absence of a change in fiscal policy, is the bond market. Requiring that the bond market, but no one else, treat the policy as firmly in place forever therefore seems exactly backward from what might be plausible. Furthermore, this adjustment to the model is not in fact very large, as is made clear by the closeness of the simulation paths for many variables in cases where this adjustment is imposed and in those where it is not. The estimated statistical model already captures the strong tendency of the federal funds rate and other short rates to move together—a relation not very different from the theoretical term structure relationship. And the connection of long rates to short rates, although it differs more between simulations, appears not to be of great importance for predicting the effects of shocks on prices and output.

Thus the exercise undertaken here is a step toward modeling private sector learning behavior that might, in principle, be useful. But because the term structure relationships are simple and well approximated in the original estimated model, it does not seem to me likely that this particular aspect of private sector expectations is of central importance in this endeavor.

The entire identified VAR literature on the effects of monetary policy runs the risk of overestimating the real effects of monetary policy. It is not hard to construct a stochastic equilibrium model in which monetary policy is neutral and certain types of technology shocks raise real interest rates and, later, lower real output. The essential ingredients are conventional Solow-residual technology shocks and increasing costs in the investment goods industry (or within-firm adjustment costs to investment). If the monetary authority did not react to such shocks, they would be a source of movements of interest rates and output in opposite directions that was not related to price behavior or to money stock behavior. One might think of the identified VAR literature on the effects of monetary policy as a search for restrictions on a macroeconomic time-series model in which some shock, labeled ''monetary policy'' and orthogonal to other shocks, moves interest rates up, money down, output down, and prices down, with possible delays in all these effects except the interest rate movement. If the data are generated by a model in which there are real shocks connecting real rates and future output movements, as I suggest, this identified VAR research strategy can easily end up confounding the real shocks with monetary policy. The variety of real effects found in this literature, and the tendency of real effects to be smaller in models estimated for countries other than the United States, gives me genuine concern that this may have happened.

Let me conclude by saying again that, despite the skeptical tone of my comments, I find this paper useful evidence on the effects of systematic changes in monetary policy that, on the whole, does weigh in favor of those effects being substantial. It is quite unlikely that monetary policy could come close to eliminating the output effects of oil, ''commodity price,'' or ''output'' shocks, despite the authors' apparent evidence to the contrary. This strong conclusion rests on the their use of an unsustainable policy as the counterfactual alternative. But very substantial delay or smoothing of the output effects via monetary policy, at the expense of more inflation, probably would be possible.

Benjamin M. Friedman: This paper by Bernanke, Gertler, and Watson is a highly useful contribution to the empirical literature of monetary policy, both for its methodological approach and for some of its specific findings. I suspect that it, like the earlier paper by Sims and Zha on which it draws, will fruitfully spur further research following this kind of empirical strategy. Indeed, as I suggest below, this way of thinking about how monetary policy affects the economy has at least one potential application that may help to inform an issue of very great importance for the practical conduct of monetary policy, both in the United States and elsewhere.

The best way to place in context the empirical strategy taken by this paper is to recall the parallel distinctions, between what is systematic and what is unsystematic and between what is anticipated and what is unanticipated, that have stood behind much of the literature of monetary policy from the past two decades. At the theoretical level, the argument made by Robert Lucas, Thomas Sargent and Neil Wallace, and others was that the only monetary policy actions that have real effects are those that are *unanticipated*. As is now well understood, this proposition rests on a variety of assumptions—for example, perfect competition and perfectly flexible wages and prices—that few actual economies of practical interest satisfy. Nevertheless, because achieving analytical precision about the failure of those assumptions and about the macroeconomic consequences of that failure is highly problematic (it is difficult to spell out precisely how competition is imperfect and why wages and prices are sticky), the presumption that only unanticipated monetary policy actions have real effects has continued to underlie—sometimes explicitly but nowadays more often implicitly—much of modern research in the field. Further, as the standard assumption of rational expectations is usually applied, any part of the conduct of monetary policy that is systematic (for example, the central bank's always raising interest rates following a decline in unemployment or a surge in inflation) is assumed to be anticipated, and so in this line of thinking it is also assumed to be without real effects.

At the empirical level, the parallel argument has been that even if such systematic monetary policy actions did affect real economic activity, it would be impossible to distinguish those effects from the independent consequences of the events to which monetary policy was reacting. (For example, to the extent that the central bank simply moves interest rates in response to prior observed inflation, any subsequent effect on real output could just as well be attributed to the inflation itself as to the consequent movement in interest rates.) Hence the appeal of the vector autoregression approach in this context is that it focuses only on those monetary policy actions determined to be *unsystematic*, in the sense that the VAR cannot explain them in terms of prior movements in other variables. One danger of this approach is that a VAR that includes too much information may overexplain the movement of monetary policy in terms of prior movements in other variables. Such a VAR will erroneously shrink the remaining component, which is taken to be unsystematic and therefore also unanticipated, to the point that it then appears to have only trivial economic consequences. But the main point is that the empirical rationale for assessing the effects of monetary policy by looking only at its unsystematic variation, which continues to be in widespread use, resonates closely with the now outdated theoretical presumption that, at least for purposes of effects on real variables, only unanticipated policy actions matter. There is an inherent congruence between the two lines of thinking.

The principal thrust of the approach taken by Bernanke, Gertler, and Watson is to sever that connection by designing a way to use the empirical VAR methodology to investigate specific aspects of *systematic* monetary policy. To be sure, the paper simply presumes, rather than shows, that systematic and therefore anticipated monetary policy actions can have real effects. But for readers who accept that there are reasons why this may be so and who do not require that the empirical model used to investigate these effects be explicitly tied to a theoretical model detailing how they come about, the resulting advance is clear. And indeed, the authors find that the specific aspect of systematic monetary policy on which they choose to focus—the central bank's response to oil price shocks and to the consequences of those shocks for prices and output—does have sizable real effects. This finding is both interesting and important. (To be clear, the *within month* response of monetary policy to an oil price shock would be unanticipated and therefore presumed to have real effects, even in a Lucas-style model. Although the paper is not specific on this distinction, I assume that the bulk of the real effects that the authors attribute to the monetary policy response to oil price shocks results from movement in the policy variable occurring after the month in which the oil price moves.)

As indicated at the outset, I suspect that this methodology has an immediate application of potentially great importance. A question that has rightly attracted widespread attention, among industrial as well as developing countries, is how price inflation affects a country's ability to maintain real economic growth. Evidence shows that above some modest level (the high single-digit range), inflation does reduce the average pace of real growth over time. A familiar view, however, is that inflation negatively affects real growth not because inflation, per se, matters in this context, but because the central bank acts to resist inflation; and in a world in which the Lucas-Sargent-Wallace assumptions do not obtain, it can only do so by slowing ("sacrificing") real output. The methodology used in this paper seems potentially able to address this question too. If so, the findings would be very valuable.

Although both the methodology and the findings of Bernanke, Gertler, and Watson's paper are highly useful, three specific aspects give cause for reservation. First, as they are at some pains to emphasize, there is substantial evidence of instability in their results across the three decades of their sample. In particular, as figure 10 clearly shows, the ''systematic'' response of monetary policy to oil price shocks in the Volcker period was far greater than either earlier or later.

A question that this instability immediately raises is whether it is reasonable to view the more energetic anti-inflationary monetary policy of the Volcker era exclusively as a response to an oil price shock. I believe that the Federal Reserve System under Paul Volcker adopted a policy broadly aimed at reducing the U.S. inflation rate, and that the rise in oil prices in 1979 and 1980 was only one element in the inflation process against which it directed its policy. The results plotted in the middle right-hand panel in figure 6, showing that the simulated response to the historical oil shock accounts for only a small part of the increase in the federal funds rate during 1981–82, are certainly consistent with this view. Because of the *post hoc ergo propter hoc* character of VAR analysis, the Bernanke-Gertler-Watson paper may attribute to the specific response (here and in other subperiods) of monetary policy to oil price shocks what was actually the more general conduct of monetary policy, based on other considerations.

The findings of subsample instability also highlight the difficulty of identifying what ''systematic'' policy means in the first place. For purely empirical purposes of extracting impulse responses and variance decomparisons from past data, systematic simply means whatever happened on average across the arbitrarily chosen sample under study. But as is the case in this paper, researchers often seek to connect this purely empirical notion of systematic behavior with the concept of policy ''rules,'' so as to go on to draw inferences about the consequences of the central bank following one rule rather than another. As a number of people (Sims, John Taylor, and I, among many others) have argued in one context or another, it is not clear that in practical settings the central bank is ever following a rule, in the crucial dual sense that its actions are not only systematic but also perceived to be so and therefore properly anticipated by the relevant public. The fact that estimating the authors' VAR over the 1976–85 sample delivers the federal funds rate response shown in the right-hand panel of the third row in figure 10 does not necessarily make this response a characterization of systematic monetary policy in any substantive sense.

A second set of reservations stems from the authors' use of oil price shocks as the principal empirical vehicle for their study of systematic monetary policy. To put it bluntly, does the Hamilton idea really make sense? For example, should one really think of the 1957–58 recession in the United States as a ripple from the 1956 Suez affair? To take Hamilton's idea seriously would require a major rethinking of most of post–World War II U.S. business cycle history—which clearly has not happened in the decade and a half since Hamilton's intriguing paper appeared. The authors of the present paper are perhaps more secure in that the role of oil prices is more plausible in at least two, possibly three, of the five recessions covered in their sample, which mostly postdates Hamilton's. Even so, I suspect that their difficulty in finding a measure of oil price shocks that satisfactorily fits the oil facts to the macroeconomic data is a warning of just this problem.

Finally, several aspects of the authors' treatment of interest rates also bear closer attention. The assumption that interest rate movements are a sufficient statistic for the channels by which monetary policy affects macroeconomic activity is, by itself, not unusual. Indeed, the authors may well overemphasize its limitations. Costs of financing (including opportunity costs) are an important factor in many kinds of spending decisions, and for this purpose interest rate fluctuations may also plausibly stand in for at least part of the relevant movement in either exchange rates or broader asset prices. While the strong rejection of the restriction excluding the federal funds rate from the output equation is somewhat surprising, the authors are presumably correct that the practical effects of imposing this restriction are small. Further, it is my conjecture that if a stock price index were included in the VAR, the data would accept this restriction. (Because the analysis in this paper depends so crucially on the role of short- and long-term interest rates, however, there is probably much to be learned from examining the coefficients of these interest rates in the output equation, as well as the impulse responses relating output to the independent components of the two interest rates. It would therefore be useful to show explicitly these key elements of the analysis.)

The potential problem, however, is the strong implied rejection of the expectations hypothesis of the term structure of interest rates, which the authors use as the organizing principle for this part of their model. Normally, within this framework, the ''term premium'' included in any specific interest rate is a substantive reflection of borrowers' and lenders' attitudes toward such features as the risk and liquidity of the underlying debt instrument. But in this paper, the term premium simply serves to undo the behavior that the built-in expectations hypothesis implies that interest rates should be following (see, for example, figure 3). Moreover, the results plotted in figure 4 for prices and the long-term rate are dramatically at variance with standard notions of how inflation expectations affect nominal interest rates. In this experiment, not surprisingly, moving from the base simulation to either the Sims-Zha simulation or the anticipated policy simulation results in far higher prices and hence much greater inflation. But in the Sims-Zha simulation the long-term interest rate is uniformly below its level in the base simulation, and in the anticipated policy simulation it even declines absolutely. So much for the notion that investors rationally anticipate the consequences of monetary policy for future inflation and incorporate the resulting inflation expectations into current bond prices!

These three sets of reservations notwithstanding, I applaud the broader methodological direction taken by Bernanke, Gertler, and Watson and retain my sense that their finding of quantitatively significant effects from systematic monetary policy is both correct and important.

General discussion: Participants generally accepted the authors' conclusion that the output declines following oil price shocks had come mainly from the responses of monetary policy to the shocks. Several also discussed the plausible magnitude of oil shock effects themselves. One issue was how much an oil price increase, or a decrease in oil supply, should affect potential output; a second was whether oil price increases reduce demand and lead to lower levels of utilization of productive capacity. Robert Hall observed that, for infinitesimal changes in oil prices, the ability of the United States to produce should not be impaired by a rise in the price of imported oil, even if it reduces oil use; the derivative of real GDP with respect to the price of oil is zero no matter how large the adjustment, with Division GDP. However, he and William Nordhaus agreed there could be effects on potential GDP as the equilibrium supply of domestic factors adjusted to the change in oil prices. George Perry added that some estimates from earlier studies, such as a reduction of several percentage points of GDP from OPEC 1, were too large to be viewed as a supply-side effect. However, taking into account the effect of an oil price increase on aggregate demand, where the price increase could be analyzed approximately like an increase in excise taxes with high-saving foreigners getting the revenue, a large short-run impact on GDP was believable. He added that the allocation of such an impact between a ''fiscal'' and a monetary effect would depend, somewhat arbitrarily, on how baseline monetary policy was defined.

Nordhaus raised several issues about the appropriateness of the various measures of oil shocks used by the authors. He suggested that almost any theory, whether Perry's that the short-run impact of increases could be regarded as a tax paid to foreigners or Sims's that it should be treated simply as an increase in input prices, should lead to some measure involving oil purchases relative to the size of the economy. This scaling makes an enormous difference. For the last three oil shocks in the sample, he calculated the increased costs of imported oil, with quantities fixed, were 1.8 percent of GDP in 1973, 1.0 percent of GDP in 1979, and 0.2 percent of GDP in 1990. Using this measure would preserve the peaks of the Hamilton series, but the shocks would be progressively smaller. Nordhaus also noted that the paper ignores the negative oil shock of 1986, when the price decline corresponded to a negative shock of 0.5 percent of GDP. He reasoned that the failure to scale the shocks, along with the fact that the positive shocks of 1986 and 1990 were quickly reversed, may explain why the responses in the two subperiods look so different in the authors' analysis. William Brainard agreed with Nordhaus's argument for scaling the shocks and added that it might be useful to construct a similar measure indicating the magnitude of the redistribution between domestic producers and consumers.

Robert Shiller observed that the stochastic properties of the oil price series seemed to have changed after the Organization of Petroleum Exporting Countries broke up in 1986. Before that, the oil price was a series of plateaus separated by sudden jumps, so that changes seem to have a lot of information. But afterward, the oil price looks like a mean-reverting process, so the movements have less information. He reasoned that the public may realize this difference, which would explain why oil price changes are no longer big news. Reflecting on the widespread concerns about oil in the 1970s and 1980s, Shiller suggested that the long view is important in economics and the best way to deal with an anomaly is to wait it out until it disappears. He suggested that may have happened with oil.

References

Andersen, Leonall C., and Jerry L. Jordan. 1968. ''Monetary and Fiscal Actions: A Test of Their Relative Importance in Economic Stabilization.'' Federal Reserve Bank of St. Louis *Review* (November): 11–24.

Bernanke, Ben S., and Alan S. Blinder. 1992. ''The Federal Funds Rate and the Channels of Monetary Transmission.'' *American Economic Review* 82(4): 901–21.

Bernanke, Ben S., and Ilian Mihov. 1995. ''Measuring Monetary Policy.'' Working Paper 5145. Cambridge, Mass.: National Bureau of Economic Research (June).

Blanchard, Olivier J. 1984. ''The Lucas Critique and the Volcker Deflation.'' *American Economic Review, Papers and Proceedings* 74(2): 211–15.

Chow, Gregory, and An-loh Lin. 1971. ''Best Linear Unbiased Interpolation, Distribution, and Extrapolation of Time Series by Related Series.'' *Review of Economics and Statistics* 53(4): 372–5.

Christiano, Lawrence J., and Martin Eichenbaum. 1992. ''Identification and the Liquidity Effect of a Monetary Policy Shock.'' In *Political Economy, Growth, and Business Cycles,* edited by Alex Cukierman, Zvi Hercovitz, and Leonardo Leiderman. MIT Press.

Cochrane, John H. 1996. ''What Do the VARs Mean? Measuring the Output Effects of Monetary Policy'' Unpublished paper, University of Chicago (August).

Darby, Michael R. 1982. ''The Price of Oil and World Inflation and Recession.'' *American Economic Review* 72(4): 738–51.

Dotsey, Michael, and Max Reid. 1992. ''Oil Shocks, Monetary Policy, and Economic Activity.'' *Federal Reserve Bank of Richmond Economic Review* 78(4): 14–27.

Ferderer, J. Peter. 1996. ''Oil Price Volatility and the Macroeconomy.'' *Journal of Macroeconomics* 18(1): 1–26.

Friedman, Benjamin M., and Kenneth N. Kuttner. 1996. ''A Price Target for U.S. Monetary Policy? Lessons from the Experience with Money Growth Targets.'' *BPEA, 1:1996,* 77–125.

Friedman, Milton, and Anna Jacobson Schwartz. 1963. *A Monetary History of the United States, 1867–1960.* Princeton University Press.

Fuhrer, Jeffrey C. 1996. ''Monetary Policy Shifts and Long-Term Interest Rates.'' *Quarterly Journal of Economics* 111(4): 1183–209.

Hamilton, James D. 1983. ''Oil and the Macroeconomy Since World War II.'' *Journal of Political Economy* 91(2): 228–48.

———. 1996a. ''Analysis of the Transmission of Oil Price Shocks Through the Macroeconomy.'' Unpublished paper. University of California, San Diego (August).

———. 1996b. "This is What Happened to the Oil Price-Macroeconomy Relationship." *Journal of Monetary Economics* 38(2): 215–20.

Hansen, Bruce E. 1997. "Approximate Asymptotic *P* Values for Structural-Change Tests." *Journal of Business and Economic Statistics* 15(1): 60–67.

Hooker, Mark A. 1996a. "Exploring the Robustness of the Oil Price-Macroeconomy Relationship: Empirical Specifications and the Role of Monetary Policy." Unpublished paper. Wellesley College (September).

———. 1996b. "What Happened to the Oil Price-Macroeconomy Relationship?" *Journal of Monetary Economics* 38(2): 195–213.

Hoover, Kevin D., and Stephen J. Perez. 1994. "Post Hoc Ergo Propter Hoc Once More: An Evaluation of 'Does Monetary Policy Matter?' in the Spirit of James Tobin." *Journal of Monetary Economics* 34(1): 47–73.

Kim, In-Moo, and Prakash Loungani. 1992. "The Role of Energy in Real Business Cycle Models." *Journal of Monetary Economics* 29(2): 173–89.

Kim, Soyoung. 1995. "Does Monetary Policy Matter in the G-6 Countries? Using Common Identifying Assumptions About Monetary Policy Across Countries." Unpublished paper. Yale University (January).

Lee, Kiseok, Shawn Ni, and Ronald A. Ratti. 1995. "Oil Shocks and the Macroeconomy: The Role of Price Variability." *Energy Journal* 16(4): 39–56.

Leeper, Eric M. 1991. "Equilibria Under 'Active' and 'Passive' Monetary and Fiscal Policies." *Journal of Monetary Economics* 27(February): 129–47.

Leeper, Eric M., Christopher A. Sims, and Tao Zha. 1996. "What Does Monetary Policy Do?" *BPEA, 2:1996*, 1–63.

Lucas, Robert E. Jr. 1976. "Econometric Policy Evaluation: A Critique." *Carnegie-Rochester Conference Series on Public Policy* 1: 19–46.

Mork, Knut Anton. 1989. "Oil and the Macroeconomy When Prices Go Up and Down: An Extension of Hamilton's Results." *Journal of Political Economy* 97(3): 740–44.

Quandt, Richard E. 1960. "Tests of the Hypothesis that a Linear Regression System Obeys Two Separate Regimes." *Journal of the American Statistical Association* 55(290): 324–30.

Romer, Christina D., and David H. Romer. 1989. "Does Monetary Policy Matter? A New Test in the Spirit of Friedman and Schwartz." In *NBER Macroeconomics Annual 1989*, edited by Olivier Jean Blanchard and Stanley Fischer. MIT Press.

Rotemberg, Julio J., and Michael Woodford. 1996. "Imperfect Competition and the Effects of Energy Price Increases on Economic Activity." *Journal of Money, Credit, and Banking* 28(4, part 1): 549–77.

Shapiro, Matthew D. 1994. "Federal Reserve Policy: Cause and Effect." In *Monetary Policy*, edited by N. Gregory Mankiw. University of Chicago Press.

Shiller, Robert J., John Y. Campbell, and Kermit L. Schoenholtz. 1983. "Forward Rates and Future Policy: Interpreting The Term Structure of Interest Rates." *BPEA, 1:1983*, 173–223.

Sims, Christopher A. 1980. "Macroeconomics and Reality." *Econometrica* 48(1): 1–48.

———. 1986. "Are Forecasting Models Usable for Policy Analysis?" *Federal Reserve Bank of Minneapolis Quarterly Review* 10(1): 2–16.

———. 1992. "Interpreting the Macroeconomic Time Series Facts: The Effects of Monetary Policy." *European Economic Review* 36(5): 975–1011.

Sims, Christopher A., and Tao Zha. 1995. "Does Monetary Policy Generate Recessions?" Unpublished paper. Yale University and Federal Reserve Bank of Atlanta (December).

Strongin, Steven. 1995. "The Identification of Monetary Policy Disturbances: Explaining the Liquidity Puzzle." *Journal of Monetary Economics* 35(3): 463–97.

Taylor, John B. 1993. *Macroeconomic Policy in a World Economy: From Econometric Design to Practical Operation*. W.W. Norton.

Tobin, James. 1980. "Stabilization Policy Ten Years After." *BPEA, 1:1980*, 19–71.

West, Kenneth D. 1993. "An Aggregate Demand-Aggregate Supply Analysis of Japanese Monetary Policy, 1973–1990." In *Japanese Monetary Policy*, edited by Kenneth J. Singleton. University of Chicago Press.