A Reconciliation of SVAR and Narrative Estimates of Tax Multipliers*

Karel Mertens and Morten O. Ravn

This draft: April 2012, first draft September 2011

Abstract

Existing empirical estimates of US nationwide tax multipliers vary from close to zero to very large. Using narrative measures as proxies for structural shocks to total tax revenues in an SVAR, we estimate tax multipliers at the higher end of the range: around two on impact and up to three after 6 quarters. We show that earlier findings of lower multipliers can be explained by an output elasticity of tax revenues assumption that is contradicted by empirical evidence or by failure to account for measurement error in narrative series of tax shocks.

Keywords: Fiscal policy, tax changes, vector autoregressions, narrative identification, measurement error

JEL Classification: E20, E32, E62, H30

---

*We thank Matthieu Bussiere, Martin Eichenbaum, James Stock, Roberto Perotti and participants at various seminars and conferences for useful comments. We also thank Jonas Fisher and Todd Walker for providing data. *Mertens*: Department of Economics, Cornell University, km426@cornell.edu; *Ravn*: Department of Economics, University College London, m.ravn@ucl.ac.uk.

1 Introduction

The empirical literature on the dynamic output effects of unanticipated changes in tax policy does not speak with one voice. Although most studies agree that tax increases are contractionary, there is considerable disagreement regarding the *size* of the effect on economic activity. Estimates of tax multipliers for the United States vary from close to zero to almost four, a range that is sufficiently wide that the literature provides only limited guidance for theory and economic policy. The broad range of estimates reflects numerous differences in methodology, including identification assumptions, model specifications, as well as sample coverage. In this paper, we use a new approach to estimate tax multipliers associated with shocks to total federal tax revenues. Our estimates imply tax multipliers of around two on impact and up to three after one-and-a-half years. Importantly, we provide a reconciliation of our estimates with previous findings in the literature.

The main challenge to measuring the aggregate effects of changes in tax policy is endogeneity of fiscal policy instruments. One strand of the literature has identified tax shocks by imposing short run restrictions in structural vector autoregressions (SVARs). In a seminal contribution, Blanchard and Perotti (2002) make assumptions on policy lags and calibrate certain parameters to identify structural innovations to taxes and government spending. Mountford and Uhlig (2009) use economic theory to derive sign restrictions on VAR impulse responses. Another part of the literature instead assumes that some exogenous changes in tax policy are observable. In a leading example, Romer and Romer (2009) construct comprehensive narrative measures of legislated changes in federal tax liabilities in the United States for the postwar period. A number of studies estimate the output effects of tax changes as the response to innovations in one of these narrative measures.[1]

[1] Recent applications of the Blanchard and Perotti (2002) identification scheme to the estimation of tax multipliers include Ilzetzki (2011), Auerbach and Gorodnichenko (2012) and Caldara and Kamps (2012). Canova and Pappa (2007) adopt sign restrictions similar to Mountford and Uhlig (2009) to identify regional tax revenue shocks. The narrative approach to the estimation of tax multipliers has also been applied to other countries: for the UK, see Cloyne (2012); for a sample of OECD countries, see Favero, Giavazzi and Perego (2011).

Unfortunately, the estimated output effects of tax shocks vary significantly both within the SVAR and narrative approaches. Blanchard and Perotti (2002) find tax multipliers that are small on impact and never exceed unity thereafter. The sign restriction approach of Mountford and Uhlig (2009) yields maximum multipliers of more than three for horizons of several years after a deficit-financed tax cut. Using the narrative approach, Romer and Romer (2010) find output increases of more than three percent approximately two years after a one percentage point cut in tax liabilities to GDP. Eliminating tax changes that are likely to be anticipated because of long implementation lags, Mertens and Ravn (2012a) find maximally two percent increases in output following a one percentage point cut in tax liabilities to GDP. Favero and Giavazzi (2012) instead find output effects of the Romer and Romer (2010) shocks that are similar to the much lower estimates of Blanchard and Perotti (2002).

Some recent studies investigate the underlying reasons for the disparity in results. Focusing on SVAR approaches, Caldara and Kamps (2012) show that the variability in the estimates can be traced to assumptions regarding the cyclical sensitivity of tax revenues. Their study however offers no new resolution to the problem of cyclical adjustment and which estimates are more plausible depends on priors for the output elasticity of tax revenues. Charhour, Schmitt-Grohé and Uribe (2012) investigate a claim made by Favero and Giavazzi (2012) that alternative assumptions regarding model specifications explain the differences between the Blanchard and Perotti (2002) and Romer and Romer (2010) estimates. They conclude that a reconciliation of the results must instead lie with identification assumptions and/or sampling uncertainty. Finally, Perotti (2011) produces a refined measure of Romer and Romer (2009)'s tax changes and finds output tax multipliers that are larger across various specifications than those in Blanchard and Perotti (2002).

We adopt an alternative approach to the estimation of tax multipliers, described in Mertens and Ravn (2012b) and Stock and Watson (2012), that integrates narrative identification into the standard

SVAR framework.[2] The key identifying assumptions are that the narrative measures correlate with tax shocks but are orthogonal to other structural shocks. The narrative tax changes are treated as proxy measures of latent structural tax shocks, which is why we refer to it as the 'proxy SVAR' approach. The main idea is to complement the usual VAR residual covariance restrictions with moment restrictions on the proxy to achieve identification. An application to US post WWII data yields estimates of tax multipliers that are large, robust and relatively precisely estimated. At medium forecast horizons, our results support tax multipliers at the higher end of the range, such as those of Mountford and Uhlig (2009) and Romer and Romer (2010). However, we find tax multipliers that are larger than these studies also in the short run.

The proxy SVAR allows us to elicit the underlying differences between the estimates produced by alternative identification schemes. Unlike the Blanchard-Perotti SVAR, the proxy SVAR does not require direct assumptions on key structural elasticities but instead estimates them. Because the specification in both SVARs are identical in every other respect, the discrepancy between results can be traced to the values of those structural elasticities. The answer lies exclusively with the output elasticity of tax revenues. The proxy SVAR estimates this elasticity to be high and rejects at the 95 percent level the lower cyclical elasticities calculated by international organizations on which Blanchard and Perotti (2002) rely. We provide several criticisms of the conventional cyclical adjustment procedures and argue that alternative methods available in the literature, while small in number, all point to high output elasticities, and therefore large tax multipliers.

Our methodology also has an advantage over existing narrative approaches because it is robust to various types of measurement error. We discuss several reasons why some error in measurement is hard to avoid when constructing the narrative measures of tax shocks, including those that concern

[2] The identification approach was outlined earlier for SVARs in an NBER lecture by Stock and Watson (2008). Stock and Watson (2012) apply the methodology to dynamic factor models for the identification of a wide range of shocks.

Perotti (2011). The proxy SVAR yields estimates of the statistical reliability of the narrative series, which measures the squared correlation between the narrative measures and the estimated structural shocks. This statistic allows for an evaluation of the quality of different available tax shock measures. We find for instance that it is important to correct for anticipated tax changes. Another issue in the calculation of the output effects of tax changes is the scaling of shocks. As in Blanchard and Perotti (2002) or Mountford and Uhlig (2009), we scale the tax shocks by their impact on tax revenues to obtain tax multipliers. Standard applications of the narrative approach instead scale the tax shocks in terms of their projected impact on tax liabilities. We quantify the measurement error bias present in the existing narrative specifications through simulations. We find that measurement error explains the differences across the narrative estimates and is the reason for the low tax multipliers estimated by Favero and Giavazzi (2012).

The key objective of this paper is to understand the dispersion of estimated tax multipliers associated with unanticipated shocks to total revenues. In doing so, we abstract from other issues relevant to the empirical characterization of the aggregate effects of tax policy shocks. For instance, we focus exclusively on unanticipated tax changes. Other studies have looked at shocks to expectations of future tax policy, e.g. Mountford and Uhlig (2009), Mertens and Ravn (2012a), Leeper, Walker and Yang (2011) or Kueng (2011). As in most previous work, there is no attempt to define more narrowly which of the many tax instruments is adjusted, as is done in Barro and Redlick (2011) or Mertens and Ravn (2012b). Finally, we restrict attention to linear models that do not allow for time-varying effects as in Auerbach and Gorodnichenko (2012). Nonetheless, the identification and measurement issues we raise are also highly relevant for extensions in any of these directions.

2 Empirical Models

The first step of our analysis is to replicate existing results on the output effects of tax shocks for the same dataset. We focus on the SVAR of Blanchard and Perotti (2002) and several narrative specifications and contrast the results with those from our new empirical model, the proxy SVAR.

2 .1 The SVAR of Blanchard and Perotti

The benchmark application of Blanchard and Perotti's (2002) methodology estimates the impact of discretionary tax shocks from a VAR using data on total tax revenues T_t, government spending G_t and output Y_t. The dynamics of the observables $Z_t = [T_t, G_t, Y_t]'$ are modeled by a VAR,

$$Z_t = \alpha' d_t + \delta' \mathbf{Z}_{t-1} + \mathcal{B}\varepsilon_t, \tag{1}$$

where d_t contains deterministic terms with coefficients α, $\mathbf{Z}_{t-1} = [Z'_{t-1}, ..., Z'_{t-p}]'$ is the vector of lagged observables, p is the number of lags, δ is a matrix of autoregressive coefficients, $\mathcal{B}$ is a nonsingular matrix of coefficients, and $\varepsilon_t = [\varepsilon_t^T, \varepsilon_t^G, \varepsilon_t^Y]'$ is a vector of structural shocks with $E[\varepsilon_t] = 0$, $E[\varepsilon_t \varepsilon_t'] = I$, $E[\varepsilon_t \varepsilon_s'] = 0$ for $s \neq t$. Let $u_t = [u_t^T, u_t^G, u_t^Y]'$ denote the reduced form residuals, which are by assumption linearly related to the structural shocks:

$$u_t = \mathcal{B}\varepsilon_t \ . \tag{2}$$

Estimates of α, δ and $E[u_t u_t']$ are straightforward to obtain by for instance OLS but the structural coefficients $\mathcal{B}$ and shocks ε_t are not identified. The requirement that $E[u_t u_t'] = \mathcal{B}\mathcal{B}'$ provides six independent identifying restrictions. Obtaining all nine elements of $\mathcal{B}$ requires at least three more identifying restrictions. Without loss of generality, we can express the reduced form errors as:

$$
\begin{aligned}
u_t^T &= \theta_G \sigma_G \varepsilon_t^G + \theta_Y u_t^Y + \sigma_T \varepsilon_t^T \ , \\
u_t^G &= \gamma_T \sigma_T \varepsilon_t^T + \gamma_Y u_t^Y + \sigma_G \varepsilon_t^G \ , \\
u_t^Y &= \zeta_T u_t^T + \zeta_G u_t^G + \sigma_Y \varepsilon_t^Y \ .
\end{aligned}
\tag{3}
$$

The parameters θ_Y and γ_Y measure the cyclical elasticities of tax revenues and spending respectively; θ_G and γ_T capture the interdependence between fiscal instruments; and ζ_T and ζ_G parametrize the contemporaneous dependence of economic activity on fiscal policy. The remaining parameters are the standard deviations of all sources of exogenous variation in the variables.

The identification strategy proposed by Blanchard and Perotti (2002) is based on restricting the values of 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 and recognition lags in fiscal policy, the parameters γ_T and γ_Y are restricted to zero. The output elasticity of tax revenues θ_Y is instead calibrated to an outside estimate of the cyclical sensitivity of revenues. Blanchard and Perotti (2002) adopt the OECD methodology described in Giorno et al. (1995) to obtain a value for θ_Y. Fixing the values of these parameters provides the three independent restrictions required to identify structural impulse responses.

We reproduce the results of Blanchard and Perotti (2002) using data from the BEA's NIPA tables on federal tax revenues, federal government consumption and investment expenditures and output, all in log real per capita terms and for the sample 1950Q1 to 2006Q4.[3] All VAR specifications have

[3] 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 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 July 29, 2011.

four lags of the endogenous variables and, as in Blanchard and Perotti (2002), include a constant, linear and quadratic trends and a dummy for 1975Q2. Because our goal is to compare results with the narrative estimates of the output response to federal tax changes recorded by Romer and Romer (2009), we use data at the level of the federal instead of the general government as in Blanchard and Perotti (2002). We use the original value for θ_Y used in Blanchard and Perotti (2002) of 2.08, even though the latter pertains to the elasticity of general government revenues.[4]

The impulse responses of output we report have the interpretation of tax multipliers, i.e. dollar changes in GDP as a ratio of the dollar changes in tax revenues. In the Blanchard-Perotti SVAR, these are obtained by dividing the response to a tax revenue shock of minus one percent by the average ratio of federal tax revenues to GDP in the sample of 17.5%. Equivalently, the numbers reflect the percent response to a tax cut that lowers tax revenues by one percentage point of GDP. Unless mentioned otherwise, we provide 95% confidence intervals that are computed using a recursive wild bootstrap using $10,000$ replications, see Gonçalves and Kilian (2004).[5]

Panel (A) of Figure 1 presents the effect on output of an exogenous tax cut in the Blanchard-Perotti SVAR. On impact, output increases by 0.48 percent in response to the tax shock, whereas the maximum output effect of 1.35 percent occurs after two years. The output increase is significantly different from zero at the 95% level for the first three years after the shock. Despite the differences in data definitions and sample coverage, these estimates are similar to the estimated impact and peak multiplier of 0.69 and 0.78, respectively, in the original paper by Blanchard and Perotti (2002). The first column of Table 1 lists the underlying estimates of the parameters of the system in (3).

[4]Using a variation of the same methodology, Follette and Lutz (2010) provide an estimate of the output elasticity of tax revenues θ_Y of 1.6 for the US at federal level. However this estimate is based on annual data.

[5]In the application of the wild bootstrap, we multiply in each artificial sample every $\hat{u}_t$ with a random variable taking on values of -1 or 1 with probability 0.5.

2 .2 Standard Narrative Approaches

A leading alternative identification strategy for estimating the dynamic effects of tax shocks is based on the narrative approach. Romer and Romer (2009) construct measures of exogenous changes in taxes from a variety of government sources by recording the (projected) impact on federal tax liabilities of legislated tax code changes. 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 debt concerns. Romer and Romer (2010) estimate the output response to changes in taxes from a univariate regression for output growth

$$\Delta Y_t = \alpha' d_t + \lambda_0 \tau_t + \lambda_1 \tau_{t-1} + \ldots + \lambda_k \tau_{t-k} + w_t \ , \qquad \text{(Romer and Romer (2010))}$$

where d_t are deterministic terms, τ_t are the narrative shocks to total tax liabilities as a percentage of GDP and the $\lambda's$ are slope coefficients. If τ_t and its k lags are exogenous, i.e. uncorrelated with the residual w_t, then OLS estimates of the λ's are structural impulse response coefficients to innovations in the measured tax changes τ_t. One can view the equation in terms of a moving average (or Wold) representation of ΔY_t in which w_t captures the effects of contemporaneous and lagged realizations of structural shocks other than those observed directly by τ_t. The required exogeneity assumption is therefore that the τ's are uncorrelated with all current and past realizations of these other shocks.

There are several possible reasons why the measure for tax shocks used by Romer and Romer (2010) may fail to satisfy the necessary exogeneity assumptions. The first is that a subset of the tax interventions are motivated as responses to inherited deficits. In practice however, several studies, including Romer and Romer (2010), Mertens and Ravn (2012a) and Favero and Giavazzi (2012), all fail to reject the hypothesis that the occurrence or size of the Romer and Romer (2010) tax changes are unpredictable by past observations of macroeconomic aggregates. Another key issue is that many changes in the tax code are legislated well in advance of scheduled implementation. In Mertens and

Ravn (2012a) we disaggregate the tax shock series into unanticipated and anticipated tax changes on the basis of the implementation lag and find evidence for macroeconomic effects of legislated tax shocks prior to their implementation. These preannounced tax changes thus reflect past tax 'news' shocks rather than surprise current tax changes which leads to a violation of the exogeneity requirement.[6] For this reason, here we only use those exogenous tax changes for which the legislation and implementation date are less than one quarter apart. The narrative measure τ_t for tax shocks is obtained by dividing the unanticipated changes in tax liabilities by previous quarter nominal GDP. In total, τ_t has 26 nonzero observations and the series is depicted in Figure 2. We estimate the Romer and Romer (2010) regression of output growth on a distributed lag of τ_t with $k = 12$ and a constant as the only deterministic term.

We also present estimates of the tax multipliers derived from two alternative empirical specifications used in the literature,

$$Z_t = \alpha' d_t + \delta' \mathbf{Z}_{t-1} + \lambda_0 \tau_t + v_t \;, \qquad \text{(Favero and Giavazzi (2012))}$$

$$Z_t = \alpha' d_t + \delta' \mathbf{Z}_{t-1} + \lambda_0 \tau_t + \lambda_1 \tau_{t-1} + \ldots + \lambda_k \tau_{t-k} + v_t \;, \qquad \text{(Mertens and Ravn (2012a))}$$

The specifications of Favero and Giavazzi (2012) and Mertens and Ravn (2012a) are vector autoregressions augmented with the contemporaneous value or a distributed lag of τ_t. In the latter, we set $k = 12$. Both these specifications include the same deterministic terms and autoregressive lags as the Blanchard-Perotti SVAR. The dynamic effects of tax shocks are obtained in each specification by tracing out the responses to a shock to τ_t. Both these specifications rely on the same exogeneity assumptions as the univariate regression.

[6]Changing the timing of the anticipated tax changes to the announcement date and combining them with the unanticipated tax changes is inappropriate since we find they have very different effects on output. We find no evidence for macroeconomic effects prior to those tax changes that we classify as unanticipated.

The estimates of the output effects of tax changes in all three narrative specifications differ from those of the Blanchard-Perotti SVAR because of the different identification strategy and because of differences in the reduced form transmission mechanism. Given the variations in time series specifications, heterogeneity in the narrative estimates is to be expected because of the differential impact of small sample uncertainty and/or model misspecifications. A more subtle issue regards the scaling of the tax shocks. In the Blanchard-Perotti SVAR, the tax shocks are scaled by their impact on actual tax revenues while the narrative studies scale the response by the impact on τ_t, i.e. the projected impact on tax liabilities assuming no change in the tax base.

Panels (B), (C) and (D) of Figure 1 depict the output responses for each model to a one percentage point shock to τ_t. The Romer and Mertens-Ravn specifications, both of which include multiple lags of the tax narrative, find impact effects on output that are only slightly higher than the Blanchard-Perotti SVAR estimates of the impact multipliers: 0.78 and 0.73 respectively. The maximum output effects, however, are instead substantially larger: 2.96 percent in the 10th quarter for the Romer specification and 2.34 percent in the eight quarter for the Mertens-Ravn specification. The confidence intervals associated with these estimates are relatively wide and easily contain the Blanchard-Perotti SVAR point estimates. The responses in the Favero-Giavazzi model are instead much closer to the Blanchard-Perotti SVAR at all horizons: the impact effect on output is 0.77 percent, but the output response never exceeds 1.17 percent. The output effects are more precisely estimated by the Favero-Giavazzi model because of a more parsimonious parametrization in the tax shocks. Despite the differences in the data used, the results are very similar to those of the original papers.

2 .3 The Proxy SVAR

We now present an alternative approach to estimating the impact of a tax shock: the proxy SVAR, which integrates the narrative identification approach into the standard SVAR framework. The proxy SVAR in this section is a straightforward application of the methodology described in Mertens and

Ravn (2012b) and Stock and Watson (2012). Unlike the standard narrative estimates of the tax multipliers, impulse responses in the proxy SVAR are based on the same VAR as the Blanchard-Perotti approach, see equations (1)-(2). However, instead of directly assuming certain values for the coefficients underlying the elements of the impact matrix $\mathcal{B}$, it exploits the informative content of narrative series of policy changes by using these as proxy measures m_t for the latent structural tax shock ε_t^T.

Without loss of generality, assume that $E[m_t] = 0$. The proxy variable m_t must satisfy

$$E[m_t \varepsilon_t^T] = \phi \neq 0 \, , \tag{4}$$

$$E[m_t \varepsilon_t^G] = 0 \, , \; E[m_t \varepsilon_t^Y] = 0 \, . \tag{5}$$

The first condition states that the proxy is contemporaneously correlated with the structural tax shock. The second condition requires the proxy to be uncorrelated with contemporaneous spending and output shocks. When these conditions hold, the proxy variable can be used for identification of the structural tax shock and the associated impulse response function. As the proxy variable for latent tax revenue shocks, we use the narrative series τ_t after removing the mean from the nonzero observations (the mean is approximately zero). Note that these identifying assumptions are weaker than those required in the standard narrative specifications. First, the proxy variable must have a nonzero correlation with the structural tax revenue shock, but the correlation does not need to be perfect. This means that τ_t does not have to contain fully accurate observations of the tax shock. Second, it is not required that m_t is uncorrelated with past structural shocks.

Implementing the identifying restrictions is straightforward. Let β_T denote the (first) column of the impact matrix $\mathcal{B}$ associated with the tax shock ε_t^T. The conditions in (4)-(5) imply that

$$\phi \beta_T = E[u_t m_t] \tag{6}$$

This condition states that the covariance between the reduced form VAR residuals and the proxy m_t is proportional to β_T. Because the extent of the correlation between the proxy m_t and ε_t^T is unknown, the constant of proportionality ϕ is unknown. Nonetheless, condition (6) provides two additional independent restrictions that suffice to trace out the response to a tax shock of any given size. In practice, the tax multipliers are easily obtained by (1) estimating the reduced form VAR, (2) regressing the reduced form residuals on the proxy variable m_t and (3) rescaling the response functions as in the Blanchard-Perotti SVAR to generate the intended effect on tax revenues. Restrictions such as (6) are always equivalent to an instrumental variables procedure, see Hausman and Taylor (1983). In this case one can alternatively view assumptions (4)-(5) as instrument validity conditions in 2SLS regressions of u_t^G and u_t^Y on u_t^T using m_t as the instrument.[7]

Figure 3 depicts the nonzero observations of the proxy variable against the corresponding structural tax shocks identified by the proxy SVAR. There is a visible positive relationship and the R squared of the associated regression line is 0.34. In Section 4 below, we will quantify the relationship between the shocks and various proxy variables using an asymptotically equivalent reliability statistic. Figure 4 presents the impulse responses of output, spending and tax revenues to an exogenous decrease in taxes, together with the 95% confidence bootstrap intervals.[8] In response to a shock to tax revenues of one percentage point of GDP, output increases by 2.00 percent on impact and rises to a maximum of almost 3.19 percent above trend after 5 quarters, before subsequently reverting to trend. Hence, at longer forecast horizons, the proxy SVAR predicts output effects that are relatively large and more in line with the results of the Romer/ Mertens-Ravn specifications than the Blanchard-Perotti and Favero-Giavazzi specifications. However, the proxy SVAR also finds substantially larger short run

[7]See Stock and Watson (2008) for the IV interpretation. Mertens and Ravn (2012b) discuss a case with n correlated proxies for n shocks. Stock and Watson (2012) consider cases with multiple proxies ('external instruments') for one structural shock.

[8]In the application of the wild bootstrap, we multiply in each artificial sample every $\hat{u}_t$ and m_t with a random variable taking on values of -1 or 1 with probability 0.5. Thus, the bootstrap inference procedure also takes into account uncertainty about identification and measurement.

effects of tax shocks than any of the other specifications. In particular, the confidence regions of the Blanchard-Perotti and proxy SVARs do not overlap for the first few quarters after the tax decrease, such that the differences across SVAR identification schemes are statistically significant.

The finding that the proxy SVAR uncovers large tax multipliers is very robust. In Mertens and Ravn (2012b), we use the same methodology to estimate the separate effects of personal and corporate income taxes based on a new narrative dataset and find similarly large output effects of cuts to either tax type. Before we make the case in favor of the larger estimates for the tax multipliers, we first examine the robustness of the finding in the context of shocks to total tax revenues:

Alternative Narrative Measures Panel (A) of Figure 5 depicts the estimated output responses when we use a few alternative versions of the unanticipated Romer and Romer (2009) exogenous tax shock series: one that excludes all tax changes that were motivated due to inherited budget concerns ('*Long run growth only*'), one that takes into account any retroactive provisions of the legislated changes ('*Retroactive*'), and a series for which the tax liabilities are scaled by previous year GDP instead of previous quarter GDP ('*Scaled by Y_{t-4}*'). None of these alternative narrative series for unanticipated tax shocks have much effect on the tax multiplier estimates.

Trend Assumptions The SVAR multiplier estimates of Blanchard and Perotti (2002) are somewhat sensitive to assumptions about trends. Panel (B) of Figure 5 shows the tax multipliers in the proxy SVAR when we switch to a stochastic trend assumption by including all variables in first differences. The main consequence is that the effect of a tax shock on output becomes permanent, with an associated long run multiplier of 3.39. However, the trend assumptions make very little difference for the point estimates for at least the first two years after the shock.

Fiscal Foresight To comply with the exogeneity assumptions required for the standard narrative approaches, we used a tax shock measure that omits all tax liability changes that were implemented more than 90 days after becoming law. Because in the proxy SVAR a zero correlation between the narrative measure and past shocks is not a requirement, the omission of tax changes that are likely to be correlated with past tax news shocks is not strictly necessary. However, this apparent advantage of the proxy SVAR is subject to two potential caveats.

The first is that the proxy SVAR also relies on the assumption that the VAR prediction errors are linearly related to the contemporaneous structural shocks, see equation (2). Several recent papers have shown that in the presence of anticipated fiscal shocks, the traditional set of conditioning variables may not contain sufficient information to satisfy this assumption.[9] We address this issue by expanding the conditioning set with variables that plausibly contain independent information on fiscal expectations. We add as a fourth endogenous variable, in turn: a measure of expected future taxes that is implied by tax exempt municipal bond yields and perfect arbitrage, constructed by Leeper et al. (2011), ('*Implicit tax rate*'); a defense sector stock returns variable, which is a series for the accumulated excess returns of large US military contractors constructed by Fisher and Peters (2010), ('*Defense returns*'); and Ramey's (2011) defense spending news variable, which contains professional forecasters' projections of the path of future military spending, ('*Defense News*').[10] Panel (C) of Figure 5 shows that including these variables has no notable effects on the estimates.

The second potential caveat is that to obtain accurate results in small samples, it is important that the correlation between the proxy and the latent unanticipated tax shock is sufficiently large. This means that good proxies of unanticipated tax shocks should have minimal predictable variation unrelated to surprise tax changes. We investigate the results for several other proxies that may differ in

[9] Leeper, Walker and Yang (2011) show that the omission of important variables can potentially produce misleading results when agents have foresight about future taxes. Ramey (2011) similarly questions the identifiability of shocks in the Blanchard-Perotti VAR in the presence of foresight about government spending.

[10] We use Leeper, Walker and Yang (2011)'s implicit tax rate variable based on bonds with maturity of one year. Since this data is only available since 1953Q2, the sample was shortened correspondingly in this case.

this dimension. In Panel (D), we use as the proxy variable the error in a regression of the (nonzero observations) of the tax narrative on four lags of the Leeper et al. (2011) implicit expected tax rate implied by municipal bond spreads. The idea is to remove any remaining predictable components of the tax narrative used in the benchmark specification.[11] We find that municipal bond spreads have little predictive power for tax changes we classified as unanticipated and the estimated tax multipliers remain close to the benchmark estimates. In Panel (E) of Figure 5 we instead use the original Romer series for exogenous tax changes. This leads to substantially lower multipliers and the point estimates are in fact very close to those of the Blanchard-Perotti SVAR in Panel (A) of Figure 1. The original Romer measure does not eliminate legislative changes with implementation lags exceeding one quarter, adding 19 observations to the proxy that we used in the benchmark.[12] The sensitivity to the inclusion of preannounced tax changes almost certainly reflects a decrease in the quality of the proxy as a measure of unanticipated shocks. A first clear indication is that a straightforward adjustment to the Romer series for expectations about future taxes almost fully restores the estimates of the benchmark proxy SVAR. In Panel (F) of Figure 5, we still use all implemented tax changes in the Romer series to construct the proxy. However, we first regress the anticipated changes on lagged observations of the Leeper et al. (2011) expected future tax rate series to eliminate the predictable component. We then merge the error in this regression with the unanticipated shocks to construct an anticipation adjusted proxy for tax shock innovations. The resulting output response is close to the benchmark proxy SVAR that used only the subset of unanticipated shocks. It turns out that, unlike the tax changes we classified as unanticipated, the anticipated tax changes are to a large extent predicted by municipal bonds spreads, such that the additional observations contain very little genuine information about unanticipated variation in taxes. In Section 4 below, we provide more formal evidence for this claim by comparing estimates of the statistical reliability of the various proxies.

[11] See Kueng (2011) for recent evidence of the predictive power of municipal bond spreads for tax rates.

[12] There are 24 observations of preannounced tax changes in the sample, but 5 occur in a quarter where there is also an unanticipated tax change observation.

3 Reconciling the SVAR Estimates of Tax Multipliers

Applying the different methodologies to the same dataset yields estimates of output responses to tax shocks that are representative for the very broad range found in the literature. We begin by isolating the reason for the difference between Blanchard and Perotti's (2002) relatively small tax multipliers and those obtained from the proxy SVAR.

3 .1 Understanding the Difference

The key feature of the proxy SVAR that makes a comparison straightforward is that, unlike the other narrative approaches, it has the same estimated reduced form transmission mechanism as the Blanchard-Perotti SVAR. Therefore, the discrepancy in tax multipliers must be apparent in the structural parameters of the contemporaneous impact matrix $\mathcal{B}$. The conditions in (4) -(5) exploited by the proxy SVAR implies two independent restrictions on $\mathcal{B}$. Whereas these are sufficient to derive the impulse response function associated with tax shocks, to identify all the parameters of the system in (3) in the proxy SVAR, we need one additional restriction. Of the three parameter restrictions imposed by Blanchard and Perotti (2002), we adopt the assumption that government spending does not react contemporaneously to changes in economic activity, i.e. $\gamma_Y = 0$. This assumption can be motivated by the presence of decision and recognition lags and seems in our view the least questionable.[13] Consequently, the discrepancy in results must manifest itself either in the value of the output elasticity of tax shocks θ_Y, the elasticity of spending to tax policy shocks γ_T, or both.

The first two columns of Table 1 provide the estimates for the structural parameters in both SVAR specifications together with 95% confidence intervals. The main result is that the proxy SVAR strongly rejects the calibrated value for the output elasticity of tax revenues, $\theta_Y = 2.08$, assumed in the Blanchard-Perotti SVAR. The point estimate for θ_Y is 3.13 with 95% percentiles of 2.73 and

[13] Assuming alternatively that $\gamma_T = 0$ in the proxy SVAR leads to the same conclusions.

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 proxy SVAR. 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. The finding that the narrative identification strategy implies a higher cyclical sensitivity of tax revenues is robust to the use of alternative tax shock measures. The first column of Table 2 lists the estimated values for θ_Y for different proxies. The point estimates for the elasticities lie within a range of 2.70 to 3.30, all significantly higher than 2.08. The only outlier is the estimate for the proxy based on the unadjusted original Romer series, which we argued above is problematic.

The comparison of the SVAR estimates suggests that the main reason for the different tax multiplier estimates is the discrepancy in the output elasticity of tax revenues θ_Y.[14] The second column of Table 1 reports the parameter estimates when we impose $\theta_Y = 3.13$ instead of 2.08 in the Blanchard-Perotti SVAR. The estimates of the structural parameters are now essentially the same. Figure 6 illustrates the impulse response from the Blanchard-Perotti SVAR when assuming a value of 3.13 instead of 2.08 and compares with the estimates from the proxy SVAR. The tax multipliers are now as good as identical across both specifications. The confidence intervals for the proxy SVAR are slightly wider than those generated by the Blanchard-Perotti SVAR because the former take into account uncertainty in identification, whereas the latter treats θ_Y and γ_T as deterministic coefficients.

We make two further observations regarding the differences between the SVARs. First, the SVARs produce very similar government spending multipliers, shown in Figure 7. In both cases, the impact spending multipliers are around 0.75 and the maximum output effect is close to 1 two quarters after

[14]Caldara and Kamps (2008, 2012) show that this elasticity is also the source of the difference between the tax multiplier estimates of Blanchard and Perotti (2002) and the sign restriction SVAR of Mountford and Uhlig (2009).

the shock. This is because the $\gamma_T = \gamma_Y = 0$ restrictions in Blanchard and Perotti (2002) suffice to identify only the government spending shock, see also Fatas and Mihov (2001). Since the proxy SVAR estimate of γ_T is approximately zero, the higher cyclical sensitivity of revenues has little effect on the response of output to a spending shock. Most applications of the Blanchard-Perotti methodology in the literature have been concerned with estimating the effects of shocks to government spending. At least for the US, the cyclical adjustment of tax revenues is not a reason to question these applications. However, in contrast to Blanchard and Perotti (2002), the proxy SVAR finds decisive evidence that the tax multiplier is larger than the spending multiplier.

The other observation regards the subsample stability of the results. Studies that rely on SVARs to estimate the response to fiscal policy shocks in the US often find them to be unstable over time. Perotti (2005) documents that a tax cut has positive output effects before 1980 and zero or even negative output effects thereafter. The left panel of Figure 8 shows that this is also the case in our application of the Blanchard-Perotti SVAR.[15] Whereas the output response in the first subsample is similar to the one in the full sample, the output response post 1980 is close to zero. The right panel of Figure 8 shows that in the proxy SVAR the evidence for instability is considerably weaker. The impact multipliers cannot be rejected to be of equal size in the subsamples as in the full sample. Only at horizons beyond two years is there some evidence that the output response is lower after 1980. Most studies argue that the output elasticity of tax revenues has increased after the Tax Reform Act of 1986, see e.g. Follette and Lutz (2010). Our point estimates for the output elasticities are 2.76 before 1980, and 3.86 after 1980 and are thus consistent with that claim.

[15] We imposed values of θ_Y of 1.75 in the pre 1980 sample, and 1.97 in the post 1980 sample. These values were take from Perotti (2005).

3 .2 Cyclical Sensitivity of Tax Revenues: High or Low?

The size of the tax multipliers estimated in SVARs hinges critically on the cyclical adjustment of tax revenues through the value of the quarterly output elasticity of tax revenues θ_Y. The proxy SVAR estimates this parameter based on a narrative measure for exogenous tax shocks. Blanchard and Perotti (2002) instead rely on an application of the OECD methodology of Giorno et al. (1995) to quarterly data. The lower value of θ_Y implied by this methodology cannot easily be explained by variations in sample coverage, as later applications yield values that are similar.[16] What may explain the difference between these existing methodologies and our estimates based on the narrative data? The OECD methodology obtains a value of θ_Y as a weighted average of the output elasticities of separate revenue components, each of which is the product of two sub-elasticities,

$$\theta_Y = \sum_i \eta^i_{T,B} \eta^i_{B,Y} \frac{T_i}{T} .$$

where T_i/T is the average total tax revenue share of revenue component i. The components are personal income taxes, social security contributions, indirect taxes and corporate income taxes. The first elasticity, $\eta^i_{T,B}$, is the elasticity of tax revenues to changes in the tax base. To account for pro- and regressivity of personal and social security taxes, Giorno et al. (1995) compute $\eta^i_{T,B}$ as the ratio of weighted averages of the marginal and average tax rates with weights derived from estimated earnings distributions. For corporate and indirect taxes, the elasticity is set to unity by assumption. This approach is a rough approximation at best, involves many ad-hoc assumptions and ignores for instance cyclical effects on tax expenditures, filing rates, income shifting and tax

[16] See for instance van den Noord (2000) and Girouard and André (2005). The same methodology is used by the International Monetary Fund, see Bornhorst et al. (2011). A closely related methodology is used to obtain estimates that are embedded in policy evaluations of the FRB/US model, see Cohen and Folette (2000) and Follette and Lutz (2010). At least one CBO document claims the same methodology is also used at the Congressional Budget Office, see CBO (2010).

compliance.[17] Furthermore, interest and dividend income, income of the self-employed as well as capital gains are excluded from the calculations. It also ignores any endogenous response of tax policy itself.[18] The second elasticity, $\eta^i_{B,Y}$ is the elasticity of the tax base with respect to GDP and is in all approaches estimated by a least squares regression of the tax base on (detrended) GDP. In the application of Blanchard and Perotti (2002), it is the lag zero OLS coefficient in regressions of the quarterly growth rates of the tax base on a distributed lag of GDP growth rates. Such regressions make no attempt at resolving problems of simultaneity and are therefore likely to yield biased estimates.[19]

In light of all these difficulties with the OECD and related methodologies, it is not surprising that the proxy SVAR detects greater cyclical sensitivity of tax revenues. As discussed by Romer and Romer (2010), the main purpose of the narrative approach is to provide a more convincing resolution to the problem of cyclical adjustment. Our estimation approach makes this resolution explicit in the context of the standard SVAR framework. We are not aware of many other studies that report estimates of the cyclical sensitivity of tax revenues based on identifying exogenous variation in either taxes or economic activity.[20] One exception is Brückner (2011), who uses rainfall and commodity prices as instruments for exogenous variation in GDP in Sub-Saharan countries and finds output elasticities of 2.5, much higher than these implied by the OECD methodology for those countries. For the

[17]For a theoretical model of how tax evasion increases the procyclicality of revenues, see Caballé and Panadés (2011). For international evidence for a procyclical tax revenue/tax base ratio due to tax evasion and other behavioral responses, see Sancak, Velloso, and Xing (2010).

[18]The alternative procedure to obtain the elasticities $\eta^i_{T,B}$ described in Follette and Lutz (2010) is more attentive to some of these issues. However it relies on a mix of ad-hoc assumptions and reduced-form regressions without corrections for simultaneity. It also depends more heavily on annual data, which complicates comparison since θ_Y is a quarterly elasticity.

[19]There is good reason to expect the OLS estimate for $\eta^i_{B,Y}$ to have a negative bias due to simultaneity. A negative bias would occur when the tax base depends positively on output but output depends negatively on changes to the tax base, a constellation that would arguably be natural.

[20]In contrast, there is a large public finance literature that studies the elasticity of taxable income to tax rates, see Saez, Slemrod and Giertz (2009) for a recent survey.

United Kingdom, Cloyne (2012) estimates an elasticity of 1.61 using narrative data on tax changes. This compares to a value of 0.76 calculated by Perotti (2005) based on the OECD method adjusted for quarterly data. For the United States, Caldara and Kamps (2012) show that the sign restriction approach of Mountford and Uhlig (2009) results in an elasticity estimate centered around 3.0, which is remarkably close to our estimate. In Mertens and Ravn (2011b), we estimate the response of US federal tax revenues to a long run identified technology shock. The implied value for the output elasticity of revenues is 3.7. All of these findings favor the higher cyclical sensitivity of tax revenues and the associated large tax multipliers found in the proxy SVAR.

Although both SVARs are by construction consistent with the covariance structure between output and tax revenues in the sample, one may worry that the higher output elasticity estimated by the proxy SVAR translates into implausible dynamics for the cyclical component of tax revenues. Figure 9 compares the cyclical components with actual tax revenues over the sample period, all in percentage deviations from trend. The cyclical component T_t^c is generated from the revenue equation in the SVAR system:

$$T_t^c = \alpha' d_t + \sum_{j=1}^{4} \delta_{TT}^j T_{t-j}^c + \sum_{j=1}^{4} \delta_{TY}^j Y_{t-j} + \sum_{j=1}^{4} \delta_{TG}^j G_{t-j} + \theta_G \sigma_G \varepsilon_t^G + \theta_Y u_t^Y \tag{7}$$

using the observed series for output Y_t and government spending G_t, the observed initial conditions as well as the estimated sequences of u_t^Y and ε_t^G. Note that the difference in the cyclical components predicted by the alternative SVARs is determined almost entirely by the value of θ_Y. If the proxy SVAR indeed exaggerates the output elasticity, one could expect the cyclical component of tax revenues to be excessively sensitive to fluctuations in economic activity. Figure 9 shows this is not the case. The standard deviation of the cyclical component in the proxy SVAR is nearly identical to the standard deviation of actual revenues, whereas in the Blanchard-Perotti SVAR it is 7% less volatile. Furthermore, the correlation with actual tax revenues is 0.94 in the proxy SVAR and 0.82 in the Blanchard-Perotti SVAR. Thus, conditional on observing output (and government spending), the proxy SVAR actually has greater explanatory power for the dynamics of tax revenues.

In a final evaluation of the proxy SVAR, we analyze the behavior of tax revenues during the 2007-2009 recession. The prior assumption is that this recession was unlikely to be caused by tight tax policy. If the value of θ_Y estimated in the proxy SVAR is implausibly high, then it will overestimate the endogenous drop in tax revenues in 2008-2009 and require large exogenous tax increases to rationalize the data. This would seem at odds with the various tax incentives provided by the federal government during the recent recession under the Economic Stimulus Act (enacted February 2008) and the American Recovery and Reinvestment Act (enacted February 2009). Figure 10 depicts output and tax revenues in deviation from their levels in 2007Q4 as well as the cyclical drops predicted by both SVARs. The cyclical responses are generated by (7) from 2008Q1 onwards based on the coefficients estimated from pre-2007 data. The latter are thus not influenced by the more recent observations. The proxy SVAR explains the observed fall in tax revenues remarkably well in terms of a purely endogenous response to output developments. It thus views the enacted tax stimuli as part of the systematic fiscal policy response typical for the US since WWII. The lower cyclical elasticity of the Blanchard-Perotti SVAR, on the other hand, explains the observed revenue drop only in part as an endogenous response to the decline in economic activity, and assigns a more important role for discretionary and supposedly unanticipated exogenous tax decreases.

While the results of Figures 9 and 10 do not allow a definitive conclusion regarding which cyclical decomposition is more realistic, they do refute the potential criticism that the tax elasticities estimated by the proxy SVAR are implausibly large. Given the problems with the cyclical adjustment procedures of international organizations and the markedly higher estimates found by the proxy SVAR and several other studies, we conclude that the evidence weighs in favor of large tax multipliers.

4 Reconciling the Proxy SVAR and Standard Narrative Estimates

The standard applications of the narrative approach do not rely on direct assumptions about cyclical elasticities, yet still deliver quite different results from the proxy SVAR. All rely crucially on the exogeneity of the narrative measure τ_t. There are however two key differences between the standard narrative models and our proxy SVAR. The first difference is the scaling of the tax shocks. The proxy SVAR estimates multipliers by scaling according to the impact on actual tax revenues, as in the Blanchard-Perotti SVAR. The narrative studies instead scale the tax shocks by their projected impact on tax liabilities. These government projections, in turn, are based on calculations assuming no effect on the tax base as a result of the policy change. In Mertens and Ravn (2012b) we show that cuts in personal and corporate taxes lead to increases in taxable incomes. For this reason, the output responses in the narrative studies underestimate the tax multiplier as calculated in the SVARs.

The second difference is robustness to various other types of measurement error. The existing narrative studies require τ_t to contain direct observations of tax shocks. The proxy SVAR instead only requires a nonzero correlation with the tax shocks such that potential measurement problems in τ_t are much less problematic. Some error in measurement certainly seems likely, as the construction of narrative shocks inevitably involves many judgment calls. Various government documents often contradict each other on the precise budgetary impact of changes in tax legislation, see Romer and Romer (2009). The narrative series may also suffer from censoring problems, for instance because it excludes changes to the tax code deemed revenue neutral, and ignores some of the less significant legislative changes. In addition, as we have already mentioned, the narrative series is based on projected changes in tax liabilities and is therefore not necessarily a good measure of actual changes in tax revenues, which is what is required to compute the tax multiplier.[21]

[21] See also Perotti (2011) on this issue. His IV approach is robust to additive measurement error, but not to arbitrary scaling of the tax shocks. Moreover it relies on excluding all dynamics from the tax revenue equation.

Measurement error leads to biased estimates in the standard narrative specifications. For example, suppose that the relationship between the narrative measure τ_t and the latent structural tax shock ε_t^T is given by a linear measurement equation,

$$\tau_t = \nu + m_t = \nu + \phi\varepsilon_t^T + \upsilon_t \ , \tag{8}$$

where ν is a constant, υ_t is random measurement error with $E[\upsilon_t] = 0$, $E[\upsilon_t^2] = \sigma_\upsilon^2$ and $E[\upsilon_t \upsilon_s] = 0$ for $s \neq t$.[22] Equation (8) allows for two types of measurement error: the additive noise υ_t and the fact that τ_t can be arbitrarily scaled. Clearly, the variable m_t satisfies the conditions in (4)-(5) and the proxy SVAR will provide unbiased estimates of the impulse response function associated with the tax shock ε_t^T. However when $\sigma_\upsilon^2 \neq 0$, including τ_t and lags thereof as regressors will instead lead to biased estimates of the slope coefficients. This is because, as in the standard narrative specifications given above, the terms involving τ_t are no longer uncorrelated with the residuals. Classical approaches to dealing with measurement error in dynamic regressions are based on instrumental variables (Maravall and Aigner (1977)), spectral methods (Hsiao (1979)), or extraction of latent factors from multiple measurements (e.g. Bernanke, Boivin and Eliasz (2005)). Neither of these alternatives are practical with a single narrative measure that is unpredictable and contains many zero observations. In addition, unless the observations in τ_t are correctly scaled, obtaining tax multipliers by tracing out the response to τ_t will produce estimates that are also wrongly scaled.

When the measurement error takes the form in (8), the proxy SVAR allows for the identification of the statistical reliability of the proxy variable m_t as a measurement of the latent tax shock ε_t^T. The reliability of m_t is defined as the fraction of the variance of the measured variable that is explained by the latent variable, and is a useful diagnostic tool to judge the quality of the narrative data. Equiv-

[22] We abstract for simplicity from potential censoring issues, see Mertens and Ravn (2012b).

alently, it is the squared correlation between the proxy m_t and the true tax shock ε_t^T. An estimator of the reliability of m_t is given by

$$\Lambda = \left(\phi^2 \sum_{t=1}^{T} \mathbf{1}_t \left(\varepsilon_t^T\right)^2 + \sum_{t=1}^{T} \mathbf{1}_t \left(m_t - \phi \varepsilon_t^T\right)^2 \right)^{-1} \phi^2 \sum_{t=1}^{T} \mathbf{1}_t \left(\varepsilon_t^T\right)^2 . \tag{9}$$

where $\mathbf{1}_t$ is an indicator function for a nonzero observation of m_t. In Mertens and Ravn (2012b), we show how an estimate of ϕ can be obtained by combining equation (6) with the restrictions implied by the estimated covariance matrix of the VAR residuals u_t. The resulting reliability Λ lies between zero and one with larger values indicating a higher correlation between the proxy and the true underlying tax shock. The statistic is asymptotically equivalent to the R squared of the regression in Figure 3.[23] Estimates of Λ allow a ranking of the different proxy measures for tax innovations according to their reliability.

The second column of Table 2 lists the estimates of the reliability of the various proxies that we considered in Section 2 .3. In the benchmark proxy SVAR, the estimated reliability of the narrative measure of the tax shocks is 0.57. The bootstrapped 95% confidence region for Λ is $0.50 - 0.61$. The implied correlation between m_t and the true underlying tax shock is 0.75. Hence, the identified tax shocks align well with the historical record of legislated federal tax changes in the US documented by Romer and Romer (2009). The reliability estimates vary across the various alternative proxies. Two narrative measures stand out for having significantly lower reliability. The reliability of the original Romer series is 0.34, which signals that excluding observations on anticipated tax changes as measures of unanticipated tax shocks is important to improve the quality of the proxy. The reliability of the Romer series in which the anticipated tax changes were adjusted for expectations implied by municipal bond yields is 0.22. Since this measure gave very similar results to

[23]The main difference is that for Λ we use an estimate of ϕ that is consistent with the estimated covariance matrix of the VAR residuals rather than the least squares estimate in the regression of the nonzero observations of m_t on the estimated ε_t^T.

those of the benchmark, it ends up being just a noisier version of the benchmark proxy. Two tax shock measures are more reliable than the benchmark proxy, the unanticipated shocks adjusted for expectations derived from municipal bond yields and the series with only tax changes motivated by long run growth objectives, but the differences are only marginal. The remaining proxies all have similar but nonetheless lower reliability than our benchmark proxy.

While the estimated reliability is relatively high, it does suggest measurement error bias is a potential concern for the standard narrative approaches. To assess the consequences of measurement error (including scaling issues) in the estimation of tax multipliers in the standard narrative approaches, Figure 11 depicts results from two counterfactual simulations. We draw $10,000$ bootstrap samples using the estimated proxy SVAR as the data generating process. First, we estimate the three alternative narrative specifications in each artificial sample using as the series for τ_t the bootstrap realization of the *measured* tax shock series, inclusive of measurement error. The blue lines represent the mean output response to a tax cut across the artificial samples. Second, we re-estimate the three narrative specifications using the bootstrap realization of the *true* structural tax shocks, i.e. without measurement error and correctly scaled. We censor these counterfactual narrative measures such that they contain the same nonzero observations as the original series. The red lines depict the resulting mean output responses. The difference between the blue and red lines captures the average effect of the measurement problems on the point estimates in all three narrative specifications. The black lines reproduce the results in actual US data depicted in Figure 1 for comparison.

The simulations reveal the source for the difference between the proxy SVAR results and the standard narrative specifications. The responses in Figure 11 show 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 proxy SVAR. The average responses when τ_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 (2012) specification, the average simulated response aligns almost perfectly with the response in the actual US data at all horizons. For the other two specifications, which contain a moving average term of τ_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 than the actual estimates but never leaves the 95% confidence bands. When the true tax shocks are used as the narrative measure (red lines), the average responses to a tax cut across all specifications are significantly higher and are all close to the true response in the data generating process in Figure 3. Another result from the simulations is that, regardless of whether τ_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 time series specifications. This finding corroborates the simulation evidence of Charhour, Schmitt-Grohé and Uribe (2012), who use an alternative data generating process based on the estimated DSGE model of Mertens and Ravn (2011a) 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.

In the case of the Favero-Giavazzi model, a straightforward correction for measurement error is simply to rescale the impulse response such that tax revenues drop by one percentage point of GDP on impact. This adjustment not only eliminates the scaling problem mentioned earlier, but in case τ_t is given by (8) also corrects for the additive error. This is because the Favero-Giavazzi model includes a single error-ridden regressor that is by assumption uncorrelated with the other regressors. Additive measurement error implies a proportional and identical attenuation bias equal to Λ in every equation such that impulse responses are correct up to scale. Figure 12 shows that the adjustment almost completely resolves the difference with the proxy SVAR. Based on the estimate for Λ, around 40% of the difference between the impact coefficients is explained by bias due to additive measurement error, whereas the remainder is due to differences between the impact on projected tax liabilities versus actual revenues. This decomposition may be different for alternative assumptions about the nature of the measurement error. Figure 12 does not report confidence bands for the adjusted Favero-Giavazzi estimates, which are very wide because the ratio of impact coefficients is very imprecisely estimated. The same adjustment is not appropriate for the other narrative specifications because they include multiple error-ridden regressors.

We conclude that a reconciliation with the findings of the existing narrative estimates can be found in a more careful treatment of measurement problems. The proxy SVAR results imply that, once measurement error is allowed for, the narrative data is supportive for relatively large tax multipliers even in the short run.

5 Concluding Remarks and Directions for Future Research

A burgeoning empirical literature on the aggregate effects of changes in tax policy has produced a range of estimates of the effects on economic activity sufficiently broad that one might question the value of the findings. In this paper, we analyze the underlying reasons for the disagreement among the various methodologies. We do this by an application of a structural vector autoregression in which tax shocks are identified by proxies based on narrative tax shock measures. Our proxy SVAR estimates large tax multipliers in US data with relatively high precision. A comparison with the popular Blanchard and Perotti (2002) approach reveals a fundamental conflict in the cyclical adjustment of tax revenues. We argue that the output elasticity of tax revenues is significantly greater than calculated by international organizations. Differences with earlier narrative studies can be explained by measurement error, which our proxy SVAR identifies in the data. The evidence in this paper is supportive for tax multipliers that are at the higher end of the range, such as those of Mountford and Uhlig (2009) and Romer and Romer (2010), and rejects the lower estimates of for instance Blanchard and Perotti (2002) and Favero and Giavazzi (2012). Unlike all these studies, however, we also find large output effects of tax changes in the short run.

There are several directions for future research. Our analysis raises concerns with the cyclical adjustment procedures of government and international institutions and calls for alternative, structural, approaches to the estimation of the output elasticity of tax revenues. This is important since this elasticity is a vital ingredient of policy evaluations, budget forecasting, and other empirical work, e.g. on fiscal consolidations by Alesina and Ardagna (2010). In focusing attention on cyclical adjustment and measurement error, we followed the common practice of studying the effects of shocks to total revenues in linear models. Other aspects of the empirical study of tax policy shocks, such as the dependence on the type of the tax instrument being adjusted or the possible time-varying nature of tax multipliers can be incorporated. Given that narrative measures of policy shocks become increasingly available, our analysis can be repeated in the future for other countries as well as for other types of shocks. Finally, our analysis is also informative about features that can improve the explanatory and predictive power of theoretical models of fiscal policy. These features are likely to include large tax multipliers and high output elasticities of tax revenues.

References

Alesina, Alberto and Silvia Ardagna, 2010, "Large Changes in Fiscal Policy: Taxes versus Spending", NBER Chapters, in: Tax Policy and the Economy Vol. 24, pp. 35–68.

Auerbach, Alan J., and Yuriy Gorodnichenko, 2012, "Measuring the Output Responses to Fiscal Policy," forthcoming, American Economic Journal: Economic Policy.

Barro, Robert J. and Charles J. Redlick, 2011, " The Macroeconomic Effects of Government Purchases and Taxes", Quarterly Journal of Economics 126(1),pp. 51–102.

Bernanke, Ben S., Jean Boivin and Piotr Eliasz, 2005, "Measuring the Effects of Monetary Policy: A Factor-Augmented Vector Autoregressive (FAVAR) Approach", Quarterly Journal of Economics, 120(1), pp. 387–422.

Blanchard, Olivier and Roberto Perotti, 2002, "An Empirical Characterization of the Dynamic Effects of Changes in Government Spending and Taxes on Output", Quarterly Journal of Economics 117(4), pp. 1329–1368.

Bornhorst, Fabian, Gabriela Dobrescu, Annalisa Fedelino, Jan Gottschalk, and Taisuke Nakata, 2011, "When and How to Adjust Beyond the Business Cycle? A Guide to Structural Fiscal Balances", Technical Notes and Manual, Fiscal Affairs, International Monetary Fund.

Brückner, Markus, 2011, "An Instrumental Variables Approach to Estimating Tax Revenue Elasticities: Evidence from Sub-Saharan Africa", forthcoming, Journal of Development Economics.

Congressional Budget Office, 2010,"The Effects of Automatic Stabilizers on the Federal Budget", CBO Report.

Caballé, Jordi and Judith Panadés, 2011. "Tax Evasion, Technology Shocks, and the Cyclicality of Government Revenues," UFAE and IAE Working Papers 870.11, Universitat Autònoma de Barcelona

Canova, Fabio and Evi Pappa, 2007, "Price Differentials in Monetary Unions: The Role of Fiscal Shocks", Economic Journal 117(4), pp. 713–737.

Caldara, Dario and Christophe Kamps, 2008, "What Are the Effects of Fiscal Shocks: A VAR-Based Comparative Analyis.", ECB Working Paper No 877.

Caldara, Dario and Christophe Kamps, 2012, "The Analytics of SVARs: A Unified Framework to Measure Fiscal Multipliers", manuscript, Board of Governors.

Charhour, Ryan, Stephanie Schmitt-Grohé and Martin Uribe, 2012, "A Model-Based Evaluation of the Debate on the Size of the Tax Multiplier", forthcoming Americal Economic Journal: Economic Policy.

Cloyne, James, 2012, "What Are the Effects of Tax Changes in the United Kingdom? New Evidence from a Narrative Evaluation", manuscript, Bank of England

Cohen, Darrel and Glenn Follette, 2000, "The Automatic Fiscal Stabilizers: Quietly Doing Their Thing", Federal Reserve Bank of New York Economic Policy Review April 2000, pp. 35—67.

Fatas, Antonio and Ilian Mihov, 2001, "The Effects of Fiscal Policy on Consumption and Employment: Theory and Evidence", CEPR Discussion Papers No. 2760.

Favero, Carlo and Francesco Giavazzi, 2012,"Measuring Tax Multipliers. The Narrative Method in Fiscal VARs". American Economic Journal: Economic Policy, forthcoming.

Favero, Carlo and Francesco Giavazzi and Jacopo Perego, 2011, "Country Heterogeneity and the International Evidence on the Effects of Fiscal Policy", IMF Economic Review November, pp. 652–82.

Fisher, Jonas D.M. and Ryan Peters, 2010, "Using Stock Returns to Identify Government Spending Shocks", Economic Journal 120(544), pp. 414–436.

Follette, Glenn and Byron Lutz, 2010, "Fiscal Policy in the United States: Automatic Stabilizers, Discretionary Fiscal Policy Actions, and the Economy", manuscript, Board of Governors

Francis, Neville and Valerie A. Ramey, 2009, "Measures of per Capita Hours and Their Implications for the Technology-Hours Debate", Journal of Money, Credit and Banking 41(6), 1071-1097.

Girouard, Nathalie and Christophe André, 2005, "Measuring Cyclically Adjusted Budget Balances for OECD Countries", OECD Economics Department Working Papers No. 434, OECD Publishing.

Giorno, Claude, Pete Richardson, Deborah Roseveare and Paul van den Noord, 1995, "Estimating Potential Output, Output Gaps and Structural Budget Balances", OECD Economics Department Working Papers no.152.

Gonçalves, Silvia and Lutz Kilian, 2004, "Bootstrapping Autoregressions with Conditional Heteroskedasticity of Unknown Form", Journal of Econometrics 123(1), pp. 89–120.

Hausman, Jerry A. and William E. Taylor, 1983, "Identification in Linear Simultaneous Equation Models with Covariance Restrictions: An Instrumental Variables Interpretation", Econometrica 51(5), pp.1527-1549.

Hsiao, Cheng, 1979, "Measurement Error in a Dynamic Simultaneous Equations Model with Stationary Disturbances", Econometrica 47(2), pp. 475–494.

Ilzetzki, Ethan, 2011, "Fiscal Policy and Debt Dynamics in Developing Countries", World Bank Policy Research Working Paper Series No. 5666.

Kueng, Lorenz, 2011, "Identifying the Household Consumption Response to Tax Expectations using Bond Prices", manuscript, University of California, Berkeley.

Leeper, Eric M., Todd B. Walker and Shu-Chun Susan Yang, 2011, "Foresight and Information Flows", manuscript, Indiana University Bloomington.

Maravall, Agustin and Dennis J. Aigner, 1977, "Identification of the Dynamic Shock-Error Model: The Case of Dynamic Regression", in: Latent Variables in Socio-Economic Models, ed. by Aigner and A. Goldberger, Amsterdam, North Holland.

Mertens, Karel and Morten O. Ravn, 2011a, "Understanding the Aggregate Effects of Anticipated and Unanticipated Tax Policy Shocks", Review of Economic Dynamics, 14(1), pp. 27–54.

Mertens, Karel and Morten O. Ravn, 2011b, "Technology - Hours Redux: Tax Changes and the Measurement of Technology Shocks.", NBER International Seminar of Macroeconomics 2010, pp. 41–76.

Mertens, Karel and Morten O. Ravn, 2012a, "Empirical Evidence on the Aggregate Effects of Anticipated and Unanticipated U.S. Tax Policy Shocks", American Economic Journal: Economic Policy, forthcoming.

Mertens, Karel and Morten O. Ravn, 2012b, "The Dynamic Effects of Personal and Corporate Income Tax Changes in the United States", manuscript, Cornell University and University College London.

Mountford, Andrew, and Harald Uhlig, 2009, "What Are the Effects of Fiscal Policy Shocks?" Journal of Applied Econometrics 24(6), pp. 960–992.

Perotti, Roberto, 2005, "Estimating the Effects of Fiscal Policy in OECD Countries", CEPR Discussion Paper No. 4842.

Perotti, Roberto, 2011, "The Effects of Tax Shocks on Output: Not So Large, But Not Small Either", NBER Working Paper No. 16786.

Ramey, Valerie A., 2011, "Identifying Government Spending Shocks: It's All in the Timing", Quarterly Journal of Economics 126(1), pp. 10-50.

Romer, Christina D., and David H. Romer, 2009, "A Narrative Analysis of Postwar Tax Changes" manuscript, University of California, Berkeley.

Romer, Christina D., and David H. Romer, 2010, "The Macroeconomic Effects of Tax Changes: Estimates Based on a New Measure of Fiscal Shocks" , American Economic Review 100(3), pp. 763–801.

Sancak, Cemile, Ricardo Velloso, and Jing Xing, 2010, "Tax Revenue Response to the Business Cycle", IMF Working Paper No. 10/71.

Saez, Emmanuel, Joel B. Slemrod and Seth H. Giertz, 2012," The Elasticity of Taxable Income with Respect to Marginal Tax Rates: A Critical Review" , Journal of Economic Literature 50(1), pp. 3-50.

Stock, James H. and Mark W. Watson, 2008, " What's New in Econometrics - Time Series", NBER Summer Institute, Lecture 7.

Stock, James H. and Mark W. Watson, 2012, "Disentangling the Channels of the 2007-2009 Recession", Brookings Panel on Economic Activity, Conference Paper March 22-23.

van den Noord, Paul, 2000, "The Size and Role of Automatic Fiscal Stabilizers in the 1990s and Beyond", OECD Economics Department Working Papers, No. 230, OECD

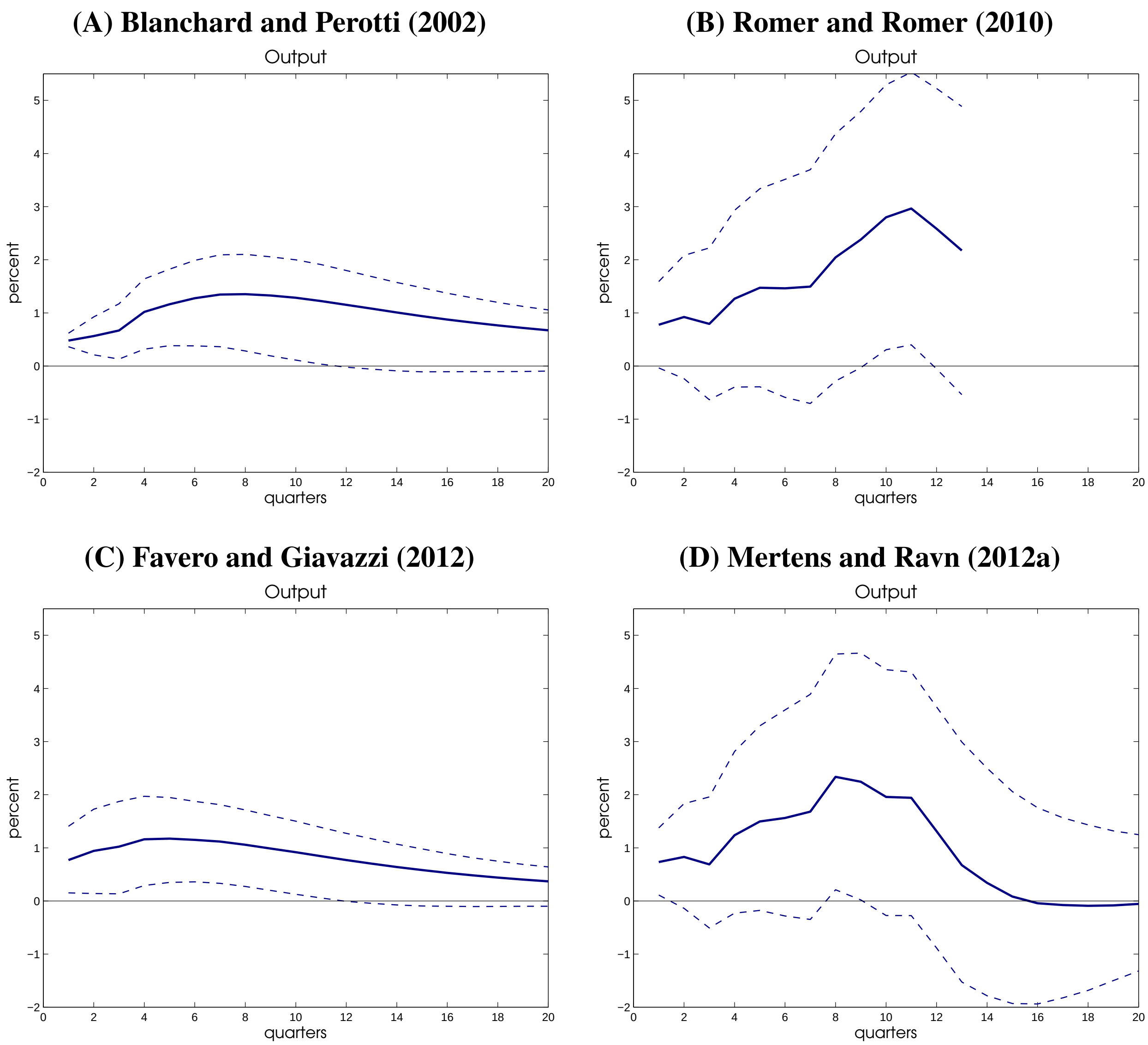


Figure 1 Replication of Existing Estimates of the Output Response to Tax Cuts. Broken lines in (A), (C) and (D) are 95% bootstrapped percentiles. Broken lines in (B) are ± 2 asymptotic standard error bands.

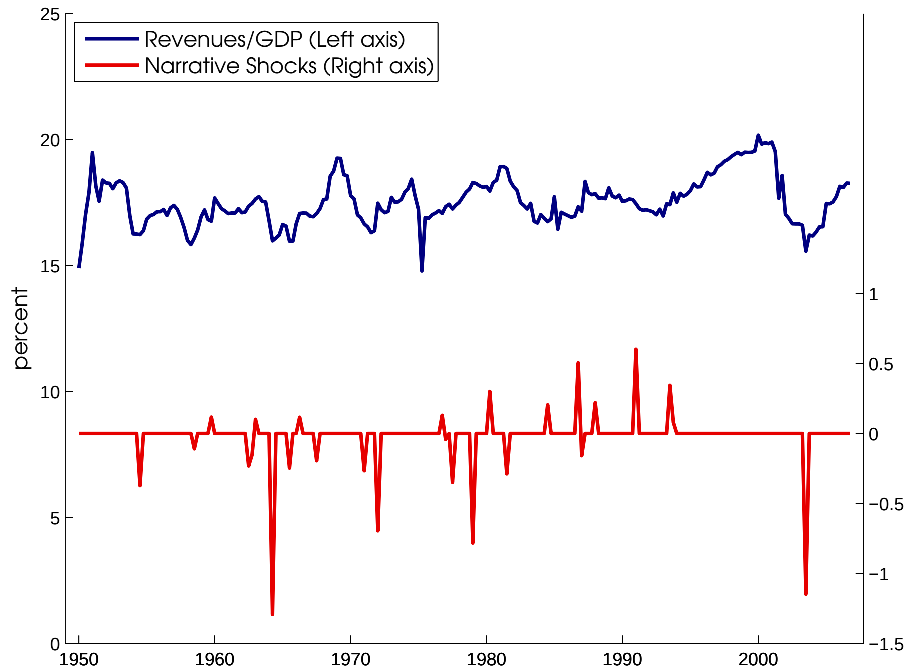


Figure 2 The Average Tax Rate and the Narrative Measure of Unanticipated Tax Shocks.

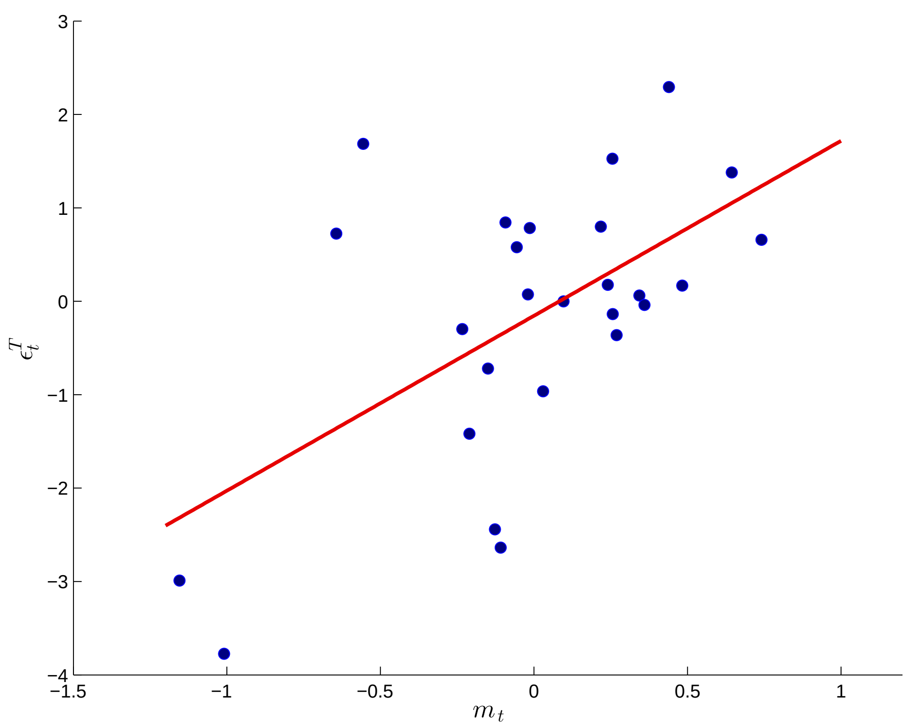


Figure 3 Identified Tax Shocks and the Proxy Measure

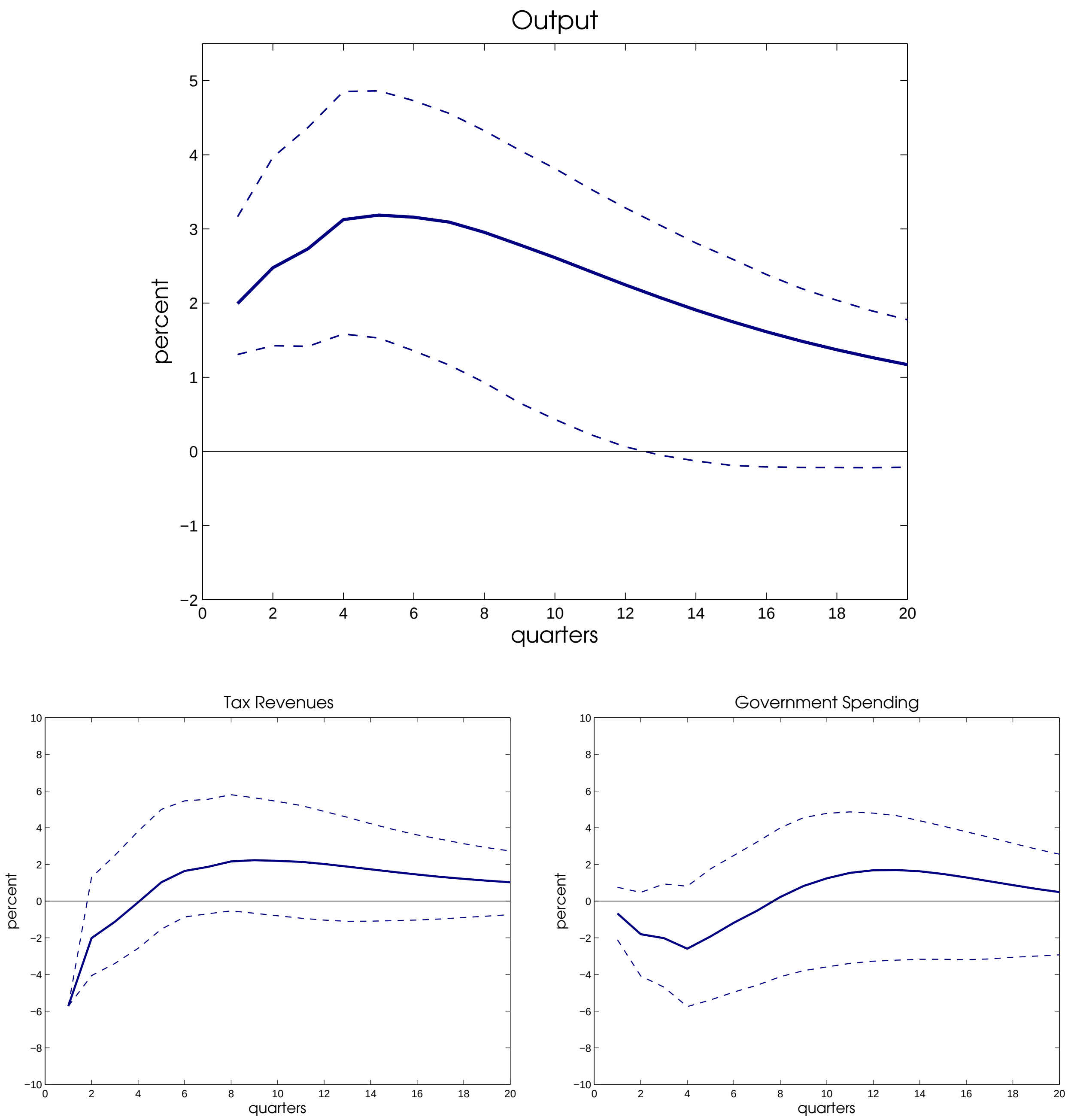
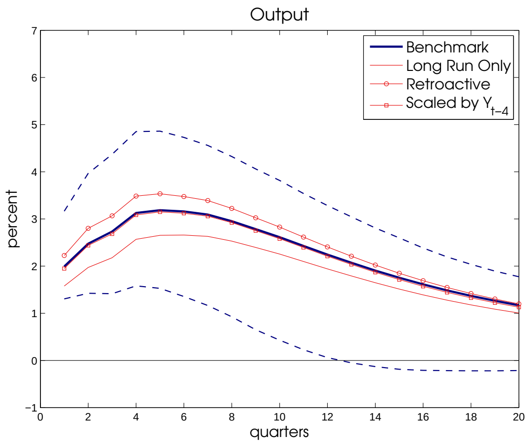
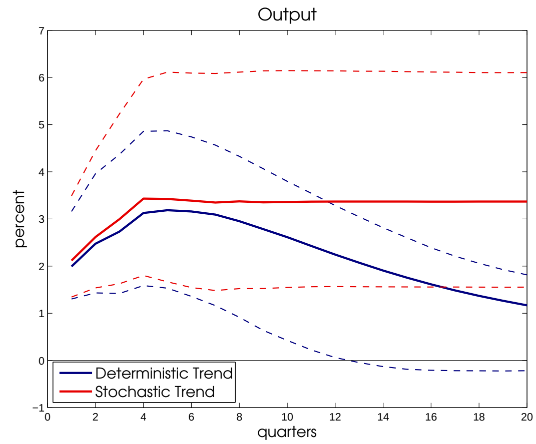


Figure 4 Proxy SVAR: Response to a Tax Cut of 1% of GDP. Broken lines are 95% bootstrapped percentiles.

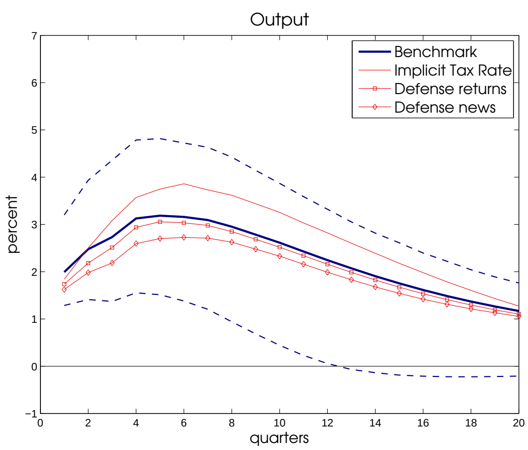
(A) Alternative Narrative Measures



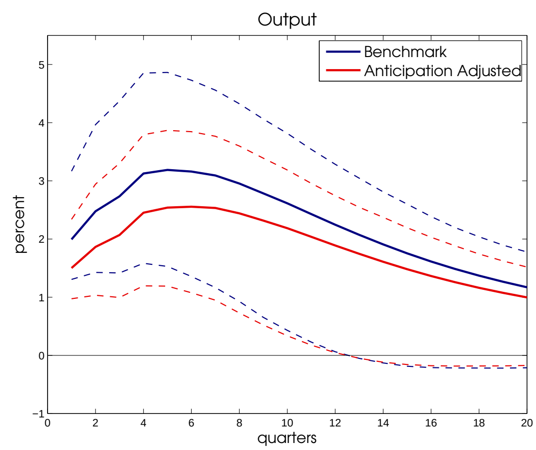
(B) Trend Assumptions



(C) Including Fiscal Foresight Variables

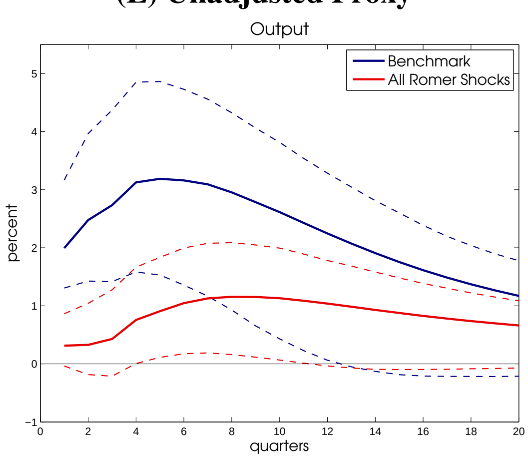


(D) Anticipation Adjusted Proxy



Using all Exogenous Romer and Romer (2009) shocks:

(E) Unadjusted Proxy



(F) Anticipation Adjusted Proxy

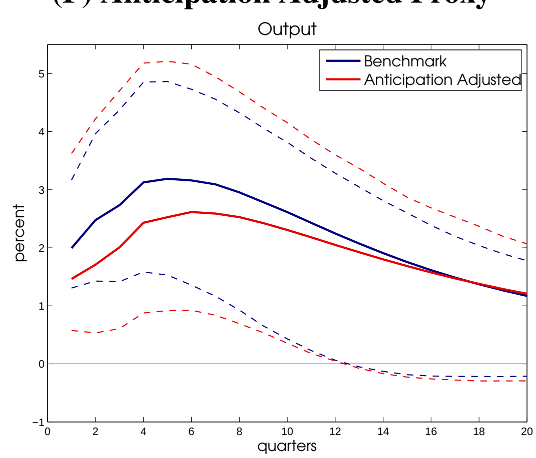


Figure 5 Robustness of Proxy SVAR. Broken lines are 95% bootstrapped percentiles.

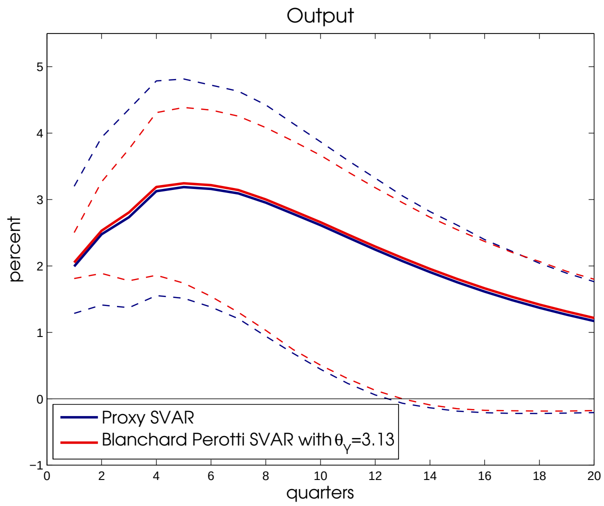


Figure 6 Tax Multiplier: Reconciling the SVAR estimates. Broken lines are 95% bootstrapped percentiles.

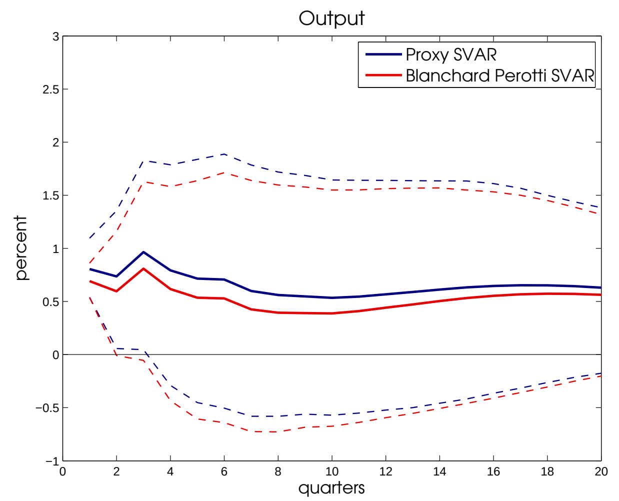


Figure 7 Response to a Spending Shock of 1% of GDP. Broken lines are 95% bootstrapped percentiles.

(A) Blanchard-Perotti SVAR **(B) Proxy SVAR**

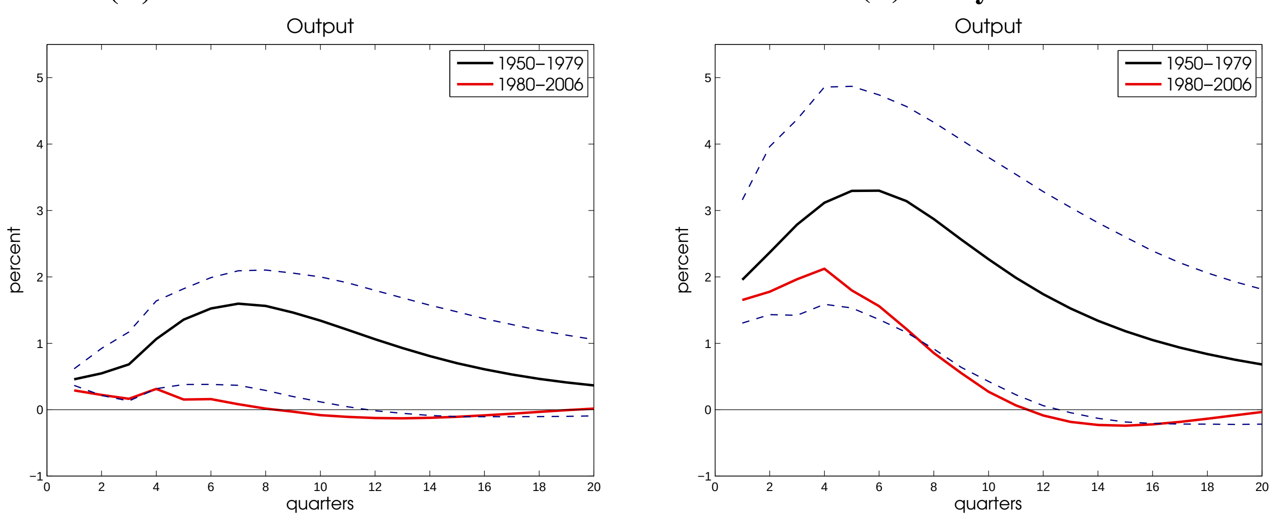


Figure 8 Subsample Stability. Broken lines are 95% bootstrapped intervals of the benchmark specification.

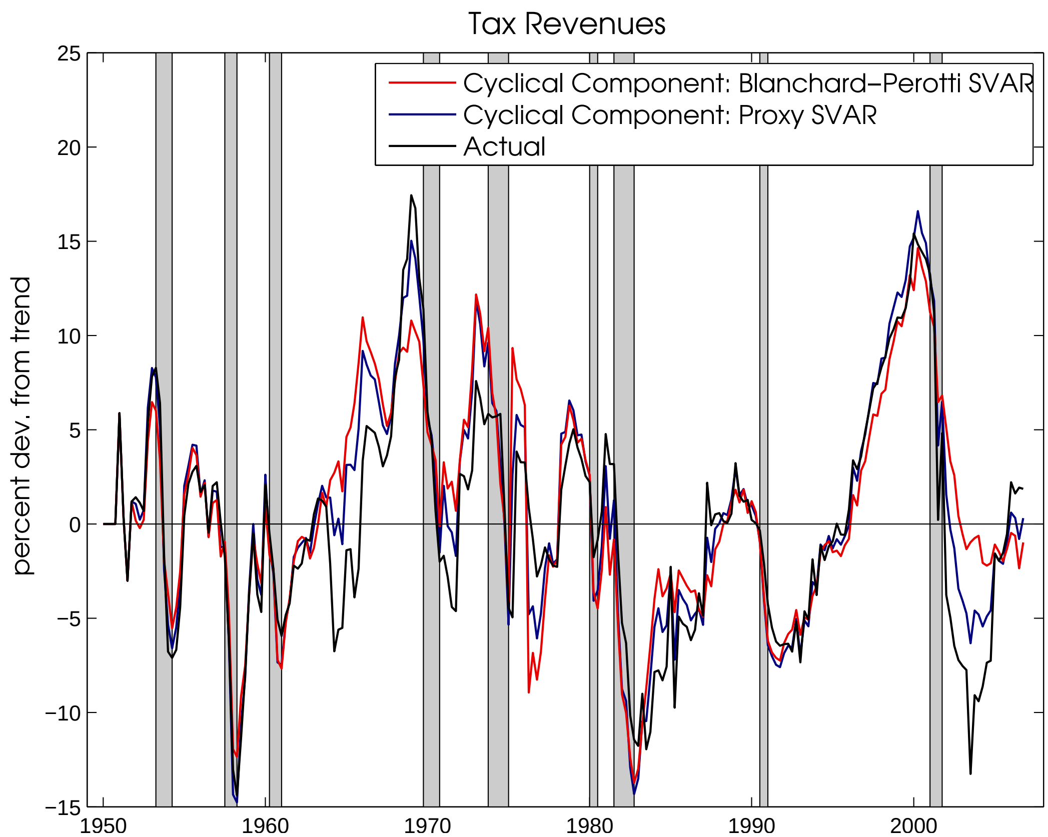


Figure 9 Actual and Cyclical Components of Tax Revenues Percent deviations from trend. Grey areas are NBER dated recessions.

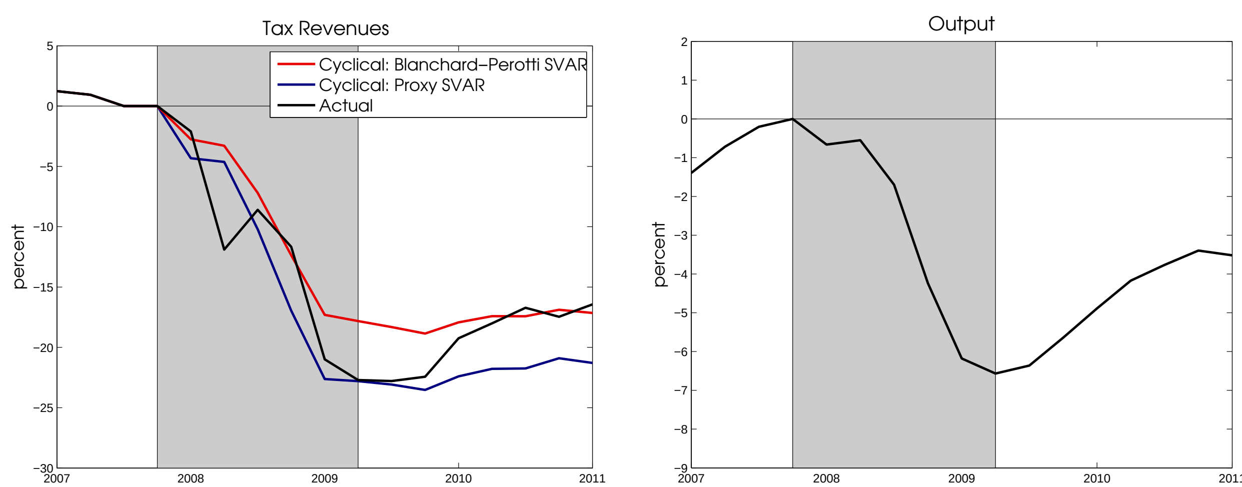


Figure 10 The Great Recession: Cyclical versus Actual Drop in Tax Revenues (Left) and Actual Output (Right). Percent deviations from 2007:Q4 levels. Grey area is the NBER dated recession.

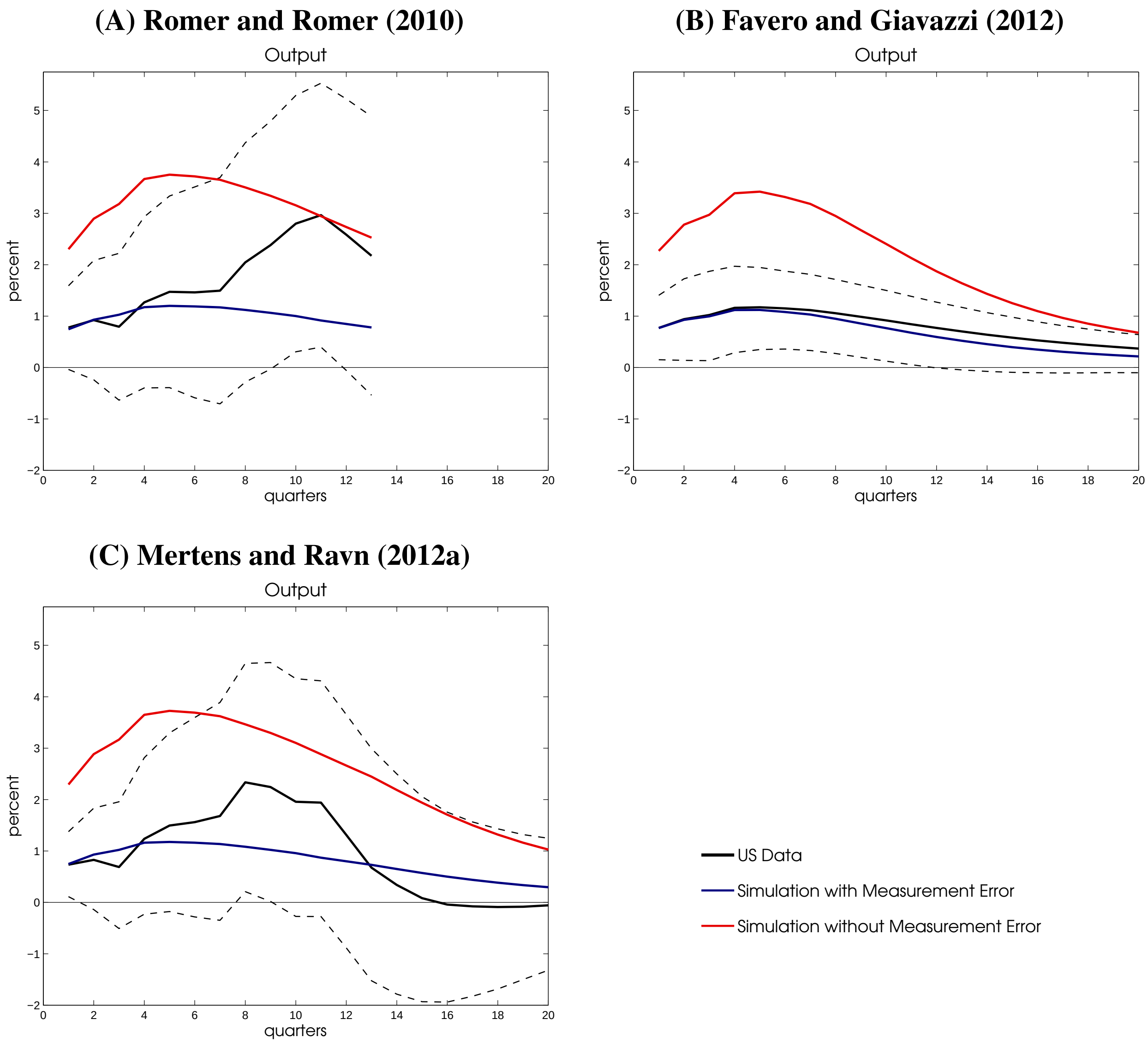


Figure 11 The Role of Measurement Error in Standard Narrative Approaches.

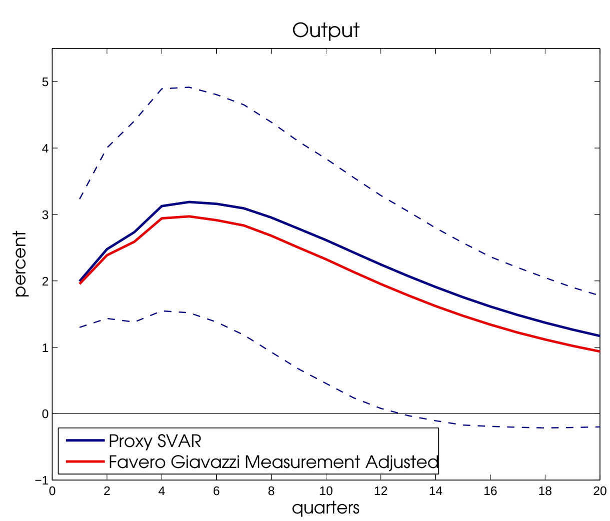


Figure 12 Proxy SVAR and Favero and Giavazzi (2012) Adjusted for Measurement Error. Broken lines are 95% bootstrapped percentiles.

Table 1 Parameter Estimates

Equation		**Proxy SVAR** Benchmark	**Blanchard-Perotti SVAR** $\theta_Y = 2.08$	$\theta_Y = 3.13$
Tax Revenue	θ_G	-0.20	-0.06	-0.13
		$[-0.35, -0.07]$	$[-0.12, -0.03]$	$[-0.19, -0.09]$
	θ_Y	3.13	2.08	3.13
		$[2.73, 3.55]$	–	–
	$\sigma_T \times 100$	2.54	2.24	2.56
		$[2.23, 2.62]$	$[2.04, 2.19]$	$[2.34, 2.51]$
Spending	γ_T	0.06	0	0
		$[-0.06, 0.17]$	–	–
	γ_Y	0	0	0
		–	–	–
	$\sigma_G \times 100$	2.35	2.36	2.36
		$[2.12, 2.30]$	$[2.13, 2.31]$	$[2.13, 2.31]$
Output	ζ_T	-0.36	-0.08	-0.36
		$[-0.57, -0.24]$	$[-0.11, -0.06]$	$[-0.43, -0.31]$
	ζ_G	0.10	0.07	0.10
		$[0.06, 0.13]$	$[0.06, 0.09]$	$[0.07, 0.12]$
	$\sigma_Y \times 100$	1.54	0.97	1.54
		$[1.21, 1.93]$	$[0.89, 0.98]$	$[1.37, 1.64]$

Values in parenthesis are 95% percentiles computed using 10,000 bootstrap replications.

Table 2 Parameter Estimates using Different Proxy Measures for Tax Shocks

Proxy Used	**Output Elasticity of Tax Revenues**, θ_y	**Reliability**
Benchmark (Figure 3)	3.13	0.57
	[2.73, 3.55]	[0.50, 0.61]
Long Run Shocks Only (Figure 4, A)	2.94	0.60
	[2.56, 3.33]	[0.56, 0.63]
Including Retroactive Provisions (Figure 4, A)	3.30	0.48
	[2.78 3.87]	[0.35, 0.54]
Scaled by Y_{t-4} (Figure 4, A)	3.14	0.57
	[2.73 3.57]	[0.45 0.61]
Benchmark, Anticipation Adjusted (Figure 4, D)	2.88	0.59
	[2.53, 3.25]	[0.53, 0.63]
All Romer Tax Shocks (Figure 4, E)	1.84	0.34
	[1.47, 2.29]	[0.25, 0.42]
All Romer Tax Shocks, Anticipation Adjusted (Figure 4, F)	2.70	0.22
	[2.07, 3.53]	[0.13, 0.30]

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