# WORKING PAPERS

## RESEARCH DEPARTMENT

**WORKING PAPER NO. 03-1**
**DATA REVISIONS AND THE IDENTIFICATION**
**OF MONETARY POLICY SHOCKS**

Dean Croushore
Federal Reserve Bank of Philadelphia

Charles L. Evans
Federal Reserve Bank of Chicago

January 2003

# FEDERAL RESERVE BANK OF PHILADELPHIA

# WORKING PAPER NO. 03-1

# DATA REVISIONS AND THE IDENTIFICATION OF MONETARY POLICY SHOCKS

Dean Croushore
Federal Reserve Bank of Philadelphia

Charles L. Evans
Federal Reserve Bank of Chicago

January 2003

# DATA REVISIONS AND THE IDENTIFICATION
# OF MONETARY POLICY SHOCKS

## ABSTRACT

Monetary policy research using time series methods has been criticized for using more information than the Federal Reserve had available in setting policy. To quantify the role of this criticism, we propose a method to estimate a VAR 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 a typically estimated measure. The impulse response functions are broadly similar across the methods. Our evidence suggests that the use of revised data in VAR analyses of monetary policy shocks may not be a serious limitation.

# Data revisions and the identification

# of monetary policy shocks[*]

Dean Croushore[†]         Charles L. Evans[‡]

December 2002

## Abstract

Monetary policy research using time series methods has been criticized for using more information than the Federal Reserve had available in setting policy. To quantify the role of this criticism, we propose a method to estimate a VAR 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 a typically estimated measure. The impulse response functions are broadly similar across the methods. Our evidence suggests that the use of revised data in VAR analyses of monetary policy shocks may not be a serious limitation.

# 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, and 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 conveniently, these shortcuts are rarely even 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.

---

[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 (1998) 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.

1

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 vintages are updated every year? This analysis does not explicitly consider how the data revision process takes place. The 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 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 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 1968-93 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 qualitatitvely 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 these are not affected by the 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 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

$$S_t = f\left(\Omega_t\right) + \varepsilon_t \tag{2.1}$$

where $\Omega_t$ is the Fed's information set at time $t$ and $\varepsilon_t$ 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 (1997); 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 $\Omega_t$, as well as the different functions $f(\cdot)$, and the correlation structure between the exogenous shock $\varepsilon_t$ and the information set $\Omega_t$.

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 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:Q4 had only been revised twice (no benchmark revision), the historical data going back through 1960 had been revised many times. As we noted in the introduction, the empirical VAR literature has neglected the effects of data revisions.[3] This omission is apparent in the monetary policy rule (2.1) since it does not reflect the vintage

---

[3]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 (2002) for examples.

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

$$S_t^T = f\left(\Omega_t^T\right) + \varepsilon_t^T \tag{2.2}$$

One reaction to this criticism is to estimate $f\left(\cdot\right)$ 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 $\Omega_t^T$ for $t < T$ were not known at time $t$. In most cases, the data have been revised;[4] this critique also holds for the approach using $\Omega_t^{T_j}$. Consequently, even with certain knowledge of $f\left(\cdot\right)$, $\varepsilon_t^T$ will differ from the true policy shock (see Rudebusch 1998 and Christiano, Eichenbaum, and Evans 1999). Assuming that the monetary authority uses a time-invariant function $f\left(\cdot\right)$ to set the policy instrument, the reaction function is

$$S_t^t = f\left(\Omega_t^t\right) + \varepsilon_t \tag{2.3}$$

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 ($\Omega_t^T$ versus $\Omega_t^t$) matters for the determination of monetary policy? Research by Croushore and Stark (2001, 2002) 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:Q4 to 1989:Q4 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 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:Q1 was initially released as 5.2 percent; in today's data

---

[4]Almost all seasonally adjusted, macroeconomic time series data are revised for a substantial period of time.

4

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 (2.2) and (2.3)? In general, estimating (2.2) using standard VAR methods will not recover the reaction function and policy shocks in (2.3). Second, how are impulse response functions from policy shocks to other macroeconomic data affected by this conflict? This question is 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.[5] Although monetary policy may plausibly respond to each new data revision as described in (2.3), 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 announced 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 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, in principle.

---

[5]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 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 (2002) 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 rerun the empirical work 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.

## 3.1. Christiano-Eichenbaum-Evans (CEE)

The benchmark CEE quarterly model consists of a recursively identified VAR in six variables: real output ($Y$), deflator ($P$), nonborrowed reserves ($NBR$), federal funds rate ($FF$), total reserves ($TR$), and commodity prices ($PCOM$), where all variables except $FF$ 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 existed 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 period, including such major revisions as switching from GNP to GDP and changing from fixed-weight real output and deflator to chain weighting. 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, 1960Q1 to 1983Q3.

The VAR is estimated and the monetary policy shocks are taken to be the orthogonalized innovations from the federal funds rate equation. Figure 3.1 illustrates the shocks that we estimate, with each of the four different lines corresponding to a different vintage data set.
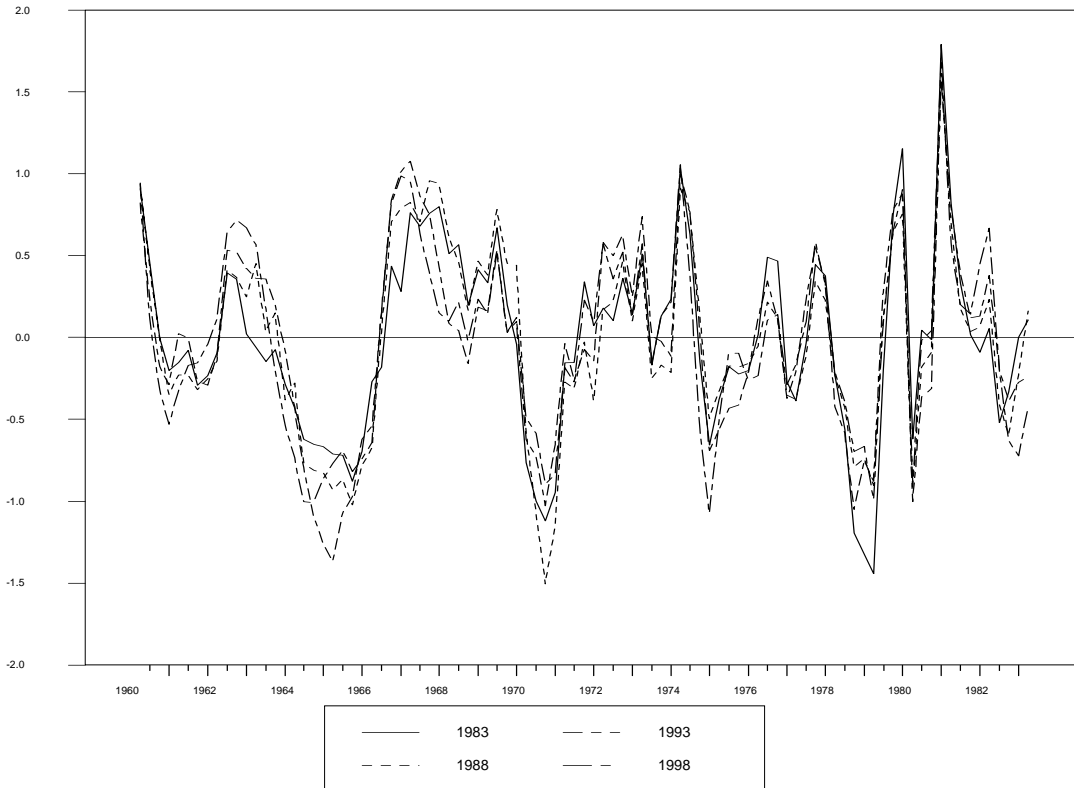
Figure 3.1: CEE Monetary Policy Shocks
The figure displays estimated monetary policy shocks from four VARs using different data vintages:
1983, 1988, 1993, and 1998. Each VAR was estimated over the sample period 1960-1983.

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 3.2) 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 long-run 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 long-run responses (out 20 quarters). Thus, impulse responses are somewhat sensitive to data revisions.
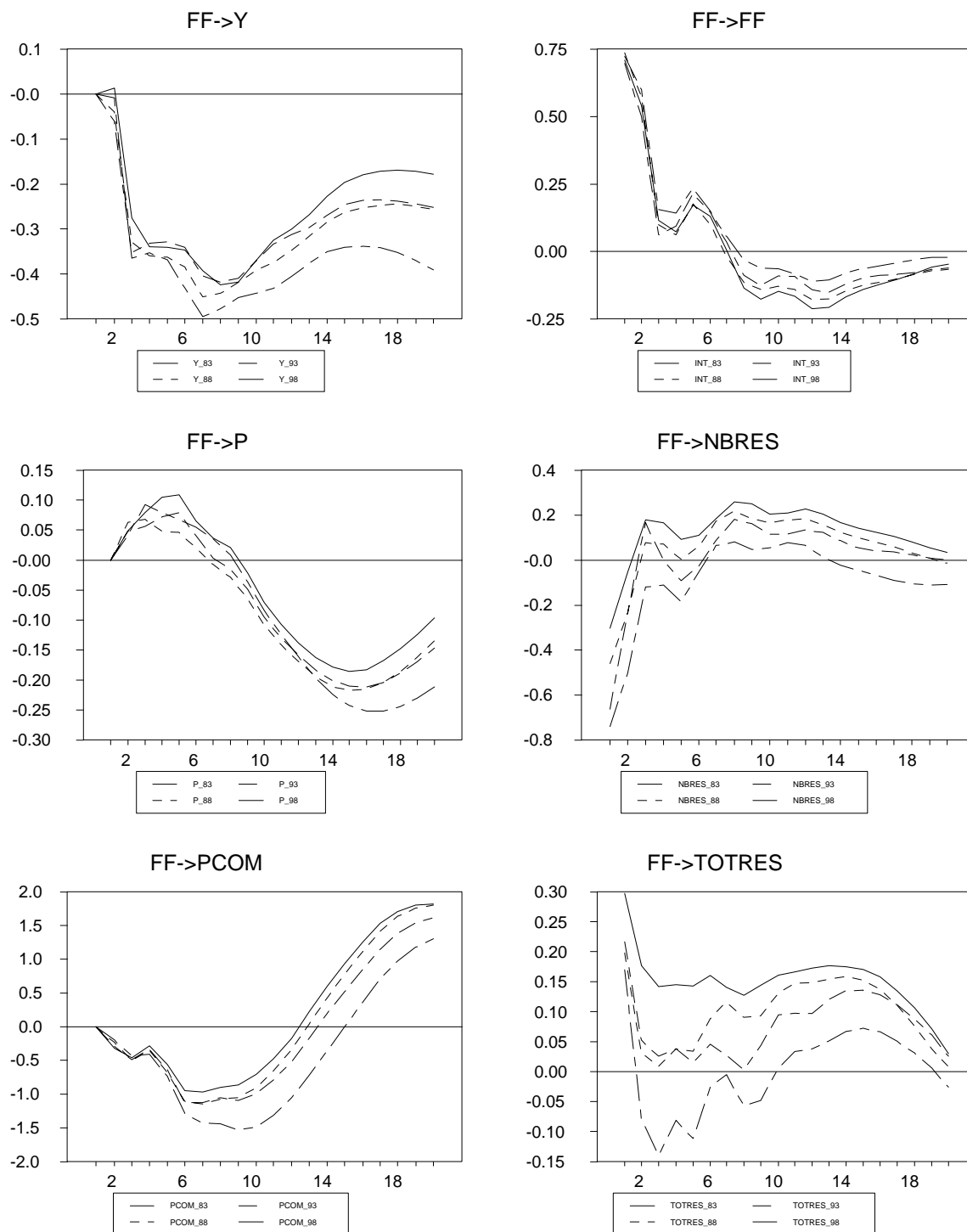
7

Figure 3.2: Impulse Responses Following a CEE Monetary Policy Shock
The figure displays the point estimates from four VARs using different data vintages: 1983, 1988, 1993, and 1998. Each VAR was estimated over the sample period 1960-1983.

8

## 3.2. Galí

The CEE model imposes no economic structure on the VAR beyond the monetary policy reaction function. But much recent empirical work has used economic theory to impose structure on the 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 output (Y), the quarterly change in the interest rate ($\Delta$FF), 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 3.3, are again quite similar across vintages of the 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 3.4, 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.
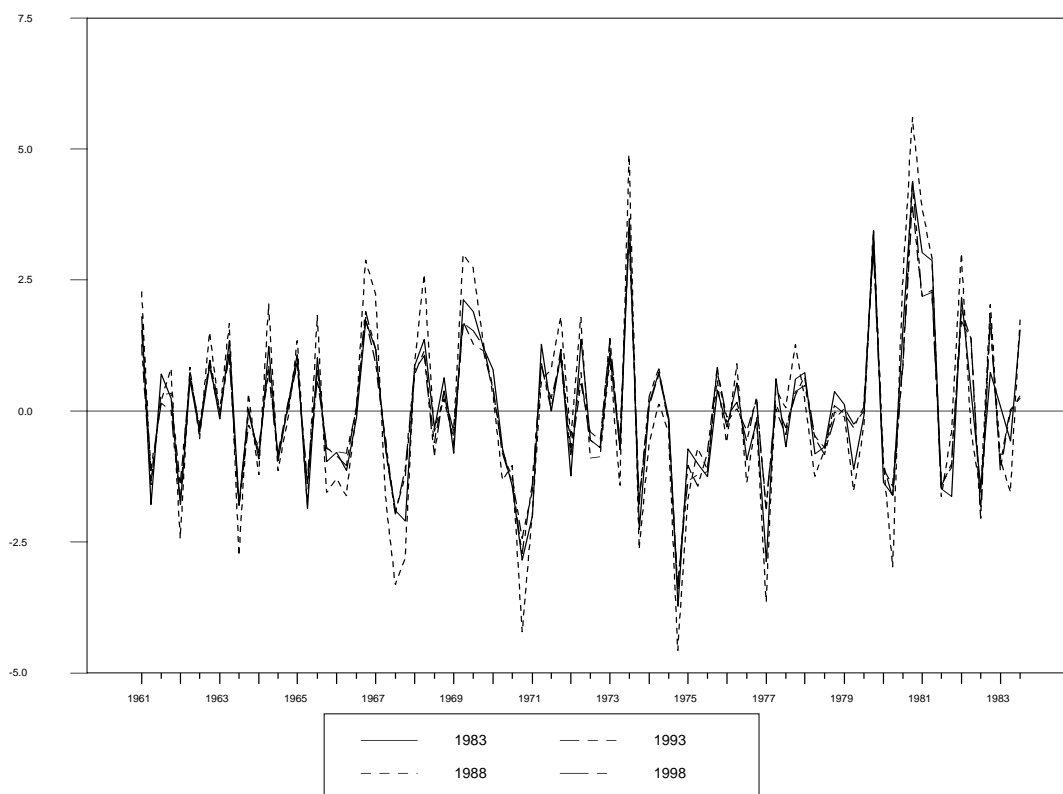
Figure 3.3: Galí Monetary Policy Shocks
The figure displays estimated monetary policy shocks from four VARs using different data vintages: 1983, 1988, 1993, and 1998. Each VAR was estimated over the sample period 1960-1983.

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 might have been in 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. To that end, we now examine the revision process more carefully, and see how VAR estimates may be affected by different types of revisions to the data.

## MP->Y



## MP->MONEY



## MP->P



## MP->FF



Figure 3.4: Impulse Responses following a Gali Monetary Policy Shock
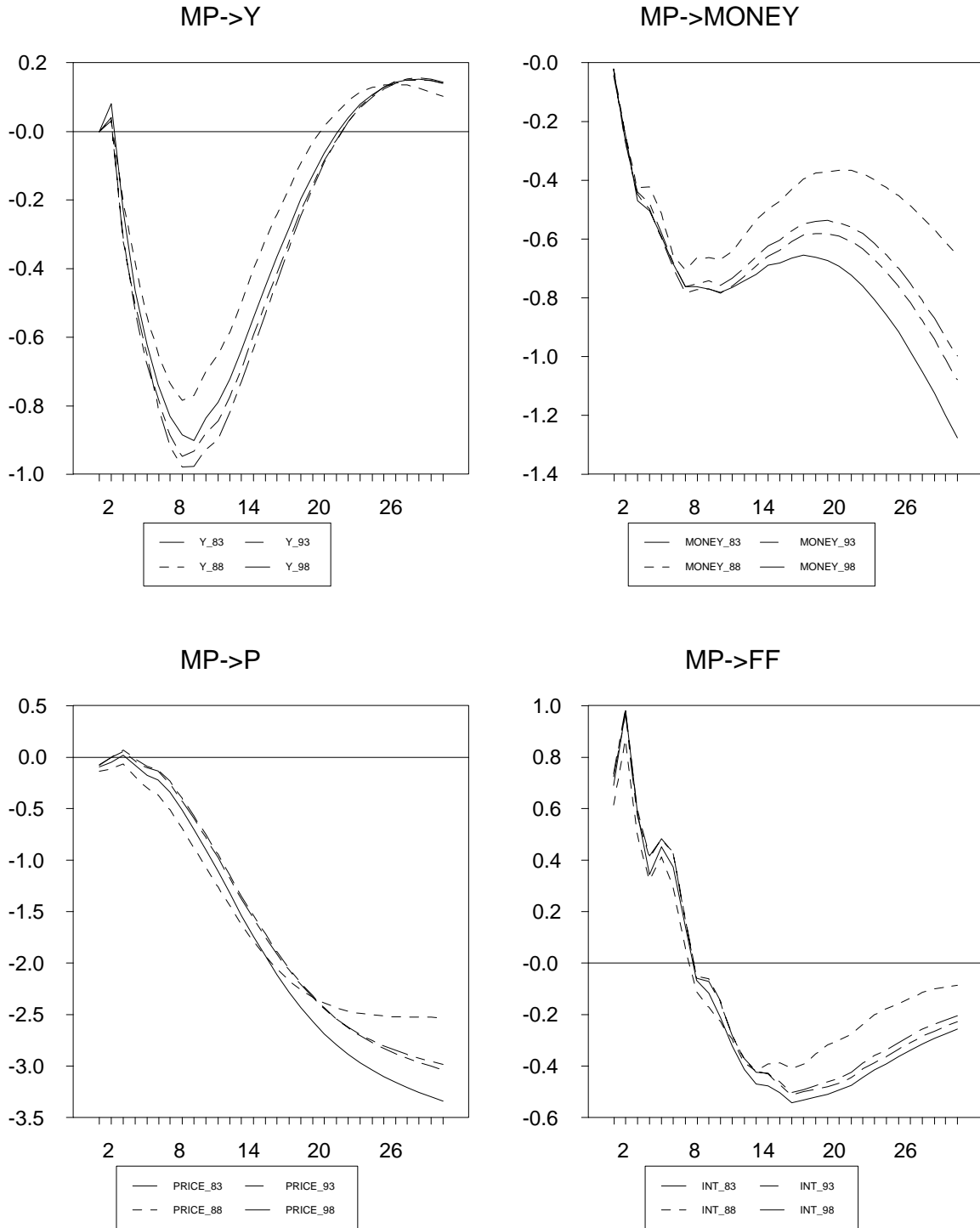The figure displays the point estimates from four VARs using different data vintages: 1983, 1988, 1993, and 1998. Each VAR was estimated over the sample period 1960-1983.

11

# 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/financial equations.

## 4.1. 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:

$$FF_t = A_{FF}(L)FF_{t-1} + A_Y(L)Y_{t-1}^t + a_Y Y_t^t + \varepsilon_{1t} \tag{4.1a}$$

$$Y_t^* = B_{FF}(L)FF_{t-1} + B_Y(L)Y_{t-1}^* + \varepsilon_{2t} \tag{4.1b}$$

Specifying the policy reaction function in real time requires explicit assumptions about the way data revisions influence policy. Equation $(4.1a)$ 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.[6] Equation $(4.1a)$ 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 $(4.1b)$ 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.

---

[6]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.

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 may be a questionable assumption, the alternatives may be worse. The assumption could be questionable because central banks 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 $Y_{t-s}^t, s \geq 0$ in equation (4.1b), 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 $\varepsilon_{1t}$ and $\varepsilon_{2t}$ shocks assumed to be exogenous, and uncorrelated with the other right-hand-side variables. The vector of exogenous shocks $\varepsilon_t = \begin{bmatrix} \varepsilon_{1t} & \varepsilon_{2t} \end{bmatrix}$ has a diagonal covariance structure.

Each period the output data are revised. The data revision process has the following form:[7]

$$Y_t^t = Y_t^* - g_t^* \tag{4.2a}$$

$$Y_t^{t+s} = Y_t^{t+s-1} + h_t^{t+s}, \quad \forall s > 0, \quad \forall t \tag{4.2b}$$

$$Y_t^* = Y_t^\mathsf{T} + h_t^{\mathsf{T}*}, \quad T = \text{latest release date.} \tag{4.2c}$$

$$g_t^* = h_t^{t+1} + h_t^{t+2} + ... + h_t^\mathsf{T} + h_t^{\mathsf{T}*} \tag{4.2d}$$

The initial data release is $Y_t^t$, and equation (4.2a) 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 (4.2b). Equation (4.2c) specifies how the latest data release $Y_t^\mathsf{T}$ is also an imperfect measure of the unobserved, true output variable $Y_t^*$. This specification readily admits further analysis with a later data release $Y^{\mathsf{T}+1}$, and thus captures the notion that $h_t^{\mathsf{T}*}$ represents a permanent wedge between measuring and observing true output $Y_t^*$. Although we do not specifically restrict the revision process $h_t^{t+s}$, equation (4.2d) states the relationship between $g_t^*$ and the revisions.

---

[7] 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.

## 4.2. Estimation difficulties with period $\mathsf{T}$-vintage data

Suppose an econometrician uses the most recent data releases to estimate equations $(4.1a)$ and $(4.1b)$ 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 $\mathsf{T}$-vintage data on $Y_t^\mathsf{T}$. Using the revised data $Y_t^\mathsf{T}$ in place of $Y_t^\mathsf{t}$ and $Y_t^*$, the system of equations becomes

$$FF_t = A_{\mathsf{FF}}(L)FF_{t-1} + A_\mathsf{Y}(L)Y_{t-1}^\mathsf{T} + a_\mathsf{Y}\,Y_t^\mathsf{T} + w_{1t}^\mathsf{T} \qquad (4.3a)$$

$$Y_t^\mathsf{T} = B_{\mathsf{FF}}(L)FF_{t-1} + B_\mathsf{Y}(L)Y_{t-1}^\mathsf{T} + w_{2t}^\mathsf{T} \qquad (4.3b)$$

The critical questions revolve around the correlation structure of the error terms $w_{1t}^\mathsf{T}$ and $w_{2t}^\mathsf{T}$ and their relationship to $\varepsilon_{1t}$ and $\varepsilon_{2t}$.[8] Given the revision process and the true laws of motion, it can be shown that

$$w_{1t}^\mathsf{T} = \varepsilon_{1t} - a_\mathsf{Y}\sum_{s=1}^{\mathsf{T}-t} h_t^{t+s} - A_\mathsf{Y}(L)\sum_{s=1}^{\mathsf{T}-t} h_{t-1}^{t+s}$$

$$w_{2t}^\mathsf{T} = \varepsilon_{2t} - h_t^{\mathsf{T}*} + B_\mathsf{Y}(L)h_{t-1}^{\mathsf{T}*}$$

When the econometrician estimates equations $(4.3a)$ and $(4.3b)$ with OLS, the estimated regression coefficients are likely to be biased. In general, the error terms $w_{1t}^\mathsf{T}$ and $w_{2t}^\mathsf{T}$ are correlated with the regressors in both equations. Note that $w_{1t}^\mathsf{T}$ contains revisions to date $t$ variables, date $t-1$ variables, and so on, which are correlated with the right-hand-side variables in equation $(4.3a)$, $Y_t^\mathsf{T}$ and $Y_{t-j}^\mathsf{T}$, $j = 1,2, ...,$ except under special assumptions. Similarly, $w_{2t}^\mathsf{T}$ contains measurement wedges — differences between the latest measure of the variable and its latent value — which will in general be correlated with the right-hand variables in equation $(4.3b)$.

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

---

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

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 (2002). 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 mix of news and noise.

Based on the outcome of the tests for news and noise, we develop consistent estimators for the parameters in equations (4.3$a$) and (4.3$b$). Clearly, the consistency of these estimates will also depend on the validity of the auxiliary assumptions about the data revision process.

## 4.3. 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 (4.1$a$),

$$FF_t = A_{\mathsf{FF}}(L)FF_{t-1} + A_{\mathsf{Y}}(L)Y^t_{t-1} + a_{\mathsf{Y}}Y^t_t + \varepsilon_{1t}$$

the econometrician can estimate this equation precisely with the vintage data $\overset{©}{Y^t_{t-s}}, s \geq 0^{\,a}$. Owing to the recursiveness assumption, $OLS$ is consistent. That is, 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 $\varepsilon_{1t}$. Consequently, the exogenous monetary policy shock $\varepsilon_{1t}$ can be recovered without being polluted by data revisions.

## 4.4. Using different data vintages as instruments

A major stumbling block in estimating equation (4.3$b$) is that true output $Y^*_t$ is never fully revealed in the period $\mathsf{T}$-vintage revisions. The error term $w^{\mathsf{T}}_{2t}$ includes the wedge terms $h^{\mathsf{T}*}_t$. If instead $Y^{\mathsf{T}}_t$ were to reveal $Y^*_t$ for some $T$ sufficiently large relative to $t$, then the measurement errors $h^{\mathsf{T}*}_t$ would disappear from $w^{\mathsf{T}}_{2t}$ 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 (usually slightly), there are data revisions. But it seems plausible to assume that there is some date beyond which all the data revisions are insubstantial. That is, real $GDP$ continues to be revised substantially as new income tax information comes in over the years. Also, seasonal adjustment procedures continue to alter the stochastic seasonals for many years. But beyond some threshold, 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 results). 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 is intended to motivate the following statistical model of benchmark data revisions:

$$Y_t^{t+s} = Y_t^* - \eta_t^{t+s}, \quad s \geq J. \tag{4.4}$$

After the threshold $J$ periods have elapsed, the reported data $Y_t^{t+s}$ measure the true $Y_t^*$ up to a measurement error $-\eta_t^{t+s}$ which is independent of $Y_t^*$ and $\eta_t^{t+s'}$ (where $s' \neq s$). This model of benchmark revisions allows us to construct an instrumental variables estimator for the output equation (4.3b). In the period $\mathsf{T}$-vintage data, let's restrict the sample period to the $Y^\mathsf{T}$ observations with $\eta^\mathsf{T}$ measurement errors, so that the error term $w_{2t}^\mathsf{T}$ is

$$
\begin{aligned}
w_{2t}^\mathsf{T} &= \varepsilon_{2t} - h_t^{\mathsf{T}*} + B_\mathsf{Y}(L)h_{t-1}^{\mathsf{T}*} \\
&= \varepsilon_{2t} - \eta_t^\mathsf{T} + B_\mathsf{Y}(L)\eta_{t-1}^\mathsf{T}, \quad t \leq T - J
\end{aligned}
$$

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

Since $FF_{t-1}$ is a valid regressor, an instrumental variables approach needs to deal with only the $Y$ data. To this end, notice that the revision errors $h_t^{\mathsf{T}*}$ are orthogonal to many other revision errors, once enough time has elapsed from the initial release of the data to ensure that all remaining revisions are orthogonal noise (across benchmark revisions). Given the data vintage denoted by $Y^\mathsf{T}$, select another data vintage $Y^{\mathsf{T}'}$. The final time period $T' < T$, by assumption (just a normalization). In practical terms, $Y^\mathsf{T}$ and $Y^{\mathsf{T}'}$ may be from the July 1998 and July 1996 releases of the National Income and Product Account data, respectively. 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, $Y_{t-s}^{\mathsf{T}'}$

16

is a valid instrument for $Y_{t-s}^{\mathsf{T}}$. That is,

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

These orthogonality conditions imply consistent estimation of the parameters in the output equation (4.3$b$).[9] Given consistent parameter estimates, the two data vintages yield two residuals $w_{2t}^{\mathsf{T}}$ and $w_{2t}^{\mathsf{T}'}$, which are error-ridden measures of the output shock $\varepsilon_{2t}$. However, the errors are independent of each other. The variance of $\varepsilon_{2t}$ 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 (4.4) is that the cross-covariances of the growth rates should be equal, $\Delta Y_t^{\mathsf{T}}\Delta Y_{t-j}^{\mathsf{T}'}$ and $\Delta Y_{t-j}^{\mathsf{T}}\Delta Y_t^{\mathsf{T}'}$.[10] Table 1 reports generalized method of moments estimates of the first four autocorrelations of $\Delta Y_t^*$ using eight moment conditions implied by

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

as well as four restrictions to estimate the just-identified means and variances of $\Delta Y_t^{\mathsf{T}}$ and $\Delta Y_t^{\mathsf{T}'}$. The data vintages are July 1996 and July 1998 releases (recorded as the August 1996 and August 1998 vintages in the Philadelphia Fed's real-time data set, as the NIPA releases come out at the end of the month and the real-time data set records vintages mid-month). 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 are similar. More interestingly, there is little evidence against the overidentifying restrictions as reported by the $J - statistic$. Consequently, the empirical evidence provides support for implementing the $IV$ estimation strategy using 1996 and 1998 vintage data.

---

[9]It should be apparent that $Y_t^{\mathsf{T}}$ and $Y_t^{\mathsf{T}^0}$ are not weak instruments for each other in this estimation strategy. For the 1996 and 1998 data vintages that we investigate below, these instruments easily passed first-stage $\mathsf{F}$-tests.

[10]Equation (4.4) refers to the log-level of real GDP. With trending real GDP, the cross-product matrices $Y_t^{\mathsf{T}}Y_{t-j}^{\mathsf{T}^0}$ will not be finite asymptotically. So it is convenient to restate the restrictions in terms of log first-differences.

Table 1: GMM estimates of autocorrelations

| Autocorrelation | $\Delta Y^*$ | $\Delta P^*$ | $\Delta NBR^*$ | $\Delta TR^*$ |
|---|---|---|---|---|
| $\rho_1$ | .54 | .89 | .14 | .13 |
| | (.09) | (.04) | (.08) | (.07) |
| $\rho_2$ | .24 | .83 | -.30 | -.30 |
| | (.12) | (.05) | (.14) | (.11) |
| $\rho_3$ | .19 | .82 | -.08 | -.09 |
| | (.10) | (.07) | (.07) | (.06) |
| $\rho_4$ | .18 | .79 | .12 | .11 |
| | (.11) | (.09) | (.07) | (.06) |
| | | | | |
| J-statistic | 1.45 | 1.16 | 1.90 | 2.53 |
| $\chi^2(4)$ p-value | (.83) | (.89) | (.75) | (.64) |

## 4.5. Computing impulse response functions

Given the parameters in equations (4.1$a$) and (4.1$b$), it is natural to compute impulse response functions from one-time exogenous shocks $\varepsilon_{1t}$ and $\varepsilon_{2t}$ at time $t$ to the paths of $FF_{t+j}, Y_t^{t+j}, Y_{t+j}^*$ 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 $Y_{t-s}^t$. 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

$$Y_t^t = Y_t^* - h_t^{t+1} - h_t^{t+2} - \ldots - h_t^T - h_t^{T*} \tag{4.5a}$$

$$h_t^{t+s} = Y_t^{t+s} - Y_t^{t+s-1}, \quad \forall s \geq 1, \quad \forall t. \tag{4.5b}$$

Given the real-time data set for $Y$ from Croushore and Stark (2001), the revision process $h_t^{t+s}$ is an observable data series for each $s \geq 1$. We assume that the $s-$revision $h_t^{t+s}$ is a stationary process that is independent of the exogenous shocks $\varepsilon_{1t}$ and $\varepsilon_{2t}$.[11] This assumption presumes $h_t^{T*}$ is unaffected by the exogenous shocks, so that its response is zero. Therefore, although the unconditional distribution of $FF_{t+j}, Y_t^{t+j}, Y_{t+j}^*$ depends on the economic shocks $\varepsilon_t$ and the measurement noises $h_t^{t+s}$ and $h_t^{T*}$, the conditional responses following an $\varepsilon_t$ shock assume that $h_t^{t+s}$ and $h_t^{T*}$ are zero.

# 5. Empirical results for recursive identification in a real-time 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 $GDP$ $(Y)$, the implicit $GDP$ 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 $T' = 1996{:}3$ and $T = 1998{:}3$. We take the benchmark threshold $J = 12$ quarters, so our estimation period runs from 1968:1 through 1993:3; the starting date is determined by the availability of real-time data and the ending date by the size of $J$ and the first vintage used as an instrument. As described in Section 4, the federal funds rate equation is estimated with real-time data (equation 4.1a), while the other equations are specified as latent variables (similar to equation 4.1b specified for a vector of data). The order of orthogonalization for studying nonpolicy shocks is $Y, P, PCOM, FF, NBR, TR$.[12]

Figure (5.1) 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). Overall, the series are remarkably similar. The correlation over the full-sample period is 0.89, and 0.74 over the more recent period 1987-93. The standard deviations of the two shocks are 75 and 87 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

---

[11] In a previous draft, we allowed the revisions to depend on the identified shocks $\varepsilon_1$ and $\varepsilon_2$, perhaps because of optimal statistical filtering rules studied by Sargent (1989). The qualitative results from that analysis were similar to the ones reported here.

[12] The contemporaneous terms in the multivariate counterparts to 4.1a and 4.1b are treated in the standard way with a recursive structure. We have also estimated the $PCOM$ equations using real-time data by $OLS$ (that is, treating PCOM as a function of measured output rather than latent output). Those results are similar to the ones reported below.
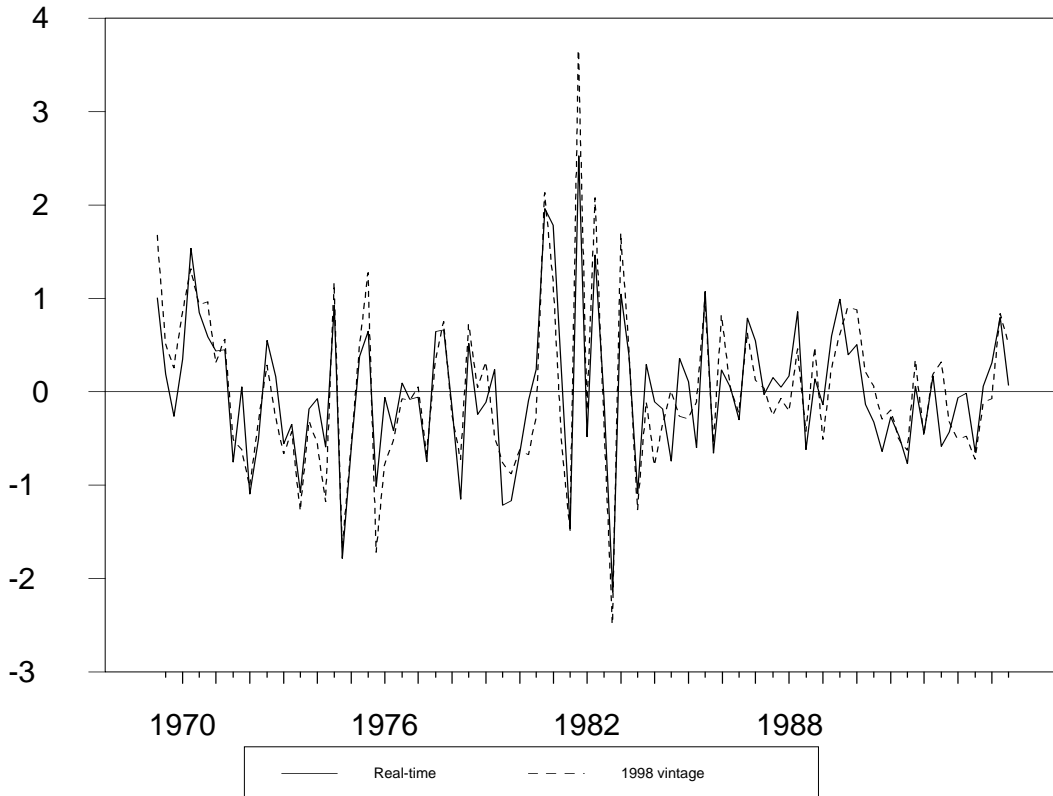
Figure 5.1: CEE Federal Funds Shocks
The figure displays estimated monetary policy shocks from two VARs estimated over the sample period 1968-1993. The solid line displays policy shocks estimated using real-time data, while the dashed line displays policy shocks from the 1998 vintage data set.

April 1974 as a date when the Federal Reserve explicitly chose to sacrifice output in order to bring an exogenous burst of inflation down. 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 120, 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.[13]

---

[13]Rudebusch (1999) finds that $FF$ residuals from a monthly VAR are quite different from forecast errors
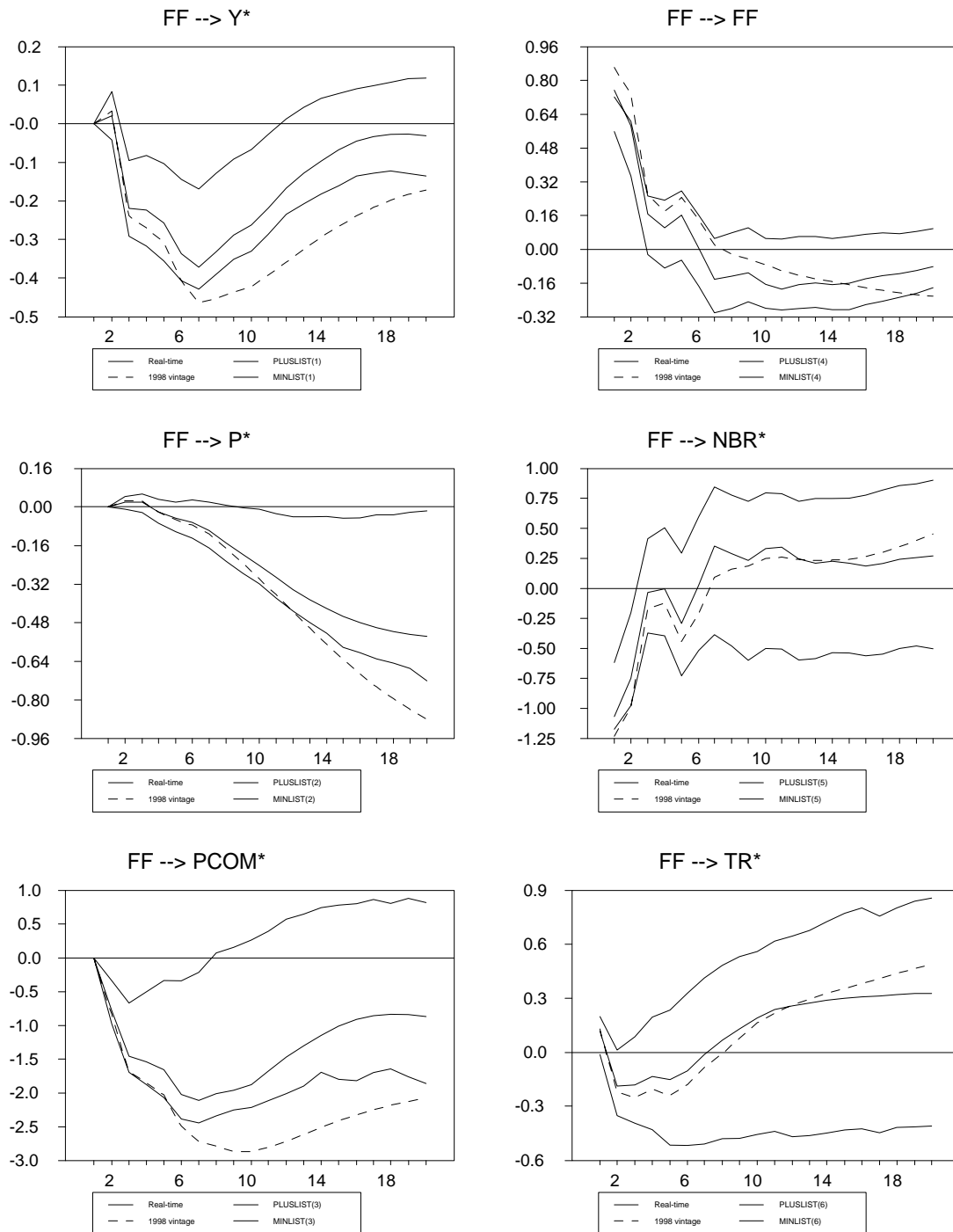
Figure 5.2: Recursively-identified FF Shock
The solid lines are the point estimate and 95 percentile bands for the impulse responses estimated using real-time data. The dashed line is the point estimate from the 1998 vintage data set.
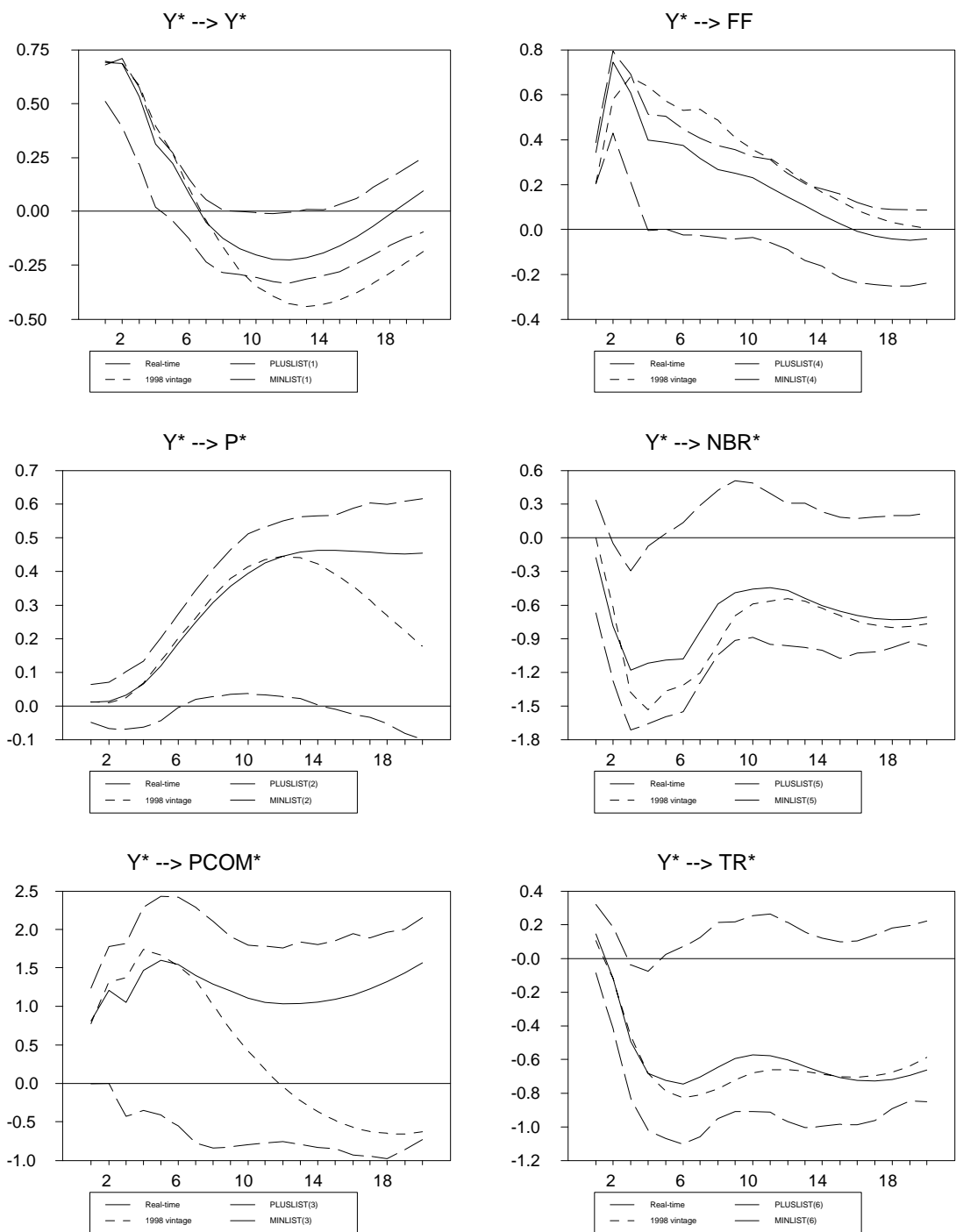
21

Figure 5.3: Recursively-identified Y* Shock
The solid lines are the point estimate and long-dashed lines are 95 percentile bands for the impulse responses estimated using real-time data. The short-dashed line is the point estimate from the 1998 vintage data set.

Figure (5.2) displays the impulse responses from the $FF$ shock for the estimated real-time system (solid line with 95 percentile confidence bands[14]) and the 1998:3 vintage estimates (dashed line). The similarity across the two VAR systems 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 second year responses of $Y^*$ and $PCOM^*$, 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. 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.

Figure (5.3) 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 (5.3). 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

---

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, evidence from futures market data do not directly shed light on the effect of real-time data for VAR policy shock measures.

[14]Bootstrap confidence bands are constructed in a straightforward manner. The data generating process is taken to be the VAR estimates in equations (4.1$a$) and (4.1$b$), the error variances estimated from the $h_t^{t+s}$ data according to equations (4.2$b$ − 4.2$d$), and estimates of the variances $\eta_t^{98}$ and $\eta_t^{96}$ based upon equation (4.4). 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 of the responses correspond to the real-time estimates.

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.

## 6.1. 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:

$$FF_t = A_{FF}(L)FF_{t-1} + A_Y(L)Y^t_{t-1} + a_Y Y^t_t + \varepsilon_{1t} \tag{6.6a}$$

$$Y^*_t = B_{FF}(L)FF_{t-1} + B_Y(L)Y^*_{t-1} + a_{FF} FF_t + \varepsilon_{2t} \tag{6.6b}$$

$$Y^t_t = Y^*_t + h^*_t \tag{6.6c}$$

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 vintage data is not consistent due to $E[Y^t_t \varepsilon_{1t}] \neq 0$: $Y^t_t$ is correlated with $\varepsilon_{1t}$ via its dependence on $FF_t$ $(a_{FF} \neq 0)$. Similarly, simultaneity causes $E[FF_t \varepsilon_{2t}] \neq 0$, which cannot be overcome by using the 1996 and 1998 vintage data: $FF_t$ is correlated with $\varepsilon_{2t}$ 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 $(4.1a, 4.1b)$.
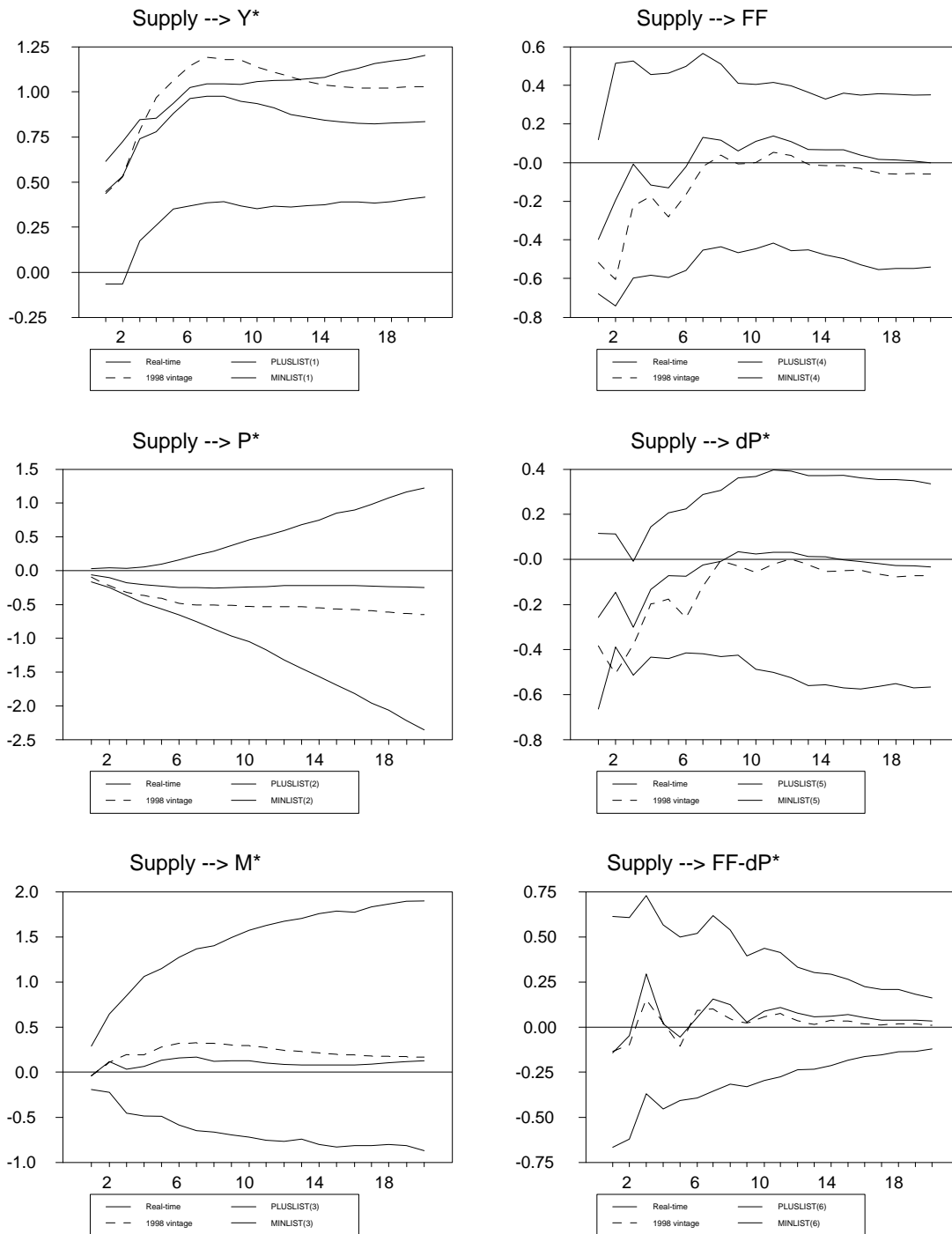
Figure 6.1: Long-run Supply Shock in Galí System
The solid lines are the point estimate and 95 percentile bands for the impulse responses estimated using real-time data. The dashed line is the point estimate from the 1998 vintage data set.

In addition, any reduced-form VAR representation for the two variables $FF$ and $Y^*$ will involve three shocks[15]: $\varepsilon_{1t}$, $\varepsilon_{2t}$, and $h_t^*$. Identifying the two economic shocks $\varepsilon_{1t}$ and $\varepsilon_{2t}$ from only two data series $FF$ and $Y^*$ is not possible without placing additional structure on the $h_t^*$ 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.

## 6.2. 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. Recall that the structural equation for output growth can be written as

$$\Delta Y_t^* = A_{11}(L)\Delta Y_{t-1}^* + A_{12}(L)\Delta FF_t + A_{13}(L)\Delta\frac{M_t^*}{P_t^*} + A_{14}(L)\left(FF_t - \Delta P_t^*\right) + \varepsilon_t^{\text{supply}} \quad (6.7)$$

Simultaneity and the contemporaneous values of $\Delta FF_t$, $\Delta\frac{M_t^*}{P_t^*}$, 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 $\Delta FF_t$, $\Delta\frac{M_t^*}{P_t^*}$, and $(FF_t - \Delta P_t^*)$. Although the latent variables are not observable, the 1998 and 1996 vintage estimation can be used to identify $\varepsilon_t^{\text{supply}}$.

Figure (6.1) 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

---

[15] Assuming $Y^*$ is an observable series is merely a simplifying device. Allowing for the latent nature of $Y^*$, the 1998 and 1996 vintage analysis can make these statements precise at the cost of additional notation.

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

[17] Since the full vector of economic shocks is not identified, the procedure described in footnote #14 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 $Y_t^{1996}$ that respect the implicit $Y_t^*$ simulations within the $Y_t^{1998}$ draws, draw normally-distributed error terms for $Y_t^{1996} - Y_t^{1998}$ 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.

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 $\Delta P^*$. 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. 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. More 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, Ben, 1986, Alternative Explanations of the Money-Income Correlation," *Carnegie-Rochester Conference Series on Public Policy* 25: pp. 49-100.

[2] Bernanke, Ben and Alan Blinder, 1992, "The federal funds rate and the channels of monetary transmission," *American Economic Review* 82: pp.

901-21.

[3] Bernanke, Ben and Ilian Mihov, 1998, "Measuring Monetary Policy," *Quarterly Journal of Economics* 113 (August): pp. 869-902.

[4] Blanchard, Olivier Jean, and Danny Quah. "The Dynamic Effects of Aggregate Demand and Supply Disturbances." *American Economic Review* 79 (September 1989), pp. 655-73.

[5] Blanchard, Olivier Jean, and Mark Watson, 1986, "Are Business Cycles All Alike?" in Robert J. Gordon, ed., *The American Business Cycle*, Chicago: University of Chicago Press.

[6] Christiano, Lawrence, Martin Eichenbaum and Charles L. Evans, 1996, "The effects of monetary policy shocks: evidence from the flow of funds," *Review of Economics and Statistics* 78 (February 1996): pp. 16-34.

[7] Christiano, Lawrence, Martin Eichenbaum and Charles L. Evans, 1999, "Monetary policy shocks: what have we learned and to what end?" Forthcoming in the *Handbook of Macroeconomics*.

[8] Croushore, Dean, and Tom Stark. "A Real-Time Data Set for Macroeconomists: Does the Data Vintage Matter?" *Review of Economics and Statistics*, 2002.

[9] Croushore, Dean and Tom Stark. "A Real-Time Data Set for Macroeconomists." *Journal of Econometrics* 105 (November 2001): pp. 111-30.

[10] Diebold, Francis X. and Glenn D. Rudebusch. "Forecasting Output with the Composite Leading Index: A Real-Time Analysis." *Journal of the American Statistical Association* 86 (September 1991): pp. 603-10.

[11] Galí, Jordi. "How Well Does the IS-LM Model Fit Postwar U.S. Data?" *Quarterly Journal of Economics* (May 1992), pp. 709-38.

[12] Hall, Robert E. "Stochastic Implications of the Life Cycle-Permanent Income Hypothesis: Theory and Evidence," Journal of Political Economy 86 (December 1978), pp. 971-87.

[13] Hamilton, James D. Time Series Analysis. Princeton: Princeton University Press, 1994.

[14] Leeper, Eric, Christopher Sims, and Tao Zha, 1997, "What does monetary policy do?" Brooking Panel on Economic Activity.

[15] Mankiw, N. Gregory, David E. Runkle, and Matthew D. Shapiro. "Are Preliminary Announcements of the Money Stock Rational Forecasts?" Journal of Monetary Economics 14 (July 1984), pp. 15-27.

[16] Mankiw, N. Gregory, and Matthew D. Shapiro. "News or Noise: An Analysis of GNP Revisions," Survey of Current Business (May 1986), pp. 20-5.

[17] Orphanides, Anthanasios, 1998, "Monetary policy rules based on real-time data," Board of Governors, FEDS working paper 1998-03.

[18] Rudebusch, Glenn D. "Do Measures of Monetary Policy in a VAR Make Sense?" International Economic Review 39 (November 1998), pp. 907-31.

[19] Sargent, Thomas 1989, "Two models of measurements and the investment accelerator," Journal of Political Economy 97: pp. 251-87.

[20] Sargent, Thomas and Christopher Sims, 1977, "Business cycle modeling without pretending to have too much a priori economic theory," Christopher Sims (ed.), New Methods in Business Cycle Research, Federal Reserve Bank of Minneapolis.

[21] Sims, Christopher A., 1986, "Are Forecasting Models Usable for Policy Analysis?" Federal Reserve Bank of Minneapolis Quarterly Review (Winter), pp. 2-16.

[22] Sims, Christopher, 1992, "Interpreting the macroeconomic time series facts: the effects of monetary policy," European Economic Review 36: pp. 975-1011.

[23] Sims, Christopher and Tao Zha, 1996, "Does Monetary Policy Generate Recessions?" unpublished manuscript.

[24] Stock, James and Mark Watson, 1989, "New indexes of coincident and leading economic indicators," NBER Macro Annual 4, MIT Press.