

Can VARs describe monetary policy?

Charles L. Evans and Kenneth N. Kuttner*

Introduction

How does monetary policy affect the economy? To answer that question requires solving a basic simultaneity problem: monetary policy affects the economy while at the same time responding endogenously to changing macroeconomic conditions. Empirically estimating the effects of policy therefore requires some observable exogenous element to policy. The narrative approach pioneered by Friedman and Schwartz (1963), and applied by Romer and Romer (1989), is one way to identify exogenous policy shifts. A more common approach, however, is the Vector Autoregression (VAR) technique developed by Sims (1972, 1980a and 1980b). In this procedure, changes in the monetary policy instrument that are not explained by the variables included in the model are interpreted as exogenous changes in policy, or policy “shocks”. Christiano, Eichenbaum and Evans (1997) provides a survey of this line of research.

One unresolved question is how well simple econometric procedures, like VARs, can describe the monetary authority’s response to economic conditions, and by extension, the policy shocks used to identify policy’s effects on the economy. There are several reasons to be skeptical of the VAR approach. VAR models (indeed, all econometric models) typically include a relatively small number of variables, while the Fed is presumed to “look at everything” in formulating monetary policy. By assuming linearity, VARs rule out plausible asymmetries in the response of policy, such as those resulting from an “opportunistic” disinflation policy. VAR coefficients are assumed to remain constant over time, despite well-documented changes in the Fed’s objectives and operating procedures.

The goal of this paper is to assess VAR accuracy in predicting changes in monetary policy, and the shock measures derived from those predictions, using forecasts from the Federal funds futures market as a basis for comparison. Section 1 reviews the VAR methodology, and its putative deficiencies. Section 2 shows that the correlation between the VAR and the futures market forecast errors can give a misleading picture of the VAR’s accuracy, and suggests looking instead at the relationship between the forecasts themselves. The results in Section 3 show that profligacy *per se* does not hurt the VAR’s performance, but reducing the number of lags *and* estimating the model over a more recent subsample can improve the model’s forecast accuracy. Section 4 discusses the time aggregation problems inherent in extracting policy surprises from Fed funds futures data. The conclusions, summarized in the final section, are that VARs mimic the futures market’s forecasts reasonably well, but that it would be misleading to use futures market forecasts as the only basis for comparison.

* Correspondence to: Kenneth Kuttner, Federal Reserve Bank of New York, 33 Liberty Street, New York, NY 10045, e-mail kenneth.kuttner@ny.frb.org; or Charles Evans, Research Department, Federal Reserve Bank of Chicago, P.O. Box 834, Chicago, IL 60604, e-mail cevens@frbchi.org. Thanks go to Rick Mishkin, Athanasios Orphanides, Chris Sims, David Tessier, and to seminar participants at Columbia University, the Bank for International Settlements, the Federal Reserve Board and the Federal Reserve Bank of New York. The views in the paper do not necessarily reflect the position of the Federal Reserve Bank of New York, the Federal Reserve Bank of Chicago, or the Federal Reserve System.

1. VARs: the technique and its critiques

The reduced form of a VAR simply involves the regression of some vector of variables, x , on lags of x :

$$x_t = A(L) x_{t-1} + u_t \quad (1)$$

$A(L)$ is a polynomial in the lag operator, and u_t is a vector of disturbances, where $E(u_t u_t') = \Omega$. In monetary applications, the x_t vector would include one (or more) indicators of monetary policy, along with the other macroeconomic and financial variables. The widely-used model of Christiano, Eichenbaum and Evans (CEE) (1996a), includes the Federal funds rate, r_t , logarithms of lagged payroll employment (N), the personal consumption deflator (P), nonborrowed reserves (NBR), total reserves (TR), $M1$, and the smoothed growth rate of sensitive materials prices ($PCOM$).¹ The funds rate equation from the CEE model is:

$$\begin{aligned} r_t = & \beta_0 + \sum_{i=1}^{12} \beta_{1,i} r_{t-i} + \sum_{i=1}^{12} \beta_{2,i} \ln N_{t-i} + \sum_{i=1}^{12} \beta_{3,i} \ln P_{t-i} + \sum_{i=1}^{12} \beta_{4,i} PCOM_{t-i} \\ & + \sum_{i=1}^{12} \beta_{5,i} \ln NBR_{t-i} + \sum_{i=1}^{12} \beta_{6,i} \ln TR_{t-i} + \sum_{i=1}^{12} \beta_{7,i} \ln M1_{t-i} + u_t \end{aligned}$$

a regression of the Fed funds rate on lagged values of all the variables included in the VAR.

In the “structural” VAR:

$$x_t = B_0 x_t + B(L) x_{t-1} + e_t \quad (2)$$

the shocks and feedbacks are given economic interpretations. The covariance matrix of the e innovations is diagonal, and contemporaneous feedback between elements of x is captured by the B_0 matrix. The equation involving the monetary policy instrument (the Fed funds rate, for example) is often interpreted as a reaction function describing the Fed’s response to economic conditions. By extension, the innovation to this equation is taken to represent “shocks” to monetary policy. A great deal of research and debate has centered on the identifying assumptions embodied in the choice of B_0 . Examples include Bernanke (1986), Sims (1992), Strongin (1995), Bernanke and Mihov (1995), Christiano, Eichenbaum and Evans (1996b), and Leeper, Sims and Zha (1996). The typical focus of this research is the impulse response functions of macroeconomic variables in response to monetary policy shocks, and how the specific identifying assumptions affect the responses.

This paper does *not* deal with the identification issue, but focuses instead on a more basic question: whether the reduced form of the VAR, and the monetary policy equation in particular, generate sensible forecasts. Rudebusch (1997) pointed out that the Fed funds futures market provides a ready benchmark for evaluating the VAR’s Fed funds rate equation.² This strategy makes sense if the futures market is efficient, in the sense that its errors are unforecastable on the basis of available information.³ The month $t - 1$ one-month futures rate, $f_{1,t-1}$, would therefore represent the rational

¹ This series was a component in the BEA’s index of leading economic indicators prior to its recent revision by the Conference Board. Recent observations of the series used here are computed using the BEA’s methodology from raw commodity price data from the Conference Board.

² Fed funds futures are known officially as Thirty-Day Interest Rate futures.

³ The findings of Krueger and Kuttner (1996) generally support this view. Results less supportive of market efficiency were reported by Sims (1996). In a regression of the average Fed funds rate on lagged monthly averages of T-bill and discount rates, and Fed funds futures rate from the middle of the previous month, the T-bill and discount rates were

Table 1
The case against VARs

Horizon, in months	Standard deviation				Average standard error of CEE shock	CEE-futures correlation
	Futures	No change	CEE model			
			in sample	out of sample		
One	13.1	19.2	23.5	27.1	20.6	0.35
Two	20.8	33.1	35.4	43.8	34.0	0.38
Three	39.6	46.8	48.2	60.7	42.1	0.47

Notes: The CEE model is estimated on monthly data from January 1961 through July 1997. The reported statistics are based on the May 1989 through December 1997 sample. The in-sample standard deviation and correlations are adjusted for the degrees of freedom used in estimation. Units are basis points.

expectation of the period t Fed funds rate conditional on information at time $t-1$. The corresponding surprise would be:

$$u^*_t = r_t - f_{1,t-1}$$

where r_t is the *average* overnight Fed funds rate in month t , consistent with the structure of the futures contract. To avoid familiar time averaging problems, the futures rate is taken from the last business day of the month.⁴

A glance at the series plotted in Figure 1 shows that the VAR's forecast errors bear little resemblance to the futures market surprises. The correlation between the two is only 0.35 for one-month-ahead forecasts, comparable to the R^2 of 0.10 reported in Rudebusch (1997).⁵ The correlations between two- and three-month ahead shocks are somewhat higher.⁶ If the futures market surprises are interpreted as the "true" shocks, this immediately calls the VAR approach into question.

A related problem is the VAR's poor forecasting performance, both in and out of sample. As shown in Table 1, the standard deviation of the regression's residuals is much higher than the futures market's forecast errors. The out-of-sample RMSE is higher still, well in excess of the RMSE of a naive "no change" forecast.

A third, less widely recognized, problem is the large standard error associated with the VAR's policy shocks. The variance of the estimated shocks, \hat{u} around the "true" errors, u , is simply:

statistically significant. When comparable point-sampled interest rates are used as regressors, however, market efficiency (i.e., the joint hypothesis that the coefficient on the lagged futures rate is 1 and the coefficients on other interest rates are zero) cannot be rejected.

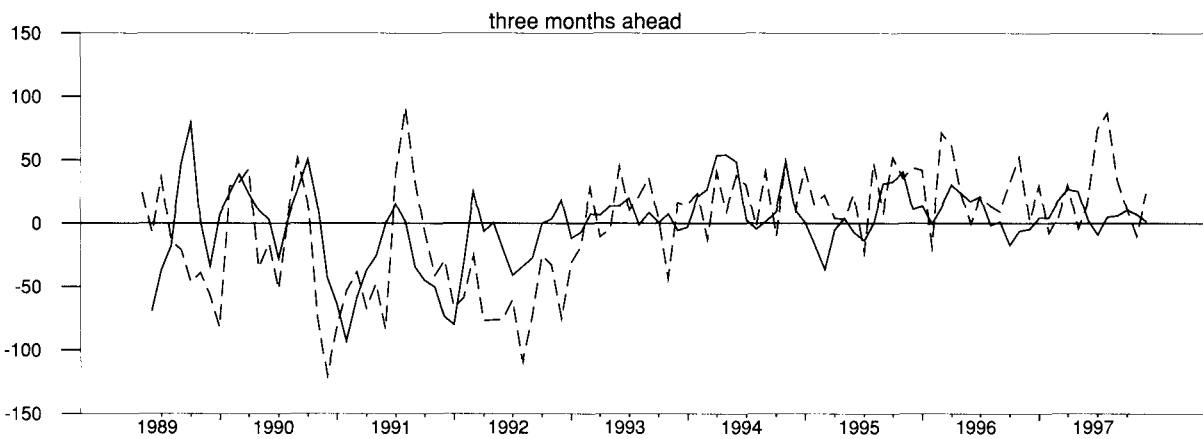
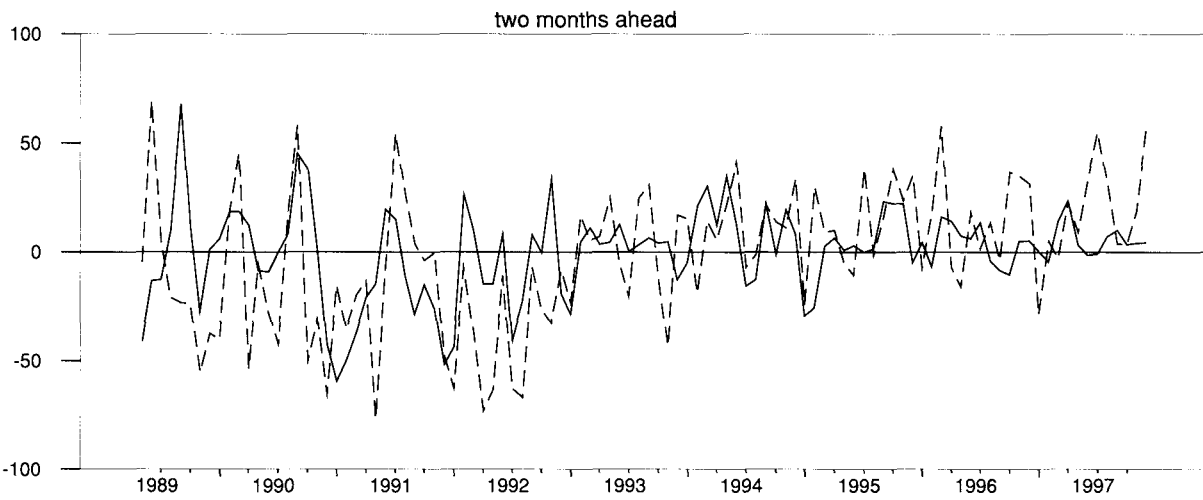
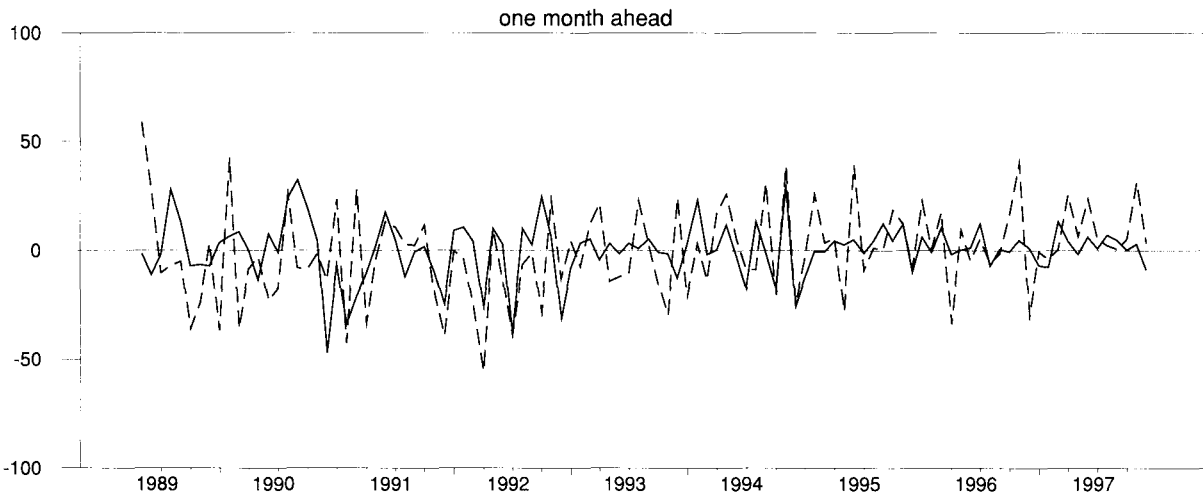
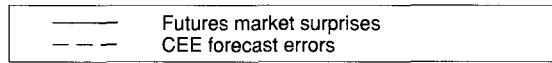
⁴ The futures rate appears to contain a forward premium of approximately 5 basis points for the one-month contract, which is subtracted from the futures rate in calculating the forecast error.

⁵ The variance and covariance estimates used to compute correlation coefficients are adjusted for the degrees of freedom used in estimation. Because computing the variance of the Fed funds futures shock does not require estimating any parameters, the result is equivalent to multiplying the unadjusted correlation by the factor $\sqrt{T/(T-k)}$ where T is the number of observations, and k is the number of VAR coefficients.

⁶ Two- and three-month ahead forecasts are obtained by regressing r_t on lags 2-12 and 3-12 of the same set of right-hand-side variables. The advantage of this shortcut is that it does not require estimating the entire VAR.

Figure 1

Fed funds futures surprises and CEE forecast errors

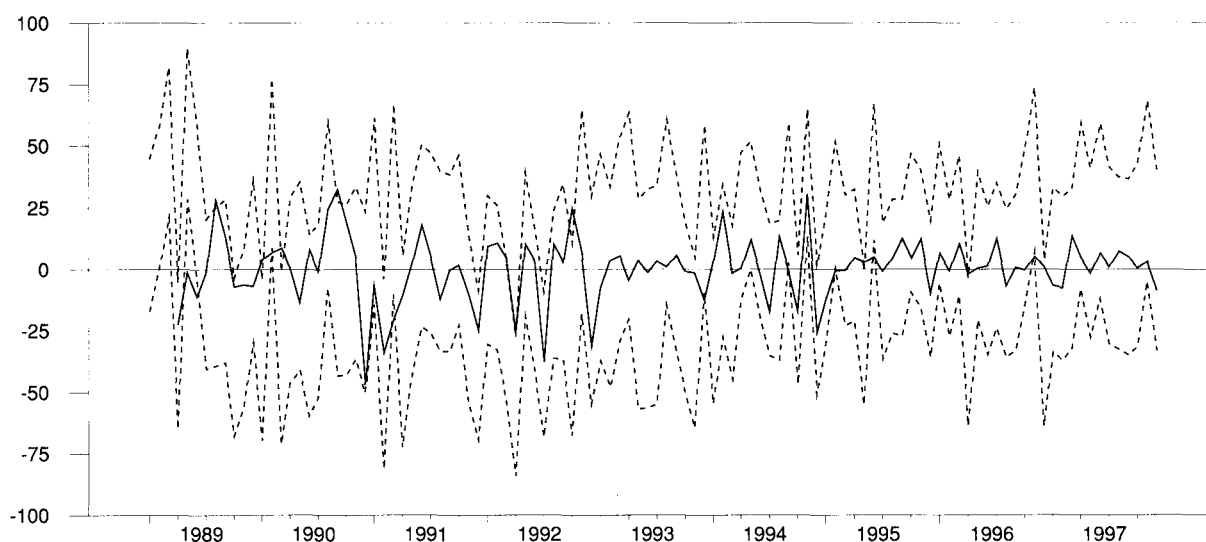


$$\begin{aligned}\text{Var}(\hat{u} - u) &= E[(\hat{u} - u)(\hat{u} - u)'] \\ &= E[X(X'X)^{-1}X'uu'X(X'X)^{-1}X'] \\ &= \sigma^2 X(X'X)^{-1}X'\end{aligned}$$

where X is the $T \times k$ matrix of variables appearing on the right-hand side of the Federal funds rate equation, and σ^2 is the variance of u .⁷ The average standard error of the shocks, reported in the fifth column of Table 1, and 95% confidence bounds around the CEE shocks are plotted in Figure 2. The estimates' imprecision is immediately visible in the figure; zero is well within the confidence bounds for most of the shocks, as are most of the futures market surprises.⁸

Figure 2

One-month Fed funds futures surprises and 95% CEE confidence bands



What accounts for these deficiencies? Several authors, notably Rudebusch (1997), Pagan and Robertson (1995), and McNees (1992), have emphasized parameter instability as a possible explanation. Rudebusch cites the VAR's profligate specification as another candidate. Nonlinearities in the Fed's reaction function, such as those arising from "opportunistic" disinflation à la Orphanides and Wilcox (1996) are another possibility, and McCarthy (1995) provides some evidence supporting this view. In addition, the VAR leaves out variables that might help forecast monetary policy. Alternatively, the estimated equation may include variables not available to investors in real time.

One response to these criticisms is the conjecture that they do not matter for impulse response functions and variance decompositions, which are the focus of most VAR analyses. After all, VARs deliver robust, relatively precise estimates of the impact of monetary policy shocks while explicitly accounting for the shock estimates' uncertainty in the computation of the impulse response functions' error bands. Unfortunately, Fed funds futures rate data do not go back far enough to make reliable comparisons between impulse response functions based on VARs with those derived from futures market shocks. Christiano, Eichenbaum, and Evans (1997) report responses based on futures

⁷ Since the regressors are not exogenous, this represents the variance of the posterior distribution conditional on realized X , given a flat prior.

⁸ The share of futures market surprises falling outside the bounds is 0.17, which represents a statistically significant deviation from the expected 0.05.

rate shocks, but with less than nine years of futures rate data, the standard errors are very large. Related work by Brunner (1997), however, found that shocks incorporating financial market and survey expectations generate impulse response functions similar to those from VARs, despite a low correlation between the measures.

Our view is that the problems are less severe than they appear, and that standard specifications can provide a better description of monetary policy than Rudebusch's results would suggest. First, the correlation between VAR and futures-market shocks is a poor gauge of the VAR's performance. Quantitatively small deviations from perfect futures market efficiency create a significant downward bias in the correlation. Second, small modifications to standard VAR specifications, such as reducing lag lengths and estimating over shorter samples, can tangibly improve the fit and precision of the models' forecasts. Finally, a time aggregation problem inherent in the futures rate can distort the timing and magnitude of shocks derived from the futures market.

2. How sensible is the correlation metric?

In using the Fed funds futures rate to evaluate the VAR's performance, one natural comparison is between the forecasts themselves: in this case, between the lagged one-month-ahead futures rate $f_{1,t-1}$ and the VAR's forecast \hat{r}_t^{VAR} . Perhaps because of the recent emphasis on policy shocks, however, assessments of VAR's performance have often involved the forecast errors rather than the forecasts themselves; see, for example, Rudebusch (1997), Brunner (1997) and Christiano et al. (1997).

At first glance, the correlation between shocks would seem to be a sensible basis for comparison; if the two procedures yielded the *same* forecasts, the shocks would be identical, and the correlation would be 1.0. Closer scrutiny shows that this measure can give a misleading picture, however; the covariance between the *shocks* has little to do with the covariance between the *forecasts*. The correlation between shocks can therefore make bad forecasting models look good, and good models look bad.

Table 2

Alternative measures of fit

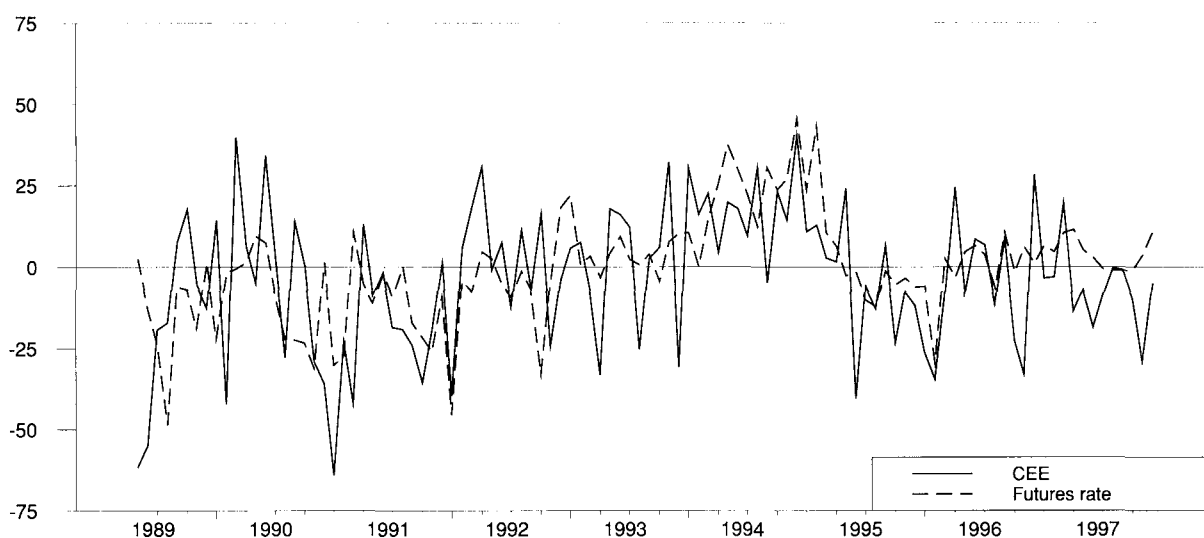
Model	Shock correlation	Δr forecasts	
		Correlation	Regression \hat{b}
CEE	0.35	0.43	0.57
No change	0.52	0	0

A comparison between the VAR and a naive "no change" forecast forcefully illustrates this point. Because the Fed funds rate is well described as an I(1) process, forecasts of the rates themselves will tend to be very highly correlated; the correlation between the forecast *changes* in the Fed funds rate, $f_{1,t-1} - r_{t-1}$ and $\hat{r}_t^{VAR} - r_{t-1}$, will therefore be more informative. As reported in the first line of Table 2, the correlation between the forecast change in the Fed funds rate is 0.43, and the shock correlation is 0.35. Sampling uncertainty associated with the estimated VAR coefficients (readily apparent as "noise" in the forecast plotted in Figure 3) will increase the variance of the VAR forecasts, however, which will reduce the correlation between forecasts. One way to eliminate the effect of parameter uncertainty is to replace the variance of $\hat{r}_t^{VAR} - r_{t-1}$ in the denominator of the correlation coefficient with the variance of $f_{1,t-1} - r_{t-1}$. The result is just the \hat{b} from the regression of the VAR forecast on the futures market's,

$$\hat{r}_t^{VAR} - r_{t-1} = a + b(f_{1,t-1} - r_{t-1}) + e_t.$$

Figure 3

Forecast one-month change in the Fed funds rate



Making this adjustment for parameter uncertainty yields a \hat{b} of 0.57, further improving the CEE model's measured fit with respect to the futures market benchmark.

How do forecasts from a "no change" forecast, $\hat{r}_t^{NC} = r_{t-1}$ compare? The implied change from this forecast is, of course, zero. Consequently, the "no change" model's forecast of the change in the Fed funds rate is uncorrelated with everything, including the forecasts from the Fed funds futures rate and the funds rate itself (hence the zeros in the second line of Table 2). On this criterion, obviously, the VAR provides the better description of monetary policy. By contrast, the correlation between the "no change" forecast errors, $r_t - r_{t-1}$, and the futures market surprises, $r_t - f_{1,t-1}$, is 0.52 – much higher than the CEE model's. Judged on this criterion, therefore, the "no change" forecast describes monetary policy better than the VAR.

How can the "no change" forecast errors be more highly correlated with the Fed funds futures surprises than the VAR's, when the VAR's forecasts are closer to the futures market's? The answer, it turns out, is that the correlation between shocks says very little about how well the VAR describes monetary policy, and a lot more about small deviations from efficiency in the Fed funds futures market.

2.1 Anatomy of a correlation

The correlation between Fed funds futures surprises, u_t^* , and forecast errors from an econometric model (e.g., a VAR), \hat{u}_t , can be written as:

$$\rho(\hat{u}_t, u_t^*) = \frac{\text{Cov}(u_t^*, \hat{u}_t)}{\sqrt{\text{Var}(u_t^*) \text{Var}(\hat{u}_t)}}$$

Since the variance of the Fed funds futures surprise is the same for each \hat{u} we consider, differences in the correlation between shocks must be attributable either to differences in the covariance between the shocks, or to the variance of the estimated errors. Substituting $r_t - \hat{r}_t$ for \hat{u}_t , the covariance term in the numerator can be written as:

$$\text{Cov}(u_t^*, \hat{u}_t) = \text{Cov}(r_t, u_t^*) - \text{Cov}(\hat{r}_t, u_t^*)$$

or in terms of the change in r ,

$$\text{Cov}(u_t^*, \hat{u}_t) = \text{Cov}(\Delta r_t, u_t^*) - \text{Cov}(\hat{\Delta} r_t, u_t^*)$$

where $\hat{\Delta} r_t = \hat{r}_t - r_{t-1}$. Writing the covariance term in this way reveals two important features.

First, the covariance between the realized change in the Fed funds rate and the futures market surprise, $\text{Cov}(\Delta r_t, u_t^*)$, mechanically builds in a positive correlation between the two shocks. Just as important, this contribution to the covariance is wholly independent of the model's forecasts. In fact, it will be positive even if the model is of no use whatsoever in forecasting the Fed funds rate, as in the case of the "no change" model discussed above.

The second key observation is that a positive covariance between the model's forecast, $\hat{\Delta} r_t$, and the futures market surprise, u_t^* , will *reduce* the covariance between the shocks. Market efficiency implies a zero covariance between u_t^* and elements of the $t - 1$ information set. In practice, however, it is highly unlikely that the sample covariance will be zero even if the market *is* efficient. Indeed, Krueger and Kuttner (1996) found that this covariance, while nonzero, was generally statistically insignificant.

Taken together, these two observations explain the "no change" forecast's surprisingly high correlation with the futures market surprises. As reported in Table 3, the covariance between Δr_t and u_t^* is 127.1, while the covariance between $\hat{\Delta} r_t$ and u_t^* is identically zero. Dividing by the relevant standard deviations yields the correlation of 0.52 – well in excess of the CEE model's, despite the zero correlation between the forecasts themselves.

An analogous breakdown for the CEE model reported on the second line of Table 3 shows that a positive covariance between the VAR's predictions and the futures market surprises partially accounts for the model's small shock correlation. The relevant covariance is 39.5; subtracting this number from 127.1 and dividing by the relevant standard deviations yields the correlation of 0.32 (without a degrees-of-freedom adjustment). Had the Fed funds futures surprises been orthogonal to the VAR forecast, the correlation would have been 0.46.

Table 3

Components of the shock correlation

Model	$\rho(\hat{u}_t, u_t^*)$	$\text{Cov}(\hat{\Delta} r_t, u_t^*)$	$\rho(\hat{\Delta} r_t, u_t^*)$	$\text{Var}(\hat{u}_t)$
No change	0.52	0	0	18.7
CEE	0.32	39.5	0.14	21.1
T-bill	0.62	-34.9	-0.12	19.9
Modified CEE	0.37	24.7	0.09	21.3

Notes: The standard deviation of the Fed funds futures shock, $\sqrt{\text{Var}(u_t^*)}$, is 13.1, and the covariance between the change in the Fed funds rate and the Fed funds futures surprise, $\text{Cov}(\Delta r_t, u_t^*)$, is 127.1. Units are basis points. Statistics are *not* adjusted for degrees of freedom.

One interpretation of this result is that the futures market is not efficient. The violation implied by this result is not quantitatively or statistically significant, however. The regression of the futures market surprise onto the VAR forecast has an R^2 of only 0.019, and the t -statistic on the CEE forecast's coefficient is only 1.41. But because standard deviations of the *shocks*, rather than the interest rate (or its change) appears in the denominator, a very small covariance can have a pronounced effect on the shock correlation.

The forecasts from a simple model involving the T-bill rate provide another illustration of the perverse properties of the shock correlation. A regression of the average Fed funds rate on two

lags of the three-month T-bill rate,⁹

$$r_t = -0.41 + 0.79 r_{t-1}^{tb} + 0.31 r_{t-2}^{tb},$$

was used to generate (in-sample) one-month-ahead predictions. Since the T-bill rate presumably incorporates expectations of subsequent months' Fed funds rate, it comes as no surprise that this equation's forecasts are highly similar to those from the Fed funds futures market; in fact, the regression of the former on the latter gives a coefficient of 0.99. Yet the correlation between the shocks is 0.62 – only 20% higher than the “no change” forecast's. Again, the nonzero covariance between the model's forecasts and the futures market shock violates the assumption of strict orthogonality, but since this covariance is negative, it increases the shock correlation.¹⁰ But the forecast's volatility is somewhat higher than the futures market's, and this reduces the correlation.

2.2 Does the VAR use too much information?

Aside from a violation of strict market efficiency, one reason for the CEE forecast's covariance with futures rate surprises is that the VAR incorporates “too much” information. The VAR forecast of the November funds rate (say) uses October's data, even though the most recent data on employment and prices is from September.¹¹ Moreover, much of these data are subsequently revised, and as Orphanides (1997) showed, the revisions can have a major impact on the fit of simple monetary policy rules. Consequently, the correlation between the futures market surprises and the VAR forecasts may be an artifact of the VAR's information advantage.

To see what impact this might have on the correlations, we re-ran the CEE equation with additional lags on payroll employment and consumer prices. (The reserves and money statistics are essentially known by the end of the month.) The results of this exercise appear in the final row of Table 3, labeled “modified CEE.” This change reduces the positive covariance between the VAR forecasts and the futures market surprises somewhat, as would be expected if it were the result of the VAR's information advantage. Substituting unrevised, real-time data in place of the revised data used here might further reduce the covariance.

3. Parsimony and parameter instability

As shown above, the positive covariance between futures market surprises and VAR forecasts partially accounts for the low correlation between the VAR forecast errors and the futures market surprises. The dissection of the correlation coefficient also revealed a second culprit. The variance of the shocks from the VAR is considerably higher than the futures market surprises. Since the square root of this variance appears in the denominator, it, too, will reduce the measured correlation.

What accounts for the VAR's inflated shock variance? One possibility is the VAR's generous parameterization – 85 parameters in the monthly CEE specification. The profligacy of the CEE model surely explains the imprecision of the shock estimates, but it alone cannot explain the shocks' implausibly high variance, so long as the “true” model is nested within it.

⁹ The regression uses last-day-of-the-month T-bill rate data, and it is estimated over the January 1961 through December 1997 sample.

¹⁰ The lagged futures rate is itself weakly (negatively) correlated with the futures market surprise.

¹¹ For this reason, Krueger and Kuttner (1996) were careful to introduce additional lags when testing futures market efficiency.

Estimating the VAR over the 1961–97 sample might, however, contribute to the shocks' volatility if the parameters changed over time. A model estimated over a sample that included the 1979–82 M1 targeting regime, for example, will almost surely be inappropriate for later periods when the Fed's weight on monetary aggregates is smaller – if not zero. The spurious inclusion of M1 could therefore introduce noise into the forecasts for later periods. Other research has turned up significant time variation along these lines. Friedman and Kuttner (1996) estimated a time-varying-parameter version of a funds rate equation, and found significant variation in the coefficient on the monetary aggregates corresponding to the shifts in targeting regimes. Instability has also been documented by Pagan and Robertson (1995).

Table 4 reports the results of shortening the lag lengths and estimating the CEE model over shorter sample periods.¹² Comparing the twelve-lag to the six-lag results for the full sample shows that greater parsimony increases the shock correlation slightly, presumably by reducing the covariance between futures market surprises and the VAR forecasts. (Obviously, eliminating *all* right-hand-side variables drives this covariance to zero.) But greater parsimony actually *reduces* the correlation between the forecasts, and the forecast RMSE falls only slightly. The reduction in the number of coefficients to be estimated shrinks the standard error drastically, however.

Table 4
Improving the VAR forecasts

	Standard deviation of shocks	Forecast RMSE	Average standard error of shocks	Forecast \hat{b}	Shock correlation
One month					
Futures rate	13.1				
CEE model					
12 lags, full sample	23.5	27.1	20.6	0.57	0.35
6 lags, full sample	21.9	26.1	15.7	0.33	0.34
6 lags, post-83	16.2	25.6	6.5	0.52	0.54
Two months					
Futures rate	20.8				
CEE model					
12 lags, full sample	35.4	43.7	34.0	0.58	0.38
6 lags, full sample	34.1	42.0	24.2	0.39	0.39
6 lags, post-83	24.1	38.8	9.1	0.67	0.53
Three months					
Futures rate	29.6				
CEE model					
12 lags, full sample	48.2	60.7	42.1	0.55	0.47
6 lags, full sample	48.0	59.8	27.6	0.40	0.54
6 lags, post-83	31.5	51.5	10.7	0.75	0.60

Notes: The reported statistics are based on the May 1989 through December 1997 sample. The in-sample standard deviation and correlations are adjusted for the degrees of freedom used in estimation. Units are basis points.

The results improve considerably when the estimation period is restricted to the January 1983 through July 1997 sample. For one thing, the forecasts are now much less noisy. At 16.2 basis points, the standard deviation of the estimated one-month-ahead shocks is now only slightly larger than the futures market's.¹³ The correlation between the shock measures rises to 0.54, but again the positive covariance between the model's forecast and the futures market surprises again prevents it

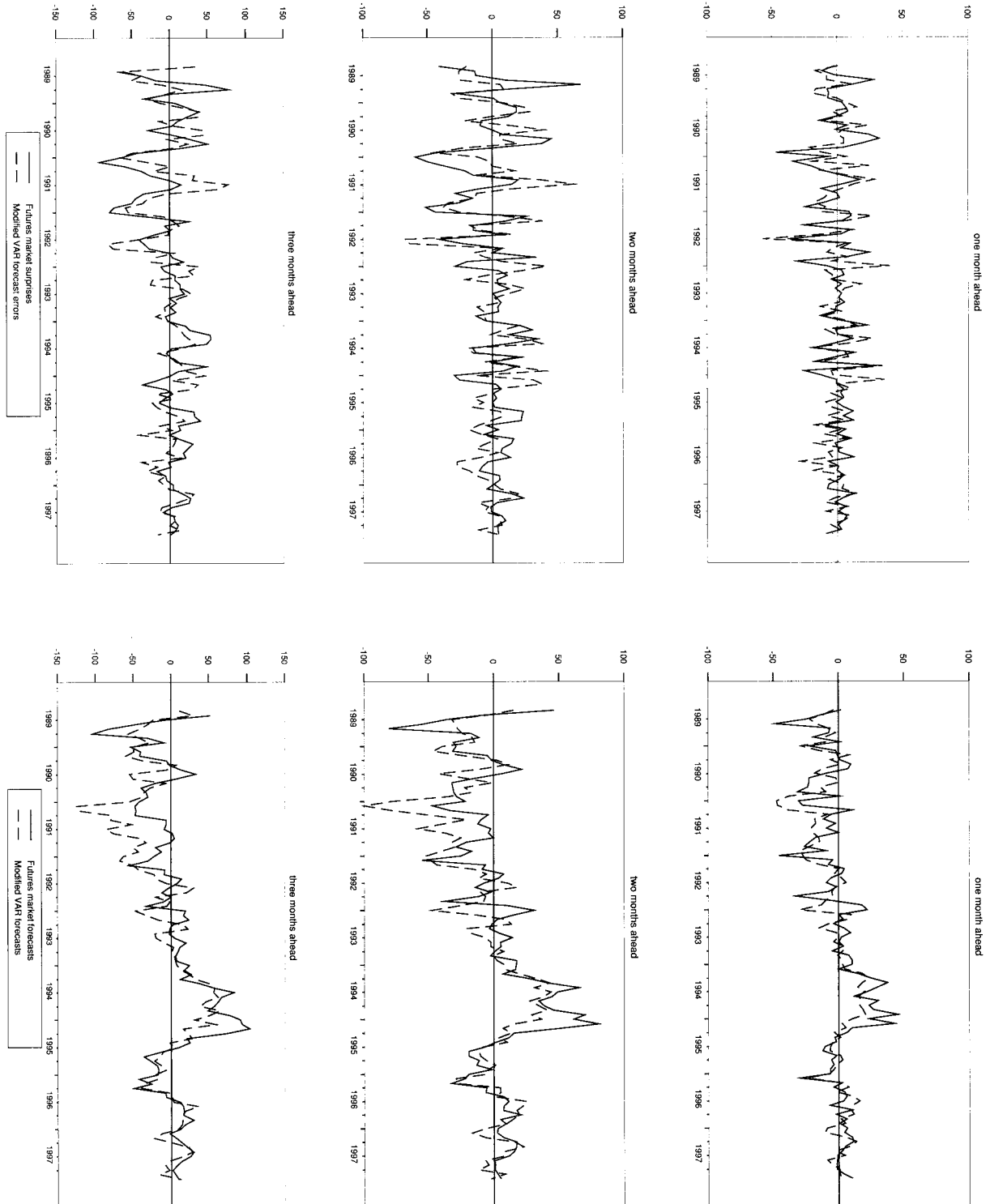
¹² As in Table 1, the correlations and standard deviations are adjusted for the degrees of freedom used in estimation.

¹³ A similar result is apparent in Figure 7 of Christiano, Eichenbaum and Evans (1997).

from rising even higher. The regression of the model's forecast on the futures market's yields a coefficient of 0.52. The VAR does even better at longer horizons. At three months, the estimated \hat{b} for the forecasts is 0.75, and forecast errors' correlation is 0.60. The forecasts and shocks plotted in Figure 4 confirm that the VAR approximately mimics the systematic and unsystematic changes in the funds rate implied by Fed funds futures rates.

Figure 4

Forecasts and errors from modified VAR equation



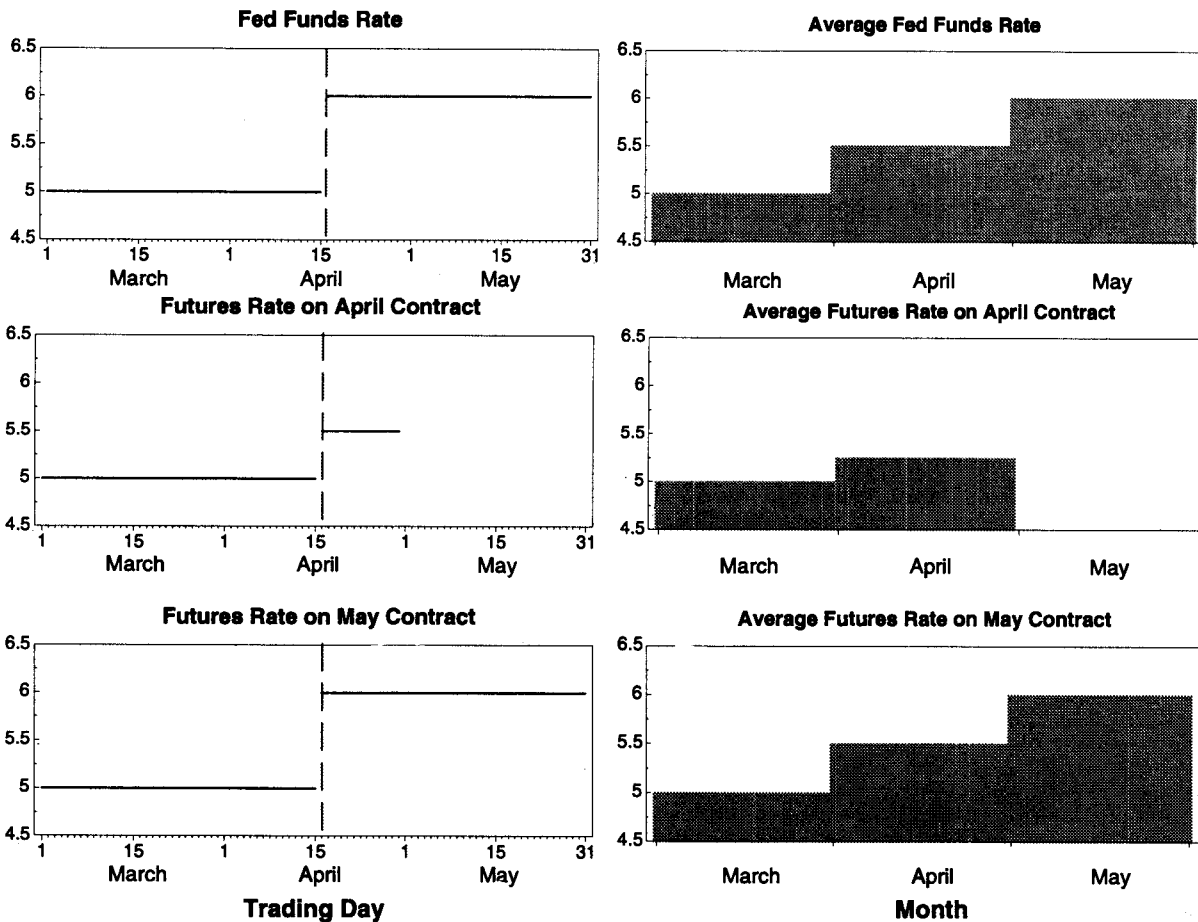
4. Time aggregation

The timing of the surprises extracted from monthly or quarterly VARs is, of course, somewhat ambiguous. At first glance, it would seem that shocks derived from the Fed funds futures rate would be free of such ambiguity. That turns out not to be true, however. The Fed funds futures contract's settlement price is based on the monthly *average* of the overnight Fed funds rate, which creates a time aggregation problem. Consequently, the timing and magnitude of policy shocks based on the futures rate are also ambiguous.

To illustrate this problem, consider the following scenario. The Fed funds rate is 5% in March, and this rate is expected to prevail through May. Now suppose that on April 16, the Fed unexpectedly raises the target Fed funds rate to 6%, and that the new rate is expected to remain in effect through May. Assume the Fed does, in fact, leave the rate at 6%. April's average Fed funds rate is 5.5%, reflecting 15 days at 5% and 15 days at 6%. The path of the Fed funds rate and the monthly averages are shown in the top row of Figure 5.

Figure 5

Financial markets' response to a Fed funds surprise



How will the futures rates respond to the surprise? With no change in the Fed funds rate expected, the futures rates corresponding to the April and May contracts will be 5% up through April 15. On April 16, the day of the surprise, the futures rate for the May contract will rise to 6%, reflecting the expectation that the 6% rate will prevail throughout May. But since the April contract is settled against April's *average* funds rate, the futures rate for the April contract will rise to only 5.5%. The paths of the futures rates are depicted in the left-hand column of the second and third rows of Figure 5.

The Fed's action on April 16 represents an unexpected increase of 100 basis points relative to expectations on April 15 and before. How should the Fed funds surprise be measured using monthly futures market data? Conceptually, this is simply the realized Fed funds rate minus its conditional expectation as measured by the futures data. But there are two complications. First, the futures contract is settled against the monthly average of the daily Fed funds rate; consequently, the realized Fed funds rate is taken to be the monthly average of the daily Fed funds rates. The second issue is whether to use point-in-time or average futures rate data in forming the conditional expectation.

Suppose we use the one-month-ahead futures rate on the last day of the previous month (e.g., the rate for the April contract as of March 31) as the conditional expectation, and measure the surprise relative to the monthly average Fed funds rate. In this example, summarized in the top panel of Table 5, the April surprise can be computed as April's average Fed funds rate (5.5%) minus the March 31 Fed funds futures rate for the April contract (5%), yielding only a 50 basis point surprise. Again using the futures rate from the last day of the previous month, the May surprise is calculated as the May average Fed funds rate (6.0%) minus the April 30 Fed funds futures rate for the May contract (6.0%), yielding no surprise. Recalling that the average funds rate increases 50 basis points in both April and May, the first 50 basis point increase is taken to be a surprise, while the second 50 basis points is anticipated. And yet, the example states clearly that the 100 basis point increase is a complete surprise on April 16. Last-day-of-month futures data, therefore, will tend to understate the magnitude of the true shock.

Table 5

Funds rate surprises using alternative measures of expectations

	Month		
	March	April	May
Average Fed funds rate	5.0%	5.5%	6.0%
Futures rate on last day of month	5.0%	6.0%	6.0%
implied Fed funds surprise	0	+50 b.p.	0
implied expected change	0	+50 b.p.	0
Average futures rate over month	5.0%	5.5%	6.0%
implied Fed funds surprise	0	+50 b.p.	+50 b.p.
implied expected change	0	0	0

An alternative way to measure the Fed funds surprise is to use the monthly average of a contract's futures rate, depicted in the right-hand column of the second and third rows of Figure 5, for the conditional expectation. This measure of the Fed fund surprise preserves the size of the shock's cumulative impact, but spreads it out over two consecutive months. As summarized in the bottom panel of Table 5, the April surprise is computed as the April average fed funds rate (5.5%) minus the March daily average of the April contract rates (5.0%), which is a 50 basis point surprise. The May surprise is the May average fed funds rate (6%) minus the April daily average of the May contract rates (5.5%), yielding a 50 basis point surprise. More generally, the surprise based on average futures rates will be a convex combination of the true shocks,

$$r_t - \bar{f}_{1,t-1} = \theta u_t + (1 - \theta)u_{t-1}$$

which implies an MA(1) structure for the average shocks,

$$r_t - \bar{f}_{1,t-1} = (1 + \phi L)e_t$$

where $\phi = (1 - \theta)/\theta$ and $e_t = \theta u_t$.¹⁴ Econometric methods, like those of Hansen and Hodrick (1980)

¹⁴ Estimating the MA(1) model gives $\hat{\phi} = 0.38$. This implies $\hat{\theta} = 0.72$, which is consistent with surprises typically occurring on the 8th day of the month.

and Hayashi and Sims (1983), exist to deal with the resulting moving-average error structure in market efficiency tests, but recovering the original “true” shock from the time-averaged data is generally not possible.

In the examples described so far, the monthly value of the Fed funds rate is taken to be the monthly average of the daily rates. How would the calculation be affected if the Fed funds rate from a single day were used in place of the monthly average? In this example, the April 30 Fed Funds rate (6.0%) minus the March 31 Fed funds futures rate for the April contract (5.0%) gives the correct 100 basis point surprise. However, other examples would generate problems with this calculation. Suppose that data were released in the first week of April that indicated the FOMC’s normal response would be to increase the Fed funds rate by 50 basis points to 5.5%; and then on April 16 the actual policy move was 100 basis points to 6%. In this case, 50 basis points is anticipated, and the other 50 basis points is a surprise. But the calculation above is unaffected by the first week’s data release, so the surprise is overstated by the amount of the mid-month’s revision to anticipated policy. Finally, since the settlement price of the Fed funds futures contract is based upon the monthly average, there is little reason to believe the futures rates would satisfy conditions of unbiasedness and efficiency relative to the last-day-of-the-month Fed funds rate.

One way to reconstruct the “true” April shock is to rescale the first measure of the surprise. Specifically, compute the surprise as the April average Fed funds rate (5.5%) minus the March 31 Fed funds futures rate for the April contract (5%), and multiply the surprise by the factor m/τ , where m is the number of days in the month and τ is the number of days affected by the change. In the scenario described above, for instance, the measured surprise of 50 basis points is scaled up by a factor of two to yield the correct 100 basis point shock. This procedure only works when the dates of policy changes (and potential changes) are known, so it would only apply to the post-1994 period in which all changes in the target Fed funds rate occurred at FOMC meetings.¹⁵ Prior to that time, most changes in the target occurred unpredictably *between* meetings. In this case, inferring the size of the “true” shock involves expectations of when the policy action occurs as well as the direction and magnitude of the change. This is beyond the scope of our analysis.

To get some sense of the quantitative importance of time aggregation, we computed the standard deviation of the rescaled Fed funds futures surprises for the post-1994 period, and compared it to the standard deviation of the unscaled shocks for the same period. The results appear in Table 6. As shown on the first line of the table, the volatility of the policy shocks rescaled in this way is dramatically higher – 28.2 basis points compared with 10.9 basis points for the unscaled shocks.

Table 6
Volatility of unscaled and rescaled Fed funds futures shocks

	Standard deviation	
	Unscaled	Rescaled
Using prior month’s futures rate		
Average of effective Fed funds rate	10.9	28.2
Average of target Fed funds rate	8.9	19.7
Using spot month futures rate	–	22.9

Notes: The reported statistics are based on the February 1994 through December 1997 period. Units are basis points.

Rescaling the shocks in this way will exaggerate the effects of any transitory deviations of the funds rate from its target (i.e., “Desk errors”), however, so it will tend to overstate the volatility of the policy shocks. If there were an FOMC meeting two days before the end of the month, for example,

¹⁵ The only exception to this was the 25 basis point increase in the target in April 1994.

and if the monthly average Fed funds rate turned out to be 1 basis point above the target, the rescaling will result in a spurious 15 basis point shock. To reduce the effect of this noise, the average *target* Fed funds rate can be used in place of the effective rate. This procedure will distort the size of the shocks only to the extent that market participants expect the average effective rate to deviate from the target. Using the target rate, the standard deviation of the rescaled policy shocks is 19.7 basis points, compared with only 8.9 for the unscaled shocks.

An alternative way to gauge the effect of time aggregation on the magnitude of funds rate surprises is to use the “spot month” contract’s price, which is based on the average Fed funds rate prevailing in the *current* month. Again, suppose that changes in the target Fed funds rate only occur immediately following an FOMC meeting, and, as in the example above, suppose that the FOMC meeting occurs on April 16. The difference between the April 16 and April 15 futures rates for the April contract would reflect the change in the expected path of the Fed funds rate over the April 16 to April 30 period. In the scenario described above, the spot month futures rate on April 15 would have been 5.0%, consistent with the “no change” expectation. On April 16, after the increase to 6.0%, the spot month futures rate would be 5.5%, since the contract’s settlement price is based on an average that includes the first 15 days of the month, when the Fed funds rate was only 5.0%. As before, scaling the difference by the factor m/τ preserves the size of the shock. The result, as reported in Table 6, is a standard deviation of 22.6 basis points – very similar to the size of the shocks computed using end-of-month futures data and the target Fed funds rate.

Making adjustments for time aggregation results in futures-market policy surprises that are roughly twice as large as those that fail to make this adjustment. This result suggests that monetary policy is not as predictable as one might have suspected, and shows that time aggregation may distort comparisons between shocks based on futures rates and those from VARs.

Conclusions

Financial market data, such as Fed funds futures rates, are potentially useful benchmarks for evaluating econometric measures of systematic and surprise movements in monetary policy. The approach is not without its pitfalls, however. One hazard involves the interpretation of the correlation between Fed funds futures surprises and VAR shocks. This correlation contains little meaningful information relevant for assessing the VAR’s description of monetary policy. As shown above, small deviations from the orthogonality condition implied by market efficiency can have a big effect on the correlation.

This is not to say that VAR’s description of monetary policy is perfect. Their forecasts are imprecise and noisy, and there is some evidence to suggest parameter instability. Shorter lag lengths and a more judicious selection of starting date can mitigate these problems, however, and the results presented here suggest more research along those lines is warranted.

One important complication arising in comparisons between VARs and futures-market forecasts is time aggregation. This problem can distort the timing and magnitude of the estimated policy surprises: point-in-time futures rate data gets the timing right, but attenuates the magnitude, while average data gets the magnitude right but distorts the timing. This observation has important implications for attempts to draw inferences about the size of policy shocks from futures market data.

While the distortion created by time aggregation may have significant effects on the contemporaneous correlation between shocks, it is unlikely that it would affect the economy’s estimated response to those shocks. Because an impulse response function can be thought of in terms of a regression of the relevant variable on a set of mutually uncorrelated shocks, merely shifting the shocks’ dating a month – or even a quarter – in one direction or another may alter the *timing* of the response but have little effect on its shape or size. Consequently, the timing ambiguities identified above are probably irrelevant for measuring the real effects of monetary policy.

References

- Bernanke, Ben S. (1986): "Alternative explanations of the money-income correlation". *Carnegie-Rochester Conference Series on Public Policy*, 25.
- Bernanke, Ben S. and Ilian Mihov (1995): *Measuring Monetary Policy*. Federal Reserve Bank of San Francisco, Working Paper, 95-09, March.
- Brunner, Allan (1997): *On the Derivation of Monetary Policy Shocks. Should We Throw the VAR Out With the Bath Water?* Unpublished manuscript, Board of Governors of the Federal Reserve System, October.
- Christiano, Lawrence J., Martin Eichenbaum and Charles L. Evans (1996a): "The effects of monetary policy shocks. Evidence from the flow of funds". *Review of Economics and Statistics*, 78(1), pp. 16-34.
- Christiano, Lawrence J., Martin Eichenbaum and Charles L. Evans (1996b): "Identification and the effects of monetary policy shocks", in M. Blejer, Z. Eckstein, Z. Hercowitz and L. Leiderman (eds.), *Financial Factors in Economic Stabilization and Growth*, Cambridge University Press, pp. 36-74.
- Christiano, Lawrence J., Martin Eichenbaum and Charles L. Evans (1997): *Monetary Shocks. What Have We Learned, and to What End?* Forthcoming in the Handbook of Macroeconomics, October.
- Friedman, Benjamin M. and Kenneth N. Kuttner (1996): "A price target for US monetary policy? Lessons from the experience with money growth targets". *Brookings Papers on Economic Policy*, pp. 77-125.
- Friedman, Milton and Anna J. Schwartz (1963): *A Monetary History of the United States, 1867-1960*. Princeton University Press.
- Hansen, Lars and Robert Hodrick (1980): "Forward exchange rates as optimal predictors of future spot rates. An econometric analysis". *Journal of Political Economy*, 88, pp. 829-53.
- Hayashi, Fumio and Christopher Sims (1983): "Nearly efficient estimation of time series models with predetermined, but not exogenous, instruments". *Econometrica*, 51, pp. 783-98, May.
- Krueger, Joel T. and Kenneth N. Kuttner (1996): "The Fed funds futures rate as a predictor of Federal Reserve policy". *Journal of Futures Markets*, 16(8), pp. 865-79.
- Leeper, Eric M., Christopher A. Sims and Tao Zha (1996): "What does monetary policy do?" *Brookings Papers on Economic Policy*, 1996(2), pp. 1-78.
- McCarthy, Jonathan (1995): *VARs and the Identification of Monetary Policy Shocks. A Critique of the Fed Reaction Function*. Unpublished manuscript, Federal Reserve Bank of New York, April.
- McNees, S. K. (1992): "A forward-looking monetary policy reaction function. Continuity and change". *New England Economic Review*, pp. 3-13, November/December.
- Orphanides, Athanasios (1997): *Monetary Policy Based on Real-Time Data*. Unpublished manuscript, Board of Governors of the Federal Reserve System, October.
- Orphanides, Athanasios and David W. Wilcox (1996): *The Opportunistic Approach to Disinflation*. Board of Governors of the Federal Reserve System, Finance and Economic Discussion Series, 96-24, May.
- Pagan, Adrian and John Robertson (1995): "Resolving the liquidity effect". *Federal Reserve Bank of St. Louis Review*, 77, pp. 33-53.
- Romer, Christina D. and David H. Romer (1989): "Does monetary policy matter? A new test in the spirit of Friedman and Schwartz". Olivier Blanchard and Stanley Fischer (eds.), *NBER Macroeconomics Annual*, pp. 121-70.
- Rudebusch, Glenn (1997): *Do Measures of Monetary Policy in a VAR Make Sense?* Unpublished manuscript, June.

- Sims, Christopher A. (1972): "Money, income and causality". *American Economic Review*, 62, pp. 540–52, September.
- Sims, Christopher A. (1980a): "Comparison of interwar and postwar business cycles. Monetarism reconsidered". *American Economic Review*, 70, pp. 250–7, May.
- Sims, Christopher A. (1980b): "Macroeconomics and reality". *Econometrica*, 48, pp. 1–48, January.
- Sims, Christopher A. (1992): "Interpreting the macroeconomic time series facts. The effects of monetary policy". *European Economic Review*, 36, pp. 975–1011.
- Sims, Christopher A. (1996): *Comment on Glenn Rudebusch's "Do Measures of Monetary Policy in a VAR Make Sense?"* Unpublished manuscript, July.
- Strongin, Steven H. (1995): "The identification of monetary policy disturbances. Explaining the liquidity puzzle". *Journal of Monetary Economics*, 35(3), pp. 463–97.