



ELSEVIER

Available online at www.sciencedirect.com

SCIENCE @ DIRECT®

Journal of Monetary Economics 51 (2004) 1107–1131

www.elsevier.com/locate/econbase

Journal of
MONETARY
ECONOMICS

Identifying VARS based on high frequency futures data[☆]

Jon Faust^a, Eric T. Swanson^b, Jonathan H. Wright^{c,*}

^a*International Finance Division, Federal Reserve Board, Washington, DC 20551, USA*

^b*Monetary Affairs Division, Federal Reserve Board, Washington, DC 20551, USA*

^c*International Finance Division, Federal Reserve Board, Washington, DC 20551, USA*

Received 6 June 2002; received in revised form 7 May 2003; accepted 3 November 2003

Abstract

Using the prices of federal funds futures contracts, we measure the impact of the surprise component of Federal Reserve policy decisions on the expected future trajectory of interest rates. We show how this information can be used to identify the effects of a monetary policy shock in a standard VAR. This alternative approach to identification is quite different, and, we argue, more plausible, than the conventional identifying restrictions. We find that a usual recursive identification of the model is rejected, as is any identification that insists on a monetary policy shock having an exactly zero effect on prices contemporaneously. We nevertheless agree with the conclusion of much of the VAR literature that only a small fraction of the variance of output can be attributed to monetary policy shocks.

© 2004 Elsevier B.V. All rights reserved.

JEL classification: C32; E52; G13

Keywords: Monetary policy; Identification; Fed funds Futures; FOMC

[☆]We are grateful to Graham Elliott, Jim Hamilton, Andy Levin, Jennifer Roush, Carl Walsh and an anonymous referee for helpful comments. We are grateful to Shing-Yi Wang for superlative research assistance and to Charles Evans for providing us with his commodity price data. The views in this paper are solely the responsibility of the authors and should not be interpreted as reflecting the views of the Board of Governors of the Federal Reserve System or of any person associated with the Federal Reserve System.

*Corresponding author. Tel.: 202-452-3605.

E-mail address: jonathan.h.wright@frb.gov (J.H. Wright).

0304-3932/\$ - see front matter © 2004 Elsevier B.V. All rights reserved.

doi:10.1016/j.jmoneco.2003.11.001

1. Introduction

Since the seminal paper of Sims (1980), much attention has been devoted to studying the effects of monetary policy on macroeconomic variables using a vector autoregression (VAR). This approach relies on making identifying assumptions relating structural shocks to the reduced form errors of the VAR. While many identification approaches have been proposed for identifying VARs, most often short-run restrictions are used. These specify that some structural shock has no contemporaneous effect on one or more variables. For example, it might be assumed that monetary policy has no contemporaneous effect on output and prices. Such identifying assumptions are used by Sims (1980) and in many other papers in the VAR literature, including Christiano et al. (1996, 1999), Bernanke and Blinder (1992) and Bernanke and Mihov (1998). Other approaches to the identification of a VAR include the use of long-run restrictions that come from neutrality or super neutrality arguments about the effects of monetary policy on real variables. For example, King and Watson (1997) make the assumption that monetary policy shocks have no long-run effect on output, and show that this long-run neutrality condition allows the identification of a very simple VAR.

Bernanke (1996) and Sims (1998) argue that while extant studies of the effects of monetary policy in a VAR differ greatly in their identifying assumptions and in the choice of the variables in the system, these studies nonetheless tend to agree on the qualitative effect of monetary policy shocks on macroeconomic variables, and, in particular, to agree that monetary policy shocks account for a small portion of the forecast error variance of output. For example, this conclusion is obtained in the recursive identification of Christiano et al. (1996, 1999), which we treat as the benchmark identification.

This important conclusion is however tempered by a concern about the validity of the identifying assumptions on which it rests. While the identifying assumptions in these papers are plausible, none is uncontroversial: we have many reasons taken from both theory and empirical evidence to believe that they will not hold exactly in the data (see, for example, Faust, 1998; Leeper et al., 1996; Uhlig, 1997). The tenuous nature of the identifying assumptions might not be troubling if the resulting conclusions appeared to be robust to minor deviations from the assumptions. Unfortunately, this robustness is difficult to check. Various authors have proposed approaches to testing robustness, and the conclusions are mixed at best. For example, Faust (1998) finds that allowing the policy shock to have a small contemporaneous effect on output allows one to identify monetary policy shocks that account for a large portion of the forecast error variance of output.¹

While the broad conclusions from the VAR literature are plausible to many of us, since the conclusions are sensitive to easily questioned assumptions, it is not clear that literature has (or should have) changed the minds of those who are inclined to disagree.

¹Some authors, such as Strongin (1995), have indeed proposed alternative identification schemes in which monetary policy shocks account for a large fraction of the forecast error variance of output.

In this paper, we maintain the standard assumption that there exists some way to identify the impulse response to a policy shock in these VAR models. We hope to bolster the credibility of conclusions about policy shocks by bringing to bear additional information from the federal funds futures market in the identification step of the exercise.

There are two key steps in our procedure. First, we measure the impulse response of the federal funds rate to the policy shock directly using federal funds rate futures data only. Second, we identify a standard VAR by imposing that the impulse responses of the funds rate to the policy shock in the VAR match the response measured from futures data. While these two steps are conceptually straightforward, carefully implementing them in practice requires dealing with several complications. In the first step, measuring the response of the funds rate to policy shocks in the futures data requires taking account of several peculiar aspects of that market. The validity of our approach rests on some substantive assumptions, which we test. In the second step, we find that the information from the futures market only partially identifies the system, so we must use inference approaches that are reliable under weak identification.

Other authors have also used high frequency financial market data to help identify the monetary policy shock in an otherwise conventional VAR. [Bagliano and Favero \(1999\)](#) and [Cochrane and Piazzesi \(2002\)](#) identify monetary policy shocks by interest rate moves around policy decisions. The primary difference in our method is that we use the futures market to measure the response of expected future interest rates to the unexpected change in the target fed funds rate, and impose the requirement that this matches the impulse responses of the funds rate to the policy shock in the VAR.

We conclude that the recursive identification of [Christiano et al. \(1999\)](#) is rejected: it is inconsistent with the evidence from the fed funds futures market. Indeed we reject any identification that insists on a monetary policy shock having an exactly zero effect on prices contemporaneously. We find that the share of the forecast error variance of output due to policy shocks is small, indeed maybe even a little smaller than is indicated by the recursive identification of Christiano, Eichenbaum and Evans. This result is striking because we use much weaker, and we argue more plausible, restrictions than in the existing literature. Our identification also eliminates the price puzzle—the finding in the benchmark recursive identification that the impulse response of prices first rises slightly but significantly, before falling.

The plan for the remainder of this paper is as follows. We discuss the identification problem in Section 2 and estimate the impulse response of the funds rate from futures data in Section 3. Our primary results are in Section 4 and we take up robustness in Section 5. Conclusions are in Section 6.

2. Identification

2.1. The simplest case

Consider the reduced form VAR,

$$A(L)Y_t = u_t, \tag{1}$$

where Y_t is $G \times 1$, $A(L) = \sum_{j=0}^{\infty} A_j L^j$ and $A_0 = I$. Following the literature we assume that $A(L)$ is invertible so that the system can be written as

$$Y_t = B(L)u_t, \quad (2)$$

where $B(L) = A(L)^{-1}$.

The identified VAR literature makes the assumption that the G reduced form errors u_t are related to structural errors by the relation: $u_t = S\varepsilon_t$, where S is full rank. One of the structural shocks is assumed to be the monetary policy shock of interest. We can take this as the first structural shock. The VAR can be written in terms of the structural shocks as

$$Y_t = B(L)S\varepsilon_t. \quad (3)$$

Call the first column of S , α ; this is the column corresponding to the policy shock. The impulse response of all variables in the VAR to the policy shock is,

$$B(L)\alpha = \sum_{j=0}^{\infty} B_j \alpha L^j.$$

This is a $G \times 1$ vector of lag polynomials and the coefficients of the g^{th} element trace out the response of the g th variable to the policy shock.

The B s are implied by the reduced form estimates and so identifying the impulse response requires picking the G elements of α . One restriction is a normalization, choosing the sign and units of the policy shock. In most work, one normalizes the standard deviation of the shock to be 1. In our work, the VAR includes the federal funds rate and we normalize the shock to have a contemporaneous +25 basis point effect on the federal funds rate.²

We complete the identification by requiring that the impulse response of the funds rate to the policy shock match the response measured in the futures market data. For now simply take it as given that we know that the response of the funds rate at time $t+h$ to a policy shock at time t is r_h , $h = 0, 1, \dots, G-1$.

To see how the r_h s allow us to identify α , note that the response at horizon h of the funds rate to the shock is $B_{h,ff}\alpha$ where $B_{h,ff}$ is the row of B_h corresponding to the funds rate. Our identification requires that

$$B_{h,ff}\alpha = r_h, \quad h = 0, 1, \dots, G-1.$$

We can stack these G equations to form

$$R\alpha = r$$

²The choice between normalizing the impact effect or the standard deviation of the shock is innocuous in the point estimates, though not in the confidence intervals. Suppose a one standard deviation shock has a +25 basis point effect in the point estimates. A 95 percent confidence interval for the effects of a one standard deviation shock need not be a 95 percent confidence interval for a 25 basis point shock. This is because the impact effect of a one standard deviation shock is stochastic. Our normalization is chosen for two reasons. It is technically convenient, and it leads to confidence intervals for something we want to learn: the effects of a given size shock. We are less interested in the effects of a 1 standard deviation shock, where the value of the standard deviation is not stipulated.

where the rows of R are the relevant row vectors $B_{h,ff}$ and the elements of r are the corresponding elements r_h . If R is of rank G , then there is a unique solution to this equation and $\alpha = R^{-1}r$.

2.2. Factors complicating inference

In the above discussion, we treated R and r as known. In practice R will be implied by the reduced form estimates of the VAR and r will be estimated from the futures market data. We must take account of uncertainty in each when doing our inference. More problematically, the identification rests on the rank condition that the rank of R is G . When we test the rank of our estimated R s below, we cannot reject rank deficiency. Thus our restrictions $R\alpha = r$ leave the system only partially identified. The reason for this is clear: the response of the funds rate at different horizons in the reduced form is very similar ($B_{h,ff} \approx B_{h+1,ff}$), so after imposing the impulse response to the shock at horizon h , one gets very little additional identifying power from also imposing the response at $h + 1$ or $h + 2$.

One might suppose that failure of the rank condition dooms inference.³ Failure of the rank condition does not doom inference, but we must take proper account of the partial identification. The most striking implication of partial identification is that we must give up on point estimation and only consider confidence intervals. Moreover, these confidence intervals must be constructed in a way that is robust to the failure of the rank condition.

2.3. Confidence intervals under partial identification

Suppose we want to learn about some scalar parameter f . This could be the share of the forecast error variance of output at horizon 48 due to the policy shock or the impulse response of prices to the policy shock at some horizon. Calling all the reduced form parameters of the VAR θ , f is a function of θ and α : $f(\theta, \alpha)$.

The vector α has an important economic interpretation—it is the contemporaneous effect of a monetary policy shock on each variable in the VAR. Economic reasoning should allow us to make *some* restrictions on the sign/magnitude of the elements of α , and so to restrict the parameter space for α to be in some set A^+ . We impose such a priori restrictions, as specified in Section 4 below. We shall argue that these are plausible, and certainly much weaker than a recursive ordering assumption that sets elements of α to zero.

The key step in forming a confidence interval for f is to form a confidence set for α from the restrictions that α must lie in A^+ and that $R\alpha = r$, taking account of uncertainty in R and r , and without relying on assumptions about the rank of R . The construction of this confidence set follows the work of Stock and Wright (2000)

³When identifying the slope parameters of simultaneous equations models using linear restrictions, individual parameters are either fully identified or valid confidence intervals for them are unbounded. In the nonlinear case (relevant to objects such as variance shares), a valid confidence interval for a parameter may be bounded even if it is not fully identified.

and is discussed in detail in the appendix. We construct a confidence set for α with 95% nominal coverage in this way, and call it A .

Now consider forming a confidence interval for f conditional on the OLS point estimate of the reduced form VAR parameters, $\hat{\theta}$. Under full identification, this would be associated with a unique estimate of f . Under partial identification, there is a range of $f(\hat{\theta}, \alpha)$ consistent with those α s in our confidence set A . Thus our confidence interval is

$$\left[\inf_{\alpha \in A} f(\hat{\theta}, \alpha), \sup_{\alpha \in A} f(\hat{\theta}, \alpha) \right].$$

Taking account of uncertainty in α and θ simultaneously requires a slightly more complicated argument. For any fixed α , the model is identified, and so we can use a conventional bootstrap to construct a 95% conditional confidence interval for $f(\theta, \alpha)$.⁴ Let this confidence interval be $[\underline{c}(\alpha), \bar{c}(\alpha)]$. Next form the outer envelope of all of these intervals across all α s in A , as $[\inf_{\alpha \in A} \underline{c}(\alpha), \sup_{\alpha \in A} \bar{c}(\alpha)]$. This confidence interval has asymptotic coverage of at least 90%, from the Bonferroni inequality, because asymptotically (i) the true α is included in A with probability 95%, and (ii) the bootstrap confidence interval has 95% coverage for any fixed α . The technique is conservative in that coverage may asymptotically be higher than 90 percent.⁵ The resulting confidence interval may be wide, reflecting in part its construction as a conservative confidence interval using the Bonferroni inequality.

3. Federal Funds Futures and the impulse response to policy shocks

This section develops the claim, taken as given in the last section, that the impulse response of the funds rate to policy shocks can be measured directly from the federal funds rate futures market.

The federal funds rate futures contract for month $t + h$ is a bet on the monthly average effective funds rate in month $t + h$, call it f_{t+h} .⁶ These contracts trade during business hours on business days. To allow for daily time indices, we maintain our convention that t is a monthly time index corresponding to data in the VAR, and write d_t for day d in month t . For simplicity, we sometimes drop the t subscript, but d with or without a subscript is always a day in month t .

Parties to the h -period contract on day d_t agree on a rate $f_{d_t}^h$ and then exchange $f_{d_t}^h - f_{t+h}$ in cash at the end of month $t + h$. With no money up front, standard no arbitrage reasoning implies that $0 = E_t[m_{t+h}(f_{t+h} - f_{d_t}^h)]$, where m_{t+h} is the stochastic pricing kernel. Re-arranging gives,

$$f_{d_t}^h = E_t f_{t+h} + \frac{\text{COV}_d(f_{t+h}, m_{t+h})}{E_t[m_{t+h}]},$$

⁴Each bootstrap replication holds α fixed but calculates a new θ from the bootstrap sample.

⁵For example, even when the true α is not in A , the confidence interval may contain the true f .

⁶The New York Fed produces the daily effective rate, and the monthly average is an average for all days in the month. The rate for non-business days is the previous business day's rate.

where cov_d is the conditional covariance seen from time d_t . Thus, as usual, the futures rate is equal to the expected future funds rate plus a risk term.

We will focus on the change in the futures rate on the day of announced changes in the Fed’s target federal funds rate. On any such day d_t we have

$$f_{d_t}^h - f_{d_t-1}^h = (E_{d_t} - E_{d_t-1})f_{t+h} \tag{4}$$

plus the change in the conditional covariance term over the day. Our first substantive assumption is that the conditional covariance term can be neglected so that there is no change-in-risk term in (4). We provide arguments and statistical tests to support this assumption in Section 5.

Under our assumption, the change in the futures market equals the change in the market’s expectation. We next assume that this change on FOMC announcement days is due to an exogenous monetary policy shock. It should be uncontroversial that any surprise in the FOMC announcement could rightly be construed, at least in part, as a policy shock. Our requirement is much stronger. There are two primary ways this could fail: the FOMC could be reacting to data hitting the market that day—the FOMC acts on the day of a surprising employment report. More problematic is the case in which the FOMC’s announcement itself reveals macro data that is private information of the Fed. We provide arguments and evidence supporting our assumption in Section 5.

With these assumptions, we have,

$$\Delta f_d^h = \Delta_d^e f_{t+h}, \tag{5}$$

where Δ is the daily change operator and $\Delta_{d_t}^e$ is the corresponding change in expectations operator: $\Delta_{d_t}^e = (E_{d_t} - E_{d_t-1})$.

Now return to the VAR. The expected funds rate at $t + h$ conditional on information in the monthly data set at time t is,

$$E_t f_{t+h} = \sum_{i=0}^{\infty} B_{h+i,ff} S \varepsilon_{t-i}.$$

The change in the expectation from day $d_t - 1$ to d_t during month t can only be due to changes in the expectations of the ε s over this day. Of course, all the past ε s (ε_{t-1} , etc.) are known at the beginning of month t , so that the change in expectations can only be due to a change in the expectation of ε_t : $\Delta_{d_t}^e \varepsilon_t$.⁷ Thus,

$$\Delta_{d_t}^e f_{t+h} = B_{h,ff} S \Delta_{d_t}^e \varepsilon_t.$$

We can write the righthand side as the change in the expectation of the policy shock, ε_{1t} , plus terms involving changes in expectations of the other shocks:⁸

$$\Delta_{d_t}^e f_{t+h} = B_{h,ff} \alpha \Delta_{d_t}^e \varepsilon_{1t} + v_{ht}. \tag{6}$$

⁷During month t , information about the month t shock accumulates, future shocks remain unknown and past shocks are already known.

⁸Formally, $v_t = B_{h,ff} S^* \varepsilon_t$, where S^* is S with the first column replaced by zeros.

Our second assumption is that $v_{ht} = 0$: neither other news, nor the policy announcement itself leads the market to reassess its view of the other shocks. Combining (5) and (6) gives

$$\Delta f_d^h = r_h \Delta_{d_t}^e \varepsilon_{1t},$$

where $r_h = B_{h,ff}\alpha$ is the impulse response of the funds rate to the policy shock at horizon h . Since this equation holds for all h , we can substitute out the unobserved quantity $\Delta_{d_t}^e \varepsilon_{1t}$ with $\Delta f_0^h / r_0$ to get

$$\Delta f_d^h = \frac{r_h}{r_0} \Delta f_d^0.$$

This says that for every day of a policy change, the change in the futures market at different horizons should be proportional and the factor of proportionality is the same every day (only the magnitude of the shock changes). We will estimate this factor of proportionality from the data and then use the normalization $r_0 = +25$ basis points to obtain the estimated \hat{r}_{hs} used in our identification.

3.1. The Federal Funds Futures data

We focus on the daily change (close-to-close) in the federal funds futures rates on the day of FOMC meetings and other announced changes in the Fed's target federal funds rate. There exist futures contracts on the current month and for 1–5 months in the future. We obtained data on the closing prices of all of these contracts from CBOT from February 1991 to August 2001. Fed funds futures now trade a few months further into the future, but they are very illiquid, and are not in our data set for the whole sample period.⁹ Our data include 98 target changes: 85 FOMC meetings and 13 intermeeting rate changes. From 1994 to 2000 inclusive, there were only 2 intermeeting rate changes. We suspect that intermeeting changes are more likely to be associated with the release of macroeconomic information rather than exogenous variation in monetary policy. Therefore we generally work with the FOMC-day changes only.

Measuring the surprise change in the target from the current month contract requires taking account of how far through the month the change occurs. The futures contract refers to the monthly average rate, and a target change later in the month has a smaller effect on the monthly average rate. Thus, we measure the surprise change in the target as¹⁰

$$\text{surprise change in target} = \Delta f_d^0 \left(\frac{\text{days in month}}{\text{days left in month}} \right).$$

⁹We could potentially gain more horizons by using eurodollar futures contracts, but these settle to a 90-day interest rate, not the federal funds rate. Besides, in testing, we find that longer dated eurodollar futures contracts are not efficient forecasts of the future spot 90-day rate, so that an important underlying assumption is not satisfied.

¹⁰If the monthly average rate is f and agents put no mass on a change except on day d out of T days in the month. If the rate starts the month at x , the close on day $d - 1$ is $f_{d-1}^0 = d/Tx + (T - d)/Tz^e$ where z

Near the end of the month, the scaling factor in parentheses is quite large. Unfortunately, futures rate data are recorded only to the nearest basis point (to the nearest half basis points since 1995). Thus, our measured changes involve measurement error that is greatly exacerbated when the time-of-month scaling factor is large. For this reason, we take the target surprise to be the change in the next month's contract whenever the change is after the 22nd of the month.

Finally, for a change on the first day of the month there is no prior day for this contract. We measure the surprise change as the difference between the same-month contract price that day and the next-month contract price on the previous day.¹¹ This approach to decomposing the target rate change into expected and unexpected components using fed funds futures is very similar to that of Kuttner (2001), though it is not the only possible way of measuring the surprise change in the target (see, for example, Söderlind and Svensson, 1997). In the context of United States data, using the fed funds futures seems the most natural approach.

3.2. Estimating the impulse responses, r_h

The futures data are summarized in Table 1. The FOMC-day surprises are small, with over 80 percent falling between -10 and plus 10 basis points. The medians are all zero. The unexpected components of the recent intermeeting changes are much larger.

Under the assumptions outlined above, the relative size of the FOMC-day change at various horizons should be the same but for a scale factor representing the size and sign of the shock. Thus, the scatter plot of any of these series against any other should be a straight line. As we see in Fig. 1, this linearity assumption appears to be reasonably well satisfied.

The failure of *exact* linearity obviously implies that our assumptions are not exactly satisfied. One reason this might be true is that there is rounding error as prices in the fed funds futures market are quoted in whole basis points (half basis points since 1995). Another factor might be the effects of illiquidity of the longer dated contracts. The horizon zero contract is least subject to this problem and, thus, is, we believe the best measured. This explains our choice of using the target surprise as the righthand side variable in the following regressions.

We regressed the FOMC-day change in the contracts for horizons 1–5 on the target surprise. We take the impulse responses as the coefficient estimates from these regressions. They are listed in Table 2, along with standard errors. Also reported are the regressions including intermeeting rate changes.

(footnote continued)

is the rate expected to prevail for the rest of the month. The close on day d after the announcement is, $f_d^0 = d/Tx + (T-d)/Tz$, where z is the announced rate. The difference in these is $\Delta f_d^0 = (T-d)/T(z - z^e)$ so the surprise change in the rate for the rest of the month is given by the formula in the text.

¹¹Our data consist of daily CBOT closing prices (3:00pm Eastern Time). Since February 1994, the FOMC's decision has been announced on the day of the meeting while CBOT is open. Following Kuttner (2001), before February 1994, we treat FOMC changes as having occurred the day after the FOMC meeting, because this is when the target rate change became known to the public.

Table 1
Summary of futures rate changes on FOMC days and days of intrameeting changes

	Target	$t + 1$	$t + 2$	$t + 3$	$t + 4$	$t + 5$
<i>FOMC day percentiles</i>						
10th	-7.30	-8.00	-6.40	-7.40	-9.40	-8.40
25th	-3.46	-2.00	-2.00	-2.50	-3.00	-3.00
50th	0.00	0.00	0.00	0.00	0.00	0.00
75th	1.15	1.00	1.00	2.00	2.00	2.00
90th	6.00	5.20	4.70	5.00	5.00	5.80
<i>Intrameeting changes, actual values</i>						
4/18/1994	10	10	10	12	14	16
10/15/1998	3.88	6	7	8	4	5
1/3/2001	-38.2	-29	-26	-21.5	-20	-14
4/18/2001	-42.5	-42	-35	-31.5	-33.5	-29

Notes: Rates are reported in basis points. The top panel reports percentiles of the distribution, the bottom panel reports the actual data for the four changes on the dates indicated. The “target” column gives the surprise changes in the target as discussed in the text.

In one month, the effect diminishes to 80 percent of the impact effect, and the effect gradually dissipates to about 60 percent of the initial effect over the next four months. We now turn to using this information to identify a VAR.

4. Results

In this section we apply the methodology to a benchmark 6-variable VAR of Christiano et al. (1999). Our data set consists of monthly observations from January 1959 through August 2001 on industrial production, CPI, the smoothed change in an index of sensitive commodity prices, the fed funds rate, nonborrowed reserves and total reserves. All of the variables, except the interest rate, are in logs, and the VAR includes 12 lags and a constant. Christiano, Eichenbaum and Evans (CEE) estimate a recursive VAR with the data in that order, calling the shock in the federal funds rate equation the monetary policy shock.

The replication of CEE’s work with our data is presented in Fig. 2, which includes point estimates from their identification and 95% bootstrap confidence intervals.¹² Our results correspond very closely to those in CEE. A policy shock that causes a 25 basis point rise in the federal funds rate causes output to fall. The peak effect on output is about $\frac{1}{4}$ of a percentage point and occurs about 18 months after the shock. There is a slight price puzzle in this VAR: prices rise slightly following the contractionary monetary policy shock, before eventually falling. Omitting commodity prices makes this puzzle worse. The impulse response of the federal funds rate to the policy shock in the CEE identification rises for one period and then is sharply

¹²Some of the VAR literature prefers 68% confidence intervals.

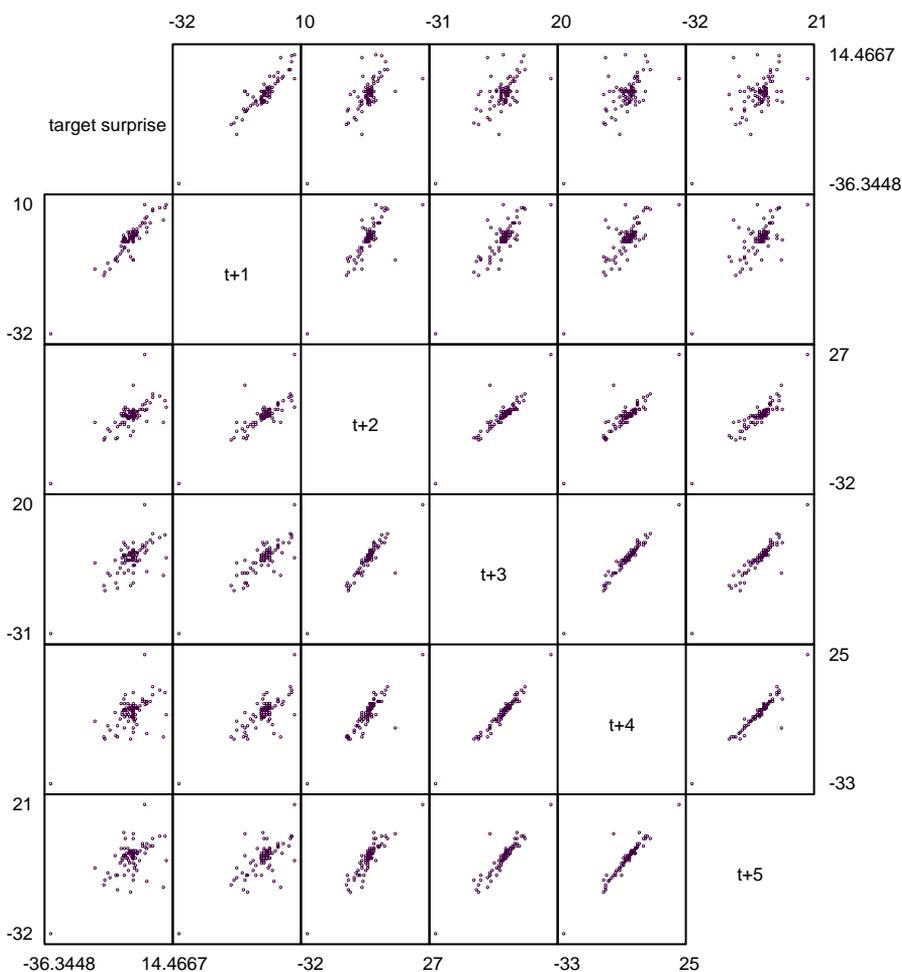


Fig. 1. Target surprise and futures rate changes (basis points).

Table 2
Measures of the impulse response of the funds rate to a policy shock

Horizon	<i>FOMC only</i>		<i>With intermeeting changes</i>	
	Rel. response	Std. err.	Rel. response	Std. err.
$t + 1$	0.80	0.04	0.83	0.02
$t + 2$	0.66	0.07	0.73	0.04
$t + 3$	0.60	0.07	0.68	0.05
$t + 4$	0.61	0.09	0.73	0.05
$t + 5$	0.55	0.09	0.67	0.06

Notes: Regression is the result of a least squares regression of the change in the futures rate on the target surprise with no intercept.

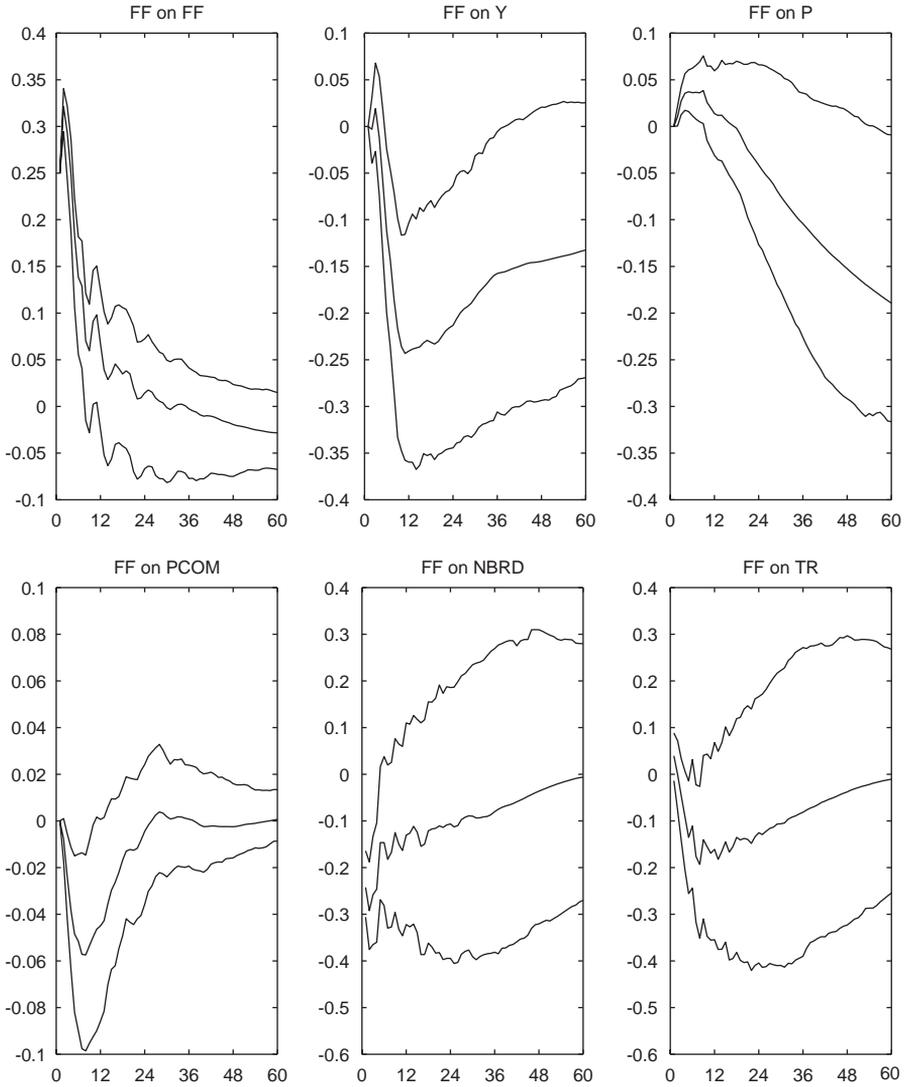


Fig. 2. Estimated impulse responses and bootstrap intervals in CEE identification.

reversed. This contrasts with the smooth decline we estimated above, a fact we will investigate further below.

4.1. Identifying the VAR using the futures market information

Remember that we can view the identification problem as choosing a vector α and that the 6 elements of α give the impact effect of the policy shock on the six variables in the VAR. The element of α corresponding to the fed funds rate is normalized to

0.25, so that a unit monetary policy shock raises the Fed Funds rate by 25 basis points. Our parameter space for α , A^+ , is bounded on the basis of *a priori* economic reasoning as follows: the elements of α corresponding to industrial production and the CPI are bounded between -0.1 and 0 , and the elements of α corresponding to commodity prices and the reserves variables are bounded between -0.25 and 0 . The requirement that all these elements of α are nonpositive says that a contractionary policy shock cannot raise output, prices, or the reserves variables contemporaneously. This assumption is commonly applied either formally or informally in the literature (e.g., Faust, 1998). The requirement that these elements of α are greater than -0.25 (or -0.1 for industrial production and the CPI) sets a bound on the absolute magnitude of the contemporaneous effects. We think larger contemporaneous effects than this are implausible. Of course, recursive identifications make the stronger restriction that there is no contemporaneous effect on variables such as output and prices that are higher in the ordering.

We test hypotheses about the rank of the matrix R using the method described in Appendix A.1. We have data on 6 futures contracts and there are six variables in the VAR, so that if the matrix R were of rank 6 then α would be just identified. The normalization of the element of α corresponding to the Fed Funds rate to 0.25 means that R has rank of at least 1. The hypotheses that R has rank 1 or 2 are clearly rejected (Table 3). The hypothesis that it has rank 3 is not rejected. Thus α is not fully identified, and this partial identification means that we will not have any point estimates and must instead be content with constructing confidence intervals that impose the conditions $R\alpha = r$ and that α lies in the parameter space A^+ , as described above and explained in detail in Appendix A2. One might suppose that these confidence intervals will be very wide. But they are still sufficiently tight to shed light on some important questions.

Fig. 3 shows 95% pointwise confidence intervals on the impulse responses to the monetary policy shock, holding the reduced form parameters fixed at their estimated values. The range shown is the closest we can come to point estimates in the current context, because θ is consistently estimable, but α is not. Our estimates of the variance share of output due to the policy shock are shown in Table 4. Our confidence intervals for the variance share (holding the reduced form parameters

Table 3
Test of the hypothesis that R has rank L in $R\alpha = r$ (p -values)

$L = 1$	985.83 (0.00)
$L = 2$	46.28 (0.00)
$L = 3$	6.47 (0.89)
$L = 4$	1.25 (0.97)
$L = 5$	0.28 (0.87)

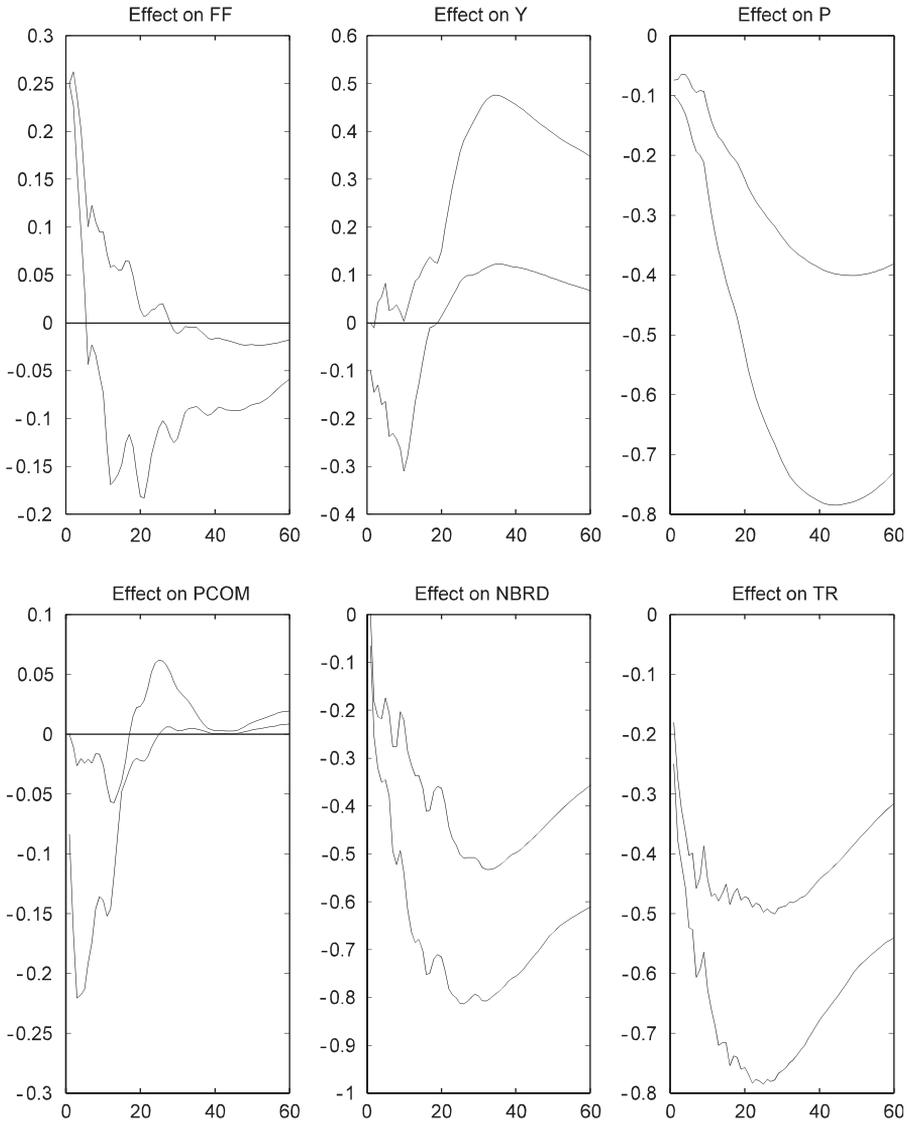


Fig. 3. Confidence intervals for impulse responses in new identification—fixed θ .

fixed at their estimated values) go from about 0-10 percent. Our identification is sufficient to conclude that, conditional on the reduced form estimates, only a small proportion of the variance of output is due to exogenous variation in monetary policy.

Fig. 4 shows the conservative 90% confidence intervals for the impulse responses to the monetary policy shock, taking simultaneous account of uncertainty in α and

Table 4
Output forecast error variance shares under alternate identifications

	v_{12}		v_{24}		v_{36}		v_{48}		v_{60}	
	Lower	Upper								
<i>CEE identification</i>										
CEE pt.	0.10		0.16		0.15		0.14		0.13	
CEE	0.03	0.21	0.06	0.33	0.05	0.32	0.05	0.32	0.05	0.33
<i>Futures-data identification</i>										
Fixed $\hat{\theta}$	0.00	0.05	0.00	0.03	0.00	0.08	0.00	0.11	0.00	0.13
Uncertain α and θ	0.00	0.15	0.00	0.14	0.00	0.22	0.00	0.28	0.00	0.29

the reduced form parameters. As explained above, these confidence intervals have asymptotic coverage of at least 90% by the Bonferroni inequality, but may in fact have higher asymptotic coverage. These confidence intervals are wider than those in Fig. 3, but still contain substantive information. In particular, the lower bound of the confidence interval for the impulse response of output to the policy shock is close to the lower bound of the CEE confidence interval for this same impulse response (Fig. 2). In this sense, the output effect of the shock at long horizons cannot be more negative than we found with the recursive identification. Also notice that the confidence interval for the impulse response of prices to the policy shock lies below zero everywhere. In this sense, there is no price puzzle. In contrast, with the recursive identification, there is a price puzzle in that the impulse response of prices first rises slightly but significantly, before falling.

Our confidence intervals for the variance share of output due to the policy shock taking simultaneous account of uncertainty in α and the reduced form parameters (shown in Table 4) are actually a little tighter than the CEE confidence intervals, although they cover essentially the same range (about 0 to 0.3). Both the upper and lower bounds of our confidence intervals for the variance share are uniformly a little lower than the CEE confidence intervals. We bring additional information to bear (the futures data) while using identifying assumptions that are much weaker in other respects, but the resulting inference about the variance share of output due to the policy shock confirms and strengthens the conclusion that only a small fraction of the variance of output is due to exogenous monetary policy shocks. In stark contrast, Faust (1998) finds that allowing a small contemporaneous response of output instead of imposing a zero effect allowed for variance shares around 2/3. Since we have no strong justification for imposing an effect that is precisely zero, the conclusion of small variance shares in the literature was not on a firm foundation. Our approach imposes restrictions very similar to Faust's and does allow contemporaneous responses of all variables. By adding that the funds rate response must match the futures market, however, we rule out the identifications with large variance shares. Faust finds that if the funds rate response to the shock is required to remain positive for 9 months, large variance shares are ruled out. The funds rate futures data provides a rationale for imposing this sort of persistence.

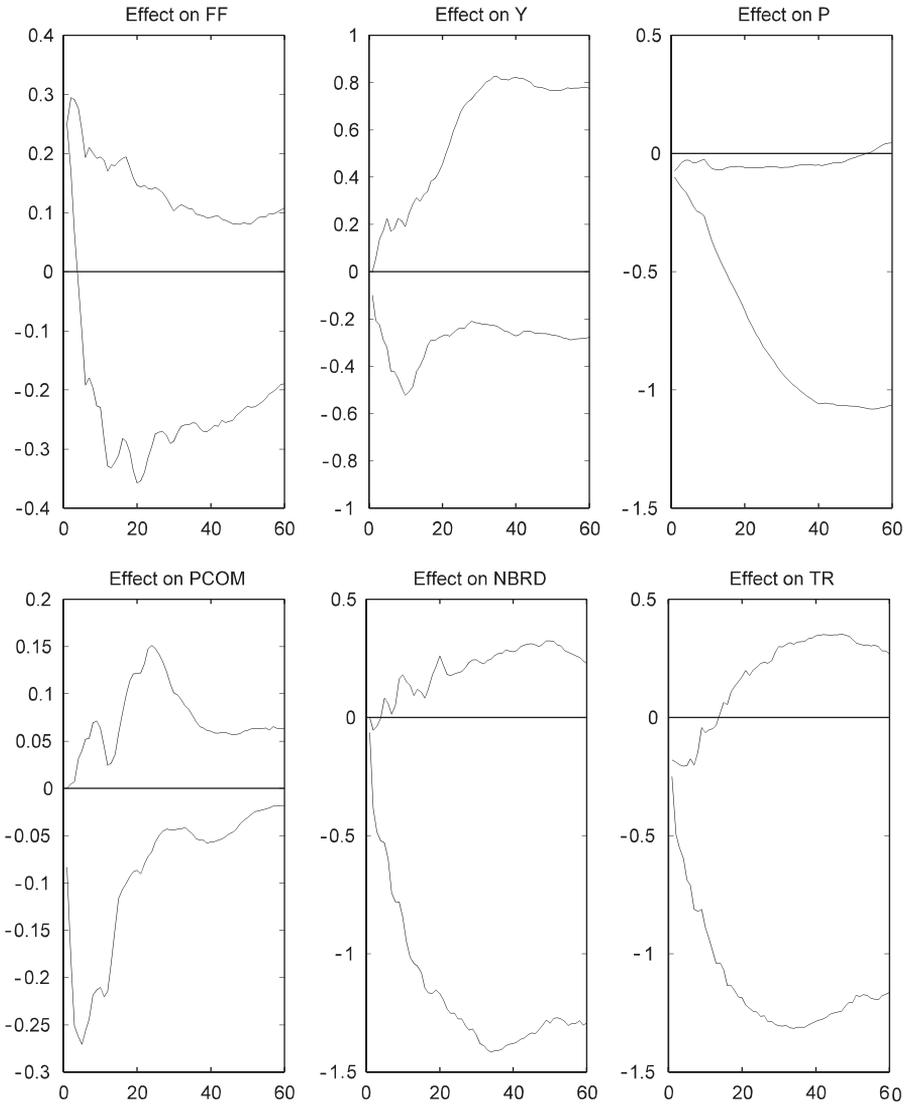


Fig. 4. Confidence intervals for impulse responses in new identification—uncertain θ .

4.2. Sensitivity Analysis: structural stability

Our paper estimates the response of the funds rate to a surprise change in the target rate over the years 1991–2001 and then imposes this on a monthly VAR estimated from 1959 to 2001. Several recent papers have considered the possibility of a structural break in the U.S. economy in the early 1980s (e.g. [Bernanke and Mihov](#),

1998; Boivin, 1999, Boivin and Giannoni, 2003; McConnell and Perez-Quiros, 2000). The impact of structural instability on the exercise that we conduct is unclear: on the one hand our inference about the identifying vector α is entirely based on recent high frequency data, but on the other hand the inference about the reduced form VAR parameters in θ would be affected by any break in the early 1980s.

To investigate the possible effects of a structural instability on our findings, we re-ran our results estimating the VAR only on data from February 1984 onward. This is a break date that Bernanke and Mihov (1998) considered. The confidence intervals that take account of uncertainty in θ are wider in the shorter subsample (as the reduced form VAR parameters are less precisely estimated) but have the same shape. For example, the confidence interval for the fraction of the 48-month variance of output that is due to the monetary policy shock is from 0.00 to 0.28 in the full sample, and from 0.00 to 0.39 in the shorter subsample. The confidence interval for the effect of a 25 basis point monetary policy tightening on prices 12 months later is from -0.07 to -0.44 (percentage points) in the full sample and from 0.11 to -0.57 in the shorter subsample.

4.3. Sensitivity Analysis: imposing additional restrictions

We have achieved partial identification of the vector α by requiring consistency with the impulse responses for interest rates measured from high frequency futures market data, normalizing the element of α corresponding to the Fed Funds rate to 0.25, and requiring that the other elements of α be non-positive and of bounded magnitude. We can experiment by adding additional restrictions in identification. For example, we could add the restriction that the element of α corresponding to industrial production is zero (i.e. the monetary policy shock has no effect on output contemporaneously), or that the element of α corresponding to the CPI is zero (i.e. the monetary policy shock has no effect on prices contemporaneously). We can in the way investigate the effects of imposing these assumptions, and test whether or not they can be maintained in the face of our futures-based identification. Restricting the element of α corresponding to industrial production to zero makes little difference to our confidence intervals for impulse responses and variance shares (though tightens the confidence intervals slightly). But, if we restrict the element of α corresponding to prices to zero, the confidence set A constructed by our identification is empty. That is, there does not exist any identification in which the monetary policy shock has a zero contemporaneous effect on prices that is consistent with the information from the futures market.

The α implied by the CEE identification is simply the fourth column of the Cholesky factor of the covariance matrix of the reduced form errors, using the CEE ordering of the variables (in which the fed funds rate comes fourth). This choice of α is not included in the confidence set A , which of course follows from the fact that restricting the element of α corresponding to prices to zero makes A an empty set.

Lastly, we note that we could go in the other direction and instead further weaken our assumptions about α . Recently, there has been some interest in the possibility

that monetary policy loosening represents cost-shocks that could boost aggregate supply and lower prices in the short-run (see, for example, Christiano and Eichenbaum, 1992; Christiano and Eichenbaum, 1997; Barth and Ramey, 2000). In order to allow for this possibility we also relaxed our bounds on α to specify that the element of α corresponding to the price level is between -0.1 and 0.1 , but not necessarily nonpositive. However this makes no difference to the results at all, because no vector α with a positive coefficient on prices is in the confidence set A .

5. Support for the identifying assumptions

Our approach to identification relies on the following three principle assumptions.

1. The VAR is an adequate representation of the reduced form.
2. The futures market provides an efficient forecast of the change in the trajectory of the funds rate, or at least risk premia in the fed funds futures markets do not change on our policy announcement days.
3. The change in the futures rates on our policy announcement days is due to the policy shock we wish to analyze. No other news moves the market and the policy announcement itself does not reveal information about other shocks.

We take up these assumptions in order in this section.

5.1. Is the VAR an adequate representation of the reduced form?

This assumption is common to the entire identified VAR literature. We will use conventional VARs and our results rest on this assumption just as the earlier work does.

Rudebusch (1998) pointed out that the funds rate forecasts implied by the futures market have little correlation with the forecasts generated from a reduced form VAR. Robertson and Tallman (2001) argue that the correlation between the two sets of forecasts can be greatly increased if the VAR is estimated with Bayesian shrinkage methods, instead of the usual least squares estimation. Shrinkage methods often enhance the forecasting performance of highly parameterized VARs. Motivated by this, we redid the analysis reported above, but replacing OLS estimates of the VAR with the posterior mean, constructed as described by Robertson and Tallman. Our substantive conclusions are not affected by this modification.

5.2. Are Federal Funds Futures Rates Efficient Forecasts?

We test the efficiency of the interest rate futures market by regressing the average effective federal funds rate for month t on the forecast for month t implicit in the fed funds futures market in the middle¹³ of months $t - 1$, $t - 2$, $t - 3$, $t - 4$ and $t - 5$.

¹³More precisely, this is the closing price on the 15th day, or next business day, of month $t - 1$.

Table 5
Forecast efficiency tests for interest rate futures

	$\hat{\alpha}$ (<i>t</i> -stat)	$\hat{\beta}$ (<i>t</i> -stat)	<i>p</i> - val ($\alpha = 0, \beta = 1$)
1 month ahead	-0.05 (-0.41)	1.00 (0.13)	0.42
2 months ahead	-0.06 (-0.31)	1.00 (-0.02)	0.30
3 months ahead	-0.03 (-0.09)	0.99 (-0.25)	0.26
4 months ahead	0.10 (0.22)	0.95 (-0.55)	0.26
5 months ahead	0.30 (0.49)	0.90 (-0.82)	0.22

Notes: The regression is $f_{t+h} = \alpha + \beta f_t^h + \text{error}$.

The regressions therefore constitute standard forecast efficiency regressions for evaluating h -step ahead forecasts of interest rates from the fed funds futures market. Under the null hypothesis of forecast rationality, the intercept should be zero and the slope coefficient should be one. The results (Table 5) support the idea that the fed funds futures market provides efficient forecasts of the trajectory of future interest rates. The null hypothesis that the slope/intercept are jointly equal to zero/one is accepted for all horizons. For all horizons the intercept is not significantly different from zero and the slope is not significantly different from one. Actually, a non-zero intercept is not a problem for our methods in itself, as this could just represent a constant risk premium; only time-varying risk premia that might vary in response to a monetary policy shock undermine our identification. Similar support for the efficiency of the fed funds futures market was reported by Gurkaynak et al. (2002); Krueger and Kuttner (1996) and Rudebusch (1998).

5.3. Is the target change purely due to the policy shock?

There are two ways that this assumption could fail. The simplest way is that other important information could hit the market on the day of announced target changes. Second, the Fed's announcement could reveal private information of the Fed about the state of the economy.

We checked whether any of important pieces of macro data were announced on the day of FOMC meetings. We find that on the FOMC days in our sample retail sales were released once, durable goods, GDP and PPI were released twice each, industrial production was released three times and CPI was released 6 times. The clear majority of FOMC days are days with no important macroeconomic data

releases. Besides, we redid our analysis omitting the FOMC meeting days on which there was a macroeconomic release, but our substantive conclusions remained unchanged. The Federal Reserve might, however, have an information advantage through earlier access to data (especially data that are produced by the Federal Reserve, such as industrial production) or through superior economic analysis provided by the Fed's staff economists.

We conducted a test of the hypothesis that policy actions on FOMC days effectively release macroeconomic news, the intuition for which is as follows. We form a measure of the surprise component of macroeconomic data announcements based on survey measures of expectations. We collect those instances where the survey is taken just before an FOMC meeting and the data come out just afterward. If the fed funds target surprise effectively *releases* macro data, then the target surprise should be correlated with the macro announcement surprise. In this case, the target surprise can be used by the market to update its expectation of the macro announcement.

The details of our test are as follows. For each FOMC meeting the test involves regressing the next monthly release of a macroeconomic indicator on (i) the target rate surprise on the day of the FOMC meeting and (ii) the forecast for that macroeconomic release made by Wrightson Associates on the Friday before the FOMC meeting. We test the hypothesis that the coefficient on the target rate surprise is equal to zero. We consider the following macroeconomic indicators: nonfarm payrolls, CPI, PPI, industrial production, retail sales, real GDP and the GDP deflator.¹⁴ In the regression corresponding to each macroeconomic indicator, we omit any FOMC meeting that occurs after the date of the release of that macroeconomic indicator for that month. Because each macroeconomic release refers to the previous month, the target rate surprise can have predictive power for the macroeconomic release only through the Federal Reserve having an information advantage, and not through any contemporaneous effects of the target rate surprise.

Applying this test, we find that the estimated coefficient on the target surprise is not significantly different from zero for any macroeconomic indicator (Table 6), except for industrial production which is significant at the 5% level, though not at the 1% level. Of course, the industrial production data are produced by the Fed and it is plausible that the Fed has an informational advantage in this regard. Overall, however, we believe that our assumption that the FOMC decision is primarily a policy shock is supported. Further results on this test for asymmetric information can be found in Faust et al. (2002).

5.4. *Alternative term structures*

Since our estimates of the impulse response of the funds rate from the futures market are subject to criticism, we also explored the sensitivity of our primary conclusion to the specifics of the imposed term structure.

¹⁴Note that there is a GDP release every month, although this data is quarterly. The first month of every quarter, there is the first release of GDP for the previous quarter while a revision, referring to this same quarter, is then released in each of the next two months.

Table 6
 Tests of the exogeneity of the monetary policy shocks (standard errors in parentheses)

Macroeconomic Indicator	Intercept	Coefficient on forecast	Coefficient on FOMC surprise
Nonfarm payrolls (mom change, 000s)	−28.75 (35.62)	1.14 (0.20)	288.50 (252.80)
CPI (mom change, percent)	0.05 (0.06)	0.65 (0.20)	−0.28 (0.30)
PPI (qoq change, ann percent)	0.02 (0.08)	0.70 (0.27)	−0.36 (0.79)
Industrial Production (mom change, percent)	0.05 (0.07)	0.75 (0.20)	1.65 (0.78)
Retail Sales (mom change, percent)	0.13 (0.09)	0.55 (0.19)	0.28 (0.92)
Real GDP (qoq change, ann percent)	0.12 (0.13)	0.98 (0.04)	−0.82 (1.18)
GDP Deflator (qoq change, ann percent)	0.07 (0.09)	0.94 (0.04)	−0.56 (0.62)

In particular, we imposed that the impulse response of the funds rate to the policy shock starts at +25 basis points and then over the next 5 months grows or diminishes linearly to 1.5, 1.25, 1, 0.75, 0.5, 0.25, 0, −0.25 and −0.5 times its impact effect. The resulting confidence intervals for the variance shares of output due to the policy shock are reported in Table 7 (these take simultaneous account of the uncertainty in α and θ). Large variance shares are only included in the confidence interval if the monetary policy shock goes to zero or reverses sign within 5 months. This is manifestly at odds with the evidence from the futures market.

A recent paper by Ellingsen and Söderström (2001) discusses the changes in the federal funds target between 1988 and 1997 and classifies some market responses as due to exogenous policy shifts, and others as an endogenous response to asymmetric information. The classification is based on their reading of the *Wall Street Journal*. Although one might be somewhat skeptical of this classification, we estimated the impulse responses r_h by regressing the FOMC-day change in the contracts for horizons 1–5 on the target surprise using only changes that Ellingsen and Söderström classify as exogenous policy shifts. Our confidence intervals for the variance shares of output due to the policy shock using these estimated impulse responses as the basis of our identification are not very different from those obtained in the baseline case (Table 4).

Table 7

Robustness check: output forecast error variance shares assuming that the impulse response of the monetary policy shock on the Fed Funds rate goes from 25 basis points to 25λ basis points, linearly, going from the month of the shock to 5 months later

λ	v_{12}		v_{24}		v_{36}		v_{48}		v_{60}	
	Lower	Upper								
1.5	0.00	0.15	0.00	0.18	0.00	0.19	0.00	0.21	0.00	0.21
1.25	0.00	0.18	0.00	0.20	0.00	0.21	0.00	0.24	0.00	0.23
1	0.00	0.23	0.00	0.24	0.00	0.24	0.00	0.25	0.00	0.25
0.75	0.00	0.30	0.00	0.30	0.00	0.28	0.00	0.29	0.00	0.30
0.5	0.00	0.37	0.00	0.35	0.00	0.30	0.00	0.31	0.00	0.34
0.25	0.00	0.43	0.00	0.39	0.00	0.31	0.00	0.34	0.00	0.37
0	0.00	0.51	0.00	0.43	0.00	0.34	0.00	0.36	0.00	0.39
-0.25	0.00	0.57	0.00	0.49	0.00	0.37	0.00	0.36	0.00	0.39
-0.5	0.00	0.57	0.00	0.50	0.00	0.39	0.00	0.36	0.00	0.39

Notes: These confidence intervals allow for simultaneous uncertainty in α and θ .

5.5. Caveats

Ultimately, all work identifying the effects of policy shocks is subject to many potential criticisms. Our goal is not to find the mythical *fully credible identification*. We have attempted to be clear about the principle assumptions our identification rests on and to support these assumptions. We argue that our approach is plausible, even more plausible than standard recursive approaches.

We are hopeful about two strategies for progress that are illustrated in this paper. First, we believe it important to generate different approaches to identification. If very different approaches give similar answers, those answers become more plausible. Second, we believe it is important to bring more information to bear than the much-studied, standard 6 or 10 macrovariables. This paper provides one approach to doing this and finds renewed support, for example, for the conclusion that policy shocks account for a small part of the variance share of output.

6. Conclusions

Fed funds futures have been used by a number of authors to analyze various aspects of monetary policy. Our motivation in this paper has been to use these data to aid in the structural identification of a standard monetary policy VAR.

The basic idea in this paper is to assume that the surprise component of the Fed's decision on FOMC day (measured from the futures market) represents exogenous variation in monetary policy. We use this high frequency data to form identifying restrictions for a monthly VAR. Specifically, we impose that the response of the fed funds rate to policy shocks must match that estimated in the futures market. The method can also apply to the response of any data in the VAR that is observed at

high frequency around a monetary policy shock (measured in this or any other way). For example, it would be possible to measure the effect of the monetary policy shock on the exchange rate and to use this information to assist in the identification of a low frequency structural VAR.

In the specific model that we have considered in this paper, we reach four conclusions. First, we reject the standard recursive identification. It appears that the standard assumption that the contemporaneous price response to the policy shock is zero is one factor at the root of this rejection—no identification which has a zero contemporaneous price response to the policy shock is consistent with the futures-based identification. Secondly, we find that the negative effect of a monetary policy tightening on output cannot be much larger than was found with the standard recursive identification. Thirdly, we find that the effect of a monetary policy tightening is to reduce the price level significantly at all horizons up to about four years, eliminating the price puzzle that arises with the standard recursive identification and suggesting that the price puzzle may have been the artifact of a false assumption that prices are contemporaneously totally inertial. Finally, we find evidence in support of the conclusion that only a small fraction of the variance of output can be attributed to monetary policy shocks. We have done so using an alternative identifying assumption that is quite different, and, we would argue, more plausible, than the conventional short-run restrictions.

Appendix A

A.1. Testing the rank of R

We wish to test the hypothesis that $\rho(R) = L$ against the alternative that $\rho(R) > L$, where $\rho(\cdot)$ denotes the rank of the argument. Assume that $T^{1/2}(\hat{\theta} - \theta) \rightarrow_d N(0, V_\theta)$. See Hamilton (1994) for primitive conditions for this convergence results and \hat{V}_θ be a consistent estimator of V_θ . The matrix R is a nonlinear function of θ and can be estimated by \hat{R} , where this denotes this same nonlinear function of $\hat{\theta}$. By the delta method, $T^{1/2}(\text{vec}(\hat{R}) - \text{vec}(R)) \rightarrow_d N(0, V_R)$, where $V_R = \text{dvec}(R)' / \text{d}\theta V_\theta \text{dvec}(R) / \text{d}\theta$.

To test the hypothesis about that rank of R , we use the test statistic

$$T \min_{P \in \pi(L)} (\text{vec}(\hat{R}) - \text{vec}(P))' \hat{V}_R^{-1} (\text{vec}(\hat{R}) - \text{vec}(P)),$$

where \hat{V}_R is $\text{dvec}(\hat{R})' / \text{d}\theta \hat{V}_\theta \text{dvec}(\hat{R}) / \text{d}\theta$ and $\pi(L)$ is the space of all conformable matrices of rank L . By Theorem 1 of Cragg and Donald (1997), under the null hypothesis, this test statistic has a χ^2 null limiting distribution on degrees of freedom.

A.2. Partial identification

Here we describe how to construct the confidence set A for the vector α when A^+ denotes the parameter space for α , the restrictions $R\alpha = r$ must be satisfied, R is estimated by \hat{R} , r is estimated by \hat{r} , R may be rank deficient, $T^{1/2}(\text{vec}(\hat{R}) -$

$\text{vec}(R) \rightarrow_d \mathcal{N}(0, V_R)$ and $T^{1/2}(\hat{r} - r) \rightarrow_d \mathcal{N}(0, V_r)$. Consider the GMM objective function

$$S(\alpha) = T(\hat{R}\alpha - r)'[(\alpha \otimes I_K)\hat{V}_R(\alpha' \otimes I_K) + \hat{V}_r]^{-1}(\hat{R}\alpha - r).$$

In standard GMM terminology, this is the continuous updating GMM objective function. The estimator $\hat{\alpha}$ that minimizes this objective function is not consistent for the true α because of the rank deficiency of the matrix R . However $S(\alpha_0)$ has a χ^2 null distribution regardless of the rank of R where α_0 denotes the true value of the vector α . Accordingly, the confidence set

$$A = \{\alpha \in A^+ : S(\alpha) \leq F_{\chi^2}\}$$

is a confidence set for α with asymptotic coverage 95%, regardless of the rank of R , where F_{χ^2} denotes the 95th percentile of a χ^2 distribution (degrees of freedom equal to the number of elements in r). This confidence set is therefore immune to the rank deficiency of R . Numerically, the way we construct the confidence set A is to draw 10 million possible α s at random uniformly from the parameter space for α (which is a 6×1 vector normalizing the coefficient on the Fed Funds rate to 0.25, bounding the coefficients on industrial production and the CPI between 0 and -0.1 and bounding the other coefficients between 0 and -0.25).

The use of such confidence sets in models that are not fully identified was proposed by [Stock and Wright \(2000\)](#), where they are referred to as S-sets. If the matrix R is rank deficient, then there exists a subspace of vectors α that are observationally equivalent to α_0 . Any vector in this subspace must be included in A with probability 95%, asymptotically. Any other vector α will be excluded from A with probability 1, asymptotically. This is a correct statement of what we do and do not know about α , when R is rank deficient. More formally, the confidence set A is unbounded with probability 0.95, asymptotically: this must be the case for any confidence set for an unidentified parameter if the confidence set is to have 95% asymptotic coverage uniformly in the parameter space ([Dufour, 1997](#)). Notwithstanding the fact that our confidence set for α is unbounded, the confidence interval for a nonlinear function of α and θ , such as a variance share, does not necessarily have to be unbounded.

References

- Bagliano, F.C., Favero, C., 1999. Information from financial markets and VAR measures of monetary policy. *European Economic Review* 43, 825–837.
- Barth, M.J., Ramey, V.A., 2000. The cost channel of monetary transmission. Mimeo.
- Bernanke, B., Blinder, A.S., 1992. The federal funds rate and the channels of monetary transmission. *American Economic Review* 82, 901–921.
- Bernanke, B., 1996. Comment on “What does monetary policy do?”. *Brookings Papers on Economic Activity* 2, 69–73.
- Bernanke, B., Mihov, I., 1998. Measuring monetary policy. *Quarterly Journal of Economics* 113, 869–902.
- Boivin, J., 1999. Revisiting the evidence on the stability of monetary VARs. Mimeo.
- Boivin, J., Giannoni, M., 2003. Has monetary policy become more effective? NBER Working Paper 9459.

- Christiano, L.J., Eichenbaum, M., 1992. Liquidity effects and the monetary transmission mechanism. *American Economic Review* 82, 346–353.
- Christiano, L.J., Eichenbaum, M., 1997. Sticky price and limited participation models of money: a comparison. *European Economic Review* 41, 1201–1249.
- Christiano, L.J., Eichenbaum, M., Evans, C.L., 1996. The effects of monetary policy shocks: evidence from the flow of funds. *Review of Economics and Statistics* 78, 16–34.
- Christiano, L.J., Eichenbaum, M., Evans, C.L., 1999. Monetary policy shocks: what have we learned and to what end? In: Taylor, J., Woodford, M. (Eds.), *Handbook of Macroeconomics*, vol. 1A.
- Cochrane, J.H., Piazzesi, M., 2002. The fed and interest rates: a high frequency identification. *American Economic Review Papers and Proceedings* 92, 90–95.
- Cragg, J.G., Donald, S.G., 1997. Inferring the rank of a matrix. *Journal of Econometrics* 76, 223–250.
- Dufour, J.M., 1997. Some impossibility theorems in econometrics with applications to structural and dynamic models. *Econometrica* 65, 1365–1387.
- Ellingsen, T., Söderström, U., 2001. Monetary policy and market interest rates. *American Economic Review* 91, 1594–1607.
- Faust, J., 1998. The robustness of identified VAR conclusions about money. *Carnegie-Rochester Series on Public Policy* 49, 207–244.
- Faust, J., Swanson, E., Wright, J.H., 2002. Does the fed possess inside information about the economy? Mimeo.
- Gurkaynak, R.S., Sack, B., Swanson, E., 2002. Market-based measures of monetary policy expectations. *Finance and Economics Discussion Series* 2002-40.
- Hamilton, J.D., 1994. *Time Series Analysis*. Princeton University Press, Princeton.
- King, R.G., Watson, M.W., 1997. Testing long run neutrality. *Federal Reserve Bank of Richmond Economic Quarterly* 83, 69–101.
- Krueger, J.T., Kuttner, K.N., 1996. The fed funds futures rate as a predictor of federal reserve policy. *Journal of Futures Markets* 16, 865–879.
- Kuttner, K.N., 2001. Monetary policy surprises and interest rates: evidence from the fed funds futures market. *Journal of Monetary Economics* 47, 523–544.
- Leeper, E.M., Sims, C.A., Zha, T., 1996. What does monetary policy do? *Brookings Papers on Economic Activity* 2, 1–6.
- McConnell, M.M., Perez-Quiros, G., 2000. Output fluctuations in the United States: what has changed since the early 1980s? *American Economic Review* 90, 1464–1476.
- Robertson, J.C., Tallman, E.W., 2001. Improving federal funds rate forecasts in VAR models used for policy analysis. *Journal of Business and Economic Statistics* 19, 324–330.
- Rudebusch, G., 1998. Do measures of monetary policy in a VAR make sense? *International Economic Review* 39, 907–932.
- Sims, C.A., 1980. Macroeconomics and reality. *Econometrica* 48, 1–48.
- Sims, C.A., 1998. Comment on “Do measures of monetary policy in a VAR make sense?”. *International Economic Review* 39, 933–941.
- Söderlind, P., Svensson, L.E.O., 1997. New techniques to extract market expectations from financial instruments. *Journal of Monetary Economics* 40, 383–429.
- Stock, J.H., Wright, J.H., 2000. GMM with weak identification. *Econometrica* 68, 1055–1096.
- Strongin, S., 1995. The identification of monetary policy disturbances: explaining the liquidity puzzle. *Journal of Monetary Economics* 35, 463–498.
- Uhlig, H., 1997. What are the effects of monetary policy on output? Results from an Agnostic Identification Procedure, CEPR Discussion Paper.