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Identification and the Effects of Monetary Policy Shocks*

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1. Introduction

In recent years there has been a resurgence of interest in constructing models of the monetary transmission mechanism. To evaluate these models and build better ones, we must know what the historical facts are about how monetary policy actions affect the economy. This paper displays some of these facts and discusses some of the difficulties involved in arguing that they are in fact facts.

Some Recent Results

In a recent paper, Christiano, Eichenbaum and Evans (1994) argue that the facts about the response of the economy to a contractionary monetary policy action can be summarized as follows. First, short term interest rates, the value of the dollar, manufacturing inventories, and unemployment rise. Second, monetary aggregates, output, employment, profits and retail sales fall. Third, the price level remains unchanged for roughly a year, before falling. Fourth, in the first half year or so after the contraction, net funds raised by the business sector in financial markets rises.

It is striking how, in many ways, these results conform with conventional wisdom regarding the empirical effects of money shocks. Some of these results, however, are difficult to reconcile with important classes of monetary models. For example, the estimated price response is difficult to reconcile with most existing flexible price models. Also, the estimated response of borrowing by the business sector is difficult to reconcile with existing models of the monetary transmission mechanism (see Christiano, Eichenbaum and Evans (1994) for a discussion). Because these facts can play such a useful role in discriminating between models, it is important to understand how they are arrived at.

The Role of Identification

The central problem in establishing the facts about the effects of monetary policy actions is that these actions often reflect policy makers' responses to nonmonetary developments in the economy. We refer to the rule which relates policymakers' actions to the state of the economy as their *feedback rule.* To the extent that a policy action is an outcome of the feedback rule, the response of economic variables reflects the combined effects of the action itself and of the variables that policy reacts to. To isolate the effects of Fed policy actions per se, we need to identify the component of those actions that is not reactive to other variables. We refer to this as the exogenous component of a monetary policy action, or, as an exogenous monetary policy shock. With this definition, monetary policy actions are the sum of two components: the feedback rule and the exogenous shock. In this paper, we interpret the question, 'how does the economy respond to a monetary policy action?' as, 'how does the economy respond to an exogenous monetary policy shock?'

It is important to distinguish between this question and a more interesting one, namely, 'what is the impact on the economy of a change in the monetary authority's feedback rule?' We attack the less interesting question that is the focus of this paper because we have to.

The reason for this is as follows. Assessing the effect of a systematic change in the monetary authority's feedback rule would be straightforward if we had data drawn from otherwise identical economies operating under the feedback rules that we are interested in evaluating. We don't. And real world experimentation is not an option. The only place we can perform experiments is in structural models. The problem is deciding on which structural model we should use. Before trusting a model's answers to hard questions, it should, at a minimum, give the right answer to simple questions. The simple question that we focus on is: how does the system respond to an exogenous monetary policy shock? Granted, giving the right answer to this question is not a sufficient condition for acting on the implications of a given model. But, this test does help narrow the field of choice and give guidance to the development of existing theory.

In this paper, we explore two general strategies for measuring exogenous monetary policy shocks. The first involves making enough assumptions (in econometric terms, *identifying assumptions*) to allow us to estimate the Fed's feedback rule. These identifying assumptions include a specification of the functional form of the feedback rule, the variables in the rule, and the variable controlled by the Fed, i.e., its policy instrument. To get the exogenous shocks that we seek, we simply subtract the action implied by the feedback rule from the actual action taken.[1] This is the type of strategy pursued in Christiano, Eichenbaum and Evans (1994). Since their calculations are based on the use of vector autoregressions (VAR), we refer to this general strategy as the 'VAR based approach'.[2]

The second general strategy for identifying exogenous monetary policy shocks is the so-called narrative approach, most recently associated with the work of Romer and Romer (1989). Under this method, one looks at a broad set of data, including, for example, the minutes of the Fed's policy deliberations. The idea is to isolate episodes in which the change in policy controlled variables was both purposeful and large, and to examine the behavior of economy afterwards. The identifying assumption needed to interpret the results of this procedure is that when the Fed makes a particularly big policy move, then all (or at least, *most*) of that move is exogenous, i.e., the feedback component of the policy action is zero or very small.

It is difficult to judge on *a priori* grounds which approach is better. The VAR approach leads to misleading results if the wrong identifying assumptions are made in specifying the Fed's policy rule.[3] A seeming advantage of the narrative approach

[1] An alternative approach to measuring exogenous monetary policy shocks is pursued in Blanchard and Quah (1989), Gali (1992), and King and Watson (1992). In this approach, less weight is placed on estimating the Fed's feedback function, and more is placed on exploiting assumptions about the long-run effects of monetary shocks.

[2] There are many other examples of the VAR approach applied to the effects of monetary policy shocks. For a recent review, see Cochrane (1994).

[3] In practice, identifying assumptions are selected in part based on whatever *a priori* knowledge one has about the sign or shape of the response of economic variables to a monetary policy action. For example, we know that a monetary contraction should be associated with a fall in the Fed's holdings of government securities and in the banking sector's holdings of nonborrowed reserves. If a particular identifying assumption leads to a policy shock measure which generated implications at

is that one is not required to formally specify a Fed feedback function. But nothing is for free. In our view there are two key problems associated with the narrative approach. First, the central identifying assumption - that large policy actions are primarily exogenous in nature - has little motivation. Indeed, Romer and Romer (1989) themselves argue that in *every* episode that they isolate, the Fed's policy action was motivated by a desire to lower inflation. Second, the narrative method - at least as applied to post war monetary policy - yields relatively little information: it delivers a few episodes of large policy actions, with no indication of their relative intensity. In contrast, the VAR approach generates many 'episodes', one for each date in the data sample, and a quantitative measure of the intensity of the exogenous shock for each date. Consequently, the VAR approach can, in principle, generate much more precise estimates of the effects of a monetary policy shock.

While the primary purpose of this paper is to evaluate the sensitivity of the results obtained by Christiano, Eichenbaum and Evans (1994) to alternative identifying assumptions, we also examine sensitivity of inference to using the narrative approach. In particular, we contrast the results obtained using the identifying assumptions in Christiano, Eichenbaum and Evans (1994) with those obtained using the index of monetary policy contractions constructed by Romer and Romer (1989).

Our Results

Our key result is that the overall qualitative findings about the impact of monetary policy shocks reported in Christiano, Eichenbaum and Evans (1994) is robust to the different identification assumptions that we consider. But on some dimensions there is some sensitivity.

Two examples illustrate the nature of the identification problems that we enounter and the nature of this sensitivity. First, disturbances in VAR equations for real GNP and the federal funds rate are positively correlated. When we treat the federal funds rate as the Fed's policy instrument, we must come to terms with the direction of

variance with this, it would be deemed inadmissable.

causation underlying this correlation: Does it reflect (i) the endogenous response of policy to real GNP via the Fed's feedback rule, or does it reflect (ii) the response of real GNP to policy?[4] Christiano, Eichenbaum and Evans (1994) assume that the answer is (i) and obtain the result that output falls after a policy induced rise in the federal funds rate. As we show below, if one assumes that the answer is (ii) then a policy induced rise in the federal funds rate drives real GNP up for about two quarters, before driving it down. We reject this implication and the underlying identifying assumption on the grounds that it is sufficiently at variance with standard models to be implausible.

Second, on several occasions in the post war era, a rise in inflation was preceeded by a rise in the federal funds rate and in commodity prices. An example is the oil shock episode in 1974. Identification schemes which treat the federal funds rate as the Fed's policy instrument and which do not include commodity prices (as leading indicators of inflation) in the Fed's feedback rule have the implication that contractionary monetary policy shocks lead to a sustained rise in the price level. Eichenbaum (1992) and Sims (1992) viewed this implication as sufficiently anomalous relative to standard theory to justify referring to it as the 'price puzzle'.[5] We show that under identification assumptions which allow for feedback from commodity prices to policy, the price puzzle disappears. This resolution of the price puzzle is consistent with a conjecture advanced by Sims (1992) and with findings in Sims and Zhou (1994).

The plan of the paper is as follows. In section 2 we discuss our version of the VAR approach to measuring monetary policy shocks. In section 3 we investigate the impact of alternative measures of shocks to monetary policy (including the Romer and Romer index) on various aggregate variables. Section 4 analyzes the impact of different monetary policy shock measures on the net flow of financial assets between

[4]There are of course intermediate possibilities under which causation is bidirectional. The version of the VAR approach that we work with excludes this possibility. Alternative approaches that do not include the ones implemented in Bernanke (1986), Gali (1992), King and Watson (1992), and one of the methods applied in Sims (1986).

[5]There do exist some models which predict a temporary rise in the price level after a monetary contraction (see Beaudry and Devereux, 1994, and Fuerst, 1992.)

sectors of the economy. In addition we consider the impact on inference of an alternative detrending technique, and of working with monthly rather than quarterly data. Section 5 contains some concluding remarks.

2. Measuring Exogenous Shocks to Monetary Policy

In this section we discuss our strategy for estimating the effects of exogenous shocks to monetary policy. The basic problem that we mu ; deal with is to how to measure the shocks themselves. In previous work we pursued a particular identification strategy which led to a particular measure of policy shocks. To investigate the robustness of our findings, here we examine alternative measures. With one exception, these correspond to imposing particular Wold causal orderings across the monetary policy shocks and the other economic variables. In the exception, we identify policy shocks with the index proposed by Romer and Romer (1989).

2.1. Identification Assumptions under Wold Causal orderings

Our basic strategy for identifying exogenous shocks to monetary policy is to focus on the disturbance term in a regression equation of the form:[6]

$$S_t = \psi(\Omega_t) + \sigma\varepsilon_{st}. \tag{2.1}$$

Here S_t is the policy instrument; ψ is a linear function; Ω_t summarizes the information set that the monetary authority looks at when setting S_t; σ is a positive number, and ε_{st} is a serially uncorrelated shock that is orthogonal to the elements of Ω_t and has unit variance. The information set, Ω_t, includes the past history of all variables in the statistical model as well as the time t realizations of a subset of those variables. To rationalize interpreting ε_{st} as an exogenous policy shock, (2.1) must be viewed as the monetary authority's rule for setting S_t. In addition, the orthogonality conditions on ε_{st} correspond to imposing a particular Wold causal structure on the policy and

[6]This discussion is based on section 2 of Christiano, Eichenbaum and Evans (1994).

other variables under which the time t policy shocks do not affect the variables in Ω_t.[7] The dynamic response of a variable to a monetary policy shock can be measured by the coefficients in the regression of the variable on current and lagged values of the fitted residuals in equation (2.1).

This procedure is asymptotically equivalent to one based on fitting a particular Vector Autoregression (VAR):

$$Z_t = A_0 + A_1 Z_{t-1} + A_2 Z_{t-2} + ... + A_q Z_{t-q} + u_t. \tag{2.2}$$

The VAR disturbance vector, u_t, is assumed to be serially uncorrelated and to have variance-covariance matrix V. The VAR disturbances are assumed to be related to the underlying economic shocks, ε_t, by

$$u_t = C\varepsilon_t, \tag{2.3}$$

where C is lower triangular and ε_t has covariance matrix equal to the identity matrix. To relate this to (2.1), suppose that S_t is the k^{th} element in Z_t. Then, ε_{st} is the k^{th} element of ε_t. In addition, Ω_t includes $Z_{t-1}, ..., Z_{t-q}$. If k > 1 then Ω_t also includes $Z_{i,t}$ for $i = 1, ..., k-1$.[8]

Parsimony dictates that only a moderately large number of variables can be included in Z_t. In our empirical analysis of the quarterly data, the vector Z_t always includes at least the following variables: the log of real GDP (Y), the log of the GDP deflator (P), minus the log of nonborrowed reserves ($NBRD$), the federal funds rate (FF), and the log of total reserves (TR).[9] When we want to assess the effect of a monetary shock on some other variable, D_t, that variable too is included in Z_t. The reason we work with $NBRD$ rather than with the log of nonborrowed reserves is to facilitate comparisons between different policy shock measures which are based upon

[7] A different class of schemes for identifying monetary policy shocks does not impose this Wold causal structure, that is, does not impose the assumption that ε_{st} is orthogonal to Ω_t. See, for example, Bernanke (1986), Gali (1992), King and Watson (1992) and Sims (1986).

[8] Equation (2.1) is proportional to the $k + 1^{st}$ equation of C^{-1} times (2.2).

[9] For a detailed discussion of the sources of all the data used in this analysis, see the Appendix in Christiano, Eichenbaum and Evans (1994).

these policy instruments. Positive FF and $NBRD$ policy shocks will both correspond to contractionary monetary policy shocks.

Given a specification of Z_t, our identification schemes differ along three dimensions. First, we consider two measures of the policy instrument, S_t: the log of nonborrowed reserves and the federal funds rate.[10] Second, we consider the effects of including a measure of commodity prices ($PCOM$) in Z_t. Third, we consider alternative specifications of the contemporaneous elements of the information set Ω_t. In conjunction with our orthogonality assumptions on ε_t, this amounts to adopting different Wold orderings of the elements of Z_t. We consider three orderings.

1. *Benchmark policy ordering* (the ordering used in Christiano, Eichenbaum and Evans (1994)). When the federal funds rate is specified as the policy instrument (S_t), ε_{st} is estimated using the ordering of the variables in Z_t given by: (Y_t, P_t, $PCOM_t$, FF_t, $NBRD_t, TR_t, D_t$). This ordering assumes that the monetary authority looks at the contemporaneous state of real economic activity (Y_t) as well as prices (P_t, $PCOM_t$) before deciding on ε_{st}. We refer to this measure of a monetary policy shock as an FF policy shock. When $NBRD$ is specified as the policy instrument, ε_{st} is estimated using the following ordering of the variables in Z_t: ($Y_t, P_t, PCOM_t$, $NBRD_t, FF_t, TR_t, D_t$). We refer to this measure of a monetary policy shock as an $NBRD$ policy shock. There are two exceptions to the above ordering. On two occasions in our analysis below, the variable D_t is an indicator of aggregate production activity: the unemployment rate and the log of employment. In those cases, we place D_t before the policy variable in the ordering. This is consistent with our assumption that monetary policy shocks do not affect prices and output in the current period.

2. *Monetary policy first* ordering: when the federal funds rate is specified as the policy instrument, ε_{st} is estimated using the following ordering of the variables

[10]See Christiano and Eichenbaum (1992) and Strongin (1992) for a discussion of the role of non borrowed reserves in monetary policy. See Bernanke and Blinder (1992) for a discussion of the federal funds rate.

in Z_t: $(FF_t, Y_t, P_t, PCOM_t, NBRD_t, TR_t, D_t)$. We refer to this measure of a monetary policy shock as an $FF1$ policy shock. The $NBRD$ specification is analogously defined and the resulting policy shock measure is referred to as an $NBRD1$ policy shock.

3. *D variable first* ordering: when the federal funds rate is specified as the policy instrument, ε_{st} is estimated using the following ordering of the variables in Z_t: $(D_t, Y_t, P_t, PCOM_t, FF_t, NBRD_t, TR_t)$. We refer to this measure of a monetary policy shock as an FFD policy shock. The $NBRD$ specification is analogously defined and the resulting policy shock is referred to as a $NBRDD$ policy shock.

2.2. The Romer and Romer index

Our final measure of monetary policy shocks is motivated by results in Romer and Romer (1989) who use minutes of the Federal Reserve Open Market Committee meetings to isolate periods in which the Fed purposefully initiated monetary contractions. For our sample period, they isolate four such dates: December, 1968; April, 1974; August, 1978; October, 1979. We follow Kashyap, Stein, and Wilcox (1993) by adding the 1966 credit crunch (1966:2) to the index of monetary contractions. In addition we add the August 1988 episode identified by Oliner and Rudebusch (1992) as the beginning of a monetary contraction. For ease of exposition, we refer to all of these episodes as Romer and Romer episodes. We compute the response of a variable to a Romer and Romer episode using the following method. Consider a VAR for the vector of variables Z_t :

$$Z_t = A(L)Z_{t-1} + \beta(L)d_t + \epsilon_t. \tag{2.4}$$

Here $A(L)$ and $\beta(L)$ are one sided polynomials in the lag operator L. The variable d_t denotes the time t value of the Romer and Romer index. This variable equals one in the period of a Romer and Romer episode, and zero otherwise. The response of Z_{t+k} to a time t Romer and Romer episode is given by the coefficient on L^k in the polynomial $[I - A(L)]^{-1}\beta(L)$. For this analysis, the ordering of variables in the Z_t vector is not relevant.

As noted in the introduction, there is room for skepticism that Romer and Romer episodes correspond to periods in which there were exogenous monetary policy shocks. For example, Romer and Romer (1989) emphasize that the periods they focus on correspond to times when the Fed was particularly concerned about inflation, suggesting that these are times when policy was tight not because ε_{st} was high, but because $\psi(\Omega_t)$ was high.

3. Monetary Policy Shocks and Aggregate Macroeconomic Variables

This section accomplishes two tasks. First, we display the measures of shocks to U.S. monetary policy corresponding to the different identifying assumptions discussed in section 2. Particular attention is paid to (i) comparing the Romer and Romer (1989) index with our VAR based policy shock measures, and (ii) documenting the role that commodity prices play in resolving the price puzzle. Second, we assess the robustness of inference regarding the effects of shocks to monetary policy on various economic aggregates.

3.1. Comparing the alternative policy shock measures

Figure 3.1 graphs the different shock measures discussed in section 2. Shock measures based on VAR's were estimated using quarterly data over the sample period 1960:1 to 1992:4. On the whole, the various policy measures that we study behave similarly, though they do display some interesting differences.

Since the policy shock measures based on VAR's are by construction serially uncorrelated, they are quite noisy. For ease of interpretation we report the centered, three quarter moving average of the shocks, i.e., $(\varepsilon_{s,t+1} + \varepsilon_{s,t} + \varepsilon_{s,t-1})/3$. Also, for convenience we include shaded regions, which begin at a National Bureau of Economic

Research (NBER) business cycle peak, and end at a trough. The upper left panel displays moving averages of the FF and $FF1$ policy shock measures in which commodity prices were included in the VAR. The upper right panel displays the analogous shocks for the case where commodity prices were excluded from the VAR. The lower panels report the corresponding shocks for the nonborrowed reserves based measures. Finally, the Romer and Romer episodes are depicted in each panel by the vertical lines (the 1974 episode is not displayed because it falls within an NBER shaded region).

Regarding the VAR shock measures, we find it useful to characterize monetary policy as 'tight' or 'contractionary', when the smoothed policy shock is positive, and 'loose' or 'expansionary' when it is negative.

According to Figure 3.1, all of the federal funds based shock measures indicate that policy tightened before each recession and eased around the time of each trough. With the exception of the 1981 - 1982 period, a similar pattern is observed for the nonborrowed reserve based shock measures.

Notice that with some important exceptions, when commodity prices are included in the analysis, the federal funds and nonborrowed reserve based policy shock measures and the Romer and Romer episodes are in rough agreement. (There is a tendency for the VAR shocks to lag the Romer and Romer epsiodes by one or two quarters.) For example, the VAR based policy shock measures agree with the Romer and Romer assessment that 1979 and 1988 were periods of tight monetary policy. If we exclude $PCOM$ from the analysis, then there is less agreement between the different measures. For example, in 1966:2 both the federal funds and nonborrowed reserve based shock measures indicate tight monetary policy when commodity prices are included. When commodity prices are excluded, then the exogenous component of policy is neutral, or slightly expansionary according to the federal funds measure.

There are three periods when there is significant disagreement between the policy shock measures: 1973, 1974 and 1981. First, Romer and Romer do not identify 1973 as a period of monetary tightening. Yet, the nonborrowed reserve based measures signal a monetary contraction. The federal funds based policy shock measures do too, but only if commodity prices are excluded from the analysis. When they are included (as they are in the benchmark specification), then the federal funds based policy shock measures indicate only a very small tightening in 1973.

Second, there is a Romer and Romer episode in 1974. This is the only such episode that occurs in the middle of a recession. The Romer's make no explicit claim about whether this policy tightening was an exogenous policy shock (i.e., a positive realization of ε_{st}) or a systematic response to the oil shock (i.e., a rise in $\psi(\Omega_t)$.)[11] However, as we noted in the introduction, to interpret subsequent events as reflecting the effects of the contraction requires the assumption that it was exogenous. Our VAR based policy shock measures suggest that it was not. These measures indicate that the Fed's policy actions in this period reflect a predictable response to the rise in commodity prices associated with the oil shock. Figure 3.1 indicates that for both VAR based policy shock measures the contraction *does* look like a shock if commodity prices are excluded. But, when commodity prices are included, policy was not unusually tight. Since there was substantial inflation after this episode, this observation helps explain the result documented below that incorporating $PCOM$ into the analysis helps resolve the price puzzle.

Third, the nonborrowed reserves based policy shock measures agree with the Romer and Romer measure that 1981 was not a period of tight monetary policy. By contrast, the federal funds based measure indicates very tight policy in 1981.

Relative to the Romer and Romer index, an important advantage of the VAR based policy shock measures is that they also identify periods of monetary expansion. For example, all of the VAR based shock measures indicate that monetary policy was expansionary at the end of the 1973-74 recession, up until 1976. This period also generates an important difference between nonborrowed reserves policy shock measures based on systems that do and do not include commodity prices. The inclusion

[11]For an elaboration of the Romers' views on this point, see their discussion on p. 149 of Romer and Romer (1989).

of commodity prices makes policy look much more expansionary in 1976. Since inflation increased after this period, this suggests a reason why incorporating commodity prices into the analysis resolves the price puzzle for nonborrowed reserves based policy shock measures. Similarly, the fact that fed funds based policy shock measures do not indicate that policy was tight in 1973 when commodity prices are included may explain why incorporating commodity prices helps those measures avoid the price puzzle.

With one exception, there is little noteworthy difference between VAR shocks computed based on the benchmark policy ordering and those based on the monetary policy first ordering. The exception is the 1973 period for the federal funds policy shock measure based on the VAR that includes $PCOM$. According to the $FF1$ measure, which assumes policy does not feedback on the contemporaneous value of $PCOM$, policy was tight. According to the FF measure, which does allow contemporaneous feedback, it was not tight. This is consistent with the idea that Fed policy actions in that episode reflected the systematic response of policy to innovations in commodity prices.

3.2. Confronting the price puzzle

Figure 3.2 displays the impulse response of the GDP deflator (P) to ten different contractionary monetary policy shock measures. The solid line is our point estimate, and the dashed lines represent plus and minus one standard error bands.[12] The left-hand column displays the effects of policy shocks in VAR systems which include commodity prices, $PCOM$; the right-hand column displays the analogous effects from systems which exclude commodity prices. Five interesting results emerge here. First, impulse response functions based on VAR's that do not incorporate $PCOM$ imply a rise in the price level that lasts for several years after a contractionary monetary

[12]These were computed using the Monte Carlo method described in Doan (1990), example 10.1, using 100 draws from the estimated asymptotic distribution of the VAR coefficients and the covariance matrix of the innovations, u_t, in (2.2). The point estimates and standard errors of our coefficients are the average and standard deviation across draws of the simulated impulse responses.

policy shock. This phenomenon is what we referred to previously as the price puzzle. Second, regardless of the identification used, the inclusion of $PCOM$ either reduces or eliminates the puzzling behavior of P relative to VAR systems which exclude $PCOM$. Third, only for an $FF1$ policy shock is the price puzzle not eliminated by the inclusion of $PCOM$. Recall that the underlying identification assumption here is that the contemporaneous portion of the Fed's feedback rule for setting the period t federal funds rate does not include P_t, Y_t or $PCOM_t$. The 'ast two findings seem consistent with Sims' (1992) conjecture that the price puzzle emerges when the Fed has more information at its disposal regarding inflation than is allowed for in the identification scheme used to measure the policy shock.

Fourth, the findings regarding the implications of the Romer and Romer episodes for the price puzzle closely resemble those discussed above. That is, a price puzzle emerges when $PCOM$ is not included in the analysis, but there is no price puzzle when $PCOM$ is included in the analysis.

Fifth, when $PCOM$ is included in the analysis, the price level hardly falls at all for several years after an exogenous monetary contraction. The biggest effect is the one implied by FF. There, the price level is essentially unchanged for the first year and a half, and is down by about 0.4 percent after three years. One way to assess the magnitude of the price effect is obtained by noting that it is much smaller than the monetary effect of a policy shock. In the next subsection we show that an FF policy contraction drives $M1$ down by about 0.4 percent after only abouı 1 year.

In the remainder of the paper, unless specifically noted, we focus on the Romer and Romer and federal funds based policy shocks, allowing for commodity prices in the analysis. The analog impulse response functions corresponding to the nonborrowed reserve based policy shocks are reported in the Appendix.

3.3. Effects on monetary variables

In order to assess the overall plausibility of our different policy shock measures we now turn to their effects on four short term money market variables. Figure 3.3 displays the effects of FF, $FF1$, and FFD policy shocks, and Romer and Romer episodes on the federal funds rate, government securities held by the Federal Reserve ($GOVSEC$), total reserves (TR), and $M1$. Several interesting results emerge here. First, for each of the federal funds based policy shock measures the increase in the federal funds rate is persistent. The rise induced by an $FF1$ policy shock is more persistent than that induced by an FF policy shock.[13] The contemporaneous rise in the federal funds rate induced by an FF or an $FF1$ shock is roughly equal to about 80 basis points. The effect is similar for the nonborrowed reserve based shock measures (reported in the Appendix), except that the initial increase in the federal funds rate is smaller (about 40 basis points). Second, consistent with interpreting the federal funds based shocks as monetary policy shocks, the Federal Reserve's holdings of government securities falls after a contractionary policy shock, i.e. a contractionary monetary policy shock coincides with open market sales of government securities. Third, total reserves and $M1$ fall for all of the policy shock measures. However, the response function of total reserves is imprecisely estimated.

Notice that the same qualitative response functions emerge for the Romer and Romer episodes: the federal funds rate increases, the Federal Reserve's holdings of government securities falls, total reserves fall, and M1 falls. There are differences pertaining to the magnitude and timing of the responses, however. Romer and Romer episodes coincide with periods in which there were large rises in the federal funds rate. The maximal impact on the federal funds rate after a Romer and Romer episode is roughly 250 basis points. In contrast, the maximal impact on the federal funds rate

[13] Since the federal funds rate is included in Z_t, the federal funds rate impulse responses are reported from VARs which do not include a seventh variable D_t. Because of this, the FF and FFD policy shocks are the same here.

induced by a $FF1$ policy shock is equal to 100 basis points.[14] The Romer and Romer episodes also induce larger effects on $GOVSEC$, TR, and $M1$ (by factors of 5 to 8) than do the federal funds based policy shocks. Notice also that the impact of a Romer and Romer episode is delayed relative to those of the federal funds based policy shocks. For example, the maximal increase in the federal funds rate occurs 7 quarters after a Romer and Romer episode; for federal funds based policy shocks, the maximal increase occurs in the second quarter after a shock.

For the subsequent analysis, it is important that we be able to rule out an alternative interpretation of VAR based policy shock measures, namely that they reflect shocks to money demand rather than to supply. According to this alternative view, (i) our policy shock measures reflect an unanticipated increase in the public's demand for money, and (ii) the subsequent reduction in economic activity is due to the interest rate effect. This interpretation runs into two difficulties. First, the response of $GOVSEC$ is perverse if our policy shock measures actually reflect shocks to the demand for money. That is, in response to the public's heightened demand for money, the Federal Reserve would be draining liquidity from short-term money markets and driving up short-term interest rates even further. If anything we expect that the Federal Reserve tries to accommodate the increased demand for reserves via increases in $GOVSEC$. Second, if the public is demanding more money, it seems very puzzling that broad aggregates like $M1$ would be falling. In light of these considerations, we conclude that the impulse response patterns displayed in Figure 3.3 strongly favor the interpretation that our benchmark policy shock measures reflect exogenous shocks to monetary policy.

[14]These responses to a Romer and Romer episode are comparable to the 350 basis point effects that Eichenbaum and Evans (1992) estimated in monthly VAR systems which included international monetary policy reaction functions over the shorter sample period 1974-1990.

3.4. Effects on aggregate variables

We now consider the effects of monetary policy shocks on various measures of aggregate economic activity and on commodity prices. Figure 3.4 displays the effects of FF, $FF1$, and FFD shocks, and Romer and Romer episodes on real GDP (Y), aggregate employment (EMP), the unemployment rate ($UNEMP$), and the change in commodity prices ($PCOM$). Recall that in the identification scheme underlying a FF policy shock, monetary policy responds systematically to the contemporaneous state of the aggregate economy. So Y and $PCOM$ are always ahead of FF or $NBRD$ in the Wold causal chain used to identify a shock to monetary policy. When employment and the unemployment rate are included in the VAR, they too are assumed to be Wold causally prior to the federal funds rate. Consequently, in these benchmark VAR specifications, FF and FFD policy shocks are the same.

A number of interesting results emerge from Figure 3.4. First, real economic activity falls steadily after a contractionary FF policy shock. Specifically, both output and employment fall, while unemployment rises. Second, after an $FF1$ policy shock, real activity appears to initially rise by a small amount and then begins a steady decline. One possible explanation for this pattern is that the $FF1$ policy shock measure fails to control adequately for contemporaneous movements in real activity that are driven by nonmonetary shocks to the system.[15] Third, $PCOM$ falls in response to an FF policy shock. But, in response to an $FF1$ policy shock it rises initially, before falling. Again, this may reflect the presence of nonmonetary shocks that are not controlled for adequately in the $FF1$ policy shock measure. The analog $NBRD$ policy shock responses displayed in the Appendix give rise to similar response patterns.

As was the case for the money market variables, aggregate economic activity responds to a Romer and Romer episode like it does to federal funds based policy shocks, except with a delay. As can be seen in figure 3.4, the initial drop in output

[15] This explanation is similar to Sims' (1992) original explanation of the price puzzle.

after a Romer and Romer episode is negligible. Employment only begins to fall substantially after eight quarters, while the unemployment rate begins its fall after about a year. This is not surprising in light of the fact that the maximal impact of a Romer and Romer episode on the federal funds rate occurs seven quarters after the shock.

Figure 3.5 displays the effects of a contractionary monetary policy shock on Retail Sales ($RSALES$), profits in the Trade Sector ($TR\ PROF$), profits in the nonfinancial sector ($NF\ PROF$), and inventories held in the manufacturing sector ($MFG\ INVT$). The results are quite robust across the federal funds based shocks and Romer and Romer episodes.[16] Retail sales fall in response to a contractionary monetary policy shock. Profits in the trade sector fall contemporaneously (except for the case of a FFD policy shock where the identification restrictions preclude such a response). Profits in the nonfinancial business sector fall substantially across all of the policy shock measures, although the negative response is delayed for $FF1$ policy shocks and Romer and Romer episodes. Consistent with the analysis above, this may reflect that (i) the $FF1$ policy shock measures do not adequately control for nonmonetary impulses to real activity, and (ii) the general pattern of a delayed response to a Romer and Romer shock.

3.5. Summary

We conclude that while there are some differences across the alternative policy shock measures considered, the basic qualitative response of the system to different policy shock measures is quite robust. There is little to be gained by using the Romer and Romer index of monetary contractions. Using these episodes rather than the federal funds based policy shock measures amounts to throwing out a large number of interesting episodes as well as information about the intensity of each episode. Not surprisingly, the resulting response functions are estimated very imprecisely.

[16]The Appendix documents the robustness of these results with repsect to nonborrowed reserve based policy shock measures.

4. The Flow of Funds

An important focus of the analysis in Christiano, Eichenbaum and Evans (1994) is the response of net funds raised by different sectors of the economy to shocks in monetary policy. Our key findings were that after a contractionary policy shock (i) net funds raised by the business sector rises for a substantial period of time before falling and (ii) the initial response of net funds raised by the household sector is not statistically significant. In this section we assess the robustness of these findings to the different measures of policy shocks.

4.1. Sectoral data concepts

Sectoral data on *net funds raised* in financial markets are reported in the Flow of Funds accounts (FOFA). These data can also be computed from the National Income and Product Accounts (NIPA) data.[17] The sectors which we consider are: nonfinancial business, household, government (federal, state and local), financial business, foreign and the monetary authority. In this study we use both the FOFA and NIPA based measures of net funds raised for the business and corporate sectors; for the other sectors we report results only for the FOFA based measures of net funds raised.

4.2. Business and Corporate Sectors

Let $BNET$ and $CNET$ denote the FOFA based measures of real, net funds raised in the business and corporate sector, respectively. The corresponding NIPA measures are denoted by $BNET^*$ and $CNET^*$. Figure 4.1 displays the dynamic response functions of $BNET$, $BNET^*$, $CNET$, and $CNET^*$ to the federal funds based policy shock measures and a Romer and Romer episode. Row 1 indicates that in response to an FF policy shock, net funds raised by the business and corporate sectors initially *rise*. This rise, which lasts for roughly six months to a year, is more persistent for

[17]The NIPA measure of net funds raised by a sector corresponds to tangible investment minus savings for that sector. Christiano, Eichenbaum, and Evans (1994) provide a more detailed comparison of the FOFA and NIPA measures.

the NIPA based measures of net funds raised.[18] Figure 4.1 also indicates that the initial rise in $BNET$ and $CNET$ generated by an $FF1$ policy shock is both more persistent and larger than the rise induced by an FF policy shock. The responses of $BNET^*$ and $CNET^*$ are also larger and somewhat more persistent. This finding may reflect the idea, discussed in section 3, that $FF1$ policy shocks are contaminated by the effects of nonmonetary shocks.

Figure 4.1 indicates that our basic findings regarding the reponse of net funds raised by the business and corporate sector are robust to working with FFD policy shocks. The major difference is that here the policy shock cannot, by assumption, affect net funds raised contemporaneously. However, in the period immediately following the FFD policy shock, $BNET$, $BNET^*$, $CNET$, and $CNET^*$ all rise for roughly a year. As with the FF and $FF1$ policy shock measures, the responses are more persistent for the NIPA than for the FOFA based measures of net funds raised. The Appendix shows that these conclusions are robust to working with nonborrowed reserve based policy measures.

Finally notice that because of sampling uncertainty, little can be said about the dynamic response functions of net funds raised by the business and coporate sectors to a Romer and Romer episode.

4.3. Other sectors in the FOFA data

Figure 4.2 displays the response of FOFA based measures of net funds raised by households ($HNET$), the government ($GNET$), financial institutions ($FINET$), and the foreign sector ($FONET$) to our different policy shock measures. A number of results are worth noting. First, none of the federal funds based policy shock measures generate statistically significant movements in $HNET$ for roughly a year.[19] This finding is consistent with the limited participation assumption in Fuerst (1992) and

[18] In the Appendix we show that a $NBRD$ policy shock generates a similar response function.

[19] The Appendix shows that this is also the case for the nonborrowed reserves based policy shock measures.

Christiano and Eichenbaum (1992), namely, that households are slow to adjust their financial portfolios immediately after the realization of a monetary policy shock.[20] In contrast to the federal funds based shock measures, we find that the contemporaneous response of $HNET$ to a Romer and Romer episode is significantly different from zero and positive. This is somewhat puzzling since the other variables we investigated (see section 2) responded to a Romer and Romer episode with a substantial lag.

Second, $GNET$ declines for about two quarters following a contractionary policy shock, i.e. at the onset of a monetary contraction, net funds raised by the government falls. As the recession induced by the policy shock gains momentum, $GNET$ rises. This is true regardless of which federal funds based policy shock measure we consider. Christiano, Eichenbaum, and Evans (1994) explore in more detail the components of the government deficit that initially fall following an FF policy shock. They report that the initial decline primarily reflects a rise personal income tax receipts.

As it turns out, the initial decline in $GNET$ is not robust to which policy shock measure we look at. For example, figure 4.2 indicates that the decline following a Romer and Romer episode is not statistically significant. In the Appendix we show that the decline is also not significant for any of the nonborrowed reserve based policy shock measures. In this sense inference about the initial response of net funds raised by the government to a monetary policy shock is fragile.

Third, we do not find a systematic response of $FINET$ to a policy shock. Fourth, for the first year, and across all of the policy shock measures that we consider, net funds raised by the foreign sector ($FONET$) rises. One interpretation of this result is that the monetary contraction in the U.S. is transmitted internationally. This could occur endogenously through international trade; or more directly, the foreign monetary authorities could respond systematically to the state of the U.S. economy and trigger a monetary contraction in their countries. Just as $BNET$ rises in the U.S. at the onset of a monetary contraction, the foreign sector may be scrambling for

[20]This assumption applies to the underlying components of HNET, in addition to HNET itself. The behavior of the components is studied in Christiano, Eichenbaum and Evans (1994).

additional funds as the resulting recession is transmitted abroad. Two recent studies provide some corroborating evidence on this point. Eichenbaum and Evans (1992) find that contractionary U.S. monetary policy shocks lead to an appreciation of the dollar and a rise in foreign interest rates. Evans and Santos (1993) find that measured productivity in the G-7 countries falls following a contractionary U.S. monetary policy shock. This response is consistent with the idea that a U.S. monetary contraction leads to a foreign monetary contraction, which drives down foreign output and - because of labor hoarding, variable capital utilization or increasing returns to scale - foreign productivity.

To summarize, this section showed that net business borrowing initially rises after a monetary contraction. Because the flow of funds accounts form a closed system, it is in principle possible to answer the question: which sectors fund this rise in borrowing? As it turns out, we do not have a robust answer to this question. For federal funds based policy shock measures, the answer seems to be that the rise in business borrowing is funded by an initial reduction in government borrowing due to a rise in personal income tax receipts. For nonborrowed reserves based measures, the answer is more ambiguous, since none of the other sectoral responses are statistically significant.

5. Analysis of monthly data and deterministic trends

In this section we establish the robustness of our results along two dimensions: (i) the use of monthly rather than quarterly data, and (ii) detrending the data assuming quadratic time trends.[21] The monthly data are of interest for a number of reasons. First, there is no reason to think that policy actions occur at the quarterly level. Second, on a priori basis, it seems to us that recursive Wold casual identification schemes are more plausible the finer the time interval being considered. Unfortunately, the FOFA and NIPA measures of net funds raised by different sectors of the economy

[21]Our results regarding robustness to time aggregation are consistent with the findings in Geweke, Miller and Runkle (1994), and Owen (1994).

are not available at a monthly frequency. Consequently we look only at the robustness of our results concerning the effects of monetary policy shocks on money market variables, aggregate economic activity and the price level.

5.1. Monthly measures of FF Monetary Policy Shocks

In section 2 we posited that the collection of variables, Ω_t, used by the Fed to set policy contains at least the past values of $\{Y, P, FF, NBRD, TR, PCOM\}$ assuming commodity prices are included in the analysis. The data on $FF, NBRD, TR$, and $PCOM$ are available at a monthly frequency. However Y (real GDP) and P (the implicit GDP deflator) are available only at the quarterly frequency. Many researchers use industrial production in place of real GDP when working with monthly data (see for example Sims (1980) and Eichenbaum and Singleton (1986)). An important disadvantage of this measure is that it covers a narrow sector of the economy. For this reason, we chose to work with monthly nonagricultural employment. Coverage issues aside, it seems reasonable to think of the Fed's policy reaction function at the monthly level as depending on employment, since a large number of policy movements (systematic and nonsystematic) have occurred on the first Friday of the month when the employment report is released.[22]

This still leaves open the issue of how to measure P. Many analyses use the Consumer Price index (CPI) as a monthly measure of the price level. However, unlike the GDP deflator, the CPI is a fixed-weight deflator. An additional problem with the CPI is that for much of the sample period it measures the cost of shelter by including a mortgage cost component. Since this cost is directly related to interest rates, a 'price puzzle' could arise simply due to the mortgage cost component of the CPI. To deal with this problem, we looked at three alternative measures of the aggregate price level: the implicit consumption expenditure price deflator (PCE), the CPI, and the CPI less shelter ($CPILSN$).

[22]When we redo the analysis using industrial production in place of employment, we obtain very similar results.

Figure 5.1 displays the dynamic response functions of various variables to a monetary policy shock computed using our benchmark identification strategy.[23] In particular, the monetary policy instrument is assumed to respond contemporaneously to employment and prices and these are assumed not to respond contemporaneously to policy shocks. Rows 1, 2 , and 3 present dynamic response functions estimated from VAR systems which include $PCOM$; Row 4 reports dynamic response functions from systems where $PCOM$ has been excluded from the analysis.

Consider first the effects of FF policy shocks when $PCOM$ is included and the aggregate price level is measured using PCE. Row 1 indicates that the estimated dynamic response functions are very similar to the analog response functions obtained using the quarterly data. The impact of a policy shock on the federal funds rate is persistent and induces a drop in nonborrowed reserves that lasts about six months, as well as a longer lasting decline in $M1$ and $PCOM$. From row 2 we see that after a delay of a few months employment also drops.

Next we consider the price response to an FF policy shock. Notice that PCE does not respond for about six months, after which it begins a steady decline. This is qualitatively similar to the response of the implicit GDP deflator discussed in section 3. Notice that when the CPI is substituted for PCE in the VAR, the price puzzle reemerges, with CPI *increasing* for about 12 months after a positive FF policy shock. Moreover, the initial rise is statistically significant. Consistent with the notion that the mortgage cost component of the CPI is playing a large role in this rise, when $CPILSN$ is used rather than CPI, the rise in the price level induced by the policy shock is not as large or as significant. Still a small 'price puzzle' remains. This may be due to the fact that $CPILSN$ (like CPI) is a fixed weight index, unlike PCE and the GDP deflator which have time-varying weights.

Row 3 of figure 5.1 displays the effects of an $NBRD$ policy shock on employment and the three price levels when $PCOM$ is included in the VAR system. As was the

[23]These were estimated from unconstrained VAR's that included 12 lags of all variables.

case for an FF policy shock, employment falls steadily following an $NBRD$ policy shock. The response of PCE is negative throughout, while CPI and $CPILSN$ rise for about six months before falling. Since all three response functions have very wide standard error bands, there is no interesting sense in which a price puzzle emerges. Similar results when $PCOM$ is excluded from the analysis.

To assess the role of commodity prices in resolving the price puzzle at the monthly level, row 4 displays the effect of an FF policy shock on employment and the three price levels when $PCOM$ is excluded from the VAR system. Notice that the price puzzle emerges for all three measures of the aggregate price level. The PCE response is borderline significant initially, rises for about six months before falling below zero after about 18 months. The initial rises in CPI and $CPILSN$ are larger, more significant, and more persistent than the PCE response. We conclude that, as with the quarterly data, including $PCOM$ plays an important role in eliminating or mitigating the price puzzle.

5.2. Allowing for deterministic trends

As a final check on the robustness of our results we re-estimated the quarterly models allowing for a quadratic time trend in the variables of interest. Figure 5.2 displays results for an FF policy shock. As can be seen our basic results are quite robust.

6. Conclusions

This paper assessed the effects of shocks to monetary policy using alternative identifying restrictions to those used in Christiano, Eichenbaum and Evans (1994). An important objective was to discover on what dimensions inference is sensitive to identification assumptions. With the exception of the Romer and Romer episodes, we confined ourselves to identification schemes that correspond to imposing Wold causal orderings on the innovations in VAR's. By no means does this exhaust the class of identifying assumptions that have been used in the literature. Alternative classes of identifying assumptions include those that involve restrictions on the long-run impact of shocks to monetary policy. (See, for example, King and Watson (1992).) An alternative class of identifying assumptions are non-recursive schemes of the type considered by Bernanke (1986), Gali (1992) and Sims (1986), among others. These are sometimes referred to as 'structural VAR's'. It would be of interest to investigate the sensitivity of inference to adopting these types of identifying restrictions, as well.

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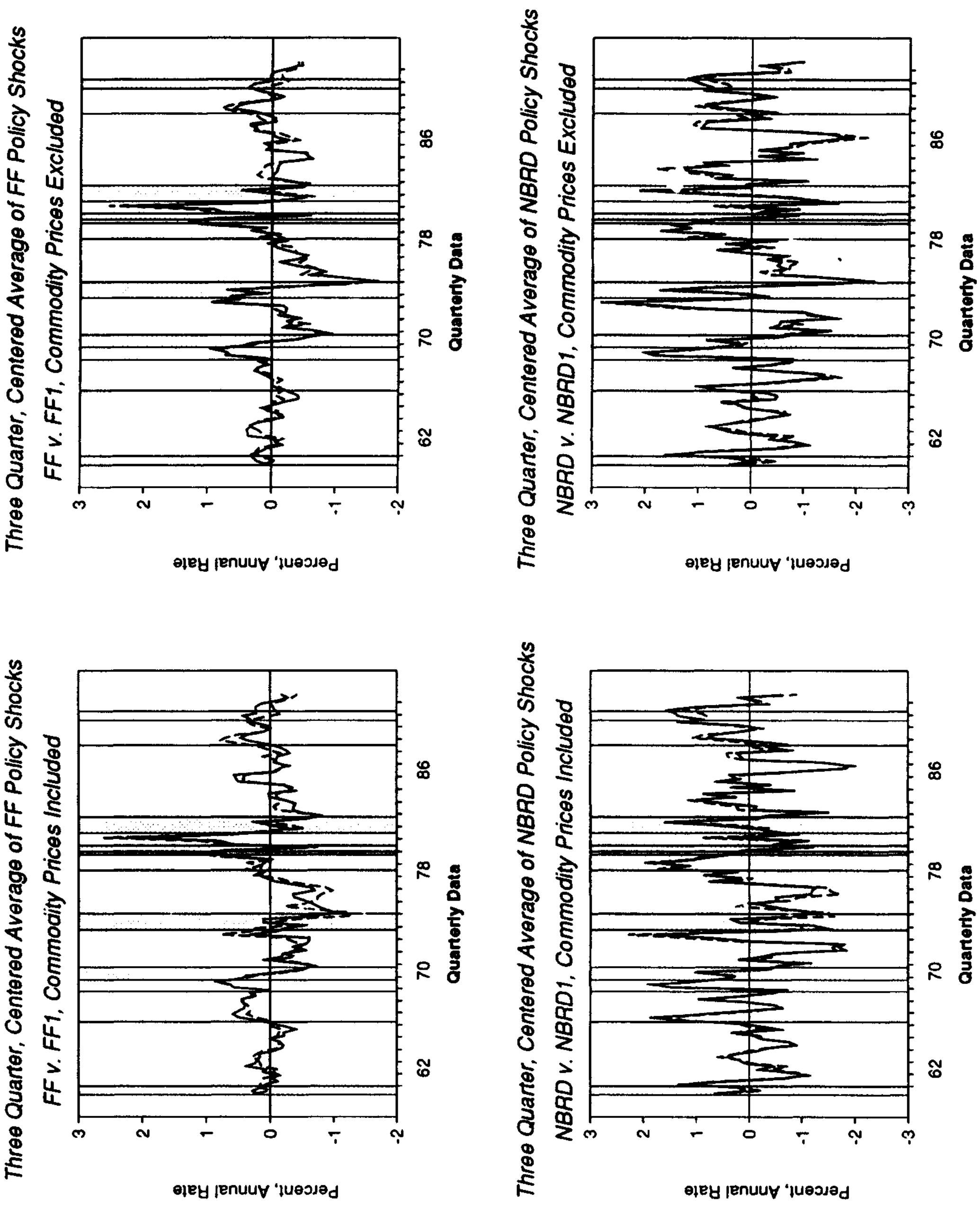
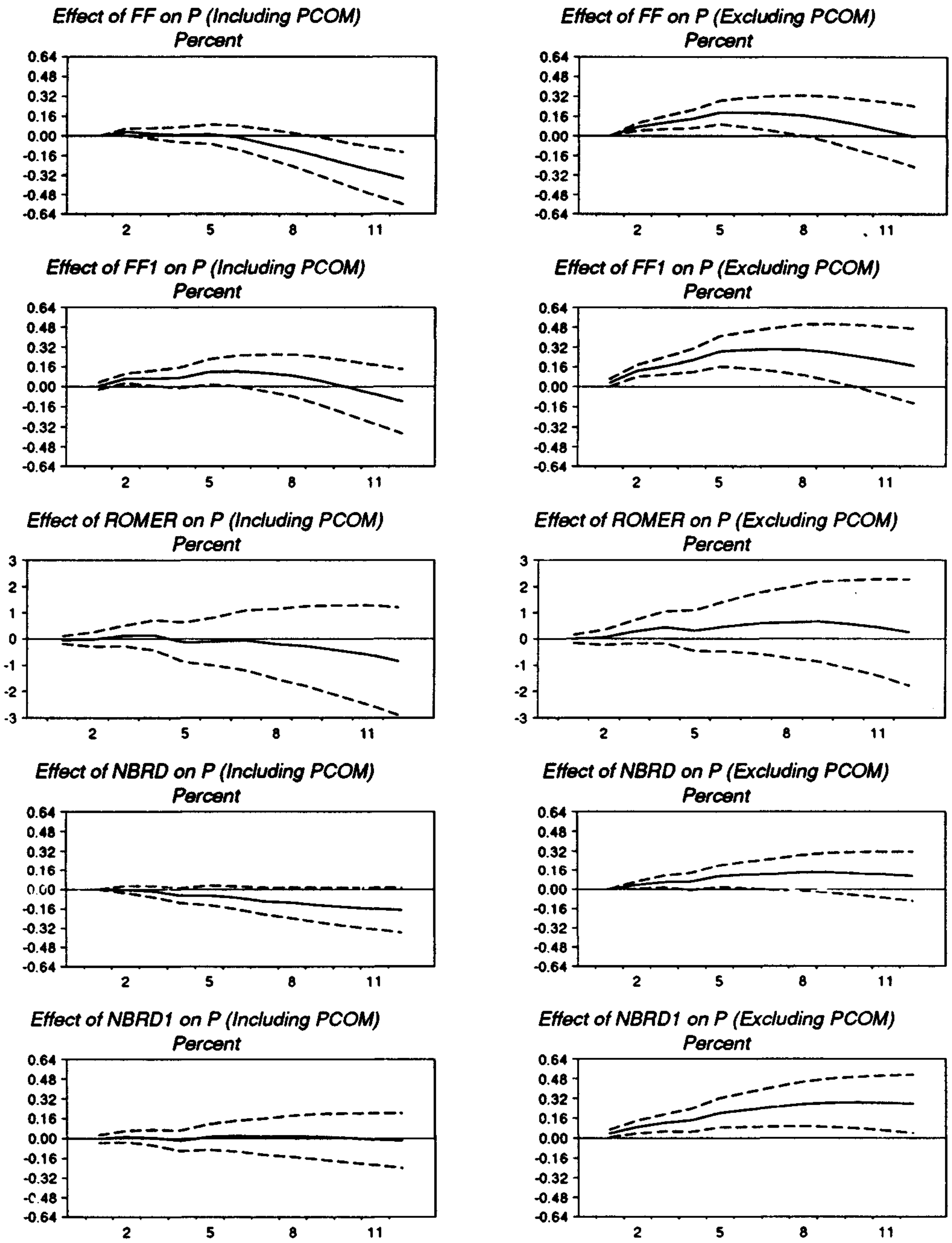
Figure 3.1: Alternative Measures of Monetary Policy Shocks[24]



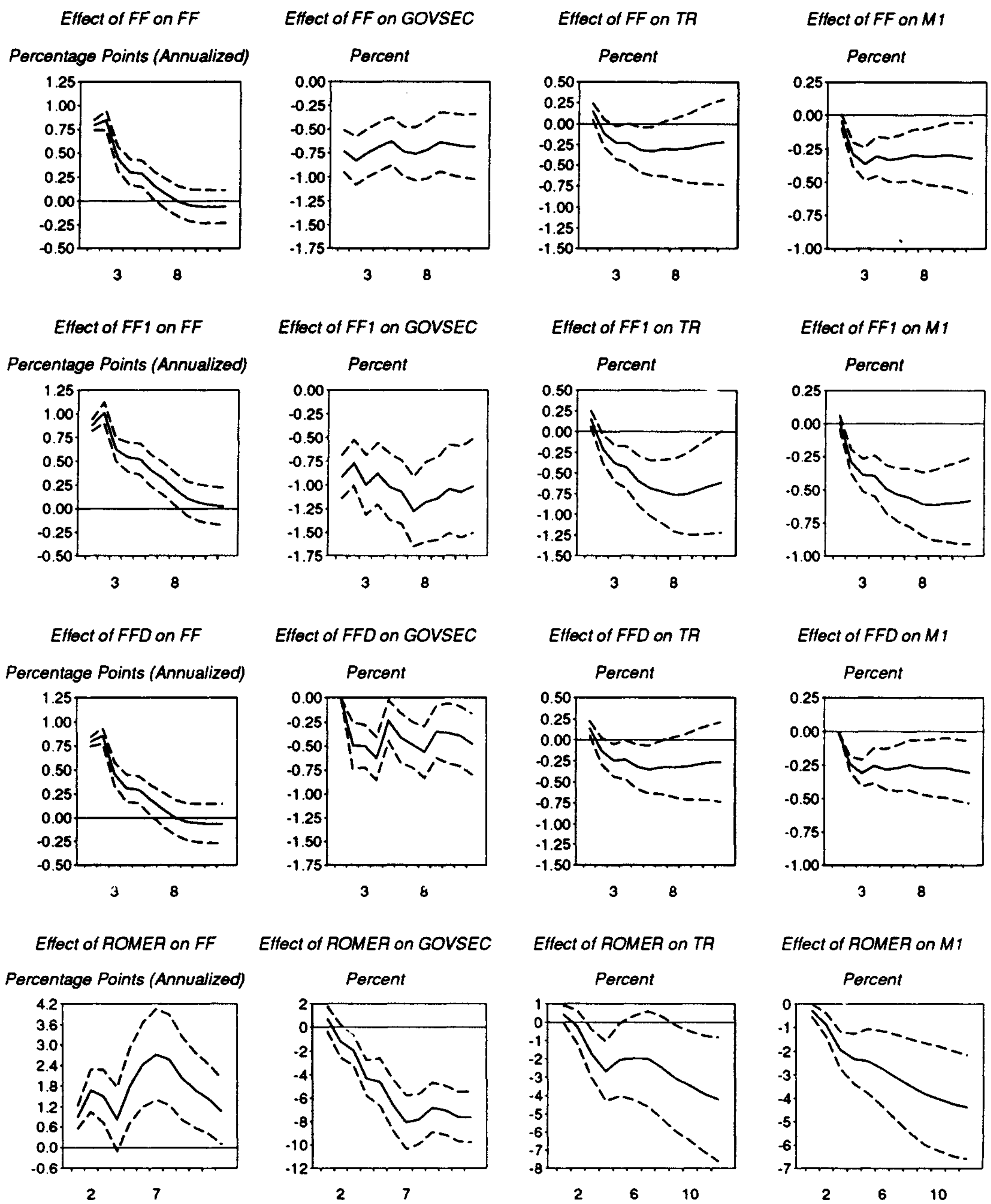
[24]The solid lines are the FF and NBRD measures of policy shocks; the dashed lines are the FF1 and NBRD1 measures of policy shocks. In each case, the three-quarter, centered moving averages are computed with equal weights applied to the time t-1, t, and t+1 orthogonalized innovations.

Figure 3.2: Effect of Policy Shocks on the Price Level[25]



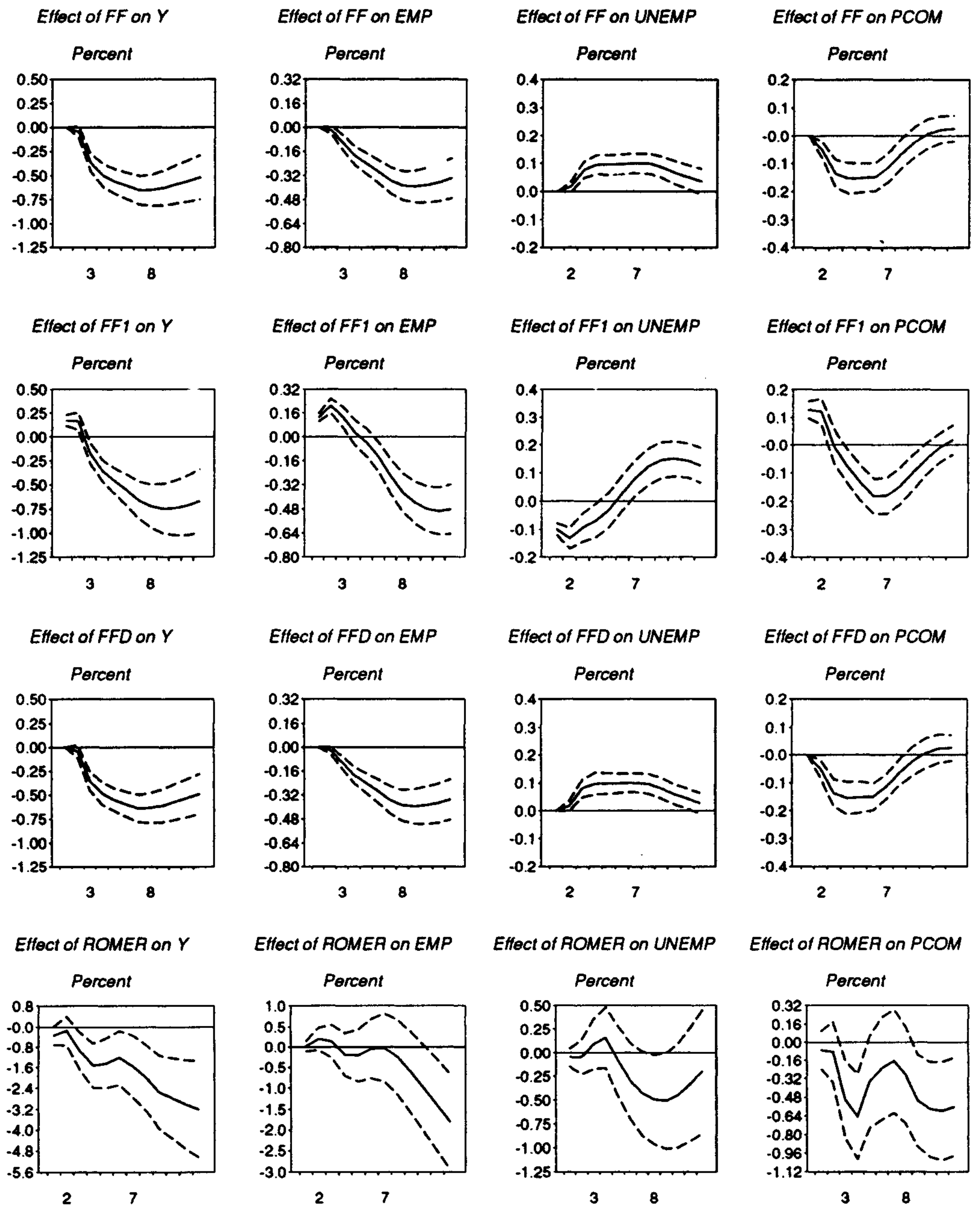
[25]The effects of alternative policy shock measures on the price level (P). The dashed lines are one-standard error bands.

Figure 3.3: Effect of Policy Shocks on Monetary Variables[26]



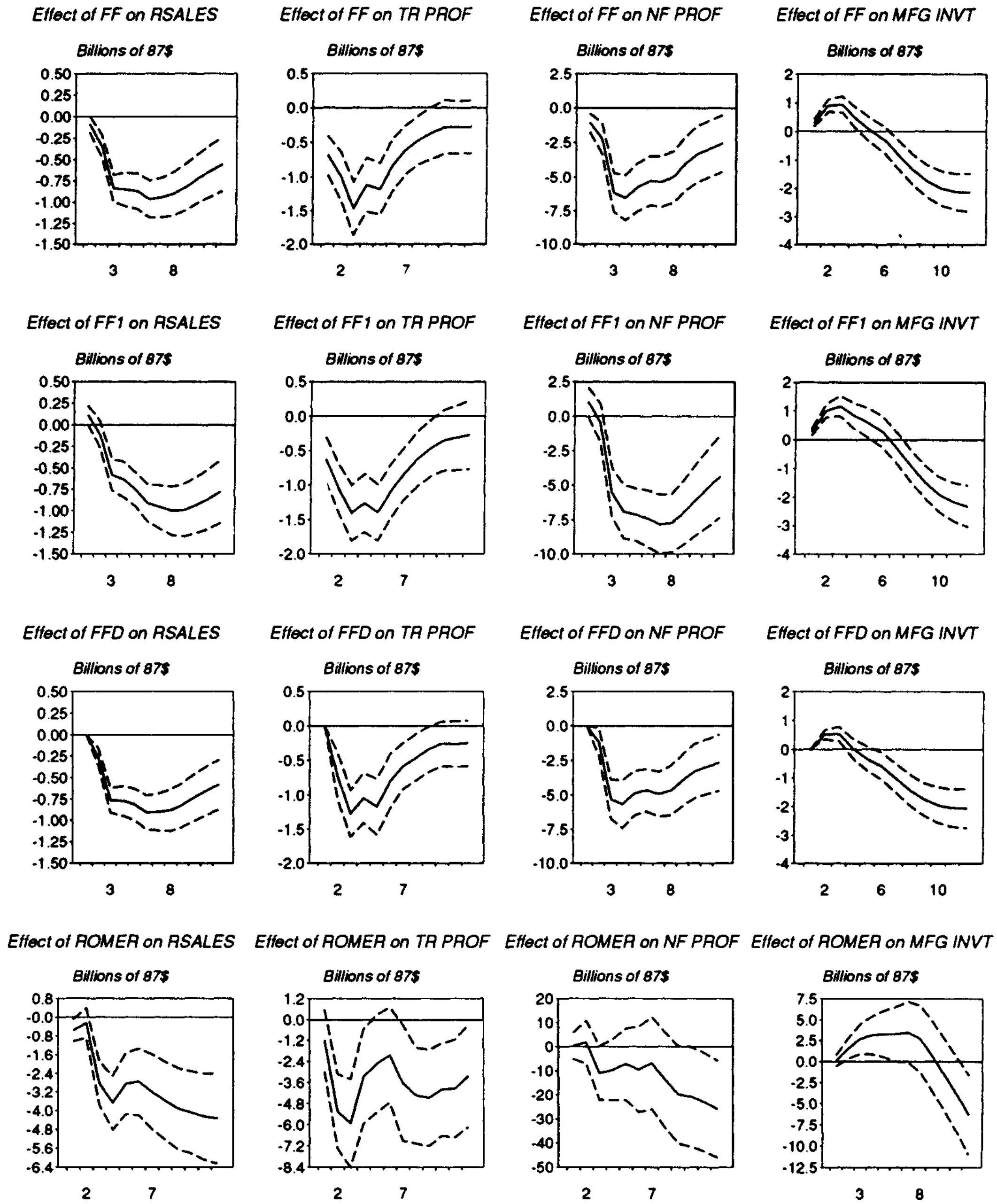
[26]The effects of alternative FF policy shock measures and the ROMER index on the Federal Funds rate (FF), government securities held by the Fed (GOVSEC), Total Reserves (TR), and M1. The dashed lines are one-standard error bands.

Figure 3.4: Effect of Policy Shocks on Macroeconomic Variables[27]



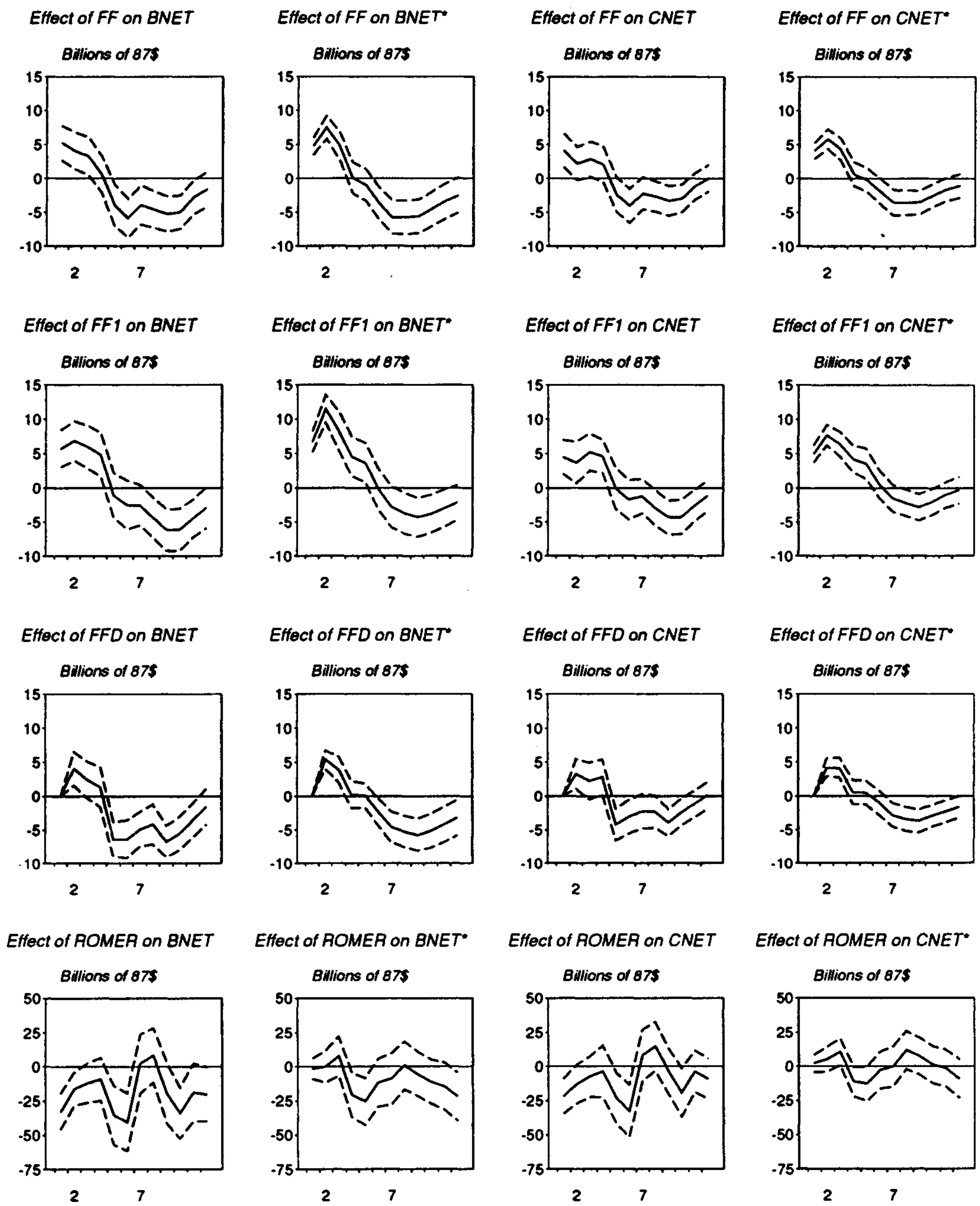
[27]The effects of alternative FF policy shock measures and the ROMER index on real GDP (Y), employment (EMP), the unemployment rate (UNEMP), and commodity prices (PCOM). The dashed lines are one-standard error bands.

Figure 3.5: Effect of Policy Shocks on Sectoral Variables[28]



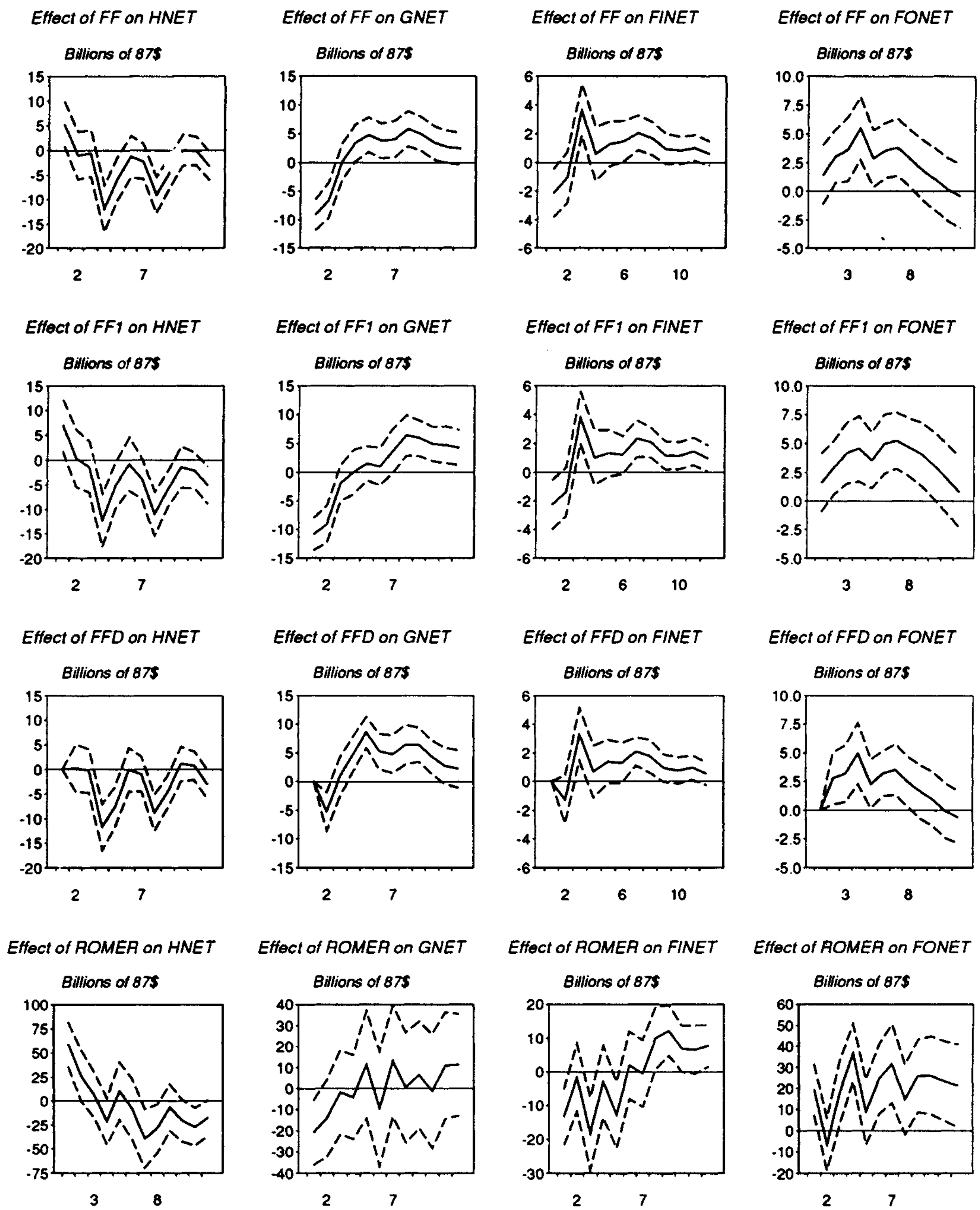
[28]The effects of alternative FF policy shock measures and the ROMER index on retail sales (RSALES), corporate profits in the trade sector (TR PROF) and nonfinancial sector (NF PROF), and manufacturing inventories (MFG INVT). The dashed lines are one-standard error bands.

Figure 4.1: Effect of Policy Shocks on Net Funds Raised by the Business and Corporate Sectors[29]



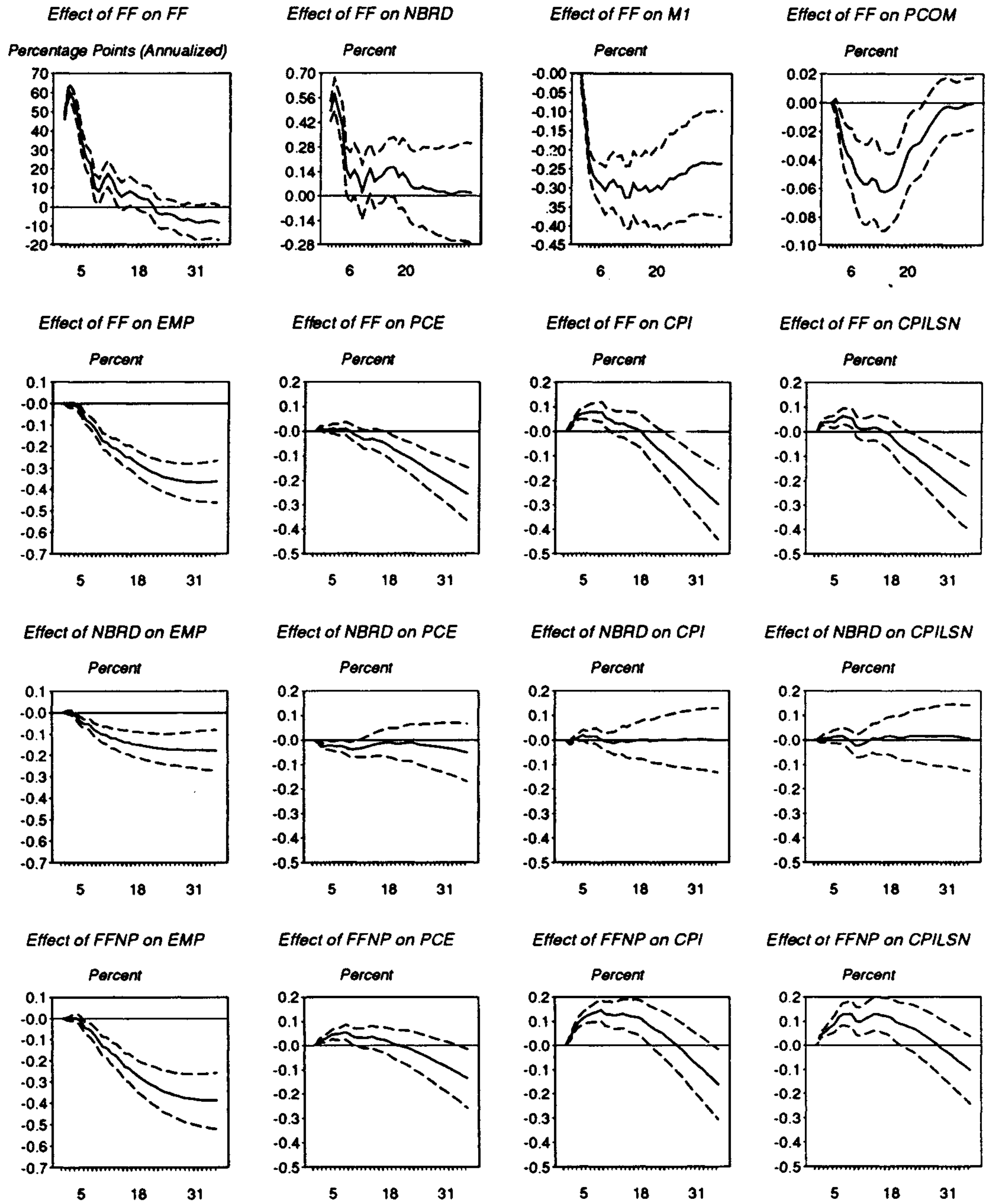
[29]The effects of alternative FF policy shock measures and the ROMER index on business sector data. The dashed lines are one-standard error bands.

Figure 4.2: Effect of Policy Shocks on Net Funds Raised by the Household, Government, Financial, and Foreign Sectors[30]



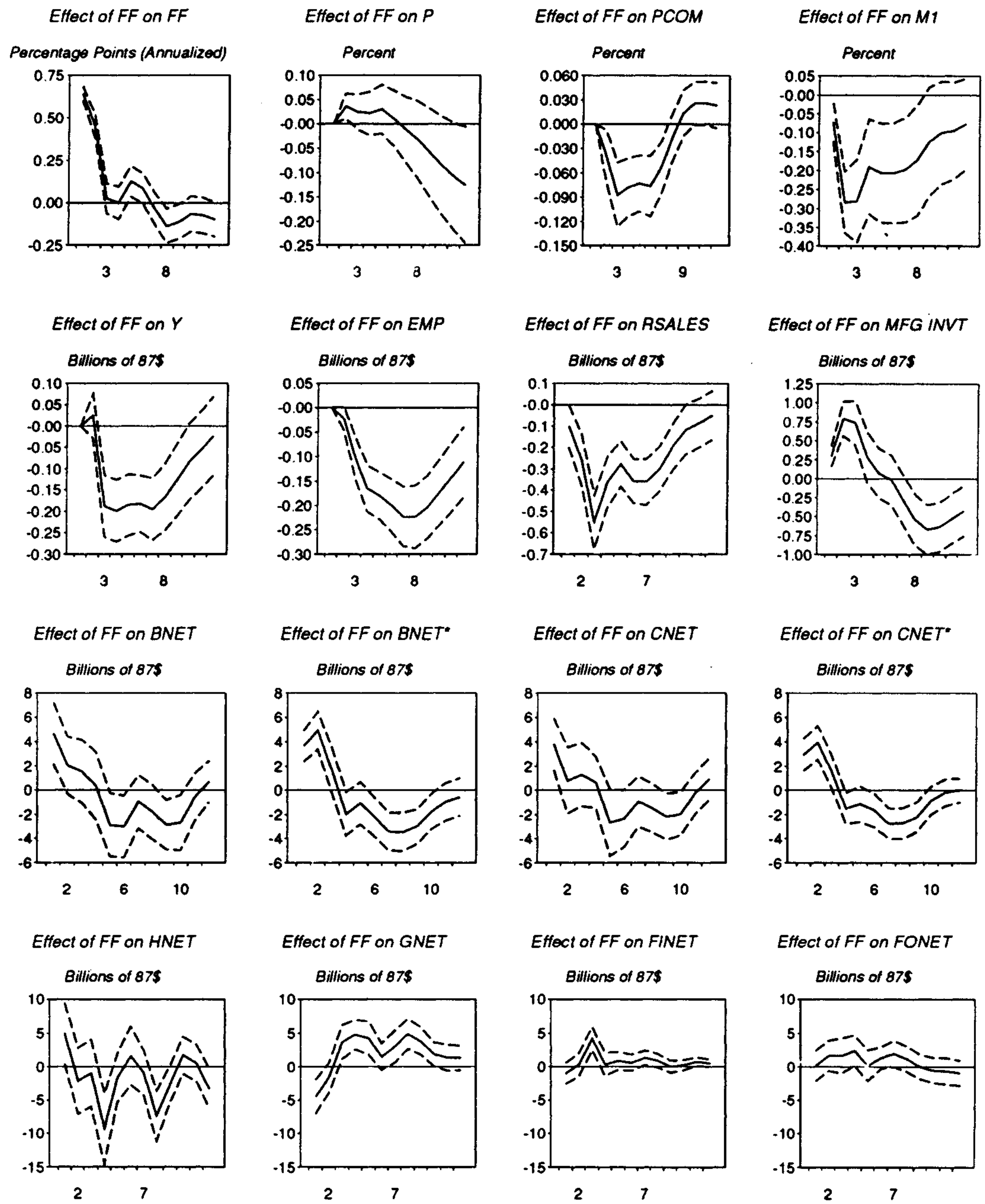
[30]The effects of alternative FF policy shock measures and the ROMER index on net funds raised in other sectors. The dashed lines are one-standard error bands.

Figure 5.1: Effect of FF Policy Shocks in Monthly Data[31]



[31]The effects of FF policy shock measures in monthly data on the Federal Funds rate (FF), NBRD, M1, PCOM, employment (EMP), implicit consumption expenditures deflator (PCE), CPI, and CPI less shelter (CPILSN). The FFNP shock is estimated from a VAR which excludes PCOM. The dashed lines are one-standard error bands.

Figure 5.2: Effect of FF Policy Shocks in VARs with Quadratic Time Trends[32]

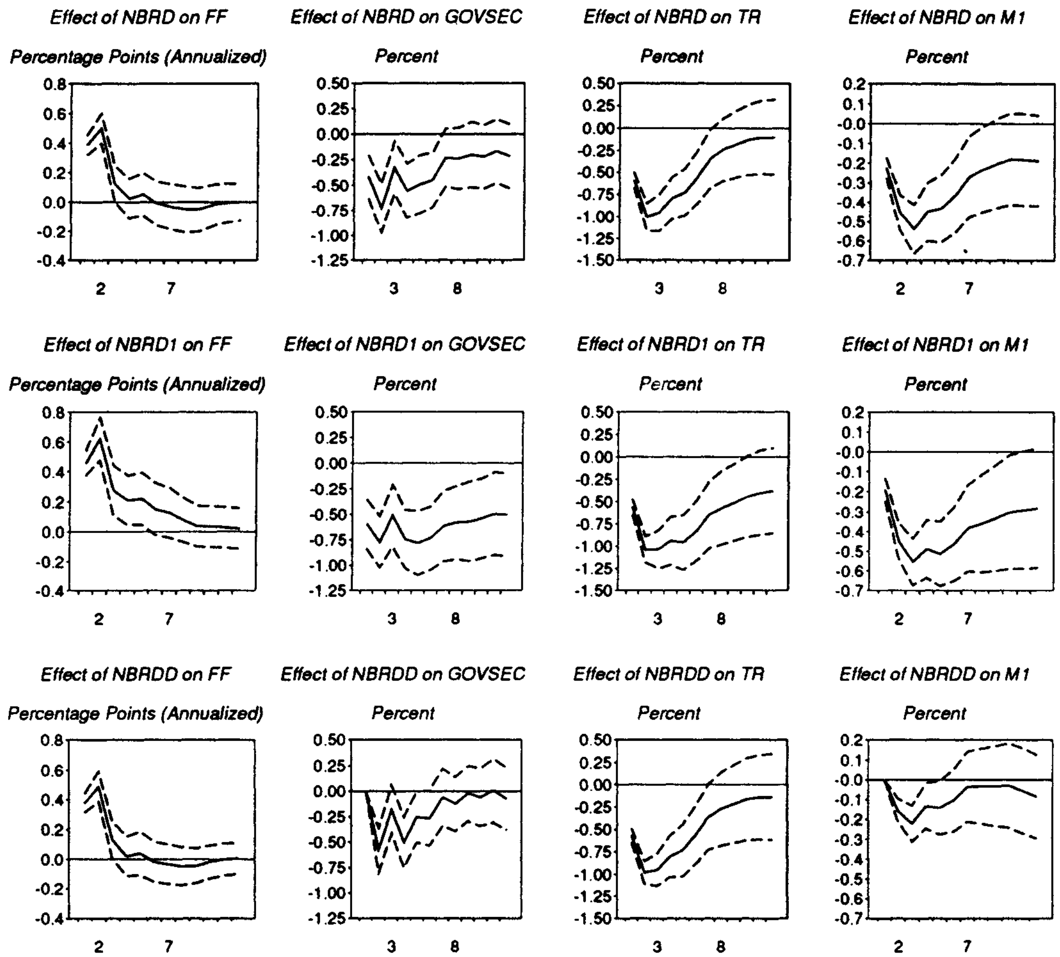


[32]The effects of FF policy shock measures when quadratic trends are accomodated in the VARs. The dashed lines are one-standard error bands.

A. Appendix

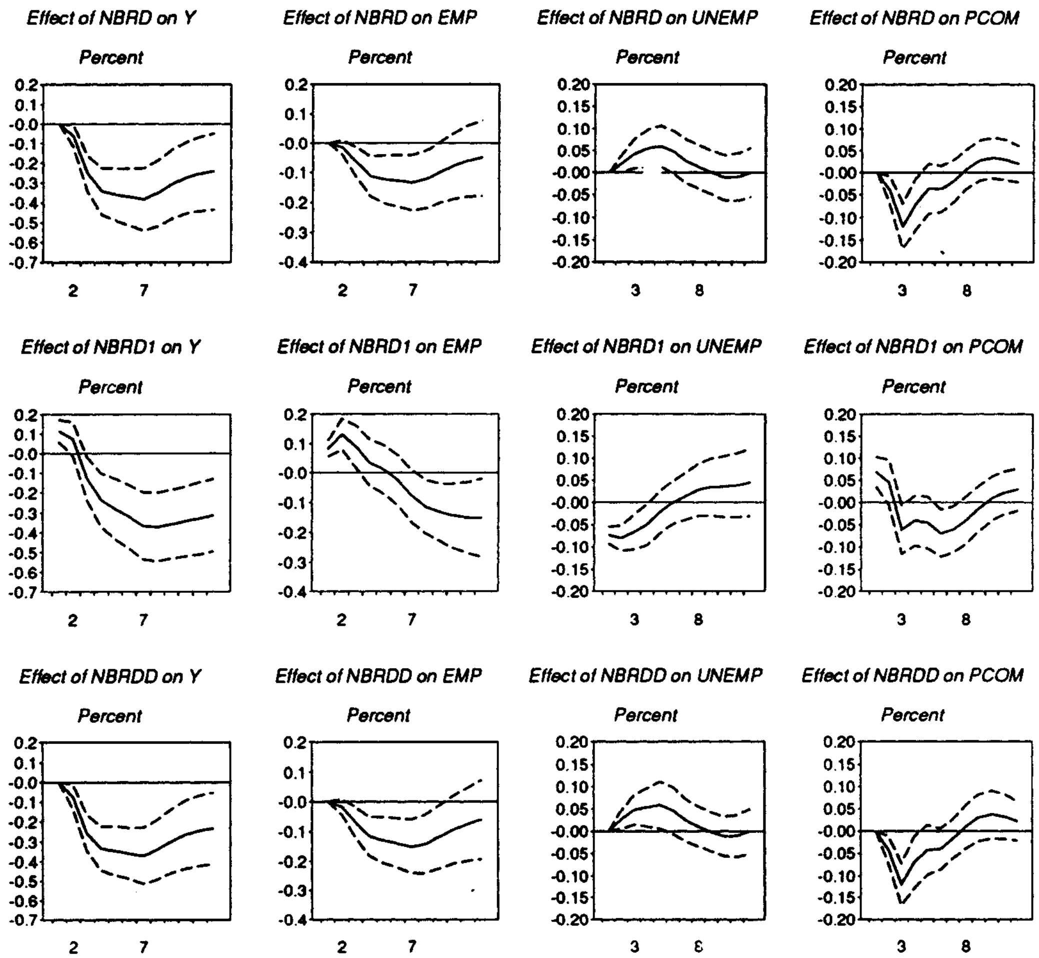
This appendix displays the dynamic response functions of various variables to positive shocks in NBRD.

Figure A3.3: Effect of Policy Shocks on Monetary Variables[33]



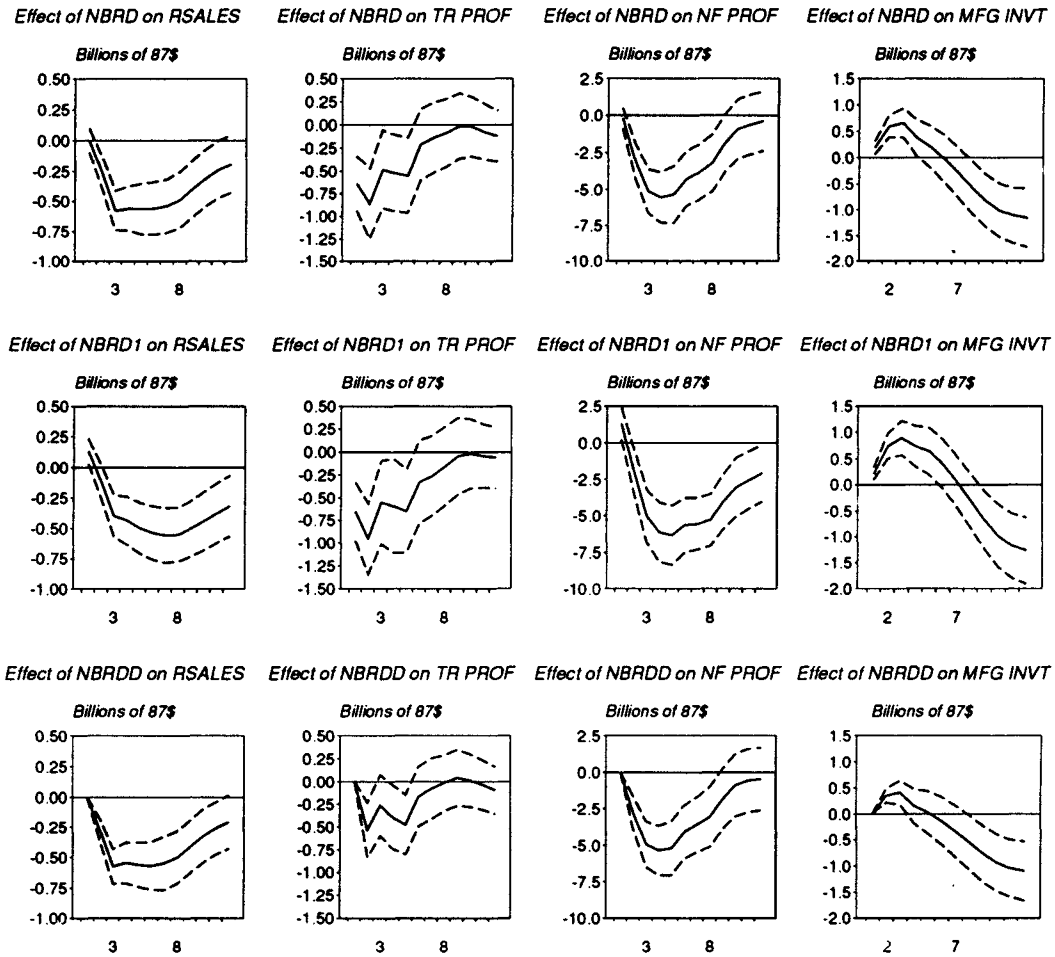
[33]The effects of alternative NBRD policy shock measures on the Federal Funds rate (FF), government securities held by the Fed (GOVSEC), Total Reserves (TR), and M1. The dashed lines are one-standard error bands.

Figure A3.4: Effect of Policy Shocks on Macroeconomic Variables[34]



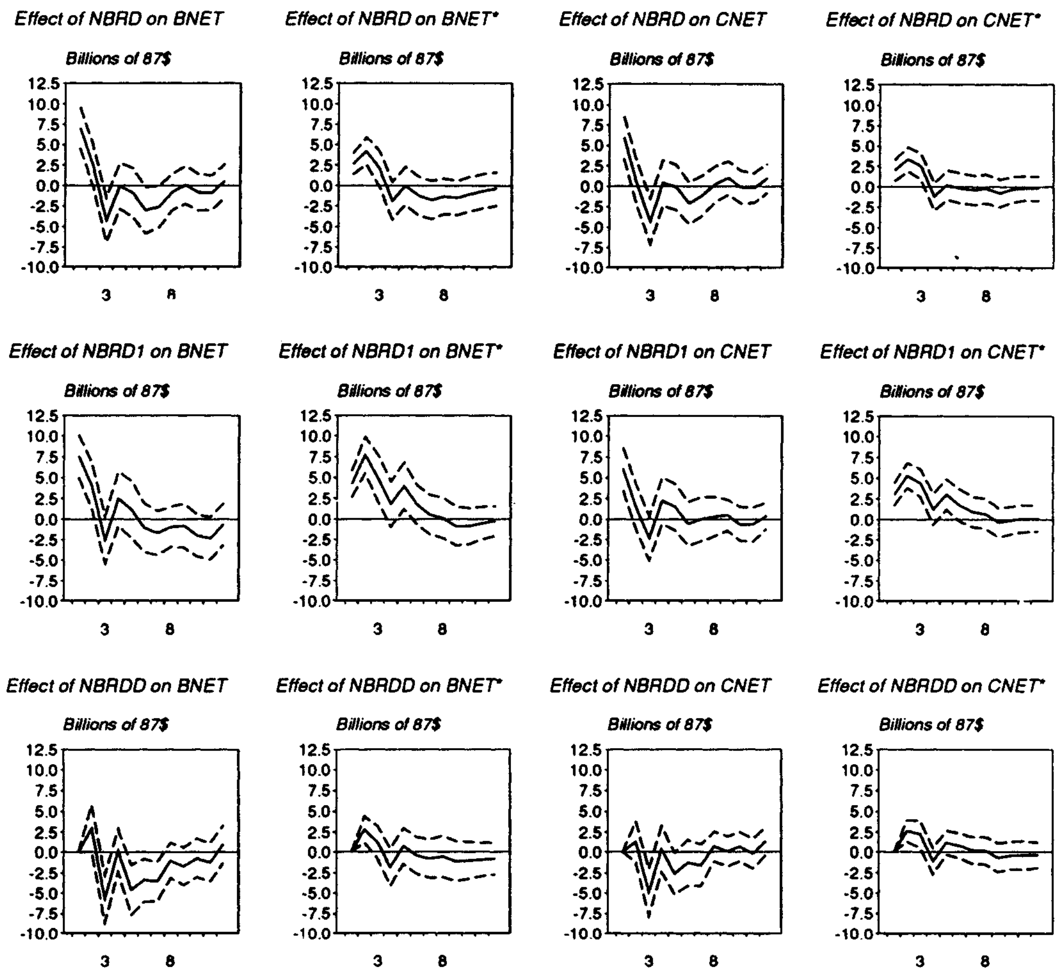
[34]The effects of alternative NBRD policy shock measures on real GDP (Y), employment (EMP), the unemployment rate (UNEMP), and commodity prices (PCOM). The dashed lines are one-standard error bands.

Figure A3.5: Effect of Policy Shocks on Sectoral Variables[35]



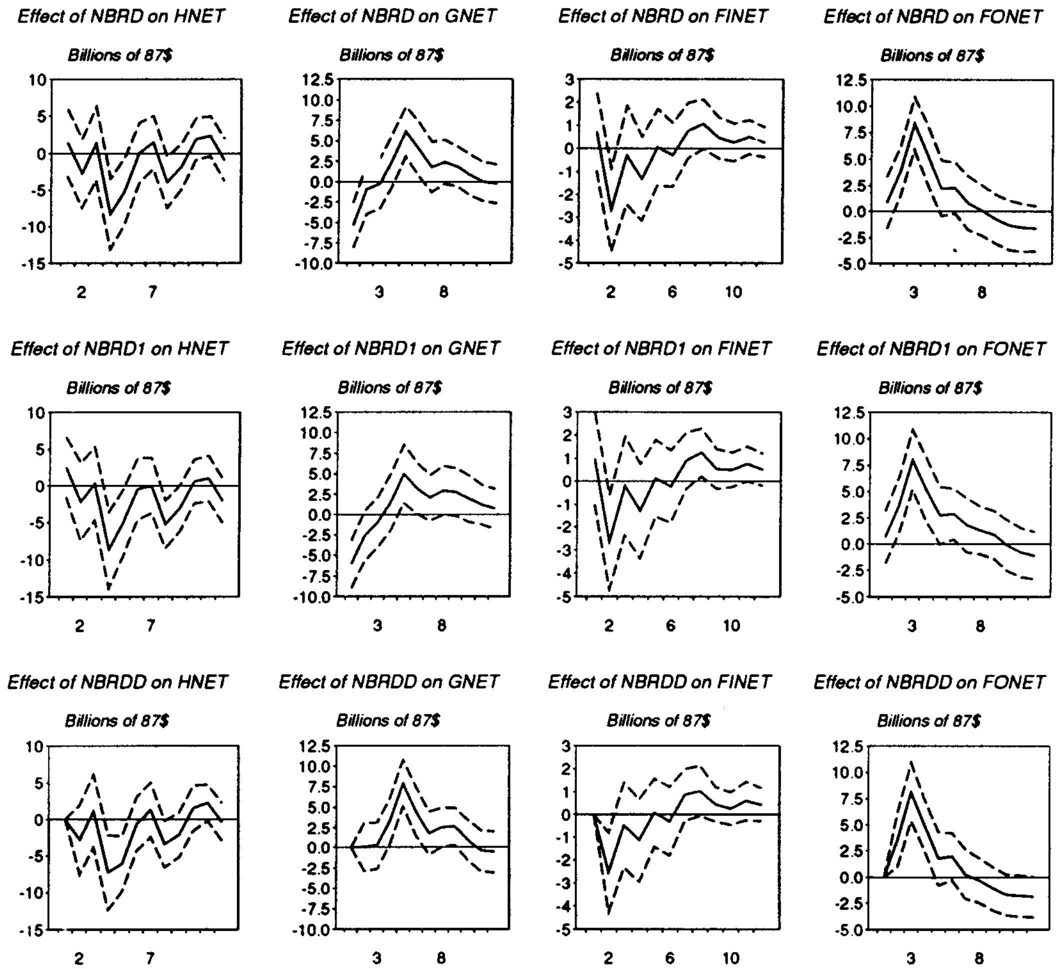
[35]The effects of alternative NBRD policy shock measures on retail sales (RSALES), corporate profits in the trade sector (TR PROF) and nonfinancial sector (NF PROF), and manufacturing inventories (MFG INVT). The dashed lines are one-standard error bands.

Figure A4.1: Effect of Policy Shocks on Net Funds Raised by the Business and Corporate Sectors[36]



[36]The effects of alternative NBRD policy shock measures on business sector data. The dashed lines are one-standard error bands.

Figure A4.2: Effect of Policy Shocks on Net Funds Raised by the Household, Government, Financial, and Foreign Sectors[37]



[37]The effects of alternative NBRD policy shock measures on net funds raised in other sectors. The dashed lines are one-standard error bands.

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