Data revisions and the identification of monetary policy shocks

Dean Croushore[a] *, Charles L. Evans[b] †

[a]Robins School of Business, University of Richmond, Virginia 23173, USA

[b]Federal Reserve Bank of Chicago, Chicago, IL 60690, USA

Monetary policy research using time-series methods has been criticized for using more information than the Federal Reserve had available. To quantify the role of this criticism, we estimate VARs with real-time data while accounting for the latent nature of many economic variables, such as output. Our estimated monetary policy shocks are closely correlated with typically estimated measures. The impulse response functions are broadly similar across estimation methods. Our evidence suggests that the use of revised data in VAR analyses of monetary policy shocks may not be a serious limitation for recursively identified systems, but presents more challenges for simultaneous systems.

JEL classification: E52
Keywords: real-time data; monetary policy shocks; VARs; data revisions; identification.

1. Introduction

Empirical research with vector autoregressions (VARs) typically ignores issues associated with data revisions and economic agents' access to only real-time data releases. An example of this is the literature on monetary policy shocks in VARs (for example, Bernanke and Blinder (1992), Sims (1992), Christiano, Eichenbaum and Evans (1996, 1999), Sims and Zha (1996) and Bernanke and Mihov (1998)). Each of these studies is based upon some data series that were not known to anyone during the period of the empirical analysis. Specifically, the data used in these studies, as well as virtually all other macroeconomic time-series research, have been revised relative to the data known at that time. Since government agencies and private sources do not provide these data conve-

*Corresponding author. Tel.: +1 804 287 1961; Fax: +1 804 289 8878; E-mail: dcrousho@richmond.edu (D. Croushore).

†We thank Ryan Hayward, James Sherma, and Judy Yoo for valuable research assistance. We thank Chris Sims for helpful comments on this research, as well as other seminar participants at Princeton University, Michigan State University, the University of Richmond, the Division of Monetary Affairs at the Board of Governors, the Federal Reserve Banks of Chicago and Philadelphia, the Southern Economic Association meetings, the American Economic Association meetings, the 2000 Econometric Society World Congress, and the Federal Reserve System Committee on Macroeconomics. This paper represents the views of the authors and should not be interpreted as reflecting the views of the Federal Reserve Bank of Chicago or the Federal Reserve System.

niently, these shortcuts are rarely questioned.[1] The real-time data collected by Croushore and Stark (2001), however, allow researchers to explore the empirical robustness of many existing macroeconomic results to this issue. Armed with the original data releases that were known at that time to business analysts, market participants, policymakers, and the rest of the interested universe, the econometrician can answer the question, how much of a difference does this make to empirical analyses of monetary policy shocks?

Addressing this question is complicated by the fact that some data are always revised, and hence the true underlying economic concept is never observed fully. For example, aggregate economic activity in the United States is not directly observable, but data on real GDP are reported and revised by the Bureau of Economic Analysis. The monetary policy shock literature has focused on how real GDP, for example, is affected by an exogenous shock to monetary policy. This is an interesting question when real GDP is taken to be an accurate measure of aggregate economic activity, but the focus should instead be on the impact of monetary policy shocks on economic activity. Consequently, when data revisions are accounted for in empirical VAR analyses, the unobserved true variable must be modeled.[2] In standard OLS estimates of autoregressions, this will induce errors-in-variables biases.

Errors-in-variables issues raise another econometric problem for identified VAR analyses, not simply the literature on monetary policy. Structural shocks are identified based upon the covariance structure of the VAR innovations. The standard method of estimating VAR innovations from the residuals, however, will include data revisions (or measurement noises). In general, the revision components will be correlated across the equations in the system. Identifying the economic shocks from the measurement noises requires more structure on the measurement process. In our empirical example, conditional on having the complete data set, the identification and estimation of the monetary policy equation is simpler than for other equations because the policy instrument is set based on observable data.

This paper considers two approaches to addressing the fact that econometricians' macroeconomic data sets are changing over time because of data revisions. The first approach is to assess the sensitivity of VAR estimates across different data vintages. For example, how do monetary policy reaction function estimates change when the sample period is fixed at 1960-1983, but the data are drawn from different vintages, with different base years, or different methodologies (for example, some vintages that use fixed-weighted data and others that use chain-weighted data)? A strength of this vintage robustness analysis is that it corresponds to typical analyses within the literature. However, this approach does not explicitly consider how the data revision process takes place, side-stepping a true real-time analysis. Our second approach considers a statistical model of data revisions and implements an alternative, real-time estimation strategy to overcome the errors-in-variables biases. Our method assumes that output, the price level, and monetary aggregates are latent variables that the data collection agency never measures precisely. Given a standard

---

[1] Diebold and Rudebusch (1991) investigate this issue for the index of leading indicators. Rudebusch (1998) criticizes VAR-based estimates of monetary policy reaction functions for ignoring this issue. Orphanides (2001) empirically assesses the importance of this issue for Taylor rule estimates.

[2] Sargent and Sims (1977) provide an early example of this environment. Sargent (1989) and Stock and Watson (1989) discuss how Kalman filter methods can be used tractably to estimate these models.

set of restrictions to identify policy and nonpolicy shocks in the absence of measurement noises, our analysis with these noises is able to identify the shocks and compute impulse responses.

Our empirical analysis of the recursively identified Christiano, Eichenbaum, and Evans (1996) system suggests that many results from the VAR literature on monetary policy are robust to these issues of real-time data availability. Specifically, our analysis of the 1960-83 estimation period using alternative data vintages (Section 3) uncovers only minor differences in monetary policy shock measures and impulse responses. Our real-time analysis of the 1968-91 period (Section 5) also finds only small differences in the estimated policy shocks between the real-time estimates and 1998-vintage estimates. The estimated effects of monetary policy shocks on variables in the system are somewhat smaller in the real-time system, but qualitatively are remarkably similar. The estimated effects of other orthogonalized shocks are also similar in the real-time system for the first three to five years of responses. After this length of time, however, the price variables in the real-time system exhibit trending behavior, while the 1998-vintage responses seem to revert to zero. So, estimated impulse responses may be sensitive to data revisions.

Our analysis of Galí's (1992) identification strategy indicates that real-time data issues present more difficulties in fully simultaneous VAR systems. When monetary policy and financial market data respond to data revisions, the Galí *IS*, monetary policy, and money demand shocks are not identified separately from the data revisions without additional restrictions. Galí's *Supply* shock is identified by long-run restrictions, and this identification is not affected by the transitory noise in data revisions. Our estimated impulse response functions following a *Supply* shock are qualitatively similar across both the real-time and a 1998-vintage system.

The paper is organized as follows. Section 2 discusses the relationship between the VAR literature on monetary policy and real-time information sets. Section 3 investigates the robustness of two VAR studies to using alternative data vintages in the estimation over the period 1960-83. Section 4 discusses difficulties raised by real-time data issues in an example, two-variable autoregression, and proposes an estimation strategy. Section 5 reports empirical results for this method applied to the Christiano, Eichenbaum, and Evans (1996) system. Section 6 examines the difficulties of identification in the non-recursive system of Galí (1992). Section 7 relates our findings to other studies. Section 8 concludes.

2. The literature and real-time data issues

The empirical literature that quantifies the effects of exogenous monetary policy shocks on the economy proceeds along the following lines. The monetary authority has a policy instrument S_t that is set as a function of the state of the economy. A general specification of the Fed reaction function is

$$S_t = f(\Omega_t) + \varepsilon_t \tag{1}$$

where Ω_t is the Fed's information set at time t and ε_t is an exogenous shock. This specification is embedded in the approaches of Galí (1992); Bernanke and Blinder (1992); Christiano, Eichenbaum, and Evans (1996, 1999); Sims and Zha (1996); Leeper, Sims and

Zha (1996); and Bernanke and Mihov (1998). The points of departure in these studies are the choices of the policy instrument S_t, the variables included in the information set Ω_t, as well as the different functions $f(\cdot)$, and the correlation structure between the exogenous shock ε_t and the information set Ω_t.

A common approach in these studies, however, is the use of a macroeconomic data set that was not consistently available during the entire period of the analysis. Each study uses a vintage data set whose variables have been revised over time, following the original data release. For example, Christiano, Eichenbaum, and Evans (1996) use a data set that was collected in mid-1993 and included real *GDP* data through the fourth quarter of 1992. Although the real *GDP* data for 1992:4 had only been revised twice, the historical data going back through 1960 had been revised many times. Ignoring the effects of alternative data vintages is apparent in the monetary policy rule (1) since it does not reflect the vintage of the data in the information set. Let T reflect the date of the data set's construction by the econometrician. Period T will often be the final observation in the data set, although this does not need to be the case. In this setting, the empirical policy rule in the existing literature should be restated as

$$S_t^T = f\left(\Omega_t^T\right) + \varepsilon_t^T \tag{2}$$

One reaction to this criticism is to estimate $f(\cdot)$ using $\Omega_t^{T_j}$ for various data vintages T_j to see if the estimates differ, while holding the full-sample period fixed. Using this approach, we provide evidence on the robustness of two VAR studies in section 3 (Christiano, Eichenbaum, and Evans (1996) and Galí (1992)).

A further criticism of most macroeconomic, time-series studies is that the data contained in Ω_t^T for $t < T$ were not known at time t. In most cases, the data have been revised; this critique also holds for the approach using $\Omega_t^{T_j}$. Consequently, even with certain knowledge of $f(\cdot)$, ε_t^T will differ from the true policy shock (see Rudebusch 1998 and Christiano, Eichenbaum, and Evans 1999).[3] Assuming that the monetary authority uses a time-invariant function $f(\cdot)$ to set the policy instrument, the reaction function is

$$S_t^t = f\left(\Omega_t^t\right) + \varepsilon_t \tag{3}$$

The notation with superscript t here indicates that the monetary authority sets the policy instrument S_t on the basis of information that is actually available to it during period t.

Are data revisions large enough that the distinction between these different information sets (Ω_t^T versus Ω_t^t) matters for the determination of monetary policy? Research by Croushore and Stark (2001, 2003) shows that both long-run views of the data and short-run views can change sharply because of data revisions. Average annual real output growth over five-year periods sometimes changes by as much as 0.5 percentage point; for example, real GDP growth from 1984:4 to 1989:4 averaged 3.0 percent according to the NIPA data set in November 1995, but was 3.5 percent according to the NIPA data set in November 2001. Over the same period, inflation (measured using the percent change in

[3]The large majority of seasonally adjusted, macroeconomic time series data are revised for a substantial period of time. Rudebusch (1998) criticizes the monetary policy shock literature for ignoring this. However, in principle, the problem is pervasive in macroeconomic time series studies generally. See Croushore and Stark (2003) for additional examples.

the GDP deflator) averaged 3.6 percent in the November 1995 data set but 3.1 percent in the November 2001 data set. So, one's view of trend growth in output and inflation may be changed dramatically by data revisions. In the short run, even larger revisions occur. For example, the growth rate of real output for 1977:1 was initially released as 5.2 percent; in the March 2000 data set it is 5.0 percent. But in between it changed from 5.2% to 7.5% (July 1977 NIPA data release) to 7.3% (July 1978) to 8.9% (July 1979) to 9.6% (December 1980) to 8.9% (July 1982) to 5.6% (December 1985) to 6.0% (December 1991) to 5.3% (January 1996) to 4.9% (July 1997) to 5.0% (March 2000). Because our measures of monetary shocks depend on the estimated relationship between the policy instrument and output growth as measured in a particular data vintage, it is clear that those measures may change considerably across data vintages when the underlying data on output are revised this dramatically. Thus research investigating monetary policy shocks using final revised data is potentially problematic.

The conflict between the empirical investigation of monetary policy and the actual setting of policy is troubling in principle. Rudebusch (1998) stresses this conflict, but provides only indirect evidence on the economic importance of the issue. To assess the economic consequences of using revised data, three questions emerge. First, how do the empirical policy shock measures and policy instrument settings differ in equations (2) and (3)? In general, estimating (2) using standard VAR methods will not recover the reaction function and policy shocks in (3). Second, is inference about the monetary transmission mechanism affected by this conflict? Specifically, how are impulse response functions from policy shocks to other macroeconomic data affected? These questions are far more difficult to assess than the first one. In most cases, computing impulse response functions from a monetary policy shock requires estimating the VAR equations for the other variables.[4] Although monetary policy may plausibly respond to each new data revision as described in (3), this assumption is somewhat more problematic for real GDP. Should we really expect that true output will be affected directly by the government's announcement that last month's released figure for real GDP was half a percentage point too high? Further assumptions about the non-policy equations are required: assumptions about the data revision process over time and the information available to economic agents at any point in time. As the discussion in section 4 indicates, the problems posed by data revisions range far beyond the monetary policy shock literature. Third, how is the identification of non-monetary policy shocks affected by data revision issues? Simple examples below suggest that VAR innovations estimated using revised data will include revision errors. Since identification of exogenous shocks is achieved by factoring particular covariance matrices of VAR innovations, the presence of additional covariation due to revision errors cannot be ignored.

[4]An exception is the two-step strategy described in Christiano, Eichenbaum, and Evans (1999) for a particular recursively identified policy rule. Although the two-step strategy is asymptotically justifiable for any impulse response function, it does require a time series on the exogenous shock. If the exogenous shock can only be identified with the aid of another variable's innovation, then the other equation must be estimated. The identification strategies of Sims and Zha (1996), Galí (1992), and Bernanke and Mihov (1998) require these additional restrictions.

3. Monetary policy shock estimates with different data vintages

Is there a simple way to see if data revisions really matter for the identification of monetary policy shocks? One way to answer this question is simply to investigate how changes in the vintage of the data affect the size of monetary policy shocks or impulse response functions.

Potentially, this issue could be important. In examining the robustness of empirical macroeconomic studies, Croushore and Stark (2003) found that some empirical results were strongly affected by data revisions. For example, some of the empirical results of Hall (1978) and Blanchard and Quah (1989) changed dramatically when alternative vintages of data were used. In both cases, the sample period used in the empirical work was not changed, only the date on which the data were measured.

To investigate the robustness of VAR results for measuring monetary policy shocks, we use the real-time data set of Croushore and Stark (2001), and re-estimate the identified VAR models of Christiano, Eichenbaum, and Evans (1996) and Galí (1992), using four alternative vintages of the data. We then examine the degree to which these alternative data sets lead to differing magnitudes for monetary shocks and the impulse responses to monetary shocks. We look at data sets that span 15 years. The data, especially NIPA data, have been revised significantly across that span, and thus could potentially have a large impact on the empirical results.

Christiano-Eichenbaum-Evans (CEE)

The benchmark CEE quarterly model consists of a recursively identified VAR in six variables: real GNP (or GDP) (Y), implicit GNP (or GDP) deflator (P), nonborrowed reserves (NBR), federal funds rate (FF), total reserves (TR), and an index of commodity prices ($PCOM$), where Y, P, $PCOM$, NBR, and TR are in logs. Using the Choleski decomposition, the causal ordering of the variables is important, and we use the CEE benchmark ordering Y, P, $PCOM$, FF, NBR, TR in everything that follows.

Our real-time data set includes the values of all six variables as they were reported in macroeconomic data sets in the fourth quarter of each of the following years: 1983, 1988, 1993, and 1998. The federal funds rate and commodity price variables are not revised, but the other four variables were revised substantially over this time period. The four vintage dates were selected to be five years apart and because each date comes between benchmark revisions of the National Income and Product Accounts (NIPA). The 1983 vintage was fixed-weighted with 1972 base year; the 1988 vintage was fixed-weighted with 1982 base year (following the December 1985 benchmark revision); the 1993 vintage was fixed-weighted with 1987 base year and with GDP rather than GNP as the main output concept after the November 1991 benchmark revision; and the 1998 vintage was chain-weighted with 1992 base year following the January 1996 benchmark revision. We maintain a common sample period for all four vintages of data, using just data from the sample that is common to all four data sets, 1960:1 to 1983:3.

The VAR is estimated and the monetary policy shocks are taken to be the orthogonalized innovations from the federal funds rate equation. Figure 1 displays three-month, centered moving averages of the shocks that we estimate, with each of the four different lines corresponding to a different vintage data set.

The figure shows that the measured monetary policy shocks differ somewhat in magnitude across the alternative data vintages, but they are qualitatively very similar. In almost every case, the shocks are of the same sign across vintages and display the same timing in terms of peaks and troughs. In a few cases, they are opposite in sign, as in the second quarter of 1963 and the third quarter of 1972. In other cases, the data points are quite a bit different quantitatively, especially from 1964 to 1965 and 1981 to 1982.

Looking at the impulse response functions (Figure 2) shows somewhat larger differences across vintages of the data. For output, the price level, and commodity prices, the short-run response to a fed funds shock is about the same across vintages, but the longer horizon response is somewhat different. For nonborrowed reserves and especially for total reserves, the short-run response to a fed funds shock is considerably different, but the differences across vintages are not as large for the longer horizon responses (out 20 quarters). Thus, impulse responses display only a modest sensitivity to alternative data vintages.

Galí

The CEE model imposes little economic structure on the VAR beyond the monetary policy reaction function. But much recent empirical work has used economic theory to impose more structure on the entire VAR, using short-run and long-run restrictions to provide identification. One such model is that of Galí (1992). The Galí model is a VAR in four variables, with the growth rate of real GNP (or GDP) (Y), the quarterly change in a short-term interest rate (ΔFF), the real interest rate, which equals the interest rate minus the quarterly inflation rate in the consumer price index (P), and the growth rate of the real money supply ($MONEY$), which equals the log of the nominal money supply (M1) minus the log of the price level. The only difference between our data and Galí's is that we use the federal funds rate, while he used the interest rate on three-month T-bills, but that difference should matter little for the empirical results.

Imposing identifying restrictions on the VAR allows one to calculate structural shocks and to generate impulse response functions. Galí imposes three long-run restrictions on the VAR: money supply shocks do not affect output, money demand shocks do not affect output, and spending shocks do not affect output. He also imposes three short-run restrictions: money supply shocks do not affect output contemporaneously, money demand shocks do not affect output contemporaneously, and the price level does not enter the money supply equation contemporaneously.

The shocks to monetary policy, shown in Figure 3, are again quite similar across the 1983, 1988, 1993, and 1998 vintages of data. The only surprise is that the 1988 vintage of the data shows somewhat larger shocks than the other three vintages. But the timing of all the shocks is identical; they differ only in magnitude.

The impulse responses in the Galí model, shown in Figure 4, differ a bit across vintages as well. Again, though, they are qualitatively the same in terms of their general paths. The revisions seem to affect the impulse response to real money balances the most.

Had the measures of monetary policy shocks and the impulse response functions across these four vintages been dramatically different, the robustness of VAR methods for measuring monetary shocks would be in greater doubt. The results from these two models, however, do not suggest that data revisions are terribly problematic for measuring monetary shocks. But the quantitative differences across vintages are enough to make us want to investigate more carefully the effect of data revisions on these empirical methods. Furthermore, these vintage estimates of the monetary policy rules still include revised data that were not available to policymakers. To assess the influence of real-time data for monetary policy rules, we now examine the revision process more carefully, and see how VAR estimates may be affected by different types of revisions to the data.

4. Estimating a recursively identified VAR with real-time data

To investigate the influence of real-time data issues for estimating VARs, we specify a two-variable, recursively identified example and impose structure on the data revision process. The methods adopted here for estimating and analyzing the two-variable system extend easily to higher order systems. The VAR includes two distinct types of variables. The first type is financial data, like the federal funds rate (FF), that are set on the basis of real-time data and do not get revised. The second data type is revised over time, like real GDP (Y): its time-series law of motion is specified in terms of an underlying, latent variable that is measured imperfectly. In one respect, the real-time policy and financial variable equations are the simplest to estimate: given the actual real-time data and recursiveness assumptions, these equations can be estimated by ordinary least squares. By placing sufficient structure on the historical data revision process, we deduce an instrumental variables estimation strategy for the nonpolicy equations.

A recursively identified VAR

In this example, we take the true data-generating process to be a two-equation identified VAR. The monetary authority sets the federal funds rate FF_t on the basis of its own past history, and the data reported for Y at time t. We will refer to Y as output, but it can just as easily be a vector of data. The law of motion for the true, unobserved output series Y_t^* is distinct from the data reporting process. The system of equations is:

$$FF_t = A_{FF}(L)FF_{t-1} + A_Y(L)Y_{t-1}^t + a_Y Y_t^t + \varepsilon_{1t} \tag{4}$$
$$Y_t^* = B_{FF}(L)FF_{t-1} + B_Y(L)Y_{t-1}^* + \varepsilon_{2t} \tag{5}$$

Specifying the policy reaction function in real time requires explicit assumptions about the way data revisions influence policy. Equation (4) is based upon data known at time t, namely, $Y_t^t; FF_{t-1}, Y_{t-1}^t; FF_{t-2}, Y_{t-2}^t, ;$ etc.[5] Equation (4) makes a strong assumption: FF will respond systematically to changes in the reported data even when the underlying Y^* does not change. The assumption may be reasonable because Y^* is not directly observed.

Equation (5) is the law of motion for Y_t^* and has two features worth noting. First, the data revisions influence Y^* indirectly through their effects on FF and monetary policy. Second but more critical, the latent variable Y^* depends upon its own history and not directly on the history of real-time data releases. This relationship might emerge in an economy where agents see the true economic allocations, while the monetary authority sees only error-ridden measures. Although this assumption has its shortcomings, the alternatives may be worse. Our assumption could be questioned because central banks

[5]Superscripts refer to the reporting vintage of the data, while subscripts refer to the observation period. Notice that the lag operator L operates on the observation date only, and not the data's vintage date.

expend many resources to measure and understand the state of their economies each period. Considering the fact that the Federal Reserve already purchases certain types of financial data from private companies, they would clearly pay to observe Y_t^* if private agents actually knew that information. An alternative line of reasoning might assume that no one in the economy observes Y^*. This could be accommodated by also including $\left\{Y_{t-s}^t, s \geq 0\right\}$ in equation (5), or simply its revisions. In this case, however, it is difficult to think about state-contingent allocations, market-clearing, or prices. This approach is worth investigation, but has not been pursued here.

This system of equations is written as a recursively identified VAR, with the ε_{1t} and ε_{2t} shocks assumed to be exogenous, and uncorrelated with the other right-hand-side variables. The vector of exogenous shocks $\varepsilon_t = \begin{pmatrix} \varepsilon_{1t} & \varepsilon_{2t} \end{pmatrix}'$ has a diagonal covariance structure.

Each period the output data are revised. The data revision process has the following form:[6]

$$
\begin{aligned}
Y_t^t &= Y_t^* + g_t^* && (6)\\
Y_t^{t+s} &= Y_t^{t+s-1} + h_t^{t+s}, \quad \forall s > 0, \quad \forall t && (7)
\end{aligned}
$$

The initial data release is Y_t^t, and equation (6) indicates that Y_t^t is an imperfect measure of Y_t^*, given the error term g_t^*. Each period $t+s$, the previously released data are revised by h_t^{t+s} according to (7). Taken together, equations (6) and (7) indicate that the latest data release Y_t^T is also an imperfect measure of the unobserved, true output variable Y_t^* and that subsequent data releases will also be imperfect measures. Although we do not specifically restrict the revision process h_t^{t+s}, the revision structure does not imply that g_t^* will ever be eliminated. Indeed, the simple observation that data continue to be revised indefinitely suggests that a permanent wedge exists between Y_t^{t+s} and Y_t^*.

Estimation difficulties with period T-vintage data

Suppose an econometrician uses the most recent data releases to estimate equations (4) and (5) by OLS. Often these data are referred to as "final, revised data"; but since the data continue to be revised, we refer to these data as period T-vintage data on Y_t^T. Using the revised data Y_t^T in place of Y_t^t and Y_t^*, the system of equations becomes

$$
\begin{aligned}
FF_t &= A_{FF}(L)FF_{t-1} + A_Y(L)Y_{t-1}^T + a_Y Y_t^T + w_{1t}^T && (8)\\
Y_t^T &= B_{FF}(L)FF_{t-1} + B_Y(L)Y_{t-1}^T + w_{2t}^T && (9)
\end{aligned}
$$

The critical questions revolve around the correlation structure of the error terms w_{1t}^T and w_{2t}^T and their relationship to ε_{1t} and ε_{2t}.[7] Given the revision process and the true laws of motion, it can be shown that

$$
\begin{aligned}
w_{1t}^T &= \varepsilon_{1t} - a_Y \sum_{s=1}^{T-t} h_t^{t+s} - A_Y(L) \sum_{s=1}^{T-t} h_{t-1}^{t+s}\\
w_{2t}^T &= \varepsilon_{2t} + \sum_{s=1}^{T-t} h_t^{t+s} - B_Y(L) \sum_{s=0}^{T-t} h_{t-1}^{t+s} + g_t^* - B_Y(L) g_{t-1}^*
\end{aligned}
$$

[6]Our data are measured in natural logarithms for our VAR estimation, except for FF. Consequently, we need to assume that these revisions take place with respect to the log of the series.

[7]See Rudebusch (1998) for a discussion of these issues with respect to the monetary policy equation.

When the econometrician estimates equations (8) and (9) with OLS, the estimated regression coefficients are likely to be biased. In general, the error terms w_{1t}^T and w_{2t}^T are correlated with the regressors in both equations. Note that w_{1t}^T and w_{2t}^T contain revisions to date t variables, date $t-1$ variables, and so on, which are correlated with the right-hand-side variables in equation (8) and (9), Y_t^T and Y_{t-j}^T, $j = 1,2, ...,$ except under special assumptions. In addition, w_{2t}^T contains measurement wedges g_t^* — differences between the initial measure of the variable and its latent value — which will in general be correlated with the right-hand variables in equation (9).

Because OLS estimators are biased, we look for alternative estimation methods, the use of which depends on the manner in which the data are constructed. Polar cases of data construction include: (1) methods by which revisions to data incorporate *news*, which is the case when the data agency uses all available data (not just its own sample measuring the data in question) to construct an optimal estimate of the data series in question; and (2) classical measurement error, in which revisions to the data reduce *noise*, which is the case when the data agency draws an unbiased sample, uses only that sample in constructing its data series, but fails to account for correlations between its data series and other data (not included in its sample) that are available at the time. Of course, data reporting agencies do not directly state which category their reporting method belongs to. Key tests of the extent to which data represent noise or news were undertaken by Mankiw, Runkle, and Shapiro (1984), Mankiw and Shapiro (1986), and Croushore and Stark (2003). The Croushore-Stark results suggest that for most macroeconomic variables, revisions between the initial release and one year later are best characterized as containing news, while revisions after one year cannot be easily characterized–they are a mixture of news and noise.

Based on the outcome of the tests for news and noise, we develop consistent estimators for the parameters in equation (9). Clearly, the consistency of these estimates will also depend on the validity of the auxiliary assumptions about the data revision process.

Using real-time data to estimate the real-time policy equation

Suppose that the econometrician has the original data for each period, as it was initially released and subsequently revised. For the monetary policy reaction function (4),

$$FF_t = A_{FF}(L)FF_{t-1} + A_Y(L)Y_{t-1}^t + a_Y Y_t^t + \varepsilon_{1t}$$

the econometrician can estimate this equation precisely with the vintage data $\left\{Y_{t-s}^t, s \geq 0\right\}$. Owing to the recursiveness assumption, OLS is consistent. That is, CEE and much of the literature with recursive-identification restrictions assume that monetary policy shocks are contemporaneously uncorrelated with output and prices. A natural additional assumption with real-time data is to assume that monetary policy shocks are contemporaneously uncorrelated with true output Y_t^* and the measurement wedge g_t^*. Consequently, since $Y_t^t = Y_t^* + g_t^*$ and $E\left[Y_t^* \varepsilon_{1t}\right] = E\left[g_t^* \varepsilon_{1t}\right] = 0$, all of the right-hand-side variables are orthogonal to ε_{1t}. Consequently, the exogenous monetary policy shock ε_{1t} can be recovered as the OLS residual without being polluted by data revisions.

Using different data vintages as instruments

A major stumbling block in estimating equation (9) is that true output Y_t^* is never fully revealed in the period T-vintage revisions. The error term w_{2t}^T includes the wedge terms g_t^*. If, instead, Y_t^T were to reveal Y_t^* for some T sufficiently large relative to t, then the measurement errors would disappear from w_{2t}^T completely. In that case, OLS estimation on the output autoregression would recover the true parameters asymptotically as well as the exogenous shocks.

Of course, data revisions never come to a final conclusion. For example, even though no new source data is being collected for 1959 real GDP, those data do get revised periodically. Specifically, when the base year is changed, or the concept is altered, there are data revisions. But it seems plausible to assume that there is some date beyond which all the data revisions are insubstantial and random. That is, real GDP continues to be revised substantially as new income tax information comes in over the years. Also, seasonal adjustment procedures require a number of years of data to eliminate the stochastic seasonals. But beyond some threshold period, it seems reasonable to assume that the adjustments are completely random with respect to previous years of benchmark revisions (which is consistent with the Croushore-Stark "news or noise" results). In our empirical work below, we select a three year threshold, which seems consistent with the way the BEA revises NIPA data using new source data information. Initial quarterly NIPA releases require assumptions about a variety of monthly data that have not yet been released, such as inventory, trade and corporate profit data. As these monthly data become available, the initial assumptions are replaced by data actuals. Some time later, small data samples give way to larger, more complete data sources that more closely approximate the sample universe, such as employment and industry breakdowns. Still later, tax, regulatory and periodic census surveys continue to refine data releases. For the NIPA data releases, most of these revisions in source data occur within three years, so we take this to be our threshold for assuming independence across benchmarks. Within a benchmark revision, however, the measurement errors may be serially correlated because of interpolation and spreading of annual source data information to quarterly measures.

This discussion motivates our statistical model of benchmark data revisions:

$$Y_t^{t+s} = Y_t^* + \eta_t^{t+s}, \quad s \geq J. \tag{10}$$

After the threshold J periods have elapsed, the reported data Y_t^{t+s} measure the true Y_t^* up to a measurement error η_t^{t+s} which is independent of Y_t^* and $\eta_t^{t+s'}$ (where $s' \neq s$, and $s, s' \geq J$). This model of benchmark revisions allows us to construct an instrumental variables estimator for the output equation (9). Two remarks are useful. First, our measurement theory assumes that new sets of η are drawn each quarter for revised data that are more than J periods from the initial release. A weaker assumption that our estimation strategy implements below is to rely on this assumption only across different benchmark vintages. Specifically, we assume that the 1995 and 1998 data vintages contain independent η^{1995} and η^{1998} measurement errors. Second, in the period T-vintage data, we restrict the sample period to the Y^T observations with η^T measurement errors, so that the error term w_{2t}^T is

$$w_{2t}^T = \varepsilon_{2t} + \eta_t^T - B_Y(L)\eta_{t-1}^T, \quad t \leq T - J$$

In this part of the sample, η_{t-s}^T revisions are correlated with Y_{t-s}^T and the standard measurement error bias result obtains for OLS estimation. However, the η_{t-s}^T are orthogonal to Y_{t-s}^*, and also the earlier vintage errors embedded in FF_{t-j} from the policy reaction function.

Within this sample period, the measurement errors embedded in FF_{t-1} are independent of the η^T measurement errors in w_{2t}^T. Consequently, estimation by an instrumental variables approach needs to deal only with the Y data. To this end, notice that the revision errors g_t^* and h_t^{t+s} are orthogonal to the η^T revision errors, once J periods have elapsed from the initial release of the data to ensure that all remaining revisions are orthogonal noise. Given the data vintage denoted by Y^T, we select another data vintage $Y^{T'}$. The final time period $T' < T$, by assumption (just a normalization). In practical terms, Y^T and $Y^{T'}$we take to be from the October 1998 and October 1995 releases of the National Income and Product Account data, respectively. In addition to new source data, the 1998 NIPA data are in chain-weighted dollars, while the 1995 NIPA data are in fixed-weight 1987 dollars. Let the estimation period range from observations 1 to $T' - J$; this means that all of the data have entered the stage of independent benchmark errors. Consequently, $Y_{t-s}^{T'}$ is a valid instrument for Y_{t-s}^T. That is,

$$E\left[Y_{t-s}^{T'}\ w_{2t}^T\right] = E\left[Y_{t-s}^{T'}\ \left(\varepsilon_{2t} + \eta_t^T - B_Y(L)\eta_{t-1}^T\right)\right] = 0.$$

These orthogonality conditions imply consistent estimation of the parameters in the output equation (9).[8] Given consistent parameter estimates, the two data vintages yield two residuals w_{2t}^T and $w_{2t}^{T'}$, which are error-ridden measures of the output shock ε_{2t}. However, the errors are independent of each other. The variance of ε_{2t} can be estimated by the sample covariance of these two residuals, and instrumental variables methods can be used to construct impulse response functions.

This model of benchmark revisions does have testable restrictions. An implication of equation (10) is that the cross-covariances of the growth rates should be equal, $\Delta Y_t^T \Delta Y_{t-j}^{T'}$ and $\Delta Y_{t-j}^T \Delta Y_t^{T'}$.[9]. Table 1 reports generalized method of moments estimates of the first four autocorrelations of ΔY_t^* using eight moment conditions implied by

$$\rho_j = E\left[\Delta Y_t^T \Delta Y_{t-j}^{T'}\right] = E\ \left[\Delta Y_{t-j}^T \Delta Y_t^{T'}\right],$$

as well as four restrictions to estimate the just-identified means and variances of ΔY_t^T and $\Delta Y_t^{T'}$. Estimates are reported for the four variables in the CEE system that are subject to data revisions: real GDP, the GDP deflator, nonborrowed reserves, and total reserves. As we mentioned above, our two data vintages come from the October 1995 and October 1998 releases of the National Income and Product Accounts. Our sample period runs from the first quarter of 1968 through the third quarter of 1991, the same sample period as our

[8]It should be apparent that Y_t^T and $Y_t^{T'}$ are not weak instruments for each other in this estimation strategy. For the 1995 and 1998 data vintages that we investigate below, these instruments easily passed first-stage F-tests.

[9]Equation (10) refers to the log-level of real GDP. With trending real GDP, the cross-product matrices $Y_t^T Y_{t-j}^{T'}$ will not be finite asymptotically. So it is convenient to restate the restrictions in terms of log first-differences.

VAR estimation in section 5. From Table 1, the autocorrelation patterns are not surprising for the latent variables. Output growth and inflation are positively auto-correlated. The strong persistence in inflation suggests wide confidence bounds on any valid inference regarding $I(0)$ or $I(1)$ behavior. The autocorrelation properties in nonborrowed and total reserves differ over this period, reflecting the many changes in operating procedures during this period. More interestingly, there is little evidence against the overidentifying restrictions as reported by the $J - statistic$. The largest test statistic is for real GDP growth, although the p-value is only 0.15. Over the longer sample period beginning in 1960, the test statistic increases with a p-value of 0.08. This is likely due to the inclusion of earlier time periods where the baskets of goods differ more substantially. Nevertheless, for our sample period, the empirical evidence provides support for implementing the IV estimation strategy using 1995 and 1998 vintage data.

Computing impulse response functions

Given the parameters in equations (4) and (5), it is natural to compute impulse response functions from one-time exogenous shocks ε_{1t} and ε_{2t} at time t to the paths of $\left\{FF_{t+j}, Y^*_{t+j}\right\}$ for all $j \geq 0$. The data revision process complicates these calculations: at time t, the policy reaction function responds to the initially reported data Y^t_t and its revisions to previously released data Y^t_{t-s}. Consequently, the response of data revisions to the exogenous shocks must be known in order to compute the response of FF. Recall that the revision process follows

$$
\begin{aligned}
Y^t_t &= Y^*_t + g^*_t && (11)\\
h^{t+s}_t &= Y^{t+s}_t - Y^{t+s-1}_t, \quad \forall s > 0, \quad \forall t. && (12)
\end{aligned}
$$

Given the real-time data set for Y from Croushore and Stark (2001), the revision process h^{t+s}_t is an observable data series for each $s > 0$. We assume that the $s-$revision h^{t+s}_t is a stationary process that is independent of the exogenous shocks ε_{1t} and ε_{2t}.[10] This assumption presumes g^*_t is unaffected by the exogenous shocks, so that its response is zero. Therefore, although the unconditional distribution of $\left\{FF_{t+j}, Y^*_{t+j}\right\}$ depends on the economic shocks ε_t and the measurement noises h^{t+s}_t and g^*_t, the conditional responses following an ε_t shock assume that h^{t+s}_t and g^*_t are zero.

5. Empirical results for recursive identification in a real-time data VAR

To investigate the implications of using real-time data in a VAR, we estimate the 6-variable CEE system described in Section 3. We use the Croushore-Stark real-time data set. The variables are real GNP (or GDP) (Y), the implicit GNP (or GDP) deflator (P), an index of commodity prices ($PCOM$), the federal funds rate (FF), nonborrowed reserves (NBR), and total reserves (TR). The data are in logs, except for FF. The two data vintages to be used as instruments for the latent variables are $T' =$ 1995:3 and $T =$ 1998:3. We take the benchmark threshold to be $J = 12$ quarters, so our estimation period

[10] In a previous draft, we allowed the revisions to depend on the identified shocks ε_1 and ε_2, perhaps because of optimal statistical filtering rules studied by Sargent (1989). The qualitative results from that analysis were similar to the ones reported here.

runs from 1968:1 through 1991:3. The starting date is determined by the availability of real-time data. With $J = 12$, our ending date could be as late as 1992:3. However, we end our sample in 1991:3, which is the final release of NIPA data using 1982 constant dollars. Our 1995 and 1998 data vintages are considerably different: the former is in fixed-weighted 1987 dollars, while the latter is in chain-weighted 1992 dollars, and there are a number of definitional differences in GDP. In addition, all of the real-time NIPA data used in the monetary policy rule have fixed-weight base years different from the 1995 and 1998 data vintages. These contrasts align well with our assumption of independent measurement errors among the real-time data and within data vintages that extend twelve quarters beyond the end of our sample.

Although the basic estimation strategy follows the discussion in Section 4, some details require further clarification. In what follows, it will be helpful to define $Z_1 = \begin{bmatrix} Y & P \end{bmatrix}'$ and $Z_2 = \begin{bmatrix} NBR & TR \end{bmatrix}'$. First, the federal funds rate equation is estimated with real-time data for each of our six variables, which is the obvious generalization of equation (4):

$$FF_t = A(L)' \begin{bmatrix} Z_{1t-1}^{t\prime} & PCOM_t & Z_{2t-1}^{t\prime} \end{bmatrix}' + a_1' Z_{1t}^t + a_2 PCOM_t + \varepsilon_{FF,t} \tag{13}$$

where $A(L)$ is a 5×1 vector polynomial in the lag operator L, which acts on the time subscript only (not the data vintage superscript); and a_1 is 2×1 vector. The monetary policy shock is $\varepsilon_{FF,t}$. Second, the structural system of nonpolicy equations can be written as

$$\begin{bmatrix} b_{11} & 0 & 0 \\ b_{21} & b_{22} & 0 \\ b_{31} & b_{32} & b_{33} \end{bmatrix} \begin{bmatrix} Z_{1t}^* \\ PCOM_t \\ Z_{2t}^* \end{bmatrix} = \begin{bmatrix} B_{11}(L) & B_{12}(L) & B_{13}(L) \\ B_{21}(L) & B_{22}(L) & B_{23}(L) \\ B_{31}(L) & B_{32}(L) & B_{33}(L) \end{bmatrix} \begin{bmatrix} Z_{1t-1}^* \\ PCOM_{t-1} \\ Z_{2t-1}^* \end{bmatrix} + \begin{bmatrix} 0 \\ 0 \\ b_{3,FF} \end{bmatrix} FF_t + \begin{bmatrix} B_{1FF}(L) \\ B_{2FF}(L) \\ B_{3FF}(L) \end{bmatrix} FF_{t-1} + \begin{bmatrix} \varepsilon_{1t}^* \\ \varepsilon_{PCOM,t}^* \\ \varepsilon_{2t}^* \end{bmatrix} \tag{14}$$

where $Z_{1t}^* = \begin{bmatrix} Y_t^* & P_t^* \end{bmatrix}'$, a 2×1 vector of latent variables; $PCOM$ is a scalar, observable basket of market prices that does not get revised; and $Z_{2t}^* = \begin{bmatrix} NBR_t^* & TR_t^* \end{bmatrix}'$ is a 2×1 vector of latent variables. Third, as the latent variables are not observed by the econometrician, the latest vintage data is used in estimation, Z_{1t}^{98} and Z_{2t}^{98}. As the discussion in Section 4 developed, for our sample period ending in 1991:3, the 1995 vintage data are valid instruments for overcoming the measurement errors in the 1998 vintage data in estimating equation (14). For the variable $PCOM$, no instruments are required. Since we assume that economic agents observe each of the latent variables in the economy and the observable federal funds rate, $PCOM$ is not contaminated by measurement errors nor is it influenced directly by measurement errors. However, since estimating the $PCOM$ equation involves the use of Z_{1t}^{98} and Z_{2t}^{98} in place of the true, latent variables Z_{1t}^* and Z_{2t}^*, Z_{1t}^{95} and Z_{2t}^{95} are used as instruments in the $PCOM$ equation.[11] Finally, b_{11} and b_{33} are lower triangular, so the order of orthogonalization for studying nonpolicy shocks is

[11]We have also estimated by OLS the $PCOM$ equation using real-time data. That is, treating $PCOM$ as a function of measured output rather than latent output, in the same way that the FF equation is estimated. Those results are similar to the ones reported below.

$Y, P, PCOM, FF, NBR, TR$. Given consistent estimates of the coefficients of equations (14), two measures of each nonpolicy structural shock can be obtained by using 1995 and 1998 vintage data. The 1998 measure is immediately obtained from the estimation results, but the 1995 measure is constructed by replacing the 1998 vintage data with the 1995 vintage data using the same consistent coefficient estimates. The variance of the structural shocks can be estimated as the covariance between the 1998 and 1995 shock measures, since the measurement error terms are independent.

Figure 5 displays the estimated FF policy shock using real-time data and an FF policy shock estimated from the fixed 1998:3-vintage data set (as a typical VAR is estimated). Unlike Figure 1 which displayed centered, moving-average errors, Figure 5 displays the actual, quarterly FF policy shocks in order to highlight the quarter-to-quarter comparisons. Overall, the series are remarkably similar. The correlation over the full-sample period is 0.88, and 0.72 over the more recent period 1987-91. The standard deviations of the two shocks are 77 and 89 basis points for the real-time and 1998:3-vintage data measures, respectively. Nevertheless, there are some notable differences. First, in the third and fourth quarters of 1974, the 1998:3-vintage data overstates the volatility of exogenous monetary policy, relative to the VAR based on data available to policymakers. Romer and Romer (1989) selected April 1974 as a date when the Federal Reserve explicitly chose to sacrifice output in order to reduce an exogenous burst of inflation. The real-time VAR residuals indicate that this was a period when the FOMC was responding in a rather typical fashion to the data they were given. Second, the three large contractionary shocks in 1980:4, 1981:2, and 1982:1 are overstated in the 1998:3 vintage data by 110, 60, and 70 basis points, respectively, when compared with the VAR based on data the FOMC had access to. Third, the two series appear to become less contemporaneously aligned since the stock market decline in 1987. The real-time exogenous tightening in 1988 leads the 1998:3 vintage by a quarter throughout the year, and the subsequent exogenous easing through 1989 is similarly misaligned. In spite of this, the general assessment of exogenous monetary policy as being tight or loose over the course of a four quarter period will not differ appreciably across these two measures.[12]

Figure 6 displays the impulse responses from the FF shock for the estimated real-time system (with its 95 percentile confidence bands as short-dashed lines) and the 1998:3 vintage estimates (without confidence bands).[13] The similarity across the two VAR systems

[12] Rudebusch (1998) finds that FF residuals from a monthly VAR are quite different from forecast errors inferred from the Federal funds futures market over the period 1989-1996. In addition to the difference in information sets across the two analyses, futures market participants do not necessarily presume that monetary policy follows a linear, time-invariant feedback rule. Consequently, Rudebusch's (1998) evidence from futures market data cannot isolate the effect of real-time data for VAR policy shock measures.

[13] Bootstrap confidence bands are constructed in a straightforward manner. The data-generating process is taken to be the VAR estimates of the nonpolicy equation (14), the FF monetary policy rule (13), the error variances estimated from the h_t^{t+s} data according to equations (11) and (12), and the variances η_t^{98} and η_t^{95} for each macroeconomic variable that is subject to data revisions based upon equation (10). In order to minimize the influence of different trends in vintage data due to changes in concept definitions, we estimated the η_t^{98} and η_t^{95} variances using only the last six years of data. In each case, we assume that $Var(\eta_t^{98}) = Var(\eta_t^{95})$, and estimate $Var(\eta_t^{98}) = \frac{1}{2}Var(Y_t^{98} - Y_t^{95})$. For each Monte Carlo draw, serially independent errors are drawn according to the DGP and simulated data series are constructed. The system of equations is estimated and impulse response functions are computed. The 95 percent confidence bands are 95 percentile bands from 500 Monte Carlo draws. The point estimates

is striking. Relative to the 1998:3 vintage estimates, the real-time FF response displays slightly less persistence. The real-time output price and commodity price responses are a bit shallower than the revised data estimates. To informally assess the uncertainty surrounding these point estimates, there are two obvious metrics. First, the reported bootstrap confidence bands around the real-time impulse responses typically cover the response paths around the 1998 vintage estimates. Exceptions to this are the responses of Y^*, P^*, and $PCOM^*$ after the first year, and the first year responses of FF. The percentile bands are somewhat wider than Christiano, Eichenbaum, and Evans (1999, Figure 2) report in a similar system for quarterly data. Nevertheless, inference about the effects of a monetary policy shock is largely unaffected because the qualitative responses are very similar. A contractionary policy shock reduces Y^* and $PCOM^*$ significantly in the first two years, while P^* most likely falls (but with very little precision in the estimates). The liquidity effect on impact is significantly different from zero, but its persistence is a bit less than CEE find in their analysis, which ignores data revisions. A second metric for assessing uncertainty focuses on the 1998 vintage estimates. Unreported 95 percentile bands around the 1998 estimates cover the real-time impulse responses. Taking account of this joint uncertainty, the real-time and 1998 vintage analyses appear to be similar.[14]

Figure 7 displays the system's response from an exogenous shock to true output Y_t^*. Except for the commodity price and deflator paths after 3 years, the responses are quite similar across estimation methods. The 95 percentile bands cover the 1998 vintage responses in most cases. The largest discrepancies involve the responses of P^* and $PCOM^*$ in Figure 7. The real-time estimates seem to be estimating a trending response from Y^* shocks to prices, while the 1998 vintage estimates revert to a stationary path. This could be the case if the real-time data estimation is more closely estimating a unit root for the price variables, than for the revised vintage data. Not surprisingly, the error bands offer no persuasive evidence on the significance of these long-horizon responses. Similar observations apply for the responses to P^* and $PCOM^*$ shocks which are not displayed. For the CEE recursive identification of monetary policy shocks and other orthogonalized shocks, there is little evidence that using real-time data in the estimation alters the literature's conclusions about the effects of monetary policy shocks on the U.S. economy during this period.

6. Galí empirical results

Real-time data issues can pose daunting identification issues for nonrecursive systems of simultaneous equations. Although Galí's (1992) assumptions are sufficient to identify four economic shocks when real-time data issues are ignored, vintage measurement issues defeat Galí's identification of all but the long-run supply shock. Simultaneity from financial market data generally creates identification difficulties unless further restrictive assumptions are made about the time series properties of measurements.

of the responses correspond to the real-time estimates.

[14]Revisions to NBR and TR are relatively small, and a referee has suggested investigating the implications of assuming that Fed policymakers know their true values. To consider this case, we take the 1998 vintage data for NBR and TR as final, and use this in the FF equation. In addition, as we do for $PCOM$, we treat Z_{2t} as observable in equation (14). The estimated impulse responses are qualitatively unchanged from our main results.

Simultaneity creates identification problems

The essence of the real-time data problem comes from the contemporaneous correlation between real allocations and financial market data that respond to measurement errors. Although the four-variable Galí (1992) system shares these problems, the previous example is simpler and can be augmented to reveal the problem:

$$FF_t = A_{FF}(L)FF_{t-1} + A_Y(L)Y^t_{t-1} + a_Y Y^t_t + \varepsilon_{1t} \tag{15}$$
$$Y^*_t = B_{FF}(L)FF_{t-1} + B_Y(L)Y^*_{t-1} + a_{FF}FF_t + \varepsilon_{2t} \tag{16}$$
$$Y^t_t = Y^*_t + g^*_t \tag{17}$$

Nonzero values for a_Y and a_{FF} imply a nonrecursive system — that is, a fully simultaneous system. OLS estimation of the policy equation using the real-time data is not consistent due to $E\left[Y^t_t\ \varepsilon_{1t}\right] \neq 0$: Y^t_t is correlated with ε_{1t} via its dependence on FF_t ($a_{FF} \neq 0$). Similarly, simultaneity causes $E\left[FF_t\ \varepsilon_{2t}\right] \neq 0$, which cannot be overcome by using the 1995 and 1998 vintage data: FF_t is correlated with ε_{2t} via its dependence on Y^t_t and Y^*_t ($a_Y \neq 0$). Therefore, the structural equations cannot be estimated directly, the way they could with the recursively identified system $(4, 5)$.

In addition, any reduced-form VAR representation for the two variables FF and Y^* will involve three shocks[15]: ε_{1t}, ε_{2t}, and g^*_t. Identifying the two economic shocks ε_{1t} and ε_{2t} from only two data series FF and Y^* is not possible without placing additional structure on the g^*_t process.[16] Consequently, the presence of an unmeasurable wedge h^*_t defeats identification of a system that would otherwise be identified in the absence of these real-time issues.

Identification of the Galí supply shock

Interestingly, the problems of simultaneity with real-time data do not defeat identification through the long-run restrictions in the Galí system. The structural equation for output growth can be written as

$$\Delta Y^*_t = A_{11}(L)\Delta Y^*_{t-1} + A_{12}(L)\Delta FF_t + A_{13}(L)\Delta\frac{M^*_t}{P^*_t} + A_{14}(L)\left(FF_t - \Delta P^*_t\right) + \varepsilon^{supply}_t \tag{18}$$

Simultaneity and the contemporaneous values of ΔFF_t, $\Delta\frac{M^*_t}{P^*_t}$, and $(FF_t - \Delta P^*_t)$ defeat the consistency of OLS estimation. But the long-run restriction that only the supply shock can permanently affect Y^*_t implies a root on the unit circle in the polynomials $A_{12}(z)$, $A_{13}(z)$, and $A_{14}(z)$, where z is a complex variable. This leads to an instrumental variables estimator using the differences of ΔFF_t, $\Delta\frac{M^*_t}{P^*_t}$, and $(FF_t - \Delta P^*_t)$. Although the latent variables are not observable, the 1998 and 1995 vintage estimation can be used to identify ε^{supply}_t.

---

[15] Assuming Y^* is an observable series is merely a simplifying device. Allowing for the latent nature of Y^*, the 1998 and 1995 vintage analysis can make these statements precise at the cost of additional notation.

[16] For example, rich patterns of serial correlation in g^*_t may identify the serially uncorrelated ε_{1t} and ε_{2t} apart from g^*_t.

Figure 8 displays the impulse responses from the estimated real-time system following a supply shock, as well as a 1998 vintage impulse response. Ninety-five percent error bands from bootstrap Monte Carlo simulations are displayed for the real-time system.[17] Although the error bands are quite large, the impulse responses for the real-time and 1998 vintage estimates are quite similar. An expansionary long-run supply shock increases Y^* substantially after two or three quarters. Only the output response is estimated with any reasonable precision. The error bands for the other responses cover zero throughout. Nevertheless, the point estimate of the price level response P^* falls, and the monetary policy instrument FF is estimated to fall initially in response to the lower inflationary pressures ΔP^*. The systematic response of monetary policy modestly constrains real activity following a technology shock: the real interest rate is primarily positive after two quarters. The rise in $M1$ is plausibly an endogenous response to the increase in output, in which case the real interest rate rise prevents a larger increase in money. Most importantly, apart from adding a good deal of additional uncertainty from the wider error bands, the real-time system is not very different from a 1998 vintage estimate.

In spite of the similarity between these responses, the lack of identification for a monetary policy shock, *IS* shock or money demand shock in a real-time data system stands in sharp contrast to the typical identification which ignores real-time data issues. To assess whether those identifications are robust to real-time data revisions requires placing more structure on the measurements. That is a subject for further research.

7. Comparison to other real-time data literature

Our estimated impulse responses following a monetary policy shock display broad robustness to allowing for data revisions in real-time monetary policy reaction functions. This feature is clear from Figures 2 and 4 which display alternative data vintage estimates of impulse responses following CEE and Galí monetary policy shocks (Section 3). In contrast with other real-time data empirical studies, a key feature of our approach in Section 6 is to recognize that data revisions are never "final" and to apply an econometric approach that recognizes this continuing uncertainty about the "true" values of key macroeconomic variables. While the Galí monetary policy shock cannot be separately identified from the measurement noises in these real-time data circumstances, the identified CEE monetary policy response is broadly similar across estimation strategies. Overall, our findings strongly suggest that the evidence on the monetary transmission mechanism from the VAR literature (as surveyed by Christiano, Eichenbaum, and Evans (1999)) continues to be valid in the presence of real-time data issues.

Much of the recent literature on real-time monetary policy has focused on the robust-

[17]Since the full vector of economic shocks is not identified, the procedure described in footnote 13 must be modified. First, the data generating process for measured output is taken to be the 1998 vintage equation. Simulations of Y_t^{1998} can be drawn. Second, to generate simulated data for Y_t^{1995} that respect the implicit Y_t^* simulations within the Y_t^{1998} draws, draw normally-distributed error terms for $Y_t^{1995} - Y_t^{1998}$ and add to the Y_t^{1998} simulations. Third, real-time data Y_t^t simulations are constructed in a similar fashion. Draw normally-distributed error terms for $Y_t^t - Y_t^{1998}$ and add to the Y_t^{1998} simulations. With Y_t^t simulations, the data revisions Y_t^{t+s} are constructed as before. This algorithm is repeated for the other data series. Given simulated real-time and vintage data, the impulse response can be computed for each Monte Carlo draw.

ness of monetary policy rules explicitly, and somewhat less on the monetary transmission mechanism. Orphanides (2001) shows how Taylor's (1993) original findings are very sensitive to his use of revised data, and that the Federal Reserve's information set, gleaned from actual Greenbook data, would have pointed to different policy prescriptions. Bernanke and Boivin (2003) find less sensitivity when they estimate forward-looking Taylor rules using Stock and Watson (2002) factors constructed from a real-time data set and its most recent "final data" counterpart. They find no appreciable differences between the real-time and final data sets for forecasting, and hence Taylor rule prescriptions. Bernanke and Boivin's focus on large data sets is somewhat more like our VAR analyses, as compared with simple Taylor rule studies where the real-time assessment of the output gap is the crucial element. In terms of the monetary transmission mechanism, Orphanides (2003a, b) and Rudebusch (2001) study a host of information difficulties facing the Federal Open Market Committee in setting monetary policy, and provide many insights on how the federal funds rate and the macroeconomy would have behaved differently in those cases. Using a small set of macroeconomic equations for theoretical tractability, these later papers provide evidence on alternative operating characteristics of the U.S. economy under these information constraints. Although the monetary transmission mechanism is embedded in the summary statistics of Orphanides and Rudebusch's simulations, they do not display alternative responses of macroeconomic variables to identified exogenous economic shocks. Another feature distinguishing our analysis from theirs is the relative treatment of "final, revised" data. Although Orphanides and Rudebusch study cases where the monetary authority is faced with measurement errors in the real-time data, both authors at some point assume that the true data is revealed to the econometrician in the latest vintage of "final, revised" data. Although this is a convenient simplifying assumption, their empirical analysis could well change with revisions to these "final data" through later vintages of data. Our analysis of Section 5 explicitly accounts for the inevitability that our most recent data will be revised in the future, and consequently provides critical new evidence of robustness.

Prior to our study, Croushore and Stark used real-time data to investigate estimated VAR impulse responses. Croushore and Stark (2003) used different vintages of GDP data to assess the robustness of Blanchard and Quah's (1989) identification of supply and demand shocks. They found that the estimated supply shock responses were very similar across data vintages; however, the demand shock impulse response functions varied substantially. Croushore and Stark's robust supply shock responses accord well with our empirical estimates showing that the Galí (1992) supply shock responses are similar when allowing for real-time data issues or when ignoring those issues (Section 6). In comparing our results for demand shocks in a VAR with long-run restrictions, our real-time analysis of Galí's non-supply shocks in Section 6 indicated that we could not identify these economic shocks from residual data revision noises. However, our analysis of different vintage estimates of Galí monetary policy shocks in Section 3 found robust results, seemingly in constrast to Croushore and Stark for the Blanchard and Quah demand shock. Unfortunately, there cannot be a clean comparison of the Galí monetary policy shock to the Blanchard and Quah demand shock. Presumably, the Blanchard and Quah demand shock represents an amalgam of monetary policy shocks as well as money demand, fiscal policy, and a large variety of general aggregate demand shocks. Interestingly, Croushore and Stark trace this lack of robustness across data vintages to varying degrees of weak instrument problems associated with the long-run identification of the demand shock. This is a vexing problem for comparing empirical analyses over different periods of time, even when the sample periods are identical. One approach is to check for the validity of the instruments at each stage of the analysis and estimation, as Croushore and Stark do. While this is an increasingly standard approach in careful empirical studies, potential difficulties may remain. For example, the initial analysis may find a valid set of instruments; but five or ten years later, the data agency may change to reporting new vintage concepts of GDP that render the identification weak. Following the approach of our paper, accounting more explicitly for the latent structure of macroeconomic data that get revised may offer a more promising and robust analysis of these real-time data difficulties. After all, one source of these weak identifications may be the residual data revision noises discussed in Section 6. Further research is necessary to document the general shape of robustness across different identification strategies.

8. Conclusions

Empirical VAR and time series research often ignores issues associated with data revisions and economic agents' access to only real-time data releases. Since government agencies and private sources do not provide these data conveniently, these shortcuts are rarely questioned. The real-time data collected by Croushore and Stark (2001) allows researchers to explore the empirical robustness of many existing macroeconomic results to this issue, but additional structure must be placed on the data revision process and assumptions regarding the information that economic agents have access to. Our empirical analyses indicate that accounting for data revisions has only a modest effect quantitatively on the recursively identified monetary policy shock measures and impulse responses we consider. Similarly robust findings were obtained for a particular long-run identification. All of these results are conditional on our assumptions about data revisions and the latent structure of the economy. A negative finding of this analysis revealed that many fully-simultaneous VAR systems that are identified when real-time data issues are ignored are actually not completely identified when vintage measurement issues are considered. Further research that allows for alternative measurement noise and data revision processes is needed to shed more light on the role of data revisions.

REFERENCES

1. Bernanke, B. and A. Blinder, 1992, The federal funds rate and the channels of monetary transmission, American Economic Review 82, 901-921.
2. Bernanke, B. and J. Boivin, 2003, Monetary policy in a data-rich environment, Journal of Monetary Economics 50, 525–546.
3. Bernanke, B. and I. Mihov, 1998, Measuring monetary policy, Quarterly Journal of Economics 113, 869-902.
4. Blanchard, O.J. and D. Quah, 1989, The dynamic effects of aggregate demand and supply disturbances, American Economic Review 79, 655-673.
5. Christiano, L., M. Eichenbaum and C.L. Evans, 1996, The effects of monetary policy shocks: evidence from the flow of funds, Review of Economics and Statistics 78, 16-34.

6. Christiano, L., M. Eichenbaum and C.L. Evans, 1999, Monetary policy shocks: what have we learned and to what end? in: J. Taylor and M. Woodford, eds., Handbook of macroeconomics, Vol. 1A (Elsevier, Amsterdam).
7. Croushore, D. and T. Stark, 2001, A real-time data set for macroeconomists, Journal of Econometrics 105, 111-130.
8. Croushore, D. and T. Stark, 2003, A real-time data set for macroeconomists: does the data vintage matter? Review of Economics and Statistics 85, 605–617.
9. Diebold, F.X. and G.D. Rudebusch, 1991, Forecasting output with the composite leading index: a real-time analysis, Journal of the American Statistical Association 86, 603-610.
10. Galí, J., 1992, How well does the IS-LM model fit postwar U.S. data? Quarterly Journal of Economics 107, 709-38.
11. Hall, R.E., 1978, Stochastic implications of the life cycle-permanent income hypothesis: theory and evidence, Journal of Political Economy 86, 971-987.
12. Leeper, E, C. Sims and T. Zha, 1996, What does monetary policy do? Brookings Papers on Economic Activity (2), 1-78.
13. Mankiw, N.G., D.E. Runkle, and M.D. Shapiro, 1984, Are preliminary announcements of the money stock rational forecasts? Journal of Monetary Economics 14, 15-27.
14. Mankiw, N.G. and M.D. Shapiro, 1986, News or noise: an analysis of GNP revisions, Survey of Current Business (May), 20-25.
15. Orphanides, A., 2001, Monetary policy rules based on real-time data, American Economic Review 91, 964–985.
16. Orphanides, A., 2003a, Monetary policy evaluation with noisy information, Journal of Monetary Economics 50, 605-631.
17. Orphanides, A., 2003b, The quest for prosperity without inflation, Journal of Monetary Economics 50, 633–663.
18. Romer, C.D. and D.H. Romer, 1989, Does monetary policy matter? A new test in the spirit of Friedman and Schwartz, in: O.J. Blanchard and S. Fischer, eds., NBER Macroeconomics Annual 1989 (Cambridge, Massachusetts, MIT Press) 121-170.
19. Rudebusch, G.D., 1998, Do measures of monetary policy in a VAR make sense? International Economic Review 39, 907-931.
20. Rudebusch, G.D., 2001, Is the Fed too timid? Monetary policy in an uncertain world, Review of Economics and Statistics 83, 203-217.
21. Sargent, T., 1989, Two models of measurements and the investment accelerator, Journal of Political Economy 97, 251-287.
22. Sargent, T., and C. Sims, 1977, Business cycle modeling without pretending to have too much *a priori* economic theory, in: C. Sims, ed., New Methods in Business Cycle Research (Minneapolis, Federal Reserve Bank of Minneapolis).
23. Sims, C., 1992, Interpreting the macroeconomic time series facts: the effects of monetary policy, European Economic Review 36, 975-1011.
24. Sims, C. and T. Zha, 1996, Does monetary policy generate recessions? unpublished manuscript.
25. Stock, J. and M. Watson, 1989, New indexes of coincident and leading economic indicators, in: O.J. Blanchard and S. Fischer, eds., NBER Macroeconomics Annual 1989 (Cambridge, Massachusetts, MIT Press) 351-394.

26. Stock, J. and M. Watson, 2002, Macroeconomic forecasting using diffusion indexes, Journal of Business and Economic Statistics 20, 147-162.
27. Taylor, J., 1993, Discretion versus policy rules in practice, Carnegie-Rochester Conference Series on Public Policy 39, 195-214.

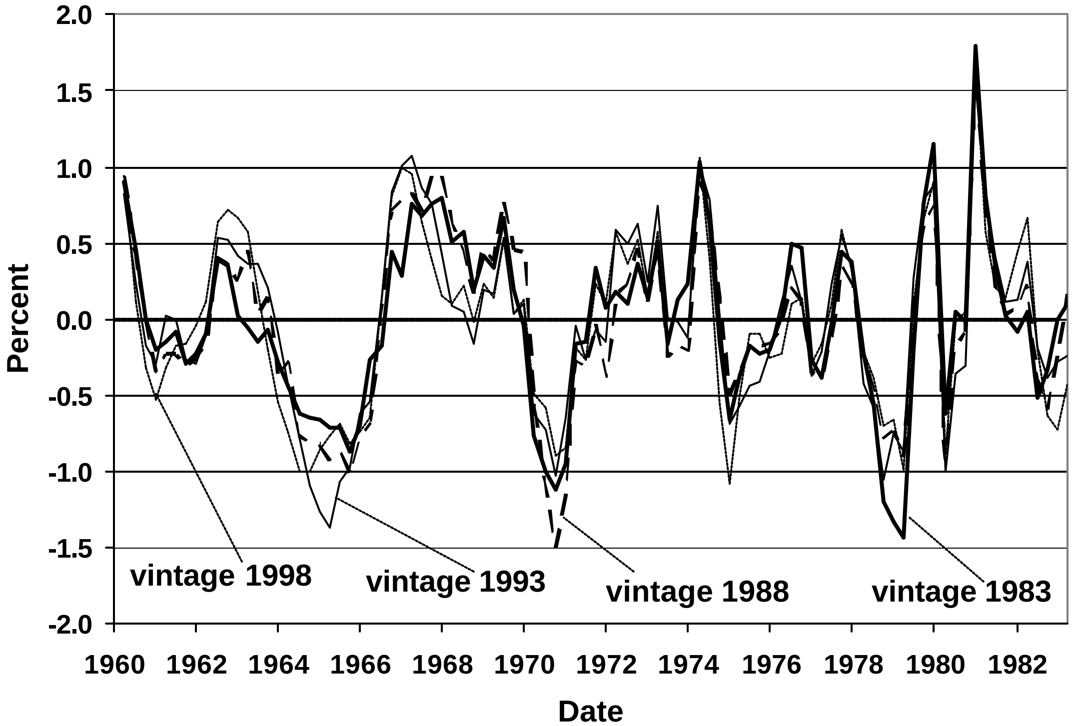

Fig. 1. CEE monetary policy shocks. Centered, three-quarter moving averages estimated from VARs using different data vintages; sample period 1960 to 1983.

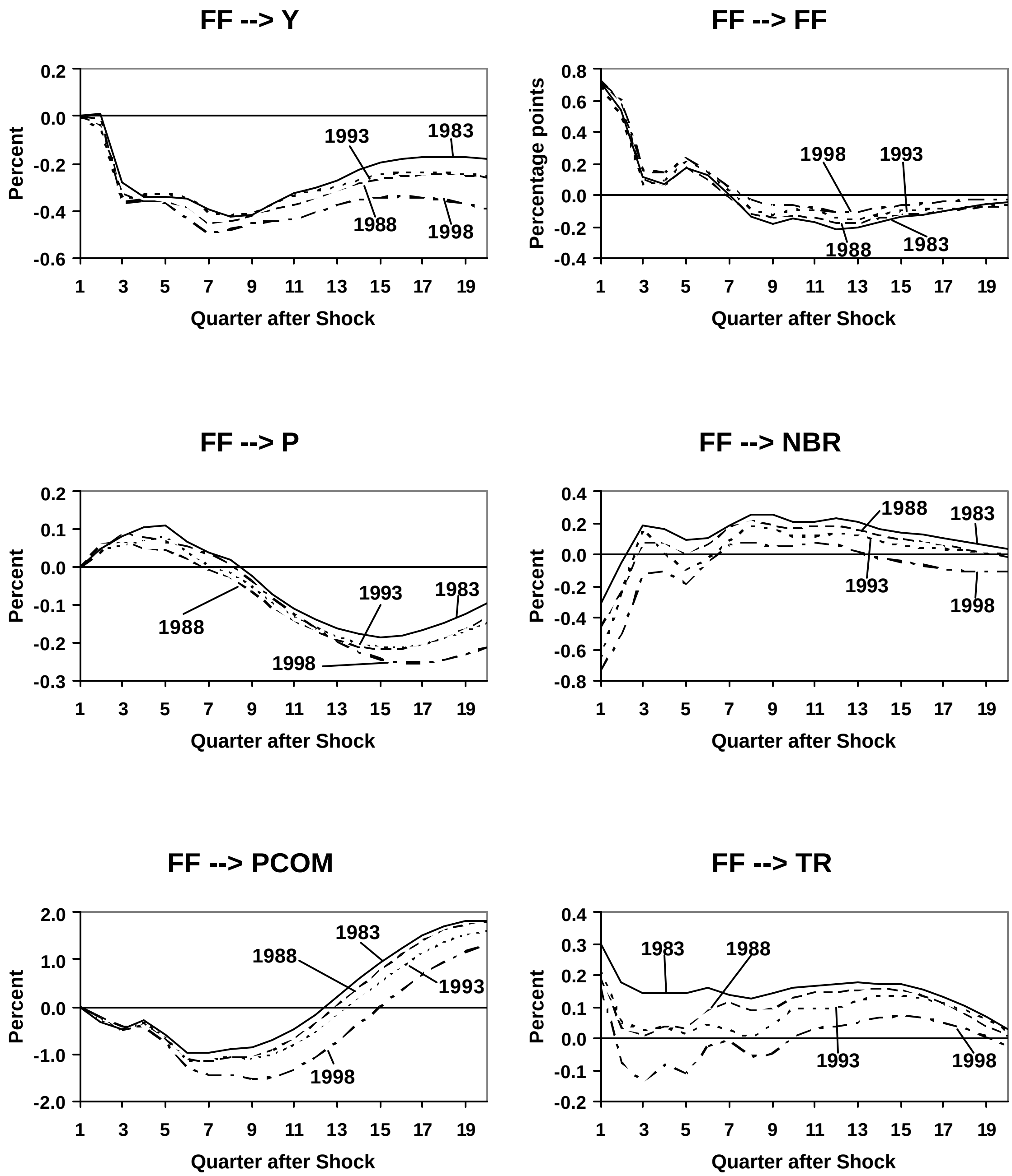

Fig. 2. Impulse responses following a CEE monetary policy shock. Estimated from VARs using different data vintages; sample period 1960 to 1983.

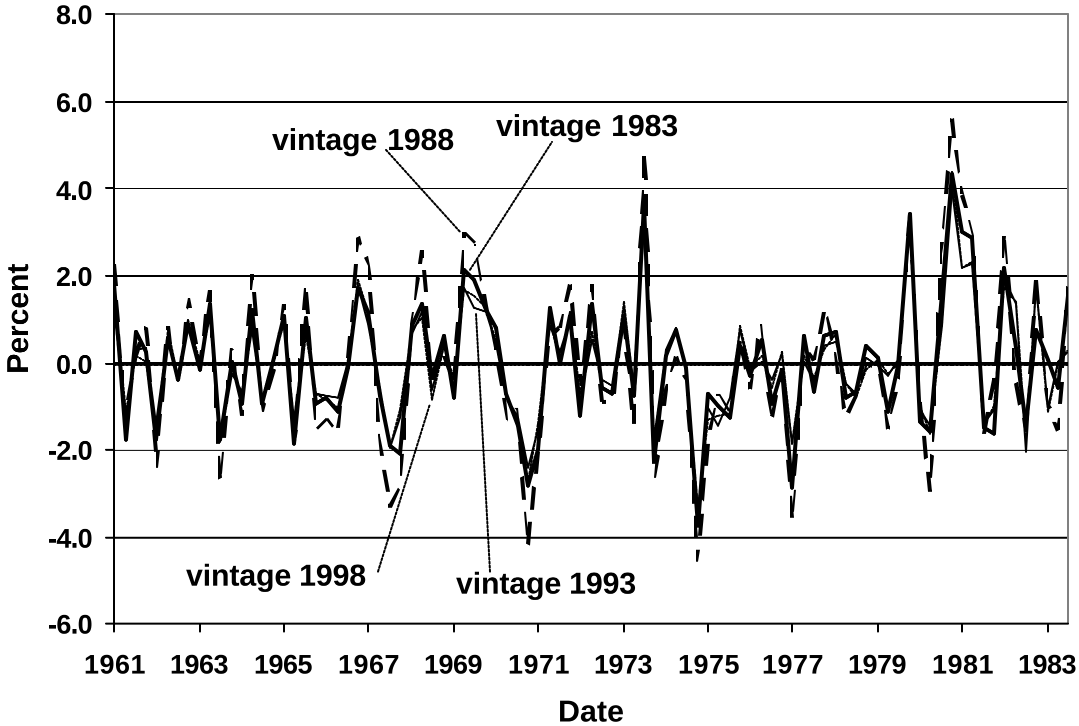

Fig. 3. Galí monetary policy shocks. Estimated from VARs using different data vintages; sample period 1961 to 1983.

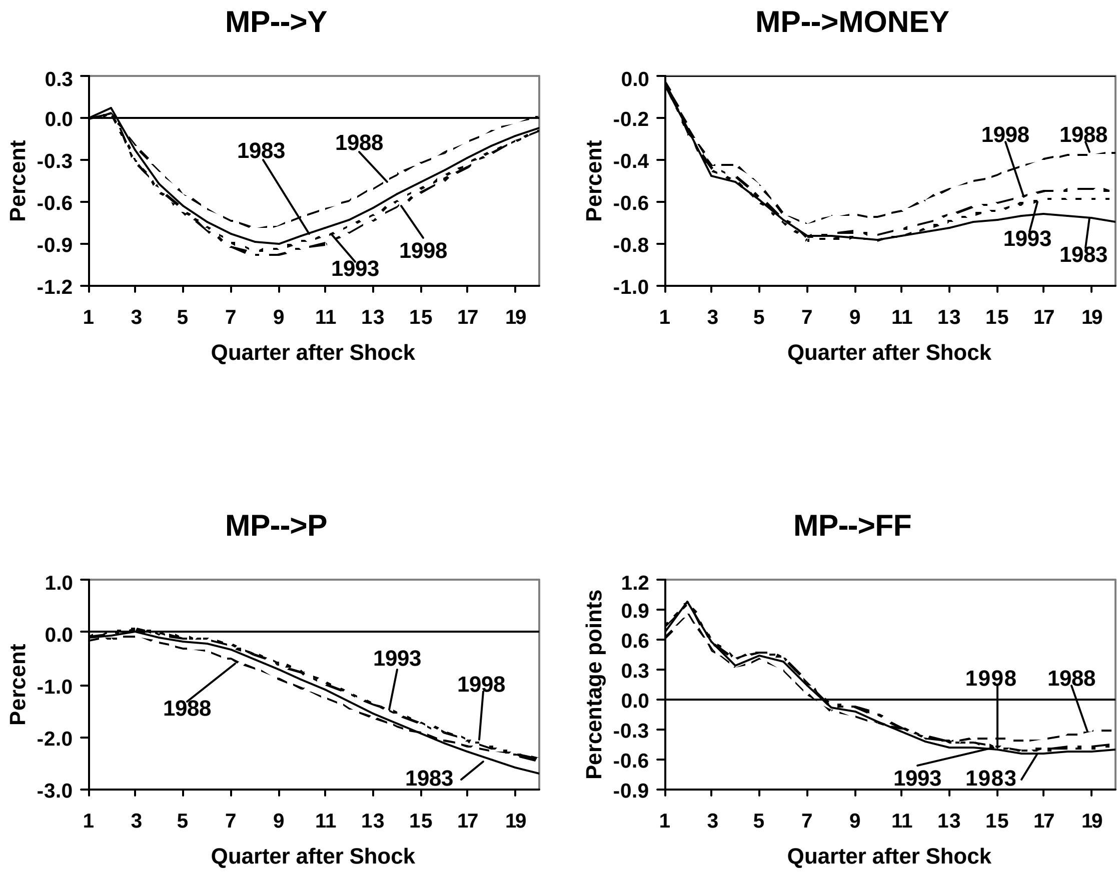

Fig. 4. Impulse responses following a Galí monetary policy shock. Estimated from VARs using different data vintages; sample period 1961 to 1983.

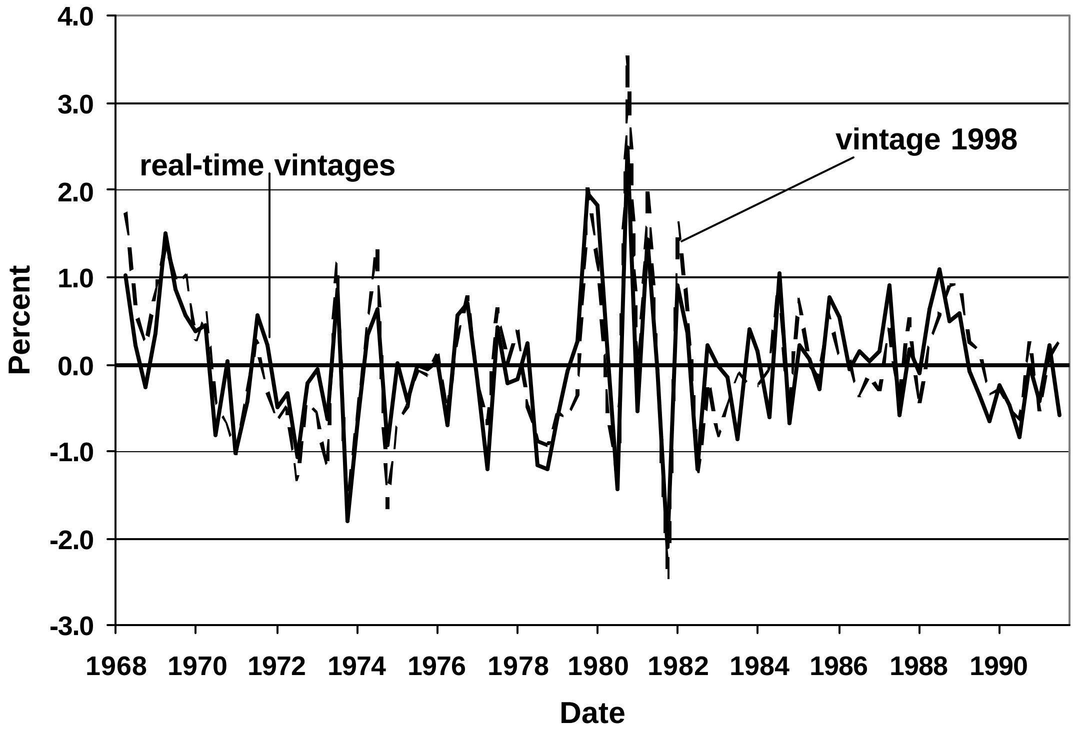

Fig. 5. CEE monetary policy shocks. Estimated from VARs using real-time data and 1998 vintage data; sample period 1968 to 1991.

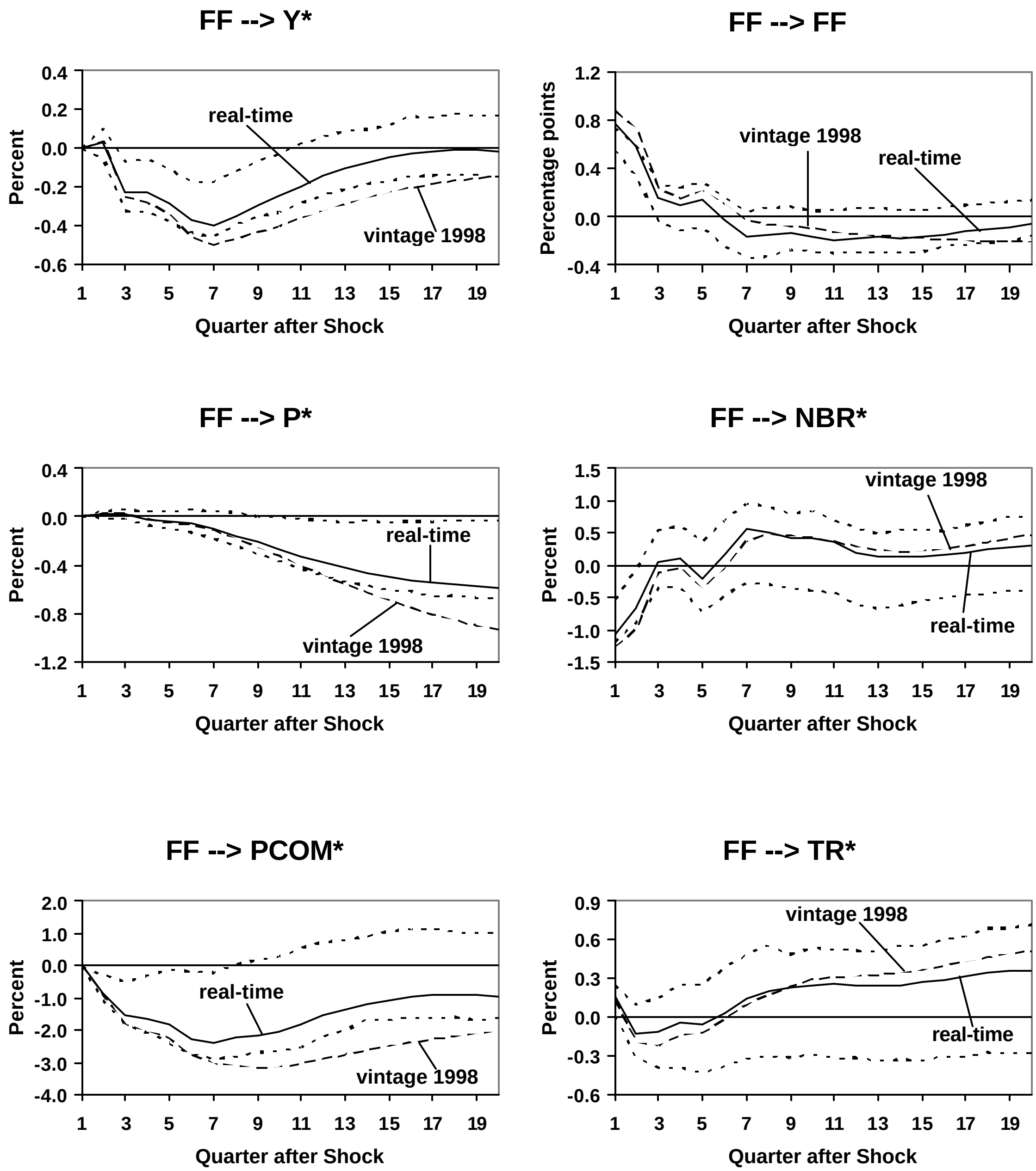

Fig. 6. Impulse responses from a recursively identified CEE monetary policy shock. Estimated from VARs using real-time data and 1998 vintage data; sample period 1968 to 1991. Dotted lines show 95 percentile bands for the real-time responses.

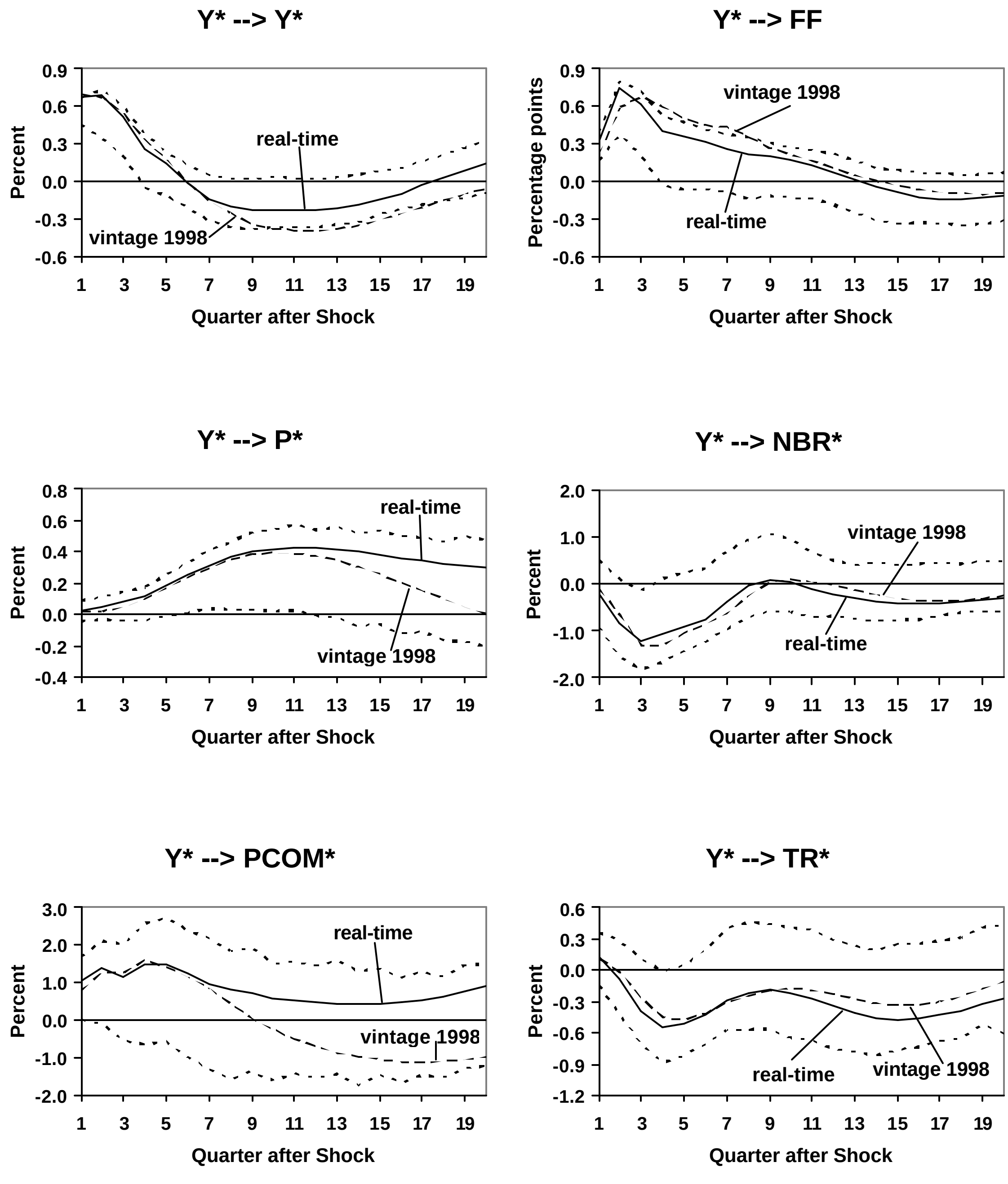

Fig. 7. Impulse responses from a recursively identified CEE output shock. Estimated from VARs using real-time data and 1998 vintage data; sample period 1968 to 1991. Dotted lines show 95 percentile bands for the real-time responses.

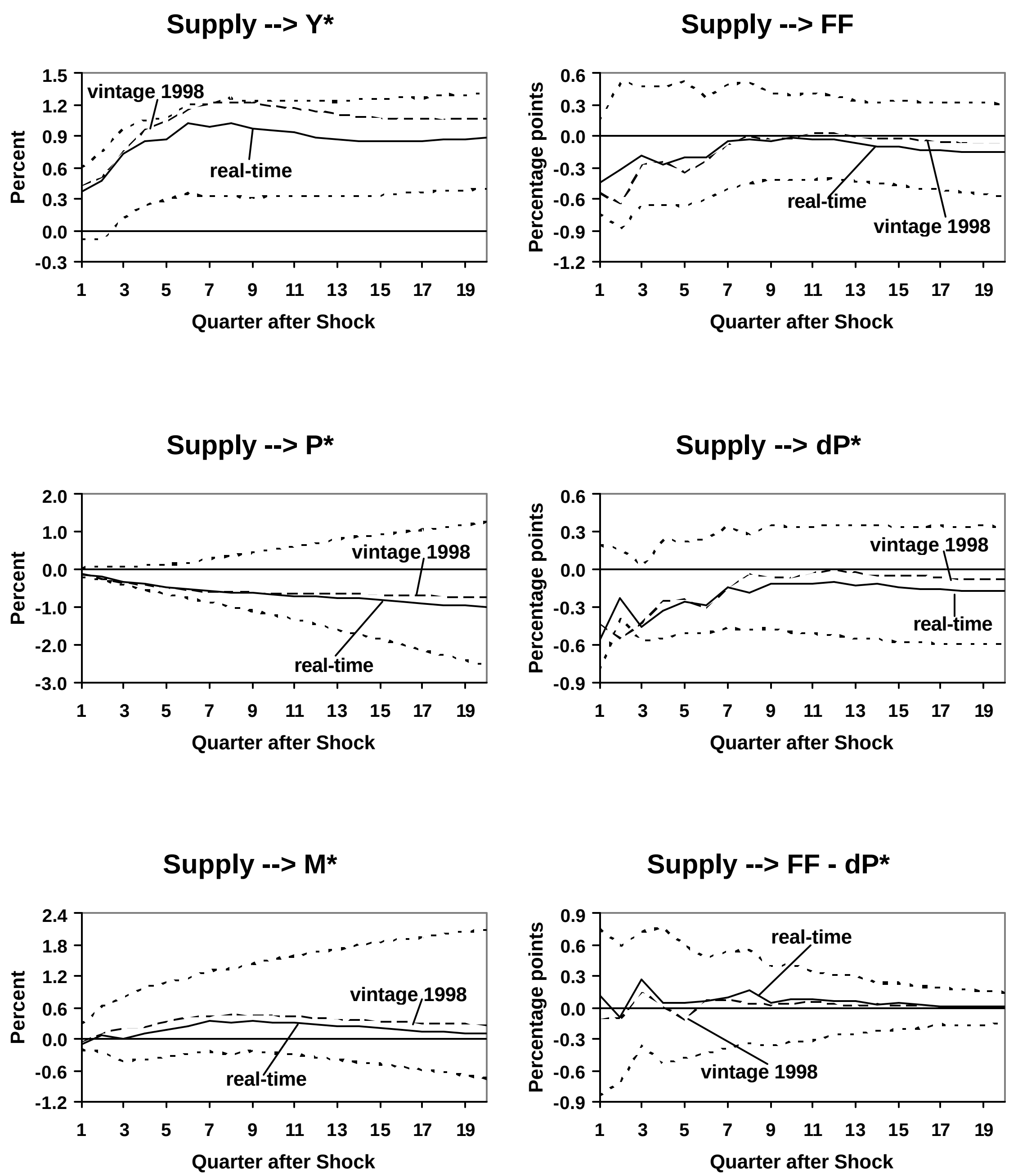

Fig. 8. Impulse responses from a long-run supply shock in the Galí system. Estimated from VARs using real-time data and 1998 vintage data; sample period 1968 to 1991. Dotted lines show 95 percentile bands for the real-time responses.

Table 1: GMM estimates of autocorrelations

Autocorrelation	ΔY^*	ΔP^*	ΔNBR^*	ΔTR^*
ρ_1	.23	.79	.16	.56
	(.08)	(.05)	(.07)	(.10)
ρ_2	.19	.76	-.35	.28
	(.08)	(.06)	(.09)	(.13)
ρ_3	.06	.76	-.10	.18
	(.08)	(.08)	(.05)	(.13)
ρ_4	.18	.68	.10	.10
	(.07)	(.08)	(.06)	(.14)
J-statistic	6.73	2.55	1.90	0.98
$\chi^2(4)$ p-value	(.15)	(.63)	(.75)	(.91)

Table 1
GMM estimates of autocorrelations

Note: Numbers in parentheses under autocorrelation coefficients are standard errors. The J-statistic tests the null hypothesis that the cross-covariances of the growth rates are equal; the associated p-values are shown below the J-statistic in parentheses.

Source: Authors' estimates.