Measuring Fiscal Shocks in Structural VARs Using Narrative Data

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1 Introduction

We describe a methodology that integrates the narrative approach to the identification of macroeconomic shocks into existing structural VAR settings while allowing for measurement error in the narrative measures of the shocks of interest. We apply our methodology to the fiscal VAR setup of Blanchard and Perotti (2002) and make use of the tax shocks constructed by Romer and Romer (2009). We find that the macroeconomic effects of unanticipated changes in taxes in the United States are significantly larger than in the typical application of the Blanchard and Perotti (2002) identification strategy. A cut in total federal tax revenues of one percent of GDP raises GDP by 2 percent in the first quarter and has a maximum output effect of 3 percent after one to two years. In contrast, the Blanchard and Perotti (2002) identification restrictions result in an impact multiplier of 0.4 percent and a maximum multiplier of 1.3 percent. We also use newly constructed data that allows identification of the distinct effects of personal income and corporate income tax changes. An unanticipated tax reform decreasing personal income taxes, but leaving cyclically adjusted corporate income taxes unchanged, has an impact GDP multiplier of 0.8 and a maximum GDP multiplier of 1.9. The corresponding numbers for a corporate tax reform are 5.7 and 6.7. Our estimates are statistically significantly different (at the 5% level) from zero and from the Blanchard and Perotti (2002) point estimates, even after accounting for uncertainty in both identification and measurement.

Our results point to a single explanation for the discrepancy in estimates between the two identification schemes. Existing fiscal SVAR approaches require either explicit or implicit assumptions regarding the values of certain structural elasticities of fiscal variables to other macroeconomic variables in order to identify the structural response to fiscal shocks. Caldara (2010) shows how the dispersion in estimates of tax multipliers in SVAR settings mostly reflect differences in untestable assumptions regarding the elasticity of tax revenues to GDP. In the Blanchard and Perotti (2002) approach, for instance, the assumed decline in tax revenues in response to a 1 percent drop in GDP is typically around 2 percent. The motivation for using values in this range is that they arise as estimates from OECD or similar studies that combine institutional features of the US tax code with estimates of the sensitivity of the tax base to fluctuations in economic activity. Our approach of using narrative data in SVARs does not require any assumptions about the output elasticities of tax revenues. Our estimate for the output elasticity of total federal tax revenues is 3.1 instead of 2. This difference is statistically significant at the 5% level and fully explains the much larger response of economic activity to a tax shock with our identification scheme. We also estimate the output elasticities for personal and corporate income tax revenues individually and find them to be significantly higher than previous estimates in the literature.

There are at least two possible reasons for why we find a higher cyclical sensitivity of tax revenues than the OECD-based work of for instance van den Noord (2000) or than others studies such as Cohen and Follette (2000) and Follette and Lutz (2010), whose methodology is according to CBO (2010) similar to the methodology of the Congressional Budget Office. The estimates derived in these studies are based on the assumption of a tax code that remains invariant to economic activity. Therefore, their usage is only valid in the presence of decision lags of at least one quarter, whereas our approach makes no such assumption. To the extent that countercyclical measures reduce tax liabilities within a quarter, the true output elasticity of tax revenues will be higher. Another objection to using estimates from the OECD or CBO methodologies in an SVAR setting is that they are derived from reduced form estimates of the sensitivity of the various tax bases to GDP. If exogenous variations in tax policy move the base and GDP in opposite directions, than the reduced form elasticity will underestimate the true elasticity. To assess the plausibility of our higher estimates, which are based on US data from 1950 to 2006, we compare the predicted endogenous response of federal tax revenues to the Great Recession across identification schemes. Our estimates entirely explain the actual drop in tax revenues observed from 2007 to 2010 as resulting from purely cyclical forces, whereas the typical Blanchard and Perotti (2002) application that assumes lower cyclical sensitivity of tax revenues substantially underpredicts the drop in total revenues and its components. Thus, our results have important implications for discerning the effects of systematic and discretionary fiscal policy measures as well as for revenue forecasting.

Various recent studies have used the data of Romer and Romer (2009) to estimate the dynamics effects of tax policy shocks. In various uni- or multivariate settings, Romer and Romer (2010) and Mertens and Ravn (2010a) find effects that are quantitatively similar to those in this paper one to three years after a shock, but the impact multipliers are generally smaller. Favero and Giavazzi (2010), on the other hand, estimate dynamic effects that are smaller and very similar to Blanchard and Perotti (2002). One main difference between all these studies is the empirical specification of the reduced form transmission mechanism. Charhour, Schmitt-Grohé and Uribe (2010) conduct a Monte Carlo study using a DSGE model as data generating process to compare the various time series specifications that use the Romer and Romer (2009) data, as well as the Blanchard and Perotti (2002) specification. They find that, conditional on correct identification, all these specifications on average accurately uncover the true theoretical response to an unanticipated change in taxes. Their conclusion is that the dispersion in estimates in the literature must be due either to small sample uncertainty or to failure to identify the same tax shocks.

We are able to rule out small sample uncertainty as an explanation for the difference between our version of the narrative identification scheme and the Blanchard and Perotti (2002) approach. We are also able to reconcile our results with those of Romer and Romer (2010), Mertens and Ravn (2010a) and in particular, Favero and Giavazzi (2010). A correct interpretation of the tax multipliers estimated in these studies requires that the narrative measures of tax shocks are meaningfully scaled and are measured with negligible error. It seems prudent to consider how possible error in measurement affects the findings. Our methodology, while requiring the same exogeneity assumptions as previous studies, produces correct impulse responses estimates as long as the measures are general linear combinations of the structural shocks of interest and potentially contemporaneously correlated errors in measurement. We estimate the statistical reliability of the series constructed by Romer and Romer (2009) and our newly constructed disaggregrated shock measures and find it is high but imperfect. The extent of the measurement error that we find produces biases in the point estimates of Romer and Romer (2010), Mertens and Ravn (2010a) and Favero and Giavazzi (2010) that make them consistent with ours. The estimation procedure proposed by Perotti (2011) is also robust to additive classical measurement error in the narrative tax shock measure. Consistent with our findings, he obtains estimates of tax multipliers that are relatively higher than Blanchard and Perotti (2002) or Favero and Giavazzi (2010). The disadvantages of his approach relative to ours is that it requires additional identification assumptions as well as a measure of shocks that are on the same scale as the true shocks to tax revenues.

While our analysis arguably constitutes an improvement in the measurement of fiscal shocks and offers at least a partial reconciliation of previous results in literature, there are many issues that we do not resolve in this paper. We are careful to focus on tax changes that were implemented shortly after being legislated. The most important assumption underlying the analysis is the exogeneity of these tax shock measures. While we and others consistently reject the hypothesis that these shocks are predictable from past observations of macroeconomic aggregates, contemporaneous exogeneity remains an untestable assumption. Leeper, Walker and Yang (2008) raise other theoretical issues regarding standard VAR-based approaches if there also other fiscal shocks that are anticipated well in advance, for instance because of long implementation lags. We discuss several reasons why these are not likely to be of grave concern in our application. More serious are problems related to the precise nature of the structural tax shocks that we estimate. While actual changes in tax policy, exogenous or endogenous, are plentiful as documented by Romer and Romer (2010), they take on many different forms. They are comprised of changes in marginal tax rates affecting individual, corporate or employment income, changes in numerous deductions or credits, excise and production taxes, and others. All of these have potentially unique effects that our broad classification into shocks affecting personal and corporate income taxes, which is dictated by data availability, is unable to isolate. This remains an important challenge for future work.

2 Estimation Strategy

Our point of departure is the traditional structural vector autoregression (SVAR). In this section we show how a narrative measure of a shock of interest provides identification restrictions that are alternatives to recursive restrictions, e.g. Fatás and Mihov (2001), short-run restrictions à la Blanchard and Perotti (2002) or sign restrictions as in Mountford and Uhlig (2009) or Pappa (2009). One key objective is to allow comparison across all identification schemes while keeping the reduced form transmission mechanism constant. Another advantage of our approach is that it quantifies the quality of identification through an estimate of the statistical reliability of the narrative data. Existing narrative studies almost always assume close to perfect reliability in measurement and differ from SVARs in both identification strategy as well as time series specification, obscuring the underlying reasons for any divergence in results.

Our primary interest in this paper is the estimation of tax shocks, but our methodology is more broadly applicable to other macroeconomic shocks for which event study data is available. In section 2 .1 we describe the estimation method in more general terms, while section 2 .2 discusses its implementation in the context of fiscal SVARs.

2 .1 General Framework

Main Assumptions The $n \times 1$ stationary vector of observables y_t has a vector autoregressive representation,

$$y_t \;=\; \delta' X_t + \mathcal{B}\varepsilon_t, \tag{1}$$

where $X_t = [y'_{t-1}\ ,\ldots,\ y'_{t-r}]'$ is the $rn \times 1$ vector of lagged observations, δ is an $rn \times n$ matrix of coefficients, $\mathcal{B}$ is an $n \times n$ nonsingular matrix of coefficients, and ε_t is an $n \times 1$ vector of structural shocks with $E[\varepsilon_t] = 0$, $E[\varepsilon_t \varepsilon'_t] = I_n$, $E[\varepsilon_t \varepsilon'_s] = 0$ for $s \neq t$. The specification in (1) omits deterministic terms for notational brevity. The $n \times 1$ vector u_t of reduced form residuals is related to the structural shocks by

$$u_t \;=\; \mathcal{B}\varepsilon_t, \tag{2}$$

Estimates of δ and $E[u_t u'_t]$ are straightforward to obtain by OLS and the requirement that $E[u_t u'_t] = \mathcal{B}\mathcal{B}'$ provides $n(n+1)/2$ independent identifying restrictions. Since the objective is partial or point identification of the elements of at least one of the columns of $\mathcal{B}$, more restrictions are needed.

The identifying restrictions we use are based on the availability of a $k \times 1$ vector of proxy variables m_t. Consider the partition $\varepsilon_t = [\varepsilon'_{1t}\ \varepsilon'_{2t}]'$, where ε_{1t} is the $k \times 1$ vector containing the shocks of interest and the $(n-k) \times 1$ vector ε_{2t} contains all other $n-k$ shocks. Without loss of generality we assume that $E[m_t] = 0$. More importantly, we assume that m_t satisfies the following properties,

$$E[m_t \varepsilon_{1t}'] = \Phi \tag{3}$$

$$E[m_t \varepsilon_{2t}'] = 0 \tag{4}$$

$$E[m_t X_t'] = 0 \tag{5}$$

where Φ is an unknown nonsingular $k \times k$ matrix. The first condition states that the proxy variables are correlated with the shocks of interest, but the correlation does not need to be perfect. The second condition requires that the proxy variables are uncorrelated with all other shocks. The third condition requires that the proxy variables are uncorrelated with lagged values of y_t.

Consider the following partitioning of $\mathcal{B}$,

$$\mathcal{B} = \left[\begin{array}{c|c} \underset{n\times k}{\beta_1} & \underset{n\times(n-k)}{\beta_2} \end{array} \right] \;,\; \beta_1 = \left[\begin{array}{c|c} \underset{k\times k}{\beta_{11}'} & \underset{k\times(n-k)}{\beta_{21}'} \end{array} \right]' \;,\; \beta_2 = \left[\begin{array}{c|c} \underset{(n-k)\times k}{\beta_{12}'} & \underset{(n-k)\times(n-k)}{\beta_{22}'} \end{array} \right]' \tag{6}$$

with nonsingular β_{11} and β_{22}. Equations (1) through (5) imply that

$$\Phi \beta_1' = \Sigma_{mu'} \tag{7}$$

where henceforth we use the notation $\Sigma_{AB} \equiv E[A_t B_t]$ for any random vector or matrix A_t and B_t. Equation (7), which is of dimension $n \times k$, provides additional identifying restrictions but also depends on the k^2 unknown elements of Φ. Because we do not wish to make any assumptions on Φ, equation (7) provides only $(n-k)k$ new identification restrictions. Partitioning $\Sigma_{mu'} = [\Sigma_{mu_1'} \;\; \Sigma_{mu_2'}]$, where $\Sigma_{mu'_1}$ is $k \times k$ and $\Sigma_{mu'_2}$ is $k \times (n-k)$, these identifying restrictions can be expressed as

$$\beta_{21}\beta_{11}^{-1} = (\Sigma_{mu'_1}^{-1}\Sigma_{mu'_2})' \tag{8}$$

where the right hand side is a function of moments of observable variables and is thus independent of Φ. In practice, estimation proceeds in three stages:

- *First Stage*: Estimate the reduced form VAR by OLS.
- *Second Stage*: Regress the reduced form VAR residuals on m_t and premultiply the estimated coefficients of the last $n-k$ equations by the inverse matrix of estimated coefficients from the first k equations to get an estimate for $\beta_{21}\beta_{11}^{-1}$.
- *Final Stage*: Use the estimates from the previous stages to estimate the objects of interest, if necessary in combination with further identifying assumptions.

The final stage depends on the precise application and we will be more specific about its implementation in fiscal SVARs in the next section.

Reliability It is useful to view our setup in the context of an augmented system consisting of the SVAR in (1) and the following linear measurement equation,

$$m_t = \Phi\varepsilon_{1t} + \upsilon_t \tag{9}$$

where υ_t is a $k \times 1$ vector of measurement errors with $E[\upsilon_t] = 0$, $E[\upsilon_t\upsilon_t'] = \Sigma_{\upsilon\upsilon'}$ and $E[\upsilon_t\upsilon_s'] = 0$ for $s \neq t$. The combination of (1) and (9) is a system of structural equations with latent variables, as discussed in Bollen (1989). In Appendix A, we show that the three stage estimation procedure corresponds to method of moments estimation of the larger system. A useful diagnostic tool in the presence of measurement error is the $k \times k$ reliability matrix,

$$\Lambda = (\Phi\Phi' + \Sigma_{\upsilon\upsilon'})^{-1}\Phi\Phi' \tag{10}$$

which is a generalization of the reliability ratio of a scalar measurement. When $k = 1$, Λ is just the fraction of the variance in the measured variable that is explained by the variance of the latent variable or also the squared correlation between the measure and the true structural shock of interest. When $k \geq 1$, the smallest eigenvalue of Λ corresponds to the smallest scalar reliability of any linear combination of m_t, see Gleser (1992). When an estimate of Λ is available, it can be used for testing the hypothesis that some linear combination of m_t has scalar reliability zero. It provides a metric for judging how closely related the proxy variables are to the true shocks, and therefore for the estimability of the structural parameters. In our fiscal SVAR application, the reliability matrix will be identified as part of the final estimation stage.

Proxy variables The requirement that m_t is not predictable by lagged values of y_t in equation (5) is not strictly necessary to obtain an estimate of $\beta_{21}\beta_{11}^{-1}$ (see Appendix A). However, we impose it for most of the analysis mainly for practical reasons. The type of applications that suits our empirical model best is one where the proxy variables are narrative measures of structural macroeconomic shocks. Typically, these measures contain many zero observations, which in our SVAR setting are interpreted as missing observations. Since the conditioning set X_t is usually large, obtaining a precise estimate of $E[m_t X_t']$ with many missing observations can be difficult. One reason for imposing (5) is therefore efficiency. Also, existing narrative analyses almost always rely on (5) for identification, so in that respect our setup is not more restrictive. Moreover, since condition

(5) constitutes a set of overidentifying restrictions, it is usually testable. Finally, assuming (5) implies that the reduced form transmission mechanism in the measurement error model is identical to existing SVARs, which facilitates comparison. The fact that m_t has missing observations affects the second estimation step but has no consequences other than a loss in efficiency in all estimators that depend on the estimate of $\beta_{21}\beta_{11}^{-1}$. If m_t contains few or no missing observations, a generally preferable approach is just to add m_t as variables in the SVAR directly. Our empirical model offers a more parsimoniously parametrized alternative for cases where this not is feasible in practice.

2 .2 Identification in Fiscal SVARs

In this section we show how various objects of interest can be identified in fiscal SVAR settings when a measure m_t of tax shocks is available. As Blanchard and Perotti (2002), we use a specification that includes tax revenues T_t, government spending G_t and output Y_t, such that $y_t = [T_t' \; G_t \; Y_t]'$. We allow for different types of tax shocks, such that T_t is generally a $k \times 1$ vector that may contain different subcomponents of total tax revenues. Unexpected changes in the endogenous variables $u_t = [u_t^{T\prime} \; u_t^G \; u_t^Y]'$ are related to the structural shocks $\varepsilon_t = [\varepsilon_t^{T\prime} \; \varepsilon_t^G \; \varepsilon_t^Y]'$ by

$$\begin{aligned} u_t^T &= \theta_G \sigma_G \varepsilon_t^G + \theta_Y u_t^Y + \Sigma_T \varepsilon_t^T \\ u_t^G &= \gamma_T' \Sigma_T \varepsilon_t^T + \gamma_Y u_t^Y + \sigma_G \varepsilon_t^G \\ u_t^Y &= \zeta_T' u_t^T + \zeta_G u_t^G + \sigma_Y \varepsilon_t^Y \end{aligned} \tag{11}$$

where Σ_T is a $k \times k$ matrix with potential nonzero off-diagonal elements, θ_G, θ_Y, γ_T and ζ_T are $k \times 1$ vectors and the remaining parameters are scalars. There are in total n^2 unknown structural parameters, where $n = k + 2$. The impact matrix $\mathcal{B}$ consists of

$$\beta_{11} = \left(I_k + \theta_Y \frac{\zeta_T' + \zeta_G \gamma_T'}{1 - \gamma_Y \zeta_G - \zeta_T' \theta_Y} \right) \Sigma_T \quad , \quad \beta_{12} = \left[\begin{array}{cc} \left(\theta_G + \theta_Y \frac{\zeta_T' \theta_G + \zeta_G}{1 - \gamma_Y \zeta_G - \zeta_T' \theta_Y} \right) \sigma_G & \frac{\theta_Y}{1 - \gamma_Y \zeta_G - \zeta_T' \theta_Y} \sigma_Y \end{array} \right]$$

$$\beta_{21} = \left[\begin{array}{c} \frac{\gamma_Y \zeta_T' + (1 - \zeta_T' \theta_Y) \gamma_T'}{1 - \gamma_Y \zeta_G - \zeta_T' \theta_Y} \\ \frac{\zeta_T' + \zeta_G \gamma_T'}{1 - \gamma_Y \zeta_G - \zeta_T' \theta_Y} \end{array} \right] \Sigma_T \quad , \quad \beta_{22} = \left[\begin{array}{cc} \left(1 + \gamma_y \frac{\zeta_T' \theta_G + \zeta_G}{1 - \gamma_Y \zeta_G - \zeta_T' \theta_Y} \right) \sigma_G & \frac{\gamma_Y}{1 - \gamma_Y \zeta_G - \zeta_T' \theta_Y} \sigma_Y \\ \frac{\zeta_T' \theta_G + \zeta_G}{1 - \gamma_Y \zeta_G - \zeta_T' \theta_Y} \sigma_G & \frac{1}{1 - \gamma_Y \zeta_G - \zeta_T' \theta_Y} \sigma_Y \end{array} \right]$$

The primary objective in this paper is estimation of impulse responses functions to meaningful shocks to tax revenues. Other objects of interest are the reliability matrix Λ and the structural parameters in (11). We distinguish between two groups of parameters: the k^2 elements of Σ_T and the remaining $n^2 - k^2$ parameters which are collected in a vector Π. Table 1 summarizes the restrictions required to identify each of these objects in four different cases. In Case I and II, T_t contains just total tax revenues as in Blanchard and Perotti (2002), such that $k = 1$. Case I describes which parameters are restricted by the Blanchard and Perotti (2002) scheme, while Case II provides the restricted parameters in our narrative SVAR.[1] Case III and IV allow for different types of tax shocks, such that $k > 1$. In appendix B, we formally prove identification under the stated assumptions for all cases.

Case I and II ($k = 1$) The Blanchard Perotti SVAR restricts the contemporaneous responses of government spending to tax shocks, γ_T, and cyclical output movements, γ_Y, as well as the elasticity of tax revenues to output, θ_Y . Based on the assumption of decision lags in fiscal policy, $\gamma_T = \gamma_Y = 0$ while θ_Y is set to an outside estimate of the output elasticity of revenues. These three assumptions allow identification of impulse responses, Σ_T and the remaining elements of Π. In the narrative

[1] We restrict attention to a comparison with Blanchard and Perotti (2002). For a comparison of Blanchard and Perotti (2002) with the alternative SVAR approaches in the literature, see Caldara and Kamps (2008).

Table 1: Identifying Restrictions

	Objects Identified	**Blanchard Perotti SVAR** Restrictions	#	**Narrative SVAR** Restrictions	#
$k = 1$		*Case I*		*Case II*	
	Response To Tax Shock	$\theta_Y, \gamma_T, \gamma_Y$	3	$\beta_{21}\beta_{11}^{-1}$	2
	Scalar Reliability Λ	–		$\beta_{21}\beta_{11}^{-1}$	2
	Structural parameters Π	$\theta_Y, \gamma_T, \gamma_Y$	3	$\beta_{21}\beta_{11}^{-1}, \gamma_Y$	3
	Structural parameter Σ_T	$\theta_Y, \gamma_T, \gamma_Y$	3	$\beta_{21}\beta_{11}^{-1}, \gamma_Y$	3
$k > 1$		*Case III*		*Case IV*	
	Responses To Orthogonal Tax Shocks	$\theta_Y, \gamma_T, \gamma_Y$	$2k+1$	$\beta_{21}\beta_{11}^{-1}, \gamma_Y$	$2k+1$
	Reliability Matrix Λ	–		$\beta_{21}\beta_{11}^{-1}, \gamma_Y$	$2k+1$
	Structural parameters Π	$\theta_Y, \gamma_T, \gamma_Y$	$2k+1$	$\beta_{21}\beta_{11}^{-1}, \gamma_Y$	$2k+1$
	Structural parameters Σ_T	Not identified		Not identified	

SVAR instead only two restrictions are required to identify the impulse response to a tax shock. These come from the estimate for $\beta_{21}\beta_{11}^{-1}$ obtained in the second estimation stage of section 2 .1 and they do not directly restrict the values of γ_T, γ_Y or θ_Y. Hence there is no assumption regarding decision lags or the output elasticity of tax revenues. The same two restrictions are also sufficient to estimate the reliability Λ of the narrative measure m_t. Identification of the structural parameters Π and Σ_T however requires one additional condition. For this purpose, we assume that decision lags in policymaking preclude government spending from reacting contemporaneously to cyclical output movements, i.e. $\gamma_Y = 0$.

Case III and IV ($k > 1$) When there are multiple shocks to taxes, we do not identify Σ_T in either identification scheme. This requires, in our view, arbitrary assumptions on how for instance personal income taxes respond contemporaneously to unanticipated changes in corporate taxes and vice versa. Assuming Σ_T is diagonal also seems unreasonable given that our narrative data below reveals significant correlation between changes in different types of taxes in the US. Therefore, when $k > 1$, it is not possible to estimate impulse responses to the tax shocks in ε_t^T. When the other parameters in Π are know, however, it is still possible to trace out the responses to linear combinations of ε_t^T with a given desired effect on the tax revenue components. We estimate responses to orthogonal tax shocks. For a given tax type k, these are unanticipated shocks to the tax code that affect cyclically adjusted taxes revenues of a certain component while leaving the cyclically adjusted tax revenues of all other $k-1$ components unchanged. The contemporaneous impact to orthogonal tax shocks on y_t is $\beta_1\Sigma_T^{-1}$. For the Blanchard Perotti SVAR this impact matrix is identified given restrictions on $\gamma_T = \gamma_Y = 0$ and θ_Y, which is now a vector containing the output elasticities of the individual tax revenue components. For the narrative SVAR with $k > 1$, this impact matrix is identified given an estimate of $\beta_{21}\beta_{11}^{-1}$ and the additional restriction that $\gamma_Y = 0$.

With the same assumptions, all elements of Π are identified in both cases and so is the reliability matrix in the narrative SVAR.

3 Empirical Results

For the variables in y_t, we use US data from the Bureau of Economic Analysis' NIPA tables on federal tax revenues, federal government consumption and investment expenditures and output, all in logged real per capita terms, and for the sample 1950Q1 to 2006Q4.[2] All VAR specifications have four lags of the endogenous variables and include a constant, linear and quadratic trends and a dummy for 1975Q2, as in Blanchard and Perotti (2002). Section 3 .1 discusses the specification that estimates shocks to total federal tax revenues, whereas section 3 .2 distinguishes between shocks to federal personal income and corporate income tax revenues.

3 .1 The Specification with Total Tax Revenues

3 .1.1 Basic Findings

Our measure m_t of exogenous shocks to tax revenues is based on Romer and Romer (2009), who record the projected impact on tax liabilities of legislated changes to the tax code from a variety of government sources. Their selection of exogenous changes in tax liabilities is based on a classification of the motivation for the tax change either as ideological or as arising from inherited deficit concerns. There are at least two possible reasons why their measure may fail to satisfy the

[2] Output is GDP in line 1 from Table 1.1.5; government spending is Federal Government Consumption Expenditures and Gross Investment in line 6 from Table 3.9.5; Total tax revenue is the sum of Federal Current Tax Receipts in line 2 of Table 3.2 and Contributions for Government Social Insurance in line 11 of Table 3.2 less corporate income taxes from Federal Reserve Banks (line 8 in Table 3.2). All series are deflated by the GDP deflator in line 1 from Table 1.1.9 and by the civilian population ages 16 obtained from Francis and Ramey (2009). The NIPA data was last revised October 29, 2010.

required exogeneity assumptions. The first is that a subset of the tax interventions are motivated as responses to historical deficits, potentially violating condition (5). Several studies, including Romer and Romer (2010), Mertens and Ravn (2010a) and Favero and Giavazzi (2010) all fail to reject the hypothesis that the occurrence or size of the change in tax liabilities are unpredictable by past observations of macroeconomic aggregates, such that this does not seem to be a practical concern. A second related issue is that many changes in the tax code are legislated well in advance of scheduled implementation. In Mertens and Ravn (2010a) we distinguish between unanticipated and anticipated exogenous tax changes on the basis of the implementation lag and find evidence for macroeconomic effects of legislated tax shocks prior to their implementation. This finding means that (5) may fail to hold for the subset of tax changes that are anticipated well in advance. For this reason, in the benchmark specification we only use those exogenous tax changes for which the legislation and implementation date are less then one quarter apart. These unanticipated exogenous tax interventions are listed in Appendix C. We scale the change in liabilities by previous quarter nominal GDP and remove the mean from the nonzero observations (the mean is approximately zero) to obtain our proxy variable m_t for shocks to total tax revenues.[3] In total, m_t has 27 nonzero observations. An F test in a regression of the nonzero observations of m_t on X_t fails to reject the hypothesis of zero slope coefficients at the 10% level. For the Blanchard Perotti SVAR, we use a value for the output elasticity of total federal tax revenues of $\theta_Y = 2$. This is higher than the estimate of 1.6 for federal tax revenues found by Follette and Lutz (2010) and closer to the value in Blanchard and Perotti (2002) of 2.08, even though the latter pertains to the elasticity of general government revenues.

[3]We use the version of the Romer and Romer (2009) that does not take into account the effects of retroactive provisions in tax legislation. Using the alternative series with retroactive provisions leads to very similar results. Also, when we scale the tax liability changes by current GDP or GDP lagged 5 quarters, the results are virtually identical.

Figure 1 shows the response for both identification schemes to unanticipated tax shock that decreases tax revenues on impact by 1% of GDP, evaluated at the mean tax revenue to GDP ratio. It also depicts 95% confidence regions for the impulse response estimates obtained by a residual bootstrap. According to the Blanchard Perotti SVAR, output increases by 0.4% on impact, gradually rises to almost 1.3% after two years and subsequently returns slowly back to the trend. The output response is considerably larger in the narrative SVAR: output increases by 2.0% on impact, rises to a maximum of almost 3.2% above trend after 5 quarters and reverts to trend after that. For both identification schemes, the output effect is significantly different from zero for multiple years after the shock. The confidence regions of both estimated impulse responses functions do not overlap for the first year after the tax decrease, such that the differences across identification schemes are statistically significant.

Table 2 provides the estimates for the structural parameters as well as the reliability of the narrative measure m_t, together with 95% confidence intervals. The first column gives the numbers for the Blanchard Perotti SVAR assuming $\theta_Y = 2$, whereas the third column contains the estimates for the benchmark narrative SVAR. Importantly, the estimated reliability of the narrative measure of the tax shocks is 0.69, which is relatively high. The implied correlation between m_t and the true underlying tax shock is 0.83. Hence, the identified SVAR tax shocks align very well with the historical record of legislated federal tax changes in the US documented by Romer and Romer (2009). The hypothesis of zero reliability, i.e. that m_t bears no relationship to unanticipated shocks to tax revenues, is very strongly rejected. Since the reduced form transmission mechanisms underlying the impulse responses in Figure 1 are identical, the disagreement in estimated tax multipliers must come from differences in the structural elasticities of the contemporaneous impact matrix. The narrative SVAR strongly rejects the value for the output elasticity of tax revenues, $\theta_Y = 2$, assumed in the Blanchard Perotti SVAR. The point estimate for θ_Y is 3.14 with 95% percentiles of 2.73 and 3.55. The other testable assumption of the Blanchard Perotti SVAR is the absence of a within quarter response of government spending to tax shocks, i.e. $\gamma_T = 0$. The point estimate for γ_T is 0.06 with 95% percentiles of -0.06 and 0.17. Thus, this assumption is not contradicted by the narrative identification scheme. The other structural elasticities are all relatively similar across identification schemes, and the remaining differences are mainly reflected in the standard deviations of the shocks.

Reconciling the SVAR Results There are two potential ways to reconcile the difference in tax multipliers between the two specifications. The first is to impose a higher value of θ_Y in the Blanchard Perotti SVAR. Panel (a) of Figure 2 provides the impulse response to a tax cut for the Blanchard Perotti SVAR that assumes the value for the output elasticity of tax revenues estimated by the narrative SVAR, i.e. $\theta_Y = 3.14$ instead of 2. Imposing this higher value produces large effects of an unanticipated decrease in tax revenues that are essentially identical to those of the benchmark narrative SVAR. Column of Table 2 gives the estimates of the structural parameters, which are in very close accordance with those of the narrative SVAR in the third column. Note that all confidence intervals are wider in the narrative SVAR compared to those of the Blanchard Perotti SVAR. This is because the latter do not reflect uncertainty in identification.

The other way of reconciling the results is by changing the narrative measure m_t used for identification of the tax shocks in the narrative SVAR. If we replace our series, which eliminates all tax changes with long implementation lags, by the original series of Romer and Romer (2009), the

Table 2: Structural Parameters

		Blanchard-Perotti SVAR		Narrative SVAR	
Equation		$\theta_Y = 2$	$\theta_Y = 3.14$	Benchmark	Using all Shocks
Tax Revenue	θ_G	-0.06	-0.13	-0.20	0.16
		$[-0.11, -0.02]$	$[-0.19, -0.09]$	$[-0.35, -0.07]$	$[0.01, 0.29]$
	θ_Y	2	3.14	3.14	1.88
		–	–	$[2.73, 3.55]$	$[1.57, 2.25]$
	$\Sigma_T \times 100$	2.23	2.56	2.53	2.19
		$[2.00, 2.18]$	$[2.34, 2.50]$	$[2.23, 2.62]$	$[1.98, 2.18]$
Spending	γ_T	0	0	0.06	-0.23
		–	–	$[-0.06, 0.17]$	$[-0.39, -0.08]$
	γ_Y	0	0	0	0
		–	–	–	–
	$\sigma_G \times 100$	2.36	2.36	2.35	2.30
		$[2.14, 2.31]$	$[2.14, 2.31]$	$[2.12, 2.30]$	$[2.05, 2.27]$
Output	ζ_T	-0.04	-0.36	-0.36	-0.05
		$[-0.09, -0.05]$	$[-0.43, -0.31]$	$[-0.57, 0.24]$	$[-0.12, 0.00]$
	ζ_G	0.07	0.10	0.10	0.07
		$[0.05, 0.09]$	$[0.07, 0.12]$	$[0.06, 0.13]$	$[0.05, 0.09]$
	$\sigma_Y \times 100$	0.95	1.53	1.55	0.92
		$[0.87, 0.95]$	$[1.37, 1.64]$	$[1.21, 1.94]$	$[0.80, 1.00]$
Reliability	Λ	–	–	0.69	0.42
		–	–	$[0.48, 0.83]$	$[0.29, 0.50]$

Values in parenthesis are 95% percentiles of the empirical distribution computed using $10,000$ bootstrap replications.

estimated tax multipliers are much lower. Panel (b) of Figure 2 plots the response in comparison with the Blanchard Perotti scheme that assumes $\theta_Y = 2$, whereas the fourth column of Table 2 lists the structural parameters. With this alternative tax shock measure, the estimated output elasticity of tax revenues is 1.87 and is not significanlty different from 2. The impulse response to a tax decrease is now virtually identical to Blanchard Perotti. Thus, when the narrative measure includes changes in the tax code that are preannounced, both SVAR identification methods pick up essentially the same shock. The broader measure m_t contains fewer nonzero observations (in total 45). If it is just an alternative measure of the same tax shock, then the discrepancy in the point estimates could simply be due to small sample uncertainty in the second estimation stage of the narrative SVAR.

The first explanation, that the output elasticity of tax revenues has been set too low in previous applications, is in our view the most plausible for several reasons. To justify including the shocks with long implementation lags as a measure of an unanticipated tax shocks, one must assume that there are no anticipation effects prior to implementation. This is in contradiction with the evidence in Mertens and Ravn (2010a) as well as almost any theoretical business cycle model with forward looking agents. There is also abundant event study evidence of income shifting across time in response to several widely anticipated tax changes, see Saez, Slemrod and Giertz (2010) for an overview. If the broader measure is just a larger sample of observations of the same underlying unanticipated shock, the difference in tax multipliers should not be statistically significant. Yet comparing Figure 1 and panel (b) of Figure 2 shows that the 95% confidence regions do not overlap for the first three quarters in both narrative SVARs. Also, the estimated reliability of m_t should be roughly similar, but Table 2 (fourth column) shows that when the broader measure is used, the reliability drops from 0.69 to 0.42, with barely intersecting confidence regions. Thus, the estimated correlation of m_t with the true unanticipated shock to taxes drops considerably. This is the expected effect when tax changes with long implementation lags are fundamentally different. Finally, when we use only the tax changes with long implementation lags as the narrative measure, the estimated effect of a decrease in taxes on output is negative (not shown). This finding is symptomatic for the inability of SVARs to uncover responses to anticipated structural shocks without properly accounting for their timing, see for instance Leeper, Walker and Yang (2008) and Ramey (2010).

Response to a Spending Shock Figure 3 plots the output response to an unexpected increase in government spending of 1% of GDP. In the Blanchard Perotti SVAR, the impact multiplier is 0.7 and the maximum output effect is 0.8 two quarters after the shock. The output multiplier never exceeds 1, but the output increase is very persistent. With the narrative identification the output effects are very similar. The impact multiplier is 0.8 and the maximum output effect is close to 1 two quarters after the shock. The 95% percentiles in either specification are very wide, and the output effect is not significantly different from zero beyond the first quarter after the shock. As a result, the difference in identification does not affect the estimated size of the spending multipliers. In both cases, the point estimates are generally consistent with the SVAR literature, see Caldara and Kamps (2008). The impact tax and spending multipliers in the narrative SVAR are significantly different at the 95% level. Thus, in contrast to Blanchard and Perotti (2002), we find evidence that the tax multiplier is higher than the spending multiplier.

Tax Shocks and Output Fluctuations One advantage of our identification approach is that the entire sequence of structural tax shocks is identified, even if the narrative measure has many missing observations. This allows the use of historical or other decompositions to asses the importance of tax shocks for output fluctuations. Figure 4 plots counterfactual output paths (in deviations from the trend) in response to shocks to tax revenues. The higher output elasticity of tax revenues in the narrative SVAR translates into a larger role of unanticipated shock to taxes in explaining output movements in the US. The variance of the tax driven output path relative to the variance of actual GDP is 0.46 in the narrative SVAR and 0.17 in the Blanchard Perotti SVAR. The tax driven output movements are in both cases positively correlated with actual detrended GDP, but the correlation is higher in the narrative SVAR: 0.64 as opposed to 0.46 in the Blanchard Perotti SVAR. Changes in tax policy do not play much of a role for output dynamics pre-1963 or in the 1990s. Tax driven output is much more closely related to actual output developments from the mid 1960s through the mid 1980s, corresponding to a period of more active use of tax policy instruments. Finally, both SVARs predict a large positive output response to the tax cuts of the early 2000s, which did not materialize in reality.

Subsample Stability Frequently, studies that rely on SVARs to estimate the response to fiscal policy shocks in the US find them to be unstable over time. In pre and post 1980s subsamples, typically a tax cut has positive output effects before 1980 and zero or even negative output effects after 1980, see e.g. Perotti (2004). The left panel of Figure 5 shows that this is also the case in our application of the Blanchard Perotti SVAR. Whereas the output response in the first subsample is similar to the one in the full sample, the output response in the second subsample is zero. The right panel of Figure 5 shows the response using the narrative identification in the pre and post 1980 subsamples. With the alternative identification scheme, the evidence for instability is considerably weaker. The impact multipliers in the subsamples are statistically speaking the same as in the full sample. Only at horizons beyond two years is there some evidence that the post 1980 output response is lower.[4]

3 .1.2 Comparison to Other Narrative Approaches

The relatively large maximum tax multipliers in the narrative SVAR are quantitatively in line with those found in previous studies using narrative data by Romer and Romer (2010) and Mertens and Ravn (2010a). The impact and shorter run multipliers, on the other hand, are considerably higher in the narrative SVAR. Favero and Giavazzi (2010) instead estimate tax multipliers using the Romer and Romer (2009) tax shocks that are more similar to those of the Blanchard Perotti SVAR. The main difference between these three previous studies is the empirical specification of the reduced form transmission mechanism. The different specifications are

$$y_t = \lambda_0 m_t + \lambda_1 m_{t-1} + ... + \lambda_j m_{t-k} + u_t \qquad \text{(Romer and Romer (2010))}$$

$$y_t = \delta' X_t + \lambda_0 m_t + \lambda_1 m_{t-1} + ... + \lambda_k m_{t-k} + u_t \qquad \text{(Mertens and Ravn (2010a))}$$

$$y_t = \delta' X_t + \lambda_0 m_t + u_t \qquad \text{(Favero and Giavazzi (2010))}$$

The dynamic effects of tax shocks are derived as impulse responses to the narrative measure m_t. All three approaches require the exogeneity assumptions (4) and (5) to hold. They also require m_t to be measured on the right scale as well as to be perfectly correlated with the true exogenous innovation to tax revenues. The Romer and Romer (2009) measure for tax shocks is based on *projected*

[4]Most studies find that the output elasticity of tax revenues has increased after the Tax Reform Act of 1986, see e.g. Follette and Lutz (2010). Our estimates for the output elasticities are 2.76 for the pre 1980 period, and 3.86 for the post 1980 period.

changes in tax *liabilities*, as opposed to *actual* exogenous changes in tax *revenues*. Moreover, various government documents often contradict each other on the precise budgetary impact of changes in tax legislation, see Romer and Romer (2009). A key advantage of the narrative SVAR is that it is robust to errors in measurement and only requires reasonably high correlation with the latent structural tax shock. Our estimate for the reliability of m_t of 0.69 suggests that estimates derived from each of the three specifications above potentially suffer from measurement error bias.

Figure 6 presents the results for each of the three alternative specifications. The black lines depict the estimated output effect of a tax cut of 1% of GDP as well as the 95% error bands.[5] We use the same dataset as for the SVARs. This includes the measure m_t that only contains the tax changes with implementation lags of less than a quarter.[6] The resulting estimates for the tax multipliers are very similar as those in the original papers: the maximum tax multiplier in response to an unanticipated tax cut in the Romer and Romer (2010) specification occurs 10 quarters after the shock and equals 3; in the Mertens and Ravn (2010a) specification, the maximum output effect is 2.4 two years after the shock; in the Favero and Giavazzi (2010) specification, the maximum tax multiplier is much lower, 1.2, and occurs one year after the shock. Based on the 95% confidence intervals, it is impossible to single out either small sample uncertainty or the assumed reduced form transmission mechanism as the main explanation for the differences in the point estimates across the three approaches.

To better understand the reason for the discrepancy between the three specifications as well as

[5]For the Romer and Romer (2010) specification, the figure depicts asymptotic two standard error bands instead of bootstrapped confidence intervals.

[6]For the Romer and Romer (2010) specification, we set $k = 12$; for the Mertens and Ravn (2010a) specification, we set r=4 and k=12; for the Favero and Giavazzi (2010) specification, we set r=4.

with our narrative SVAR approach, Figure 6 depicts additional results from two counterfactual simulations. We draw 10,000 bootstrap samples using the estimated narrative SVAR specification as the data generating process and estimate the three alternative narrative specifications in each artificial sample. The blue lines represent the mean response to a tax cut when the series for m_t used in each simulation is a bootstrap sample of the measured tax shock series. The red lines show the mean responses when we instead use the true underlying structural tax shocks as the counterfactual measure m_t, censored such that it contains the same nonzero observations as the measured tax shock series. The difference between the blue and red lines therefore captures the average effect of measurement error on the point estimates in all three narrative specifications.

Two conclusions emerge from the simulations. The first one regards the reason for the differences between the narrative SVAR results in this paper and the previous narrative studies. Figure 6 reveals that measurement error generates large attenuation biases in the specifications used in the literature. Moreover, the extent of the measurement error bias statistically explains the difference in tax multipliers with the narrative SVAR. The average responses when m_t contains measurement error (red lines) in all three cases lie well within the confidence bands of the impulse response estimates. For the Favero and Giavazzi (2010) specification, the average simulated response aligns almost perfectly with the response in real data at all horizons. For the other two specifications, which contain a moving average term of m_t, the simulated output effects are very similar to the actual estimates for horizons up to one year. At longer horizons, the simulated response is lower then the actual estimates but never leaves the 95% confidence bands. When the true tax shocks are used as the narrative measure (blue lines), the average responses to a tax cut across all specifications are significantly higher and are all relatively close to the true response in the data generating process. All three specifications however imply slightly upward biased tax multiplier estimates in small samples. Nonetheless, the simulations show that the robustness of the narrative SVAR approach to measurement error is an advantage that is highly empirically relevant.

The second conclusion from the simulations is that, regardless of whether m_t contains measurement error and despite the differences in the reduced form transmission mechanism, the average simulated responses are quantitatively always very similar across all three specifications. This finding corroborates the simulation evidence of Charhour, Schmitt-Grohé and Uribe (2010), who use an alternative data generating process based on the estimated DSGE model of Mertens and Ravn (2011) to evaluate the ability of the different specifications to uncover the theoretical response to an unanticipated tax shock. They also find that the assumed reduced form transmission mechanism is an unlikely source of the difference in estimates in the literature. Our results support their view that a reconciliation of the findings in the literature requires a focus on issues of identification and small sample uncertainty.

Besides the advantages of the narrative SVAR in terms of (1) comparability to a large SVAR literature, (2) robustness to measurement error, (3) inference that accounts for uncertainty in identification and (4) estimation of the entire shock sequence allowing variance decompositions, it is worth pointing out one other advantage. Specifications that rely on moving averages of the narrative measure to help capture the shock transmission, such as Romer and Romer (2010) or Mertens and Ravn (2010a), typically suffer more from estimation uncertainty. This was also pointed out by Charhour, Schmitt-Grohé and Uribe (2010) and is also apparent in the confidence intervals of Figure 6. An additional advantage of the SVAR approach, and at the same time of the Favero and Giavazzi (2010) specification, is that it exploits the available data more efficiently. On the other hand, relying on the informational content of the endogenous lagged regressors to capture the transmission requires the existence of a VAR representation. Unlike specifications with moving average components, our approach is for instance not well suited to estimate the effects of anticipated fiscal shocks unless the set of conditioning endogenous variables is appropriately expanded, see Leeper, Walker and Yang (2008). In principle, fiscal foresight can also invalidate the use of SVARs for estimating the effects to unanticipated shocks. Simulation evidence in Mertens and Ravn (2010b) and Charhour, Schmitt-Grohé and Uribe (2010) suggests that the latter is not a serious problem in practice.

3 .2 The Specification with Personal and Corporate Income Tax Revenues

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4 Tax Revenues in the Great Recession

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5 Conclusion

TO BE COMPLETED

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A The Measurement Error Model

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B Identification

Case I: Blanchard and Perotti SVAR, $k = 1$

Case II: Narrative SVAR, $k = 1$

Case III: Blanchard and Perotti SVAR, $k > 1$

Case IV: Narrative SVAR, $k > 1$

C Disaggregated Tax Shocks

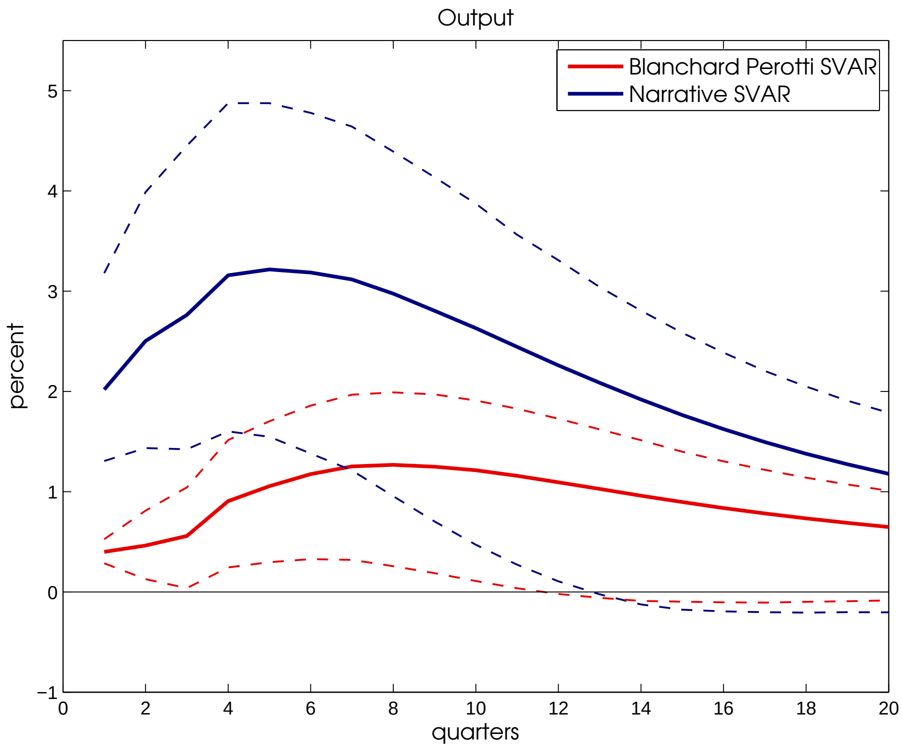

Figure 1: Response to Tax Cut of 1% of GDP. Broken lines are 95% percentiles

(a) Blanchard-Perotti with $\theta_Y = 3.14$ (b) All Romer and Romer (2009) shocks

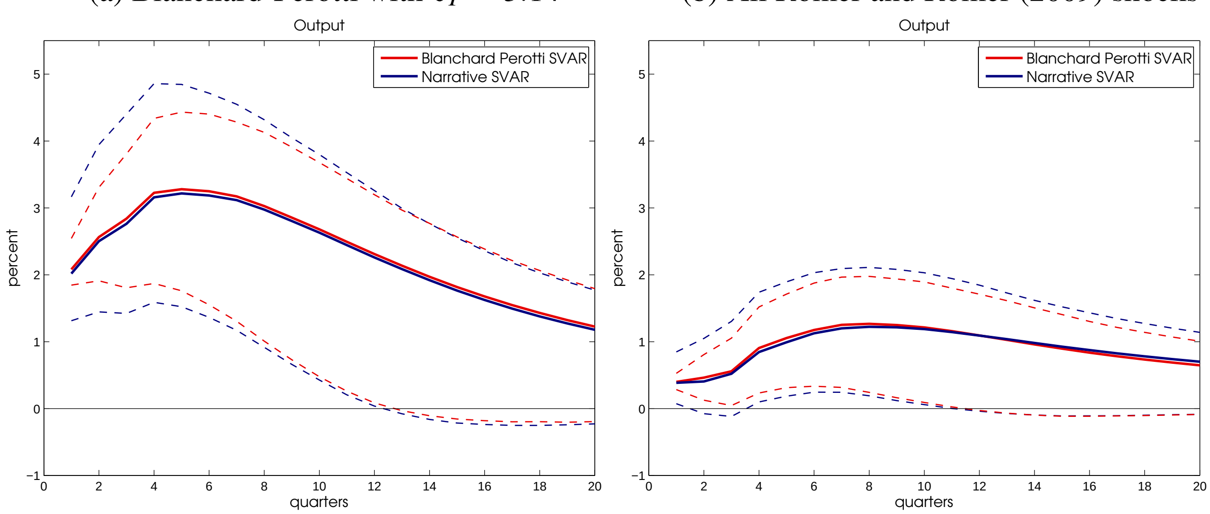

Figure 2: Response to Tax Cut of 1% of GDP. Broken lines are 95% percentiles

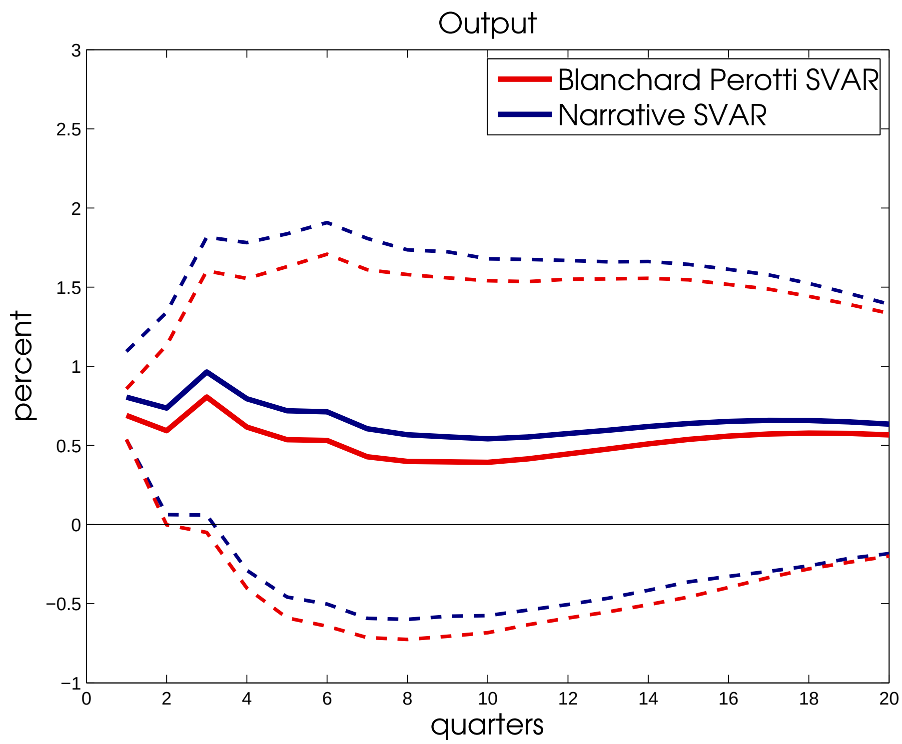

Figure 3: Response to Government Spending Increase of 1% of GDP

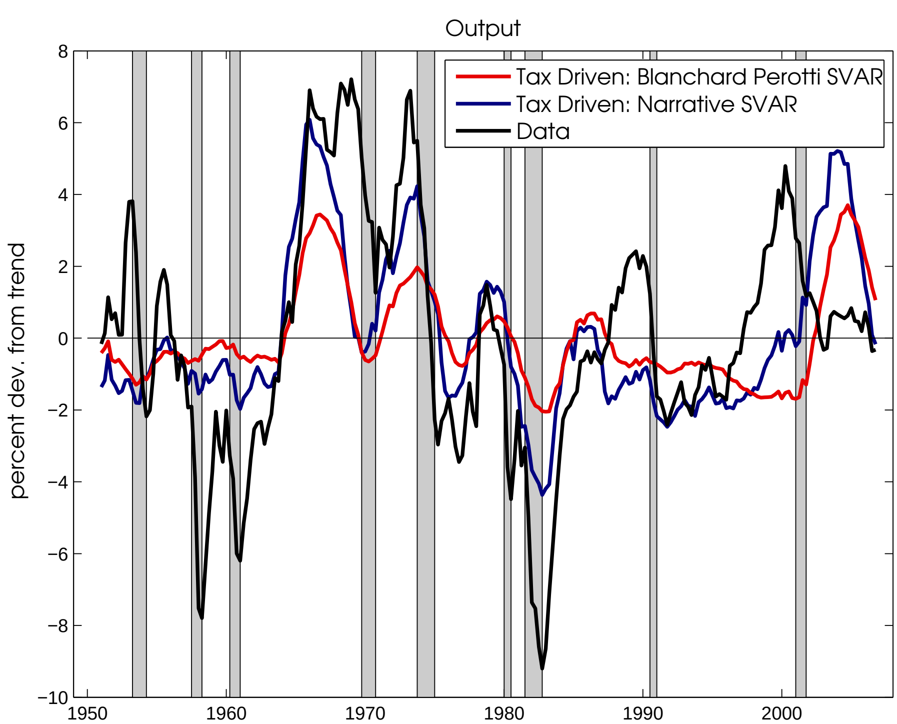

Figure 4: The Tax Driven Cycle

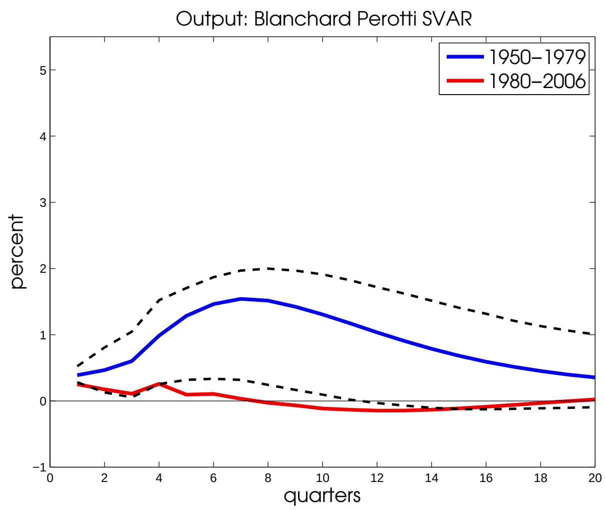

Figure 5: Response to Tax Cut of 1% of GDP, Subsample Stability. The 95% bands shown are for the full sample response.

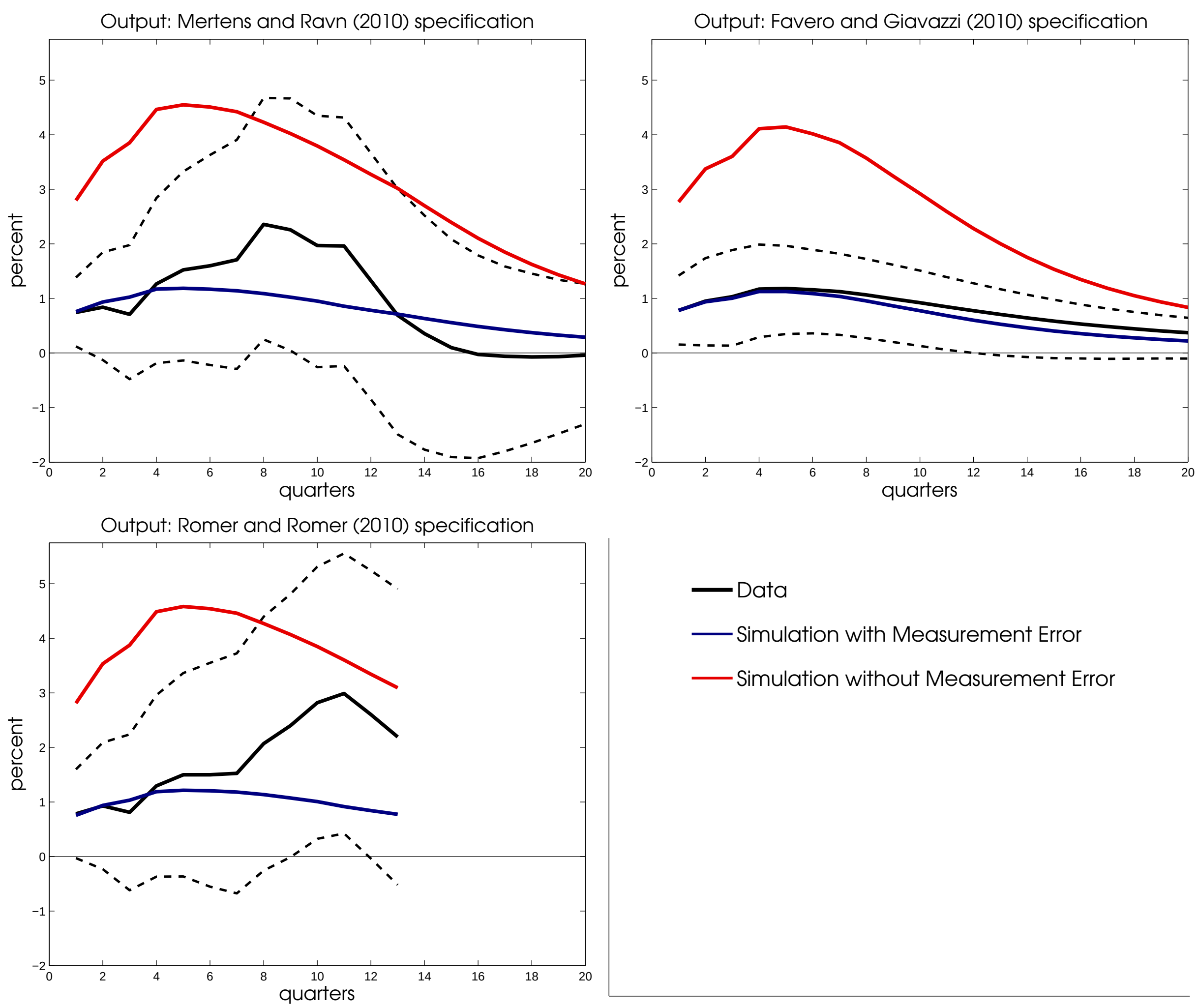

Figure 6: Response to Tax Cut of 1% of GDP: Alternative Narrative Specifications

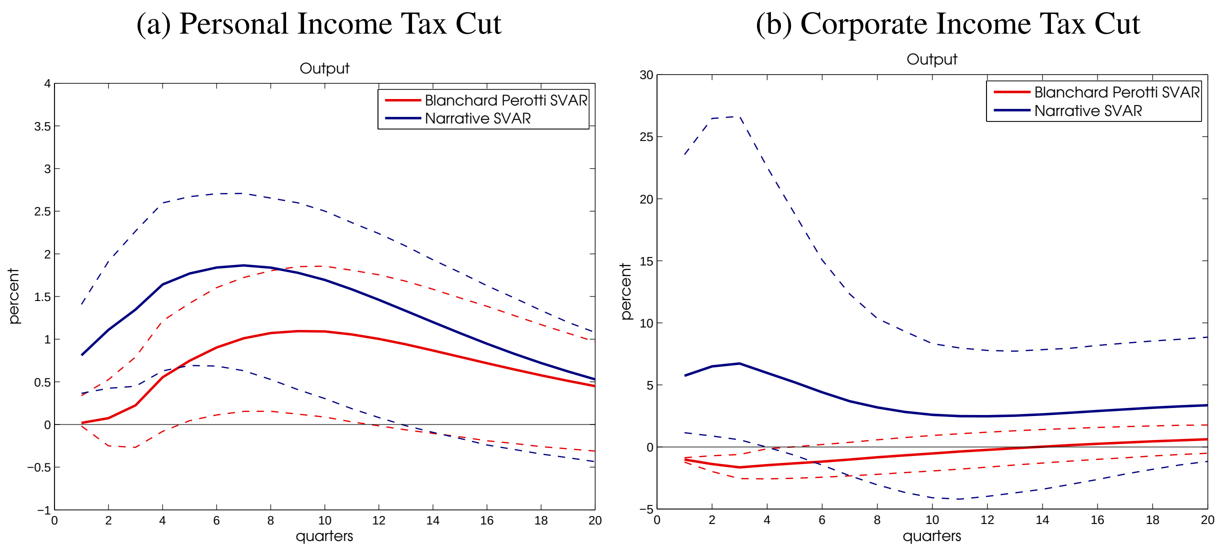

Figure 7: Response to Tax Cut of 1% of GDP

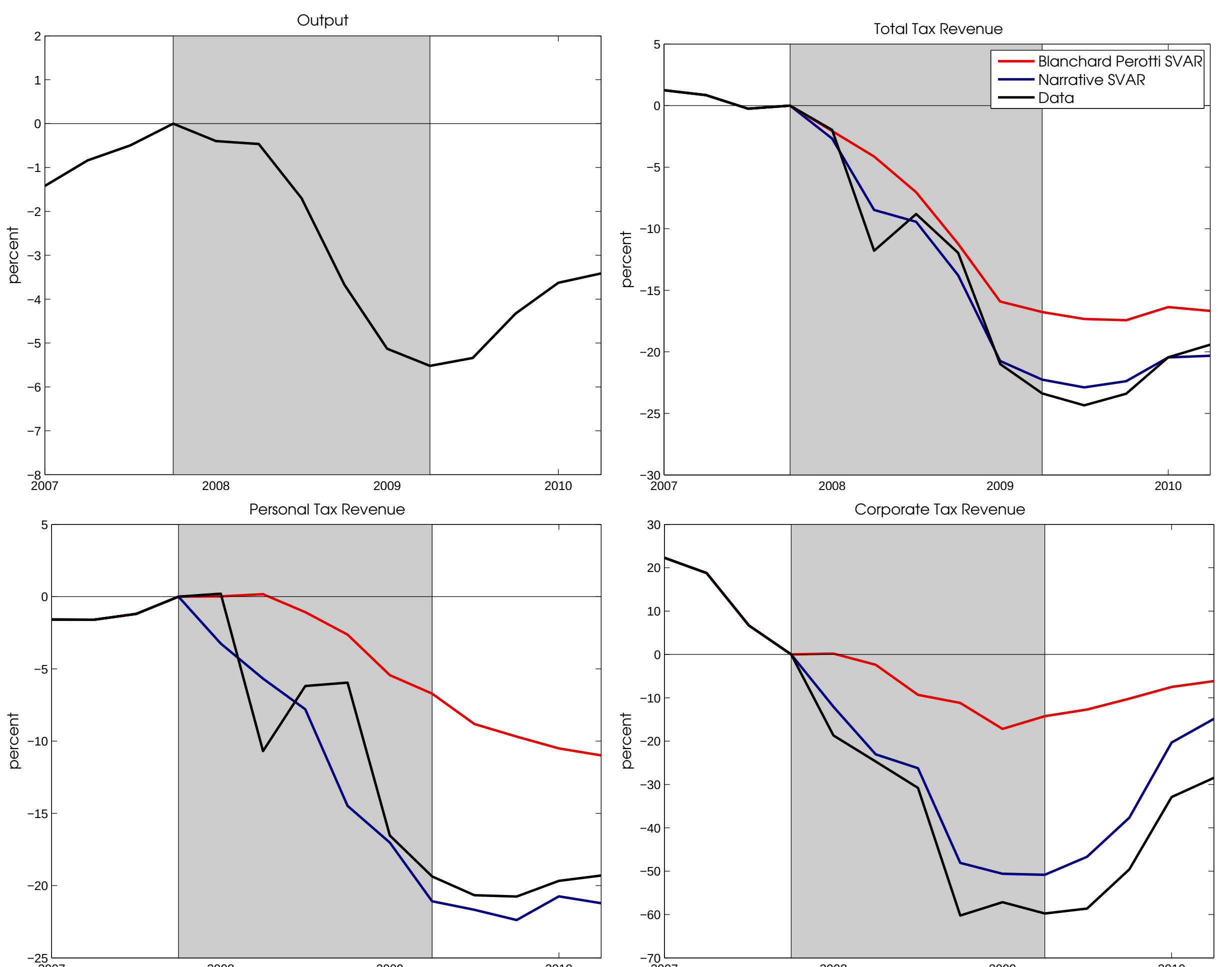

Figure 8: Endogenous Response of Tax Revenues to the Great Recession