A Search for Timing Effects of Macroeconomic Shocks in the U.S.*

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Abstract

This paper addresses whether the response of output to various shocks has timing or season-contingent effects; that is, does a particular shock have a different effect on output if it originates in a particular calendar quarter? We study, in turn, shocks to: government spending, government revenue and monetary policy. For spending shocks, there is very little evidence of a timing effect. For revenue shocks, there is no evidence of a timing effect. For the final shock, we find a timing effect, which is consistent with existing research. However, omitting the early part of the Volcker disinflation, 1980-1983, eliminates this effect.

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1 Introduction

A current and central question in macroeconomics is whether wage changes of employees, which happen infrequently for at least some workers, are state dependent or time dependent.[1] State-dependent changes occur because individual or aggregate conditions put the current wage far off from its desired level. Time-dependent changes, on the other hand, occur for reasons related to time but orthogonal to current conditions.

The real effects of monetary policy are likely to be smaller if these changes are state rather than time-dependent.[2] With wage rigidity, monetary non-neutrality exists because many workers have wages that are far off their target wage, which is sometimes called the target flexible wage. If the workers that update their wages are the ones that are farthest from their targets, this will imply weak monetary non-neutrality. In contrast, Christiano, Eichenbaum and Evans (2005) and Edge, Laubach and Williams (2003) find time-dependent wages implies strong monetary non-neutrality.

One compelling piece of empirical evidence for time-dependent wages is presented in Oliveri and Tenreyro (2007). Using a structural vector-autoregression (VAR) and U.S. data, they show that exogenous interest rate policy shocks that originate earlier in the year (e.g. April) have a greater impact on real GDP than those shocks arising later in the year (e.g. September). We call this a season-contingent or timing effect of monetary policy. In their benchmark specification, for example, an exogenous 25 basis point reduction in the federal funds rate leads to maximal 0.5 percent increase in real GDP (eventually) if the shock originates in the second calendar quarter. If instead the same shock originates in the fourth calendar quarter, the maximal real GDP response is 0.1 percent. Oliveri and Tenreyro (2008) also provide evidence of season-contingent effects in Japan.

Oliveri and Tenreyro further argue that the season-contingency in responses to monetary policy shocks comes from season (time)-dependent wage adjustment. From existing research, we learn that greater wage stickiness implies stronger monetary non-neutrality. In their model, the bulk of nominal wage adjustments typically happens at the end of the calendar year, but many fewer firms adjust wages in the first half. With nominal wages nearly fixed entering the beginning of the next calendar year, the early part of the year is likely to see greater real effects of monetary policy changes, because nominal wages will not respond immediately.

[1] The same question exists regarding price changes of goods.

[2] See, for example, Takahashi (2009). Caplin and Spulberg (1987) and Goloslov and Lucas (2007) show similar results regarding nominal prices.

Moreover, Oliveri and Tenreyro provide anecdotal evidence that wage setting is actually concentrated at the end of the calendar year. The most direct evidence comes from a survey of a number of New England businesses in 2003. They report that most of the businesses surveyed make decisions on employees' wages between October and December. Durant et al (2009) find a similar pattern in EU countries that over 50 % firms adjust their wage contracts in January. [3]

Thus, Oliveri and Tenreyro's (2007) novel finding of season-contingent effects may be explained by bunching of wage changes during particular months of the calendar year. This explanation goes to the crux of a central question in macroeconomics: state- versus time-dependent nominal rigidities.

In light of the centrality of the question, our paper conducts an extensive search for evidence of timing effects of macroeconomic shocks as well as examines the existing evidence related to monetary policy shocks. We limit ourselves to searching for timing effects on real GDP because of our interest in time dependent wage setting. A search for timing effects on interest rates, for example, would more likely be an indicator of season-contingent monetary policy than of seasonal wage bunching.

Our rationale for examining other macro shocks is that if seasonal bunching of wage changes causes timing effects for monetary policy shocks, then this wage bunching is also likely to cause similar timing effects for other shocks. This is because, in many models, the degree of nominal rigidity influences the output response to a host of other shocks, including technology and government spending (e.g. Liu and Phaneuf (2008)).

Using this insight, our paper first estimates the response of output to two shocks, allowing for timing effects for each shock. First, we study government spending shocks, which are identified by assuming that changes in non-policy variables cannot affect the policy variable (government spending) in the same period.[4] Second, we study a government revenue, or tax, shock identified via the narrative approach by Romer and Romer (2008, 2009). By studying the historical record, e.g. Congressional testimony and presidential speeches, the authors parse tax changes into exogenous and endogenous components. The tax shocks occur primarily in the first and third quarters in the sample, which limits our study accordingly.

We find negligible evidence of timing effects in both cases. For the spending shock, we test for seasonal dependence in both the earlier and the later part of the impulse response

[3]Barattieri, et.al. (2009) on the other hand provide evidence against seasonal wage bunching in the U.S.

[4]Blanchard and Perotti (2002) and Fatas and Mihov (1998), for example, use this identification.

functions for each of the four calendar quarters, which gives eight tests in all. Seven of the eight fail to reject seasonal independence, i.e. the tests find no timing effect, at a 20% significance level. For the tax shock, we have four tests because limited data forces us to exclude second and fourth calendar quarters. All four tests fail to reject seasonal independence at a 25% level.

The balance of the paper attempts to reconcile our finding of no timing effects of spending and revenue shocks with Oliveri and Tenreyro (2007)'s finding of strong timing effects for monetary policy shocks. As a result, we find that their result may be due to several monetary policy shock outliers. In particular, omitting the early part of the Volcker disinflation, 1980-1983, eliminates the timing effect. Moreover the importance of this period may be due to two major monetary policy reversals, both of which occurred in the fourth calendar quarter during these few years. Our approach follows several VAR studies which control for special events in order to identify the typical response of macroeconomic variables to exogenous shocks.[5]

In the next section, we describe the seasonal structural VAR used in the paper. Section 3 considers each macroeconomic shock in turn and section 4 concludes.

2 A Statistical Framework for Timing Effects

Our general framework is a seasonal structural VAR model. By "seasonal", we refer to models where the coefficients vary according to the quarter of the year.

The structural form of the model can be written as the following:

$$A_0^q Y_t = \sum_{p=1}^{k} A_p^q Y_{t-p} + \epsilon_t^q, \tag{1}$$

where Y_t is a n by 1 vector composed of macro variables of interest. The matrix, A_0^q, governs the contemporaneous interactions between variables in the system. The lag coefficients are summarized in matrices, $\{A_p^q\}_{p=1}^k$, where p is the variable for the order of lags and k is the maximum lag. The structural disturbance is given by the vector, ϵ_t^q. Without loss of generality, we assume that $\epsilon_t^q \sim i.i.d.\ N(0, I), \forall q, t$, where I is the identity matrix. The superscript, q, stands for the quarter of the year, where $q = j$ if t corresponds

[5]Examples of this include Becksworth, et.al. (2009), Blanchard and Perotti (2002) and Irons and Faust (1999), and we discuss each later in the paper.

to the jth quarter of the year. Such a setting indicates that coefficients in the model are season-dependent. To be exact, the coefficients vary according to q.

The identification of various shocks can be obtained by imposing restrictions on the coefficient matrices. To identify the government spending shock, we employ a short-run restriction $a\ l\acute{a}$ Blanchard and Perotti (2002). It requires that government spending do not respond to changes of other macro variables in the current period, due to decision and/or implementation lags. This assumption imposes a restriction on the matrix, A_0^q, such that $A_0^q(i,j) = 0, \forall j \neq i, \forall q$, where i is the position of the equation for government spending, and j's are positions of variables in the equation.

The government revenue shock, or the tax shock, is identified by using a method in Romer and Romer (2009). Romer and Romer use narrative evidence to construct a time series for autonomous tax changes. Such exogenous changes will affect other macro variables, but there is no causation in reverse. Therefore, it is straightforward to identify the tax shock by imposing that $A_p^q(i,j) = 0, p = 0, 1, ..k, \forall j \neq i, \forall q$, where i is the position of the equation for the tax variable, since those tax changes are exogenous.

Another short-run restriction is used to identify the monetary policy shock, following Christiano, Eichenbaum and Evans (1999, 2005) and others. We use the federal funds rate as the policy instrument. Based on the assumption, the federal funds rate responds to contemporaneous changes of other variables in the system, but the other variables cannot respond to contemporaneous federal funds rate changes. This restriction implies that $A_0^q(i,j) = 0, \forall i \neq j, \forall q$, where j is the corresponding position of the federal funds rate in every equation, and i's are the positions of equations in the system.

In practice, we identify the mentioned shocks separately in different models. When applying the short run restrictions, i.e, identifying the spending shock and the monetary policy shock, we estimate the reduced form of the model (1),

$$Y_t^q = B^q(L)Y_{t-1}^q + e_t^q, \tag{2}$$

where $B^q(L) = B_1^q + B_2^q L + ... + B_k^q L^{k-1}$ is the polynomial of reduced-form coefficient matrices, and L is the lag operator. The reduced-form residual is denoted by e_t^q. It is straightforward to verify that $B_p^q = (A_0^q)^{-1}A_p^q$ and $e_t^q = (A_0^q)^{-1}\epsilon_t^q$. We then estimate $\{B_p^q\}_{p=1}^k$ with equation-by-equation OLS. The matrix A_0^q is identified by applying Cholesky decomposition on the variance matrix of e_t^q. When identifying the revenue (tax) shock, we estimate the structural form directly with restrictions on the corresponding coefficients. Unless otherwise noted, we estimate quarter-dependent models, which means there are four different sets of estimates for A_0 and $A(L)$.[6]

An important indication of season-dependent coefficients in model (1) is that the variables' dynamics vary according to the calendar quarter in which a shock originates. Impulse responses trace out these dynamics. To compute the impulse responses, it is convenient for us to rewrite the model in a season-invariant form. To this end, we define

$$X_T = \left[Y_T^{1\prime},\ Y_T^{2\prime},\ Y_T^{3\prime},\ Y_T^{4\prime}\right]^{\prime},$$

where Y_T^q represents the observation of variable $\{Y\}$ in the qth quarter in the Tth year, and the symbol, "′" represents the transpose operator. Therefore, the annualized form of system (1) can be written as

$$\Xi_0 X_T = \Xi(L)\ X_{T-1} + \tilde{U}_T. \tag{3}$$

The coefficient matrices, Ξ_0 and $\Xi(L) = \Xi_1 + \Xi_2 L + ... + \Xi_{\tilde{k}} L^{\tilde{k}-1}, \tilde{k} = roundup\{k/4\}$, can be obtained by re-arrange the matrices A_0^q and $\{A_p^q\}_{p=1}^k$, where L is the lag operator. The disturbance, $\tilde{U}_T$, is constructed by stacking ϵ_t^q appropriately.[7] As in traditional VAR models, the impulse responses can be easily computed based on the Wold representation of equation (3). The corresponding confidence intervals are computed by Monte Carlo sim-

[6] Allowing VAR coeffients to vary across calendar quarters implies a large number of parameters. However, our focus is on estimating and analyzing impulse responses. Sims (1980) points out that an impulse response is a better device studying the dynamics in VAR models. To this end, we compare impulse responses with four seasons to one with two semesters. The results are consistent with what we report in the benchmark model.

[7] As a simple example, if the model for quarterly data is a VAR (4),

$$A_0^q Y_t^q = A_1^q Y_{t-1}^q + A_2^q Y_{t-2}^q + A_3^q Y_{t-3}^q, q = 1, 2, 3, 4,$$

we have

$$\begin{aligned}
\Xi_0 &= \begin{bmatrix} A_0^1 & 0 & 0 & 0 \\ -A_1^2 & A_0^2 & 0 & 0 \\ -A_2^3 & -A_1^3 & A_0^3 & 0 \\ -A_3^4 & -A_2^4 & -A_1^4 & A_0^4 \end{bmatrix} \\
\Xi_1 &= \begin{bmatrix} A_4^1 & A_3^1 & A_2^1 & A_1^1 \\ 0 & A_4^2 & A_3^2 & A_2^2 \\ 0 & 0 & A_4^3 & A_3^3 \\ 0 & 0 & 0 & A_4^4 \end{bmatrix} \\
\tilde{U}_T &= \begin{bmatrix} \epsilon_t^1 \\ \epsilon_t^2 \\ \epsilon_t^3 \\ \epsilon_t^4 \end{bmatrix}
\end{aligned}$$

ulation.

In the following section, we discuss the timing effects of spending shocks, tax shocks and monetary policy shocks in turn. Further details about data and model specifications are discussed case by case.

3 A Search for Evidence of Timing Effects

As explained in the introduction, whether wage changes are time dependent or state dependent has potentially great impact on how policy makers interpret the real effects of monetary policy. The season-contingent effects of real GDP to interest rate shocks could be due to season (and ergo time) dependency of wage setting.

In this section, we apply the statistical model from section 2 to three different shocks. Each shock has been considered in previous work assuming seasonal independence, i.e. no timing effect; moreover, each previous study finds statistically significant effects on real GDP of each respective shock.

For consistency, we use the same variables and sample periods as used in previous work[8]. We do this to avoid rigging our results towards not finding seasonal dependence. It turns out that for each shock, extending the sample diminishes the shock's real effects. Using more recent data might therefore lead us to find no timing effects when it is simply the case that real effects in all seasons declined in both economic and statistical significance.

3.1 Blachard-Perotti Identification of Spending Shocks

Following Blanchard and Perotti (2002), we identify government spending shocks, or more concisely spending shocks, by assuming that spending do not respond to other variables within the quarter due to decision and implementation lags in policy.[9] This can be implemented as a recursive identification assumption with spending ordered first in the vector-autoregression and entries in the first row of the A_0 matrix set equal to zero (except the first one).

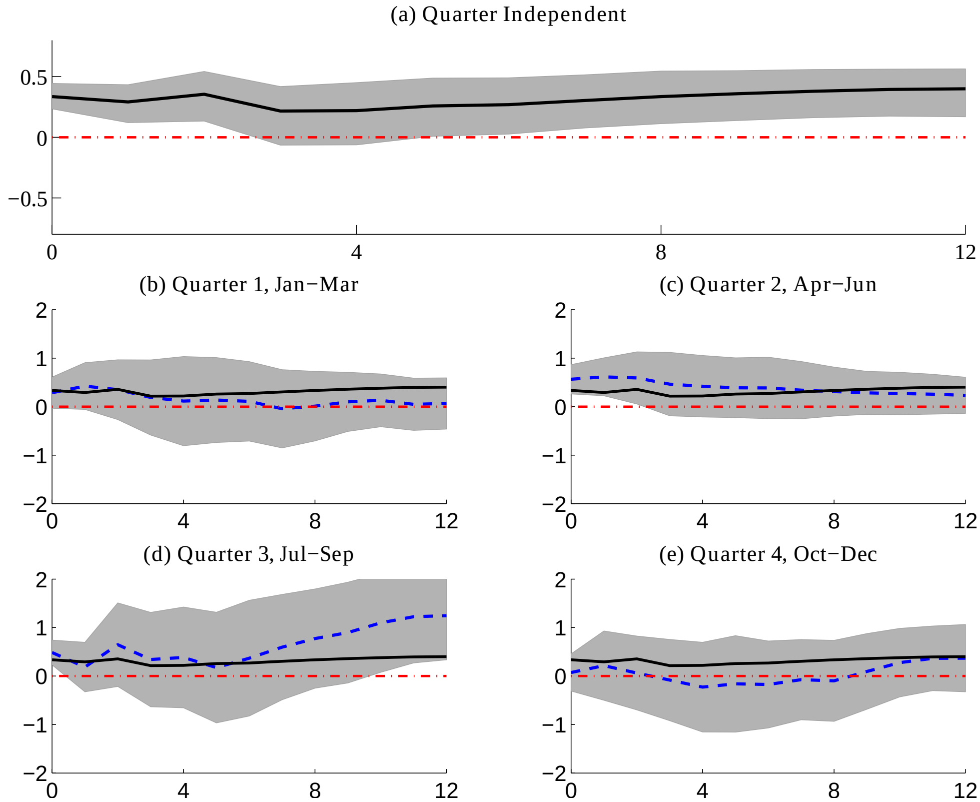
Again, following Blanchard and Perotti, we use a three-variable VAR with four lags consisting of real government spending, real government revenue and real GDP.[10] All the

[8]The exception is when we exclude the 1980-1983 period for the monetary policy shock.

[9]See also Fatas and Mihov (1998).

[10]Data details are contained in the appendix.

Figure 1: Response of real GDP to exogenous spending increase, quarter dependent (by quarter of shock origination) and quarter independent



Notes: Each shock is a 1% increase in government spending. Solid line is the quarter-independent response. Dashed line is the quarter-dependent response. Shaded region in (a) is the 80% confidence interval computed based on sample standard deviations. Shaded regions in (b)-(e) are the quarter-dependent confidence intervals. Sample is 1960-1996.

series are detrended by removing a linear trend.[11] The sample is quarterly and runs from 1960 through 1996.

Panel (a) of figure 1 plots the impulse response of real GDP to a 1% increase in spending. In this panel, the shaded region contains the 80% confidence interval under the assumption of seasonal independence. GDP increases contemporaneously with the shock and remains above zero over the plotted horizon. The confidence interval lies above the zero response line for most periods.

This panel corresponds to and is consistent with the upper right panel of figure V from

[11] As alternative, we also consider a quadratic trend and a HP-filter trend, and the result is robust to different detrending schemes.

Blanchard and Perotti.

Panels (b)-(e) of figure 1 plot the impulse response to the same shock allowing for timing effects. Thus we implement the statistical procedure from section 2 using the Blanchard-Perotti identification. In each of these panels, the dashed line is the quarter-dependent response and the shaded region is the 80% confidence interval for this response. For comparison, we plot the quarter-independent response as the solid line (i.e. exactly the solid line from panel (a)). In each season, the confidence interval for the quarter-dependent response contains the quarter-independent impulse response. We interpret this as an absence of statistical support for timing effects of spending shocks.

While figure 1 is instructive, we would like a formal test against the null hypothesis of no timing effect. To this end, we construct a statistic based on the maximum difference (in absolute value) between the quarter-contingent and standard quarter-independent impulse responses, following Oliveri and Tenreyro (2007). The test statistics is defined as

$$D_H = \max_{k \in H} |x_k^q - x_k|$$

where x_k^q is the k-period response of output for a shock originating in calendar quarter q and x_k is the k-period quarter-independent response. Here, H denotes the horizon considered for the impulse response. Table 1 reports results for two different horizons: (i) year zero through one, $H = \{0, 1, \ldots, 7\}$; (ii) year two through three, $H = \{8, 9, \ldots, 15\}$. The distribution of the D-statistics under the null hypothesis is difficult to compute, so we rely on the Monte Carlo method. We simulate a sample with the same length of the data based on a quarter-independent model. The simulated sample is then estimated both as a quarter-dependent model and as a quarter-independent model. Therefore, we can compute a new value, D^s, where the superscript, s, means "simulated". The p-value reported in Table 1 correspond to the frequency of cases simulated D^s exceeding observed D in a simulation with 1,000 replications.

In only one of the eight possible cases is the p-value less than 10%, while others are well above 20%. In particular, when the shock originates in the third calendar quarter and over the later horizon, the p-value equals 0.019. As a whole, we see very little evidence of a seasonal effect in response to spending shocks; moreover, acknowledging this timing effect here generates a new puzzle. The single case of a seasonal effect of the spending shock occurs when the shock originates in the second-half of the year. This is puzzling because in existing research that does find a seasonal effect for monetary shocks, the strongest effect occurs for shocks originating in the first half of the calendar year. The timing of

Table 1: p-values for differences in GDP impulse responses (D_H statistics), by quarter of origination and by shock

	Quarter of Shock Origination			
	First	Second	Third	Fourth
Government Spending Shock				
Year 0-1 horizon	0.467	0.310	0.503	0.283
Year 2-3 horizon	0.227	0.477	0.019	0.325
Tax Shock				
Year 0-1 horizon	0.394	–	0.536	–
Year 2-3 horizon	0.304	–	0.564	–
Monetary Policy Shock				
including 1980-1983				
Year 0-1 horizon	0.037	0.027	0.091	0.198
Year 2-3 horizon	0.034	0.270	0.005	0.346
Monetary Policy Shock				
excluding 1980-1983				
Year 0-1 horizon	0.397	0.278	0.442	0.211
Year 2-3 horizon	0.720	0.478	0.321	0.227

Notes: Null hypothesis that quarter-contingent impulse response is identical to quarter-independent impulse response over various horizons. p-values computed via bootstrap. (-) appears when there is insufficient data for shocks originating in corresponding calendar quarter.

seasonalities in impulse responses to spending shocks misses the timing of documented seasonalities in wage adjustments.[12]

3.2 Romer-Romer Identification of Tax Shocks

The second shock we consider is a change in government spending. As with spending and monetary policy, tax policy changes are often made in response to current macroeconomic conditions and, therefore, cannot in themselves be thought of as exogenous. Romer and Romer (2009) attempt to parse out endogenous from exogenous Federal tax changes using written records from the executive and legislative branch. Examples include presidential speeches and Senate Finance Committee transcripts.

Romer and Romer (2009) catalog fifty-four exogenous tax changes and measure these quantitatively in terms taxes as a fraction of that period's GDP. Their sample is quarterly and runs from 1945 through 2007. They find a strong negative, and statistically significant, effect on real GDP of exogenous tax increases. In their benchmark VAR, they find that a 1% exogenous increase in taxes reduces real GDP with a maximal impact of approximately 3% that occurs approximately 10 quarters following the shock.[13]

These tax shocks provide an excellent environment to test the hypothesis that macroeconomic shocks have timing effects. First, the data are quarterly. Second, Romer and Romer find a strong effect of the shock on real GDP without allowing for timing effects.

Their VAR does not allow for timing effects; however, it is reasonable to think that differences in wage rigidity over the calendar year will translate into different real effects of tax shocks depending on the shocks' timing.[14]

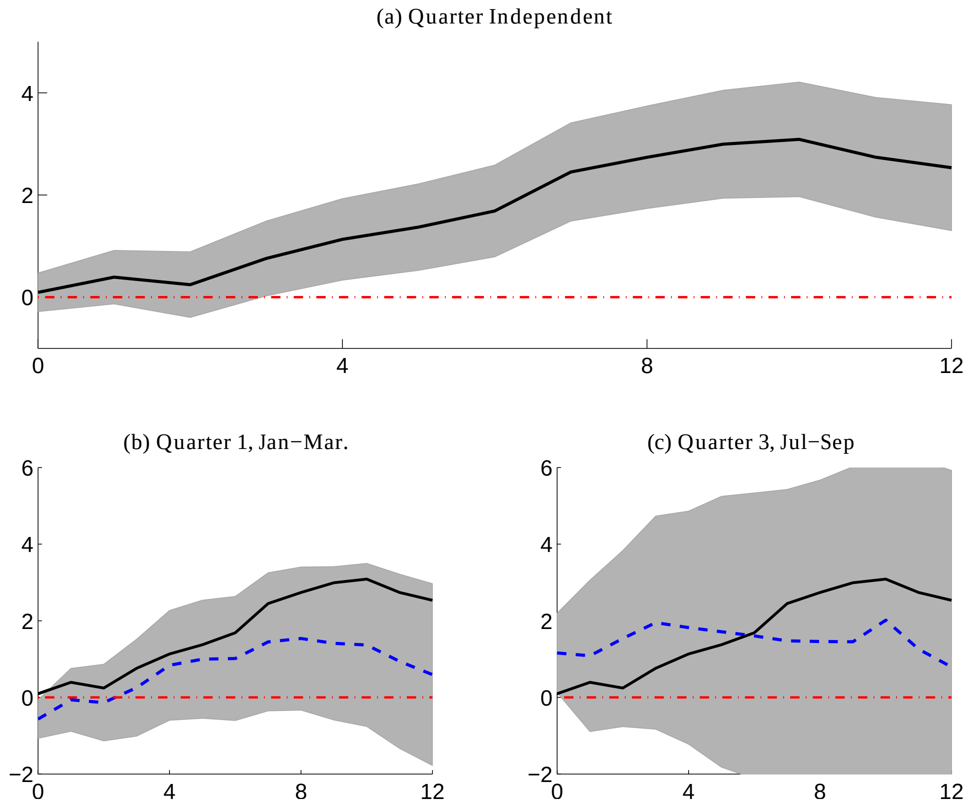
Our analysis is limited to the quarter-independent case and the quarter-dependent case with the shock originating in either quarter one or quarter three. This is because there is a paucity of non-zero exogenous shocks in the other two quarters. Over the entire sample period, only 27% of the observations take on non-zero values. Moreover, 78%

[12]Nakamura and Steinsson (2008) document that the frequency of price changes decreases along quarters, which actually indicates the effect of monetary policy might be stronger in the second half of the year. However, this piece of empirical evidence cannot be reconciled by the fact that there is lack of evidence in favor of timing effect for other shocks presented in following sections of our paper

[13]Their relevant figure for our purpose is Figure 6(c).

[14]One open question, to our knowledge, is how wage rigidity affects an economy's response to tax shocks. The answer to this question is likely to depend on the details of the tax, such as where its incidence is borne. In some models and for some types of taxes, wage rigidity may have not affect on the response of real GDP to tax shocks. In this case, our VAR could correctly find no season-contingent effects even though they are present for other shocks.

Figure 2: Impulse response of real GDP to exogenous tax cut, quarter dependent (by quarter of shock origination) and quarter independent.



Notes: Each shock is a 1% decrease in the tax-to-GDP ratio. Solid line is the quarter-independent response. Dashed line is the quarter-dependent response. Shaded region in (a) is the 80% confidence interval computed based on sample standard deviations. Shaded regions in (b)-(c) are the quarter-dependent confidence interval. Sample is 1945-2007.

of these non-zero shocks occur in the first and third calendar quarters. In quarter two and quarter four, there are only seven and eight non-zero values, respectively. Following Romer and Romer, we study a VAR containing their tax measure and real GDP.

Figure 2 plots the responses of real GDP to exogenous tax reductions in three cases. Panel (a) contains the quarter-independent response and is very similar to the impulse response reported in Romer and Romer (2009). Panels (b) and (c) contain the responses when the shock arrives in January through March and July through September. In each case, the point estimates imply that the tax cut stimulates output. The impulses have similar quantitative magnitudes and general shapes. From these figures, we draw no evidence in favor of season-contingent effects of tax shocks.

As with the spending shocks, we also use the D-statistics to test for season-dependent impulse responses. Table 1 contains p-values for the null hypothesis of seasonal independence in response to tax shocks. None of the four tests are able to reject seasonal independence at any conventional significance level.

3.3 Recursive Identification of Monetary Policy Shocks

In this subsection, we study the only shock which exhibits a timing effect–a monetary policy shock; however, in the succeeding sub-section, we show that this timing effect disappears when we exclude the first few years of the Volcker disinflation from the sample.

Following Oliveri and Tenreyro (2007), we consider a VAR (4) model with four variables in the order: the GDP deflator inflation rate, detrended real GDP, the spot commodity price growth rate, and the federal funds rate. Real GDP is detrended by using a linear trend.[15] The data is quarterly, and following Oliveri and Tenreyro, our sample runs from 1966-2002. Identification of the monetary policy shock comes from the standard assumption that other variables do not respond to exogenous innovations to the federal funds rate within the quarter. In contrast to the spending identification, here the policy variable is placed at the bottom of the system, and entries on the last column of the A_0 matrix are all zeros (except the last one).

Panel (a) of figure 3 plots the impulse response of real GDP to a 1% exogenous decrease in the federal funds rate, where the VAR is assumed to be quarter-independent. By assumption, the period zero response is zero. After period zero, output increases over time with its maximal response occurring approximately two years following the shock. The confidence interval lies above the zero response for most of the periods plotted. Thus, panel (a) is in line with standard structural VAR findings based on this identification assumption and over this time period.

Panels (b)-(e) of the figure plot the impulse responses to the same shock allowing for timing effects. Shocks originating in the first two calendar quarters have much larger real effects than shocks originating in the final two calendar quarters. For example, consider the quarter 2 case, i.e. panel (c). For several quarters following the shock, the output response is more than three times as large than if one were to assume quarter-independence. For a shock originating in quarter 4, i.e. panel (e), the output response is very close to the quarter-independent response across all the plotted periods. Oliveri and Tenreyro pro-

[15]In unreported robustness results, we found that using either a quadratic or a HP-filter trend delivers similar results.

vided the original evidence of the timing effect of monetary policy shocks. Our figure 3 provides impulse responses that demonstrate the same phenomenon as their figures one through five.

In addition to the impulse responses, the D-statistics for the monetary policy shocks in table 1 also demonstrates a timing effect. This result is shown under the "*including* 1980-1983" sub-heading. For the first three calendar quarters, we reject the hypothesis of quarter-independence in the output responses for either one or both of the horizons considered at the 5% significance level.

We report similar D-statistics that support timing effects of monetary policy shocks as Oliveri and Tenreyro (2007), although they find these effects for all four calendar quarters.[16] Beyond this VAR evidence, Oliveri and Tenreyro also provide a potential explanation. If nominal wage contracting happens annually at the end of the year, then the real effects of monetary policy shocks should be largest when a shock arrives immediately after the wage contracting has occurred. That is, unanticipated policy shocks should have their greatest effect if they occur in the first half of the year. In the same paper, Oliveri and Tenreyro further provide direct evidence of this type of wage setting based on a 2003 survey of firms in New England and three other less direct sources.

While Oliveri and Tenreyro's results provide compelling evidence, two factors motivate a more detailed study of this finding of season-dependent responses. First, earlier in the paper, we demonstrated the absence of evidence for timing effects in response to either spending or revenue shocks. This lack of evidence would be particularly puzzling if wage contracting were concentrated at the end of the calendar year. Based on current macroeconomic theory, the response of the economy to other shocks should depend on the degree of nominal rigidity in the economy as well.[17] Second, Barattieri, et.al. (2009) provide evidence based on SIPP data that there is little seasonal bunching of wage setting.

In the next subsection, we reconcile the timing effect of monetary policy shocks and absence of timing effects of two other shocks by showing that the latter timing effect may be due to a few outliers.

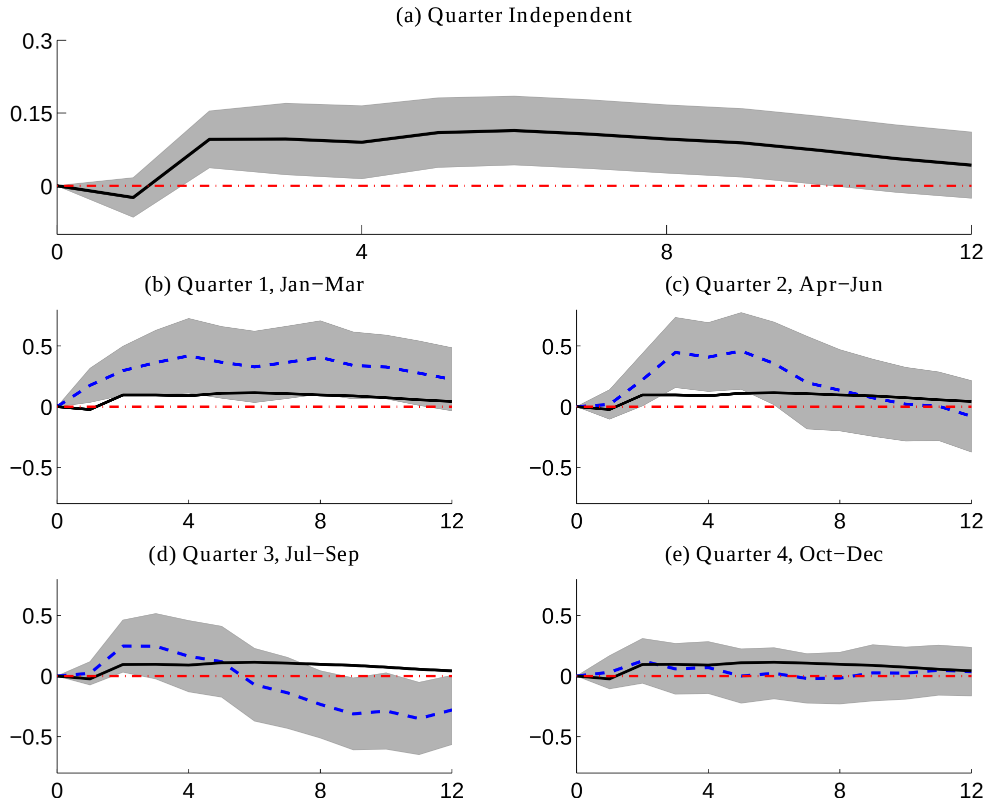
3.4 Monetary policy shocks, excluding 1980 through 1983

Our reading of the historical record of post-WWII U.S. monetary policy informs our statistical work below. We discuss this record presently. First, the largest interest rate changes

[16]See the GDP column of their table 1.

[17]See, for example, Liu and Phaneuf (2008) for a relevant discussion of technology shocks.

Figure 3: Response of real GDP to an exogenous interest rate reduction, including 1980-1983, quarter dependent (by quarter of shock origination) and quarter independent



Notes: Each shock is a 1% decrease in the federal funds rate. Solid line is the quarter-independent response. Dashed line is the quarter-dependent response. Shaded region in (a) is the 80% confidence interval computed based on sample standard deviations. Shaded regions in (b)-(e) are the quarter-dependent confidence interval. Sample is 1966-2002.

occur mainly in the early 1980s and mainly in the later part of the calendar year. Table reports the six largest quarter-on-quarter changes in the federal funds rate between 1954 and 2008. Each of the reported interest rate changse are at least three times greater than the sample standard deviation, which is 0.94, of $\Delta R(t)$. For comparison, we also list the real GDP growth two quarters following each of the extreme interest rate changes.[18] Four of the five largest interest rate changes (three standard deviations or greater) occur in July through December. The two largest shocks occur in the fourth quarter. One is four and the other is six times the sample standard deviation.

[18]The sample standard deviations of real GDP growth (annualized as in table 2) is 3.69.

Although these quarter three and four interest rate changes are huge, the output changes that follow (six month later) are not especially large. For each of these interest rate changes, with the exception of 1980:Q3, the associated change in GDP is less than two standard deviation of the typical change.

How do the season-contingent VAR estimates reconcile the July through December feature of some very volatile interest rate shocks followed by not particularly large changes in real GDP?[19] The real effects of these large shocks must have a smaller effect on GDP than the relatively modest interest rate changes that occur between January and June. The effect of these extreme values, since they are included in the sample, might show up as a timing effect when the seasonal structural VAR is estimated over the whole sample.

This leads to two questions: (i) why are large interest rate changes concentrated in the early 1890s? (ii) why are these changes concentrated in the later part of the calendar?

First, the Federal Reserve moved from a passive to an active monetary policy between 1980 and 1983. Despite this structural policy change, in our VAR analysis above as well as in Oliveri and Tenreyro (2007), there is a single monetary policy rule.[20] Since the systematic part of monetary policy changed in the early 1980s, large unexplained shocks are likely to be inferred from a policy rule that does not change.

The explanation for the season-dependency of the changes lies in the history of the Volcker years. We rely on Goodfriend (1993) for this history.[21] The Volcker disinflation began with an initial sequence of interest rate hikes in October of 1979 that continued until April 1980. The total increase was large, but the change occurred relatively gradually. Hence, no interest rate changes in this period appear in table 2. In mid-1980 there was the first policy reversal. The interest rate was cut approximately eight percentage points between April and August of 1980. This interest reduction shows up in table 2 in the fifth row, for the third quarter of 1980.

This easing was short-lived. As Goodfriend explains, "the reversal signaled an inflation scare induced by the excessively aggressive easing." In response, in quarter four 1980, there was another policy reversal-this time in the direction of disinflation. "The Fed

[19] Of course, table 2 does not contain innovations to an interest rule, so the values are not exogenous shocks; however, we believe the table is instructive and motivates well the robustness exercise.

[20] This raises an important issue of whether the 1980 through 1982 interest rate changes are innovations to a time-invariant monetary policy rule (as the season-contingent VAR specification assumes) or whether the 1980-1982 should be interpreted as a transition period between two separate rules. In fact, many researchers prefer the latter interpretation. Clarida, Gali and Gertler (2000) and Taylor (1999), for example, distinguish the earlier period as a time when monetary policy was insufficiently responsive to changes in inflation when setting the federal funds rate.

[21] Cook (1989) contains another detailed discussion of the period.

Table 2: Largest Federal Funds Rate Changes, 1954-2008

$\Delta R(t)$	Year	Quarter of change	$\Delta Y(t+2)$
6.02	1980	Oct - Dec	-3.13
-4.00	1981	Oct - Dec	2.14
-3.51	1982	Jul - Sep	4.90
-3.04	1975	Jan - Mar	6.71
-2.86	1980	Jul - Sep	8.03

Notes: Δ denotes the change relative to the previous quarter. $R(t)$ denotes the average, during period t, of the (respective) monthly effective Federal Funds rates. $Y(t+2)$ denotes the log real GDP two quarters following the corresponding Federal Funds rate.

began an aggressive easing," as Goodfriend explains.

Thus far, there have been two major interest rate change: the first down and the second up. Both occur in the latter half of 1980. A third policy reversal was yet to come (in 1981), and again in the latter half of the year. It is perhaps not a coincidence that this occurs after nine months to one year after the last interest rate hike. Conventional wisdom holds that there is a delay in the real effects of monetary policy. In fact, the unemployment rate increased from 5.9% in November of 1979 to 7.8% in July of 1980. As effect of the massive interest rate spike, the contractionary policy in late 1979 began to have real effects in mid-1980, by late 1980 the Federal Reserve reduced interest rates again. These large policy reversals occurred in the second halves of 1980 and 1981.

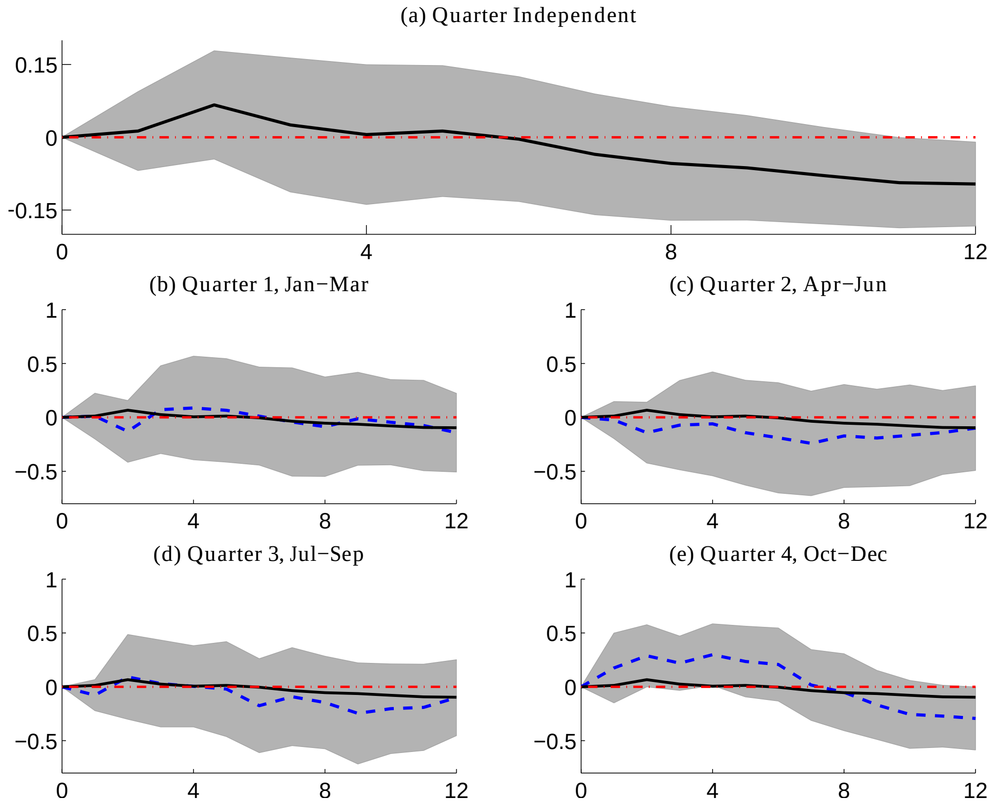
A number of VAR studies control for the outlier effect of atypical events. Blanchard and Perotti (2002) use dummy variables in their VAR study of fiscal policy to control for the effects of the 1975 tax cut. Irons and Faust (1999), in their study of the effect on U.S. monetary policy of the president's political party, use a dummy variable to control for the 1980 introduction of credit controls. Becksworth, et.al. (2009) control for the effects of spikes in the monetary base related to Y2K and 9/11 on the effect of monetary policy on long term interest rates

In light of the above discussion, we redo the analysis from section 3.3 excluding 1980 though 1983.[22] .These results appear in figure 4.[23]

[22] In the appendix, we show the result does not change if we use time dummies to control the effect of Volcker Disinflation

[23] Contemplating the above discussion of a policy regime change, a reader might think that merely dropping 1980-1983 is an insufficient remedy for the misspecification. This would be a reasonable point. To

Figure 4: Response of real GDP to an exogenous interest rate reduction, excluding 1980-1983, quarter dependent (by quarter of shock origination) and quarter independent



Notes: Each shock is a 1% decrease in the federal funds rate. Solid line is the quarter-independent response. Dashed line is the quarter-dependent response. Shaded region in (a) is 80% confidence interval computed based on sample standard deviations. Shaded regions in (b)-(e) are the quarter-dependent confidence interval. Sample is 1961-2002 and excludes 1980-1983.

address that, we did try estimation over the pre-1980 and post-1983 sub-periods separately; however, there are insufficient data to split the sample. For the pre-1980, for example, there are less than twenty observations for each season. There are not so many degrees for freedom left. As a consequence, the estimation of impulse responses suffers seriously from the badly-estimated lag coefficients. When we estimated the model, we could not compute standard errors on impulse responses because very often the bootstrapped impulse responses were explosive. As an attempt to account for the possible "regime-change" during the Volcker Disinflation period, we add time dummies and its interactions with other lag variables in the model. The result is reported in Appendix A.2. The result is very similar to what we obtain in the benchmark model here.

This subsection of our paper shows that timing effects of policy shocks are due to outliers, resulting from misspecification. By correcting part of the misspecification (by dropping 1980-1983), we find the timing effects disappear. If it were feasible to correct the entire misspecification, it is unlikely, although conceivable, that the timing effect would reappear.

Without the 1980-1983 period, the quarter-independent response shows a hump-shaped increase in real GDP following a interest rate reduction. This panel has the same qualitative features as the previous corresponding one with these few years included, i.e. figure 3(a). Next, figure 4(b)-(e) plots the impulse responses with quarter-dependence. Without these few years, there is no statistically significant systematic differences depending on the calendar quarter of the shock's origination. In fact, examining the point estimates, the fourth quarter generates the largest real effect of the monetary policy shock. On the other hand, quarter four had the smallest response when 1980-1983 was included.

The lack of robustness of the timing effects to small changes in the sample can be seen by looking at the corresponding D-statistics. The final two rows of table 1 provide the p-values for seasonal dependence in GDP impulse responses when the 1980-1983 years are excluded. The lowest p-value in these two rows is 0.258; thus, for each of the four different calendar quarters, the season-contingent impulse response is statistically indistinguishable from the season-independent one. The extreme interest rate changes between 1980 and 1983 appear to drive the timing effect.

Note in figure 4(a) is that there is no statistically significant effect of the shock on output. Although figure 4(a) finds that, for that specification, we find no significant effect in the quarter-independent model, this is not robust. Minor changes to the specification, sometimes result in a statistically significant effects on output.[24] Because of this lack of robustness, we do not draw a broad conclusion that without 1980-1983 money appears to be neutral. On the other hand, minor specification changes do not support the seasonality patterns described in existing research when these few years are included.

Appendix A.3 contains analysis of monetary policy shocks and spending shocks with alternative specifications. We find little evidence in favor of timing effect for either shocks. Here is a summary of these finding. For each of the alternative specifications, the impulse response to a monetary shock lies everywhere within the 80% confidence interval. There are four (out of thirty-two possible) cases where p-values for the D-statistics are less than 10%. These four, however, vary substantially across quarter of shock origination; thus, we see no systematic seasonal pattern. Also, none of the p-values are less than 5%. For the alternative specification of the spending shock, the result is very similar to the specification in the main text.

[24]See figures A.3 and A.5 in appendix A.3 for examples.

4 Conclusion

Based on this paper's search, the only evidence in U.S. data supporting season-contingent responses to shocks concerns monetary policy. This matches evidence in Oliveri and Tenreyro (2007). These effects disappear, however, if a few years in the early 1980s are excluded. Next, there is no substantial timing effect for the spending or revenue shock.

Put concretely:[25] there were twenty different tests, each corresponding to a different combination of impulse horizon, shock and calendar quarter; only one test rejected seasonal-independence at a 10% level and that test failed to reject at a 5% level.

There are three additional things worth noting.

First, one open question is whether one can reconcile a timing effect with respect to monetary policy shocks and an absence of timing effects with respect to other shocks. One possibility is to replace sticky wages, as offered by Oliveri and Tenreyro (2007), with rational but inattentive agents. In other contexts, researchers have recently used inattentiveness to explain the inertial behavior of inflation as well as the differential responses of variables to monetary policy and technology shocks.[26]

In terms of the timing effect, if agents were inattentive in a season-dependent way with respect to monetary policy shocks but their attentiveness had no seasonal dependence with respect to other shocks, then perhaps reconciliation could be achieved. This explanation would not appear to explain the imperative role of the Volcker disinflation in finding a timing effect for the monetary policy shock.

Second, while our paper documents the lack of corroborating evidence for season-contingent effects of macroeconomic shocks in U.S. data, Oliveri and Tenreyro (2008) find evidence using Japanese data. Their structural VARs show, in Japan, that monetary policy shocks that occur in the first half of the year have a greater effect than those in the second half. The paper focuses, as in Oliveri and Tenreyro (2007), on monetary policy shocks measured with a short-term interest rate in a period that includes the Volcker disinflation. Moreover, Oleveri and Tenreyro (2008) explain that in Japan there is greater flexibility in wages in the second relative to the first half of the year. This season-contingent wage rigidity could explain that season-contingent response of monetary policy. Their paper does not consider other macroeconomic shocks.

Third, one might look for timing effects for other shocks. One obvious candidate is a technology shock identified by a long-run restriction, as implemented in Fisher (2006), for

[25]That is, after putting aside the full sample monetary policy shocks.

[26]See, for example, Mackowiak and Wiederholt (2008), Paciello (2007) and Sims (2003).

example. We attempted this identification strategy in the course of our research. However, the estimated impulse responses have large standard errors, as discussed in Chari, et.al. (2008). This problem is exacerbated significantly when the sample size is effectively cut by three-fourths when the VAR is allowed to be season-dependent.

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Appendix

A.1 Data and Sample Period

For the analysis of spending shocks, the sample period is from 1960Q1 to 1996Q4. All the data are from the National Income and Product Accounts (NIPA). Nominal government spending is measured by the sum of nominal government consumption and nominal government investment. The nominal tax is measured by the current tax receipt. The data of nominal GDP is directly from NIPA. Each variable is divided by the GDP deflator to get its real value.

For the analysis on tax shocks, the data for real GDP is from NIPA. The data for tax changes is available on David Romer's website. The sample period is from 1945Q1 to 2007Q4.

For the analysis of monetary policy shocks, the sample period runs from 1966Q1 to 2002Q4. The data for the GDP deflator and real GDP are from NIPA. The data for the federal funds rate are from the Federal Reserve Bank of St. Louis website. Quarterly averages of the monthly data are used in the analysis. The data for spot commodity price index are from the Commodity Research Bureau website.

A.2 Using Dummies for the Volcker Disinflation Period

In this section, we consider an alternative method to treat the Volcker disinflation. Our new finding turns to be very similar to the result obtained in the "excluding 1980-1983" model in section 3.4.

The conduct of monetary policy and the behavior of the private sector changed dramatically during the Volcker Disinflation period. Hence, it is not reasonable to assume the underlying DGP remained unchanged. To account for the effect of the period of turmoil, we introduce a time dummy, X_t. It equals one when the current period is within 1979Q4 to 1983Q4, and equals zero otherwise. The seasonal structural VAR model becomes the following:

$$A_0^q Y_t^q = A^q(L) Y_{t-1}^q + D^q X_t^q + \Gamma^q(L) X_t^q Y_{t-1}^q + \epsilon_t^q, q = 1, 2, 3, 4. \tag{A.1}$$

In the system (A.1), the early part of the Volcker disinflation is treated as a different regime, and its effect is separated from "regular" monetary policy shocks. We use the dummy variable, X_t^q, and its iteractions with other explanatory variables, $X_t^q Y_{t-1}^q$, to account for the effects of this separate regime on macro variables. Such effects can be traced

Table A.1: P-values for differences in GDP impulse responses to monetary policy shock, with time dummy for 1979Q4 to 1983Q4

	Quarter of Shock Origination			
	First	Second	Third	Fourth
Monetary Policy Shock with time dummy				
Year 0-1 horizon	0.104	0.337	0.294	0.154
Year 2-3 horizon	0.265	0.465	0.116	0.290

Notes: Null hypothesis that quarter-contingent impulse response is identical to quarter-independent impulse response over various horizons. p-values computed via bootstrap.

out by analyzing the parameter estimates, $\hat{A}_0^q$, $\hat{A}^q(L)$, $\hat{D}^q$ and $\hat{\Gamma}^q(L)$. Here, we report only the results based on the analysis of "regular" monetary policy shocks.

Figure A.1 draws impulse responses to monetary policy shocks, along with the corresponding confidence intervals, after controlling for the effect of the Volcker disinflation. As in the "excluding 1980-1983" case from section 3.4 of the paper, we find the responses to monetary policy shocks do not show strong season-contingency. For each of the four calendar quarters, the estimated quarter-independent response lies well within the 80% confidence band of the quarter-dependent responses. Furthermore, the D-statistics reported in table A.1 also confirm this finding.

A.3 Result Based on Alternative Specifications

In our benchmark model, we find no evidence for timing effects of spending shocks or tax shocks. After removing the early years of Volcker Disinflation, the timing effect of monetary policy shocks disappear as well. In this section, we test the robustness of our benchmark result by using different model settings.

The finding in this section can be summarized as follow: for each of the alternative specifications, the impulse response to a monetary shock lies everywhere within the 80% confidence interval. There are four (out of thirty-two possible) cases where p-values for the D-statistics are less than 10%. These four, however, vary substantially across quarter of shock origination; thus, we see no systematic seasonal pattern. Also, none of the p-values are less than 5%. For the alternative specification of the spending shock, the result is very similar to the specification in the main text.

A.3.1 Monetary Policy Shocks

In the benchmark model, we assume there are stochastic trends in the GDP deflator and commodity prices, and we use growth rates of the two price series. In this section, we assume there are deterministic (linear) trends in the two series, and use linearly detrended series. This setting is the same as Olivei and Tenreyro (2007)'s benchmark model. Adopting their specification gives us similar results as our benchmark model. To save space, we only report the results when we remove the early years of Volcker Disinflation. Figure A.2 shows impulse responses of real GDP to monetary policy shocks in both the quarter-independent and the quarter-dependent models. In all four cases considered in the quarter-dependent model (panel (b) to (e)), the quarter-indepedent responses lie in the 80% confidence interval of the quarter-dependent impulse responses. Table A.2 reports the p-values of the D-test constructed in the main text. In seven out of eight cases studied, we cannot reject the null of no quarter-dependence at conventional levels. The only exception is for the longer horizon in the third quarter, where the p-value is $.057$. It does not match the timing of wage adjustment reported in Olivei and Tenrenryo. Furthermore, this small p-value does not indicate strong effect of monetary policy shocks, because it is due to an "output puzzle", i.e. output falls in response to expansionary monetary policy.

We are also concerned with if our results hinge on some particular observations. Therefore, we remove observations in 1979 along with observations from 1980 to 1983. The purpose is to further control for the effect of Volcker Disinflation, since Paul Volcker assumed the position in late 1979. It turns out including 1979 or not has little effect on our conclusion. Figure A.3 and A.4 report the results based on the Olivei and Tenreyrosetting and our benchmark setting respectively. There is no season-dependence in reponses to monetary policy shocks in each case. The D-tests in table A.2 further confirm this finding.

We include a measurement of commodity prices in the benchmark model to control the possible "price puzzle". For the sake of robustness, we estimate a three-variable model when commodity prices are excluded. An additional benefit of estimating a smaller system is that we estimate fewer parameters. This change does not alternate our conclusion that quarter-dependence disappears when the Volcker Disinflation period is removed. Impulse responses in this model are plotted in figure A.5. Just as in previous cases, none of the quarter-dependent responses are significantly different from the quarter-independent ones in this setting. Among p-values for the D-test reported in table A.2, we find the value for responses to the second quarter shock in the longer horizon

Table A.2: P-values for Differences in GDP Impulse Responses, by quarter and by shock

	Quarter of Shock Origination			
	First	Second	Third	Fourth
Monetary Policy Shock				
OT Setting but Excluding 1980-1983*				
Year 0-1 horizon	0.199	0.200	0.106	0.177
Year 2-3 horizon	0.297	0.669	0.057	0.108
OT Setting but Excluding 1979-1983				
Year 0-1 horizon	0.258	0.188	0.104	0.437
Year 2-3 horizon	0.560	0.065	0.149	0.056
*DH Setting** but Excluding 1979-1983*				
Year 0-1 horizon	0.350	0.286	0.368	0.332
Year 2-3 horizon	0.568	0.500	0.267	0.412
Three Variables Excluding 1980-1983				
Year 0-1 horizon	0.439	0.251	0.376	0.223
Year 2-3 horizon	0.714	0.077	0.506	0.166
Spending Shock with Five Variables				
Year 0-1 horizon	0.204	0.171	0.067	0.274
Year 2-3 horizon	0.363	0.320	0.040	0.396

Notes: Null hypothesis that quarter-contingent impulse response is identical to quarter-independent impulse response over various horizons. p-values computed via bootstrap. *OT setting denotes the benchmark model specification used by Olivei and Tenreyro (2007). **DH setting denotes our benchmark model specification.

is close to 5%. Nevertheless, this small p-value does not imply that real GDP responds strongly to the monetary policy shock, either. There is an ”output-puzzle” related to the second quarter shocks, which itself is not significantly different from zero. The sign reversal between the quarter-independent and the quarter-dependent model causes the small p-value.

As a summary for the robustness check for monetary policy shocks we have conducted so far, none of the plotted responses show evidence in favor of seasonal-contigent effect from various macro shocks. In table A.2, three p-values (out of possible 32) have been less than 10%. However, there is no systematic pattern across specifications. One is for quarter two, one is for quarter three and one is for quarter four. None of them is less than 5%. We find similar evidence of seasonal-independence in these alternative specifications as in the benchmark model.

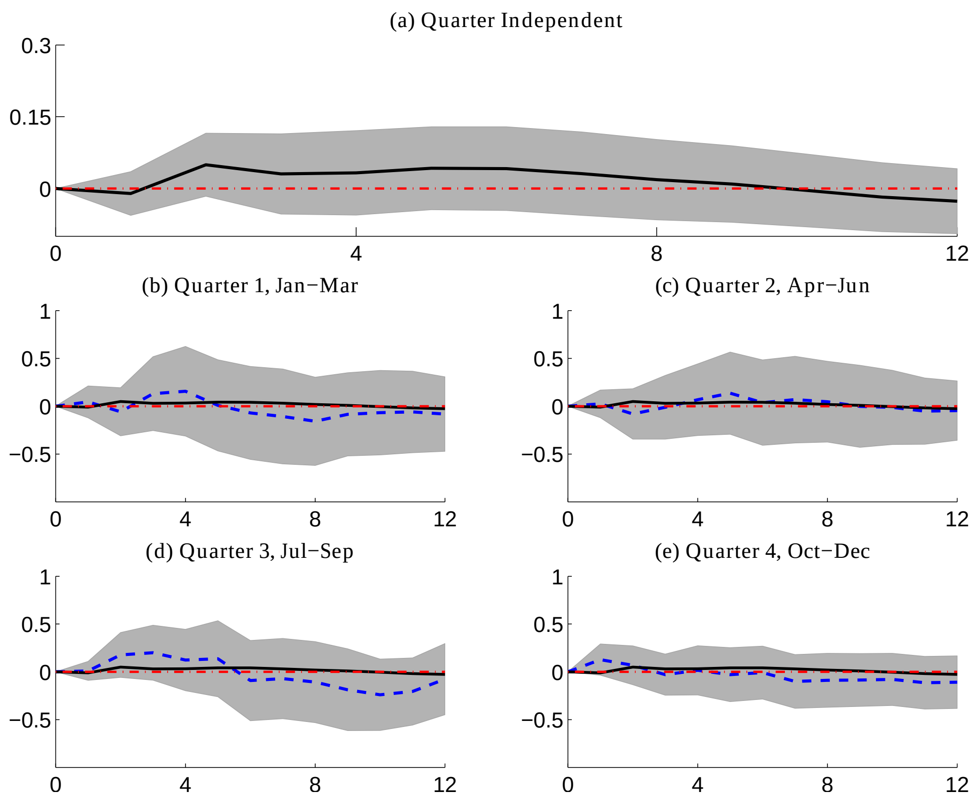
Also, one aspect of the benchmark specification, when we exclude 1980-1983, is that there is no statistically significant output response at any horizon in the quarter-independent model. A reader may ask, does this mean that the early Volcker disinflation is necessary to find that money is not neutral? Although it is true in the benchmark case, this finding is not robust to alternative specifications. Figures A.3 and A.5 give two alternative specifications where the early Volcker years are excluded and we do see that money is not neutral.

A.3.2 Spending Shocks

Next, we consider robustness with respect to spending shocks. We extend the VAR to include five variables by including linearly detrended real consumption and linearly detrended real investment. In this model, the responses of real GDP show similar evidence of seasonal-indepdence as the benchmark model does. The plots of impulse responses are shown in figure A.6, and the p-values of the D-test are listed in table A.2.[27]

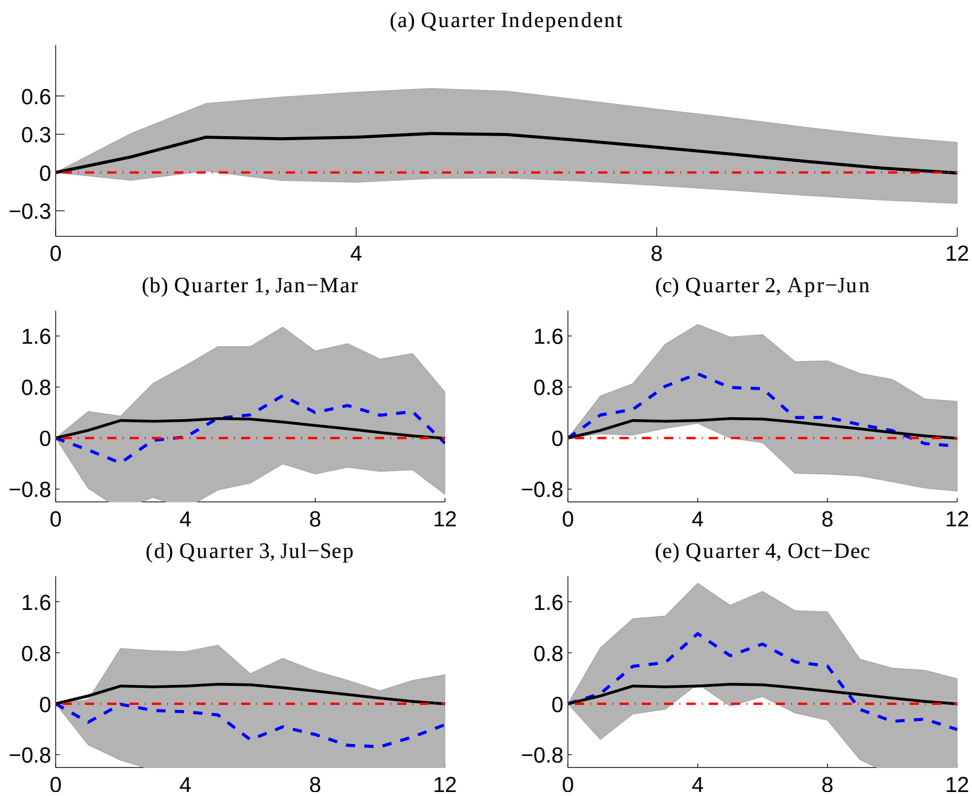
[27]The response of real GDP shows no seasonal pattern, but responses of real investment (not plotted) dip down in responses to the third quarter shock, although this finding is not statistically significant.

Figure A.1: Response of real GDP to an exogenous interest rate reduction, with time dummy for 1979Q4 to 1983Q4, quarter dependent (by quarter of shock origination) and quarter independent



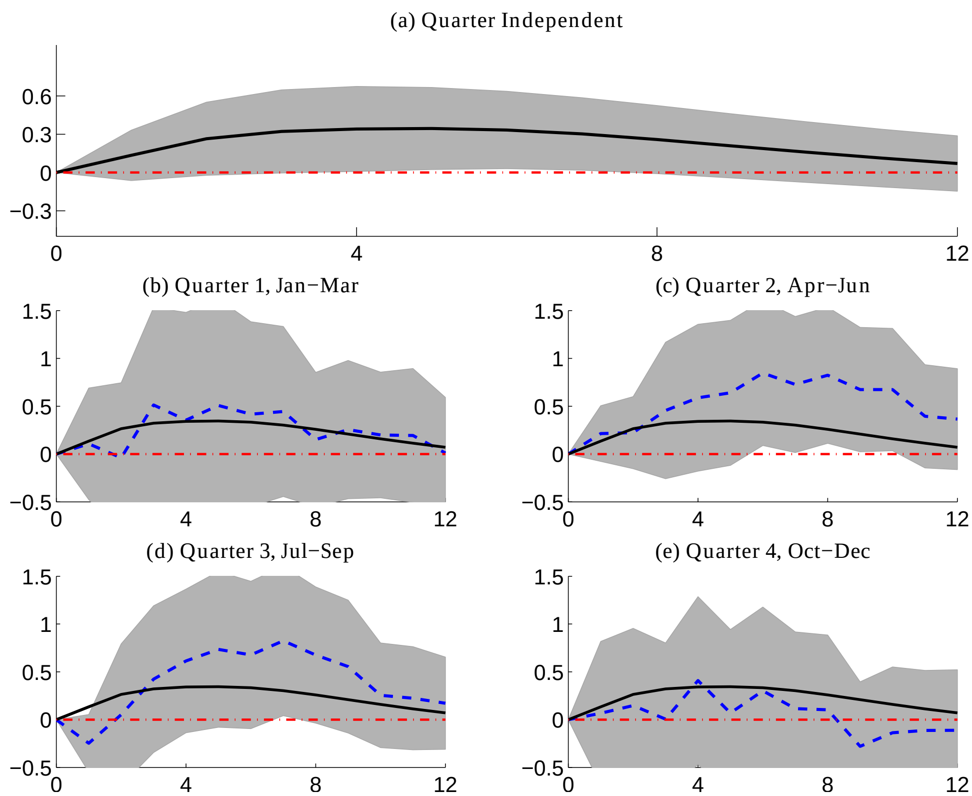
Notes: Each shock is a 1% decrease in the federal funds rate. Solid line is the quarter-independent response. Dashed line is the quarter-dependent response. Shaded region in (a) is 80% confidence interval computed based on sample standard deviations. Shaded regions in (b)-(e) are the quarter-dependent confidence interval. Sample is 1966-2002.

Figure A.2: Response of real GDP to an exogenous interest rate reduction, with 1980Q1 to 1983Q4 removed, Olivei and Tenrenyro setting, quarter dependent (by quarter of shock origination) and quarter independent



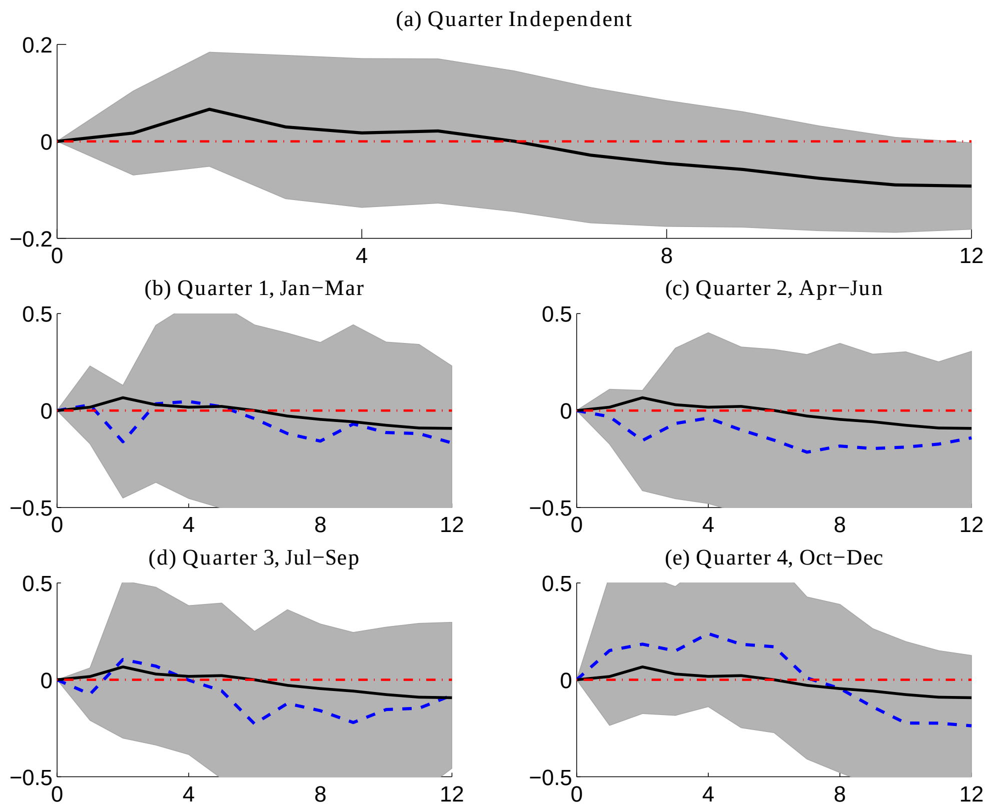
Notes: Each shock is a 1% decrease in the federal funds rate. Solid line is the quarter-independent response. Dashed line is the quarter-dependent response. Shaded region in (a) is 80% confidence interval computed based on sample standard deviations. Shaded regions in (b)-(e) are the quarter-dependent confidence interval. Sample is 1966-2002.

Figure A.3: Response of real GDP to an exogenous interest rate reduction, with 1979Q1 to 1983Q4 removed, Olivei and Tenreyro setting, quarter dependent (by quarter of shock origination) and quarter independent



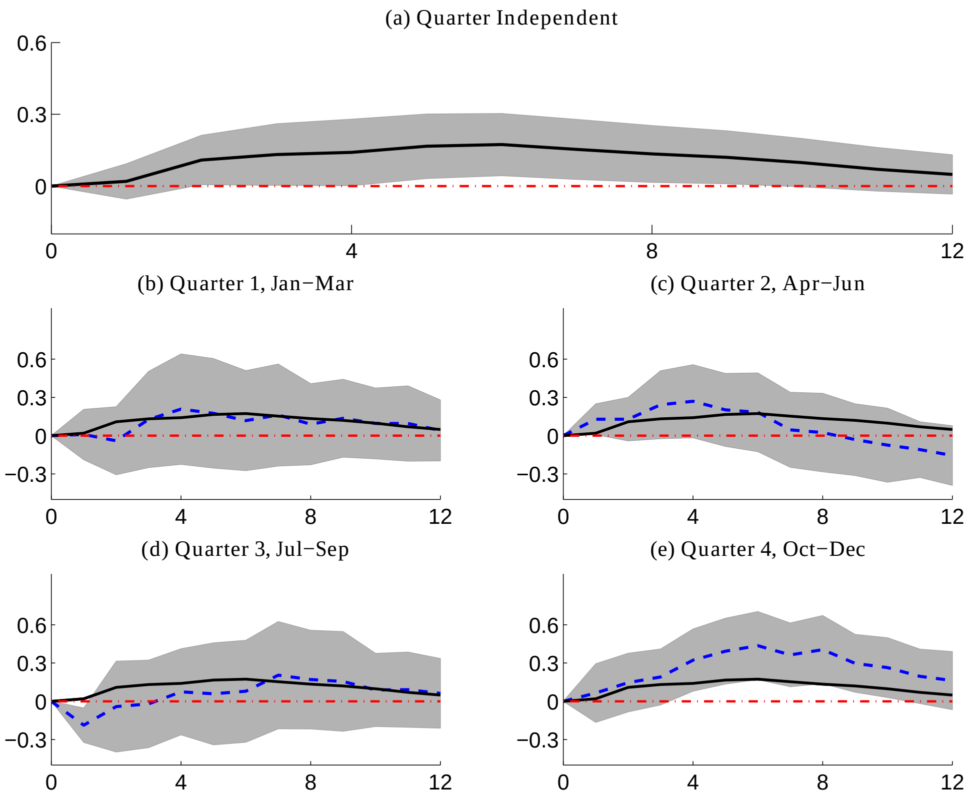
Notes: Each shock is a 1% decrease in the federal funds rate. Solid line is the quarter-independent response. Dashed line is the quarter-dependent response. Shaded region in (a) is 80% confidence interval computed based on sample standard deviations. Shaded regions in (b)-(e) are the quarter-dependent confidence interval. Sample is 1966-2002.

Figure A.4: Response of real GDP to an exogenous interest rate reduction, with 1979Q1 to 1983Q4 removed, benchmark setting, quarter dependent (by quarter of shock origination) and quarter independent



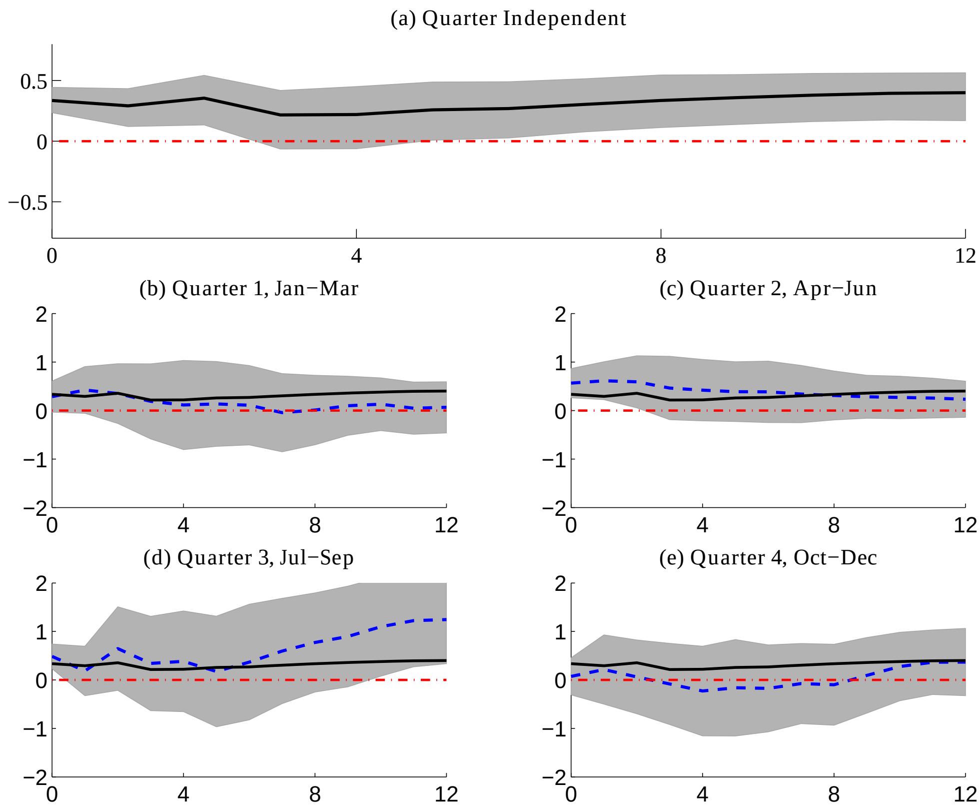
Notes: Each shock is a 1% decrease in the federal funds rate. Solid line is the quarter-independent response. Dashed line is the quarter-dependent response. Shaded region in (a) is 80% confidence interval computed based on sample standard deviations. Shaded regions in (b)-(e) are the quarter-dependent confidence interval. Sample is 1966-2002.

Figure A.5: Response of real GDP to an exogenous interest rate reduction, with 1980Q1 to 1983Q4 removed, three-variable system, quarter dependent (by quarter of shock origination) and quarter independent



Notes: Each shock is a 1% decrease in the federal funds rate. Solid line is the quarter-independent response. Dashed line is the quarter-dependent response. Shaded region in (a) is 80% confidence interval computed based on sample standard deviations. Shaded regions in (b)-(e) are the quarter-dependent confidence interval. Sample is 1966-2002.

Figure A.6: Response of real GDP to exogenous spending increase, five-variable system, quarter dependent (by quarter of shock origination) and quarter independent



Notes: Each shock is a 1% increase in government spending. Solid line is the quarter-independent response. Dashed line is the quarter-dependent response. Shaded region in (a) is the 80% confidence interval computed based on sample standard deviations. Shaded regions in (b)-(e) are the quarter-dependent confidence intervals. Sample is 1960-1996.