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# TAX MULTIPLIERS:
# PITFALLS IN MEASUREMENT AND IDENTIFICATION

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## ABSTRACT

We contribute to the literature on tax multipliers by analyzing the pitfalls in identification and measurement of tax shocks. Our main focus is on disentangling the discussion regarding the identification of exogenous tax policy shocks (i.e., changes in tax policy that are not the result of policymakers responding to output fluctuations) from the discussion related to the measurement of tax policy (i.e., finding a tax policy variable under the direct control of the policymaker). For this purpose, we build a novel value-added tax rate dataset and the corresponding cyclically- adjusted revenue measure at a quarterly frequency for 14 industrial countries for the period 1980-2009.

On the identification front, our findings favor the use of narratives à la Romer and Romer (2010) to identify exogenous fiscal shocks as opposed to the identification via SVAR. On the (much less explored) measurement front, our results strongly support the use of tax rates as a true measure of the tax policy instrument as opposed to widely-used, revenue-based measures, such as cyclically-adjusted revenues. While tax multipliers tend to be very small (in absolute value) or even positive when using cyclically-adjusted revenues, they are significantly negative (i.e., tax policy is contractionary) when using tax rates.

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# 1 Introduction

In the aftermath of the global financial crisis and ensuing recession triggered by the fall of Lehman Brothers on September 15, 2008, many governments across the world implemented aggressive countercyclical fiscal policies. More recently, large fiscal deficits and concerns about debt sustainability in industrial countries, particularly in Europe, have shifted the tone of the discussion from stimulus to fiscal adjustment. Since then – and following the influential paper by Giavazzi and Pagano (1990) – there has also been a revival of studies suggesting the possibility that fiscal adjustments might have no effect on output, or be even expansionary, especially when driven by spending cuts.[1] As a result of the policy focus first on fiscal stimulus and later on fiscal consolidation, there has also been a flourishing of studies estimating fiscal multipliers, both on the spending and the taxation side.

The main challenge and point of contention among researchers has been how to address the possible endogeneity of fiscal policy or, to put it differently, how to identify fiscal policy shocks (i.e., changes in fiscal policy variables that are not directly or indirectly related to output changes). Two main approaches have been used to address these endogeneity problems:

1. One strand of the literature has followed the seminal contribution of Blanchard and Perotti (2002) by imposing short term restrictions in the context of structural vector autoregressions (SVAR). On the expenditure side, the Blanchard-Perotti approach assumes that discretionary government spending requires at least one quarter to respond to news about the state of the economy. On the taxation side, the output elasticity of tax revenues is used to differentiate "discretionary" changes in taxation (also referred to as changes in cyclically-adjusted revenues) from those driven by endogenous reactions of tax revenues to output fluctuations. The Blanchard-Perotti approach is by far the most popular identification strategy due to its ease of implementation and data availability (e.g., Perotti, 2004; Favero and Giavazzi, 2007; Mountford and Uhlig, 2009; Favero and Giavazzi, 2010; Ilzetzki, 2011; and Ilzetzki, Mendoza, and Vegh, 2013). This strategy has been criticized on the basis that most changes in government spending and taxes are actually anticipated by agents (e.g., Ramey and Shapiro, 1998; Leeper, Walker, and Yang, 2008; Ramey, 2011; Auerbach and Gorodnichenko, 2011; and Riera-Crichton, Vegh, and Vuletin, 2015).

[1] See, for example, McDermott and Wescott (1996), Giavazzi, Jappelli, and Pagano (2000), von Hagen and Strauch (2001), von Hagen, Hughes, and Strauch (2002), Lambertini and Tavares (2003), Ardagna (2004), Alesina and Ardagna (2010), and IMF (2010).

2. Another strand of the literature has developed around the so-called "natural experiment" approach. This identification strategy assumes that some exogenous changes in fiscal policy are, in fact, observable. On the spending side, studies have typically focused on particular spending categories, such as military buildups, that are unlikely to respond to output fluctuations (e.g., Barro, 1981; Ramey and Shapiro, 1998; Hall, 2009; Ramey, 2011; and Barro and Redlick, 2011). On the taxation side, Romer and Romer (2010), hereafter RR, use the narrative record – such as presidential speeches and congressional reports – to identify, on a case-by-case basis, the nature of legislated federal tax changes in the United States from 1945 to 2007. RR identify tax changes exogenous to the business cycle, either because they were passed for long-run growth reasons or involve increases seeking to reduce an inherited budget deficit. On tax changes induced by long-run growth considerations, they argue that: "[t]he quintessential exogenous change might be a tax cut motivated by a belief that lower marginal tax rates will raise output in the long run. Such an action is fundamentally different from the countercyclical actions [...] because the goal is to raise normal growth, not to offset shocks acting to reduce growth relative to normal." Regarding deficit-driven tax changes, they argue that "[a]n inherited deficit reflects past economic conditions and budgetary decisions, not current conditions or spending changes. If policymakers raise taxes to reduce such a deficit, this is not a change motivated by a desire to return growth to normal or to prevent abnormal growth. So it is exogenous."

Our paper contributes to the literature on tax multipliers. Building on the above discussion on *identification*, we revisit the merits of the SVAR versus the "natural experiment" approach.[2] To implement the latter, we use as our starting point the action-based fiscal consolidation episodes identified by an IMF study in 15 industrial countries for the period 1980-2009 and then develop our own narrative to ensure exogeneity. This strategy is in the spirit of RR's identification of tax changes driven by inherited fiscal deficits.

Our main and novel contribution, however, is related to the much less explored issue of *measurement* of tax policy (i.e., finding a tax policy variable under the direct control of the policymaker). As discussed in Kaminsky, Reinhart, and Vegh (2004) and Vegh and Vuletin (2012), an obvious, yet critical, observation is that policymarkers' main tax instrument is the tax *rate*. While policymakers control and legislate on tax rates, tax revenues are a policy *outcome* not under the policymakers'

[2] In this dimension, our paper is thus related to the recent contribution of Mertens and Ravn (2013).

direct control. To fix ideas, define real tax revenues ($R$) as follows:

$$R_t = TAX\ RATE_t \cdot TAX\ BASE_t. \tag{1}$$

where $TAX\ BASE_t$ denotes the real tax base. The policymaker controls $TAX\ RATE$ but not $TAX\ BASE$ and, hence, does not control $R$.[3] Taking logarithmic changes, it follows from equation (1) that

$$r_t - r_{t-1} = (tax\ rate_t - tax\ rate_{t-1}) + (tax\ base_t - tax\ base_{t-1}), \tag{2}$$

where $r$, *tax rate*, and *tax base* are the log of real tax revenues ($R$), tax rate ($TAX\ RATE$), and the real tax base ($TAX\ BASE$), respectively. Equation (2) indicates that the percentage change in tax revenues ($\Delta r_t \equiv r_t - r_{t-1}$) can be decomposed into the sum of the percentage change in the tax rate ($\Delta tax\ rate_t \equiv tax\ rate_t - tax\ rate_{t-1}$) and the percentage change in the tax base ($\Delta tax\ base_t \equiv tax\ base_t - tax\ base_{t-1}$).

Given the lack of readily-available cross country data on tax rates, the standard measure utilized in the tax multiplier literature to capture discretionary changes in tax policy is the so-called cyclically-adjusted revenues (e.g., Giavazzi and Pagano, 1990; Alesina and Perotti, 1997; Blanchard and Perotti, 2002; Alesina and Ardagna, 2010; Favero and Giavazzi, 2010; Perotti, 2011; and Ilzetzki, 2011). The change in cyclically-adjusted revenues is typically calculated as:

$$\Delta cyclically\text{-}adjusted_t = r_t - r_{t-1} - \eta\,(y_t - y_{t-1}), \tag{3}$$

where $y$ is the log of real output and $\eta$ is the historical (i.e., average) output elasticity of tax revenues. The first two terms on the right-hand side of (3) denote the percentage change in tax revenues. The third term aims at capturing the percentage change in tax revenues associated with GDP-driven changes in the tax base. In principle, this cyclically-adjusted measure offers an intuitive way of dealing with the fact that the tax base moves endogenously with the business cycle. The idea is, of course, that once tax revenues are cyclically-adjusted, changes will reflect the discretionary action of policymakers. Indeed, assuming that $tax\ base_t = \eta \cdot y_t$, it follows that $\Delta cyclically\text{-}adjusted_t = \Delta tax\ rate_t$.[4,5] In other words, cyclically-adjusted revenue changes seem to capture discretionary changes in tax policy (i.e. changes in tax rates).

[3] This concern is particular to tax policy, since goverment spending is, in principle, the appropriate policy *instrument* on the spending side.

[4] Assuming that $tax\ base_t = \eta \cdot y_t$ implies, of course, that $\eta$ is the elasticity of the tax base with respect to GDP.

[5] To see this, replace (2) in (3) and use the fact that $\Delta tax\ base_t = \eta \cdot \Delta y_t$.

While appealing at first, revenue-based measures of tax policy, such as cyclically-adjusted revenues, suffer from important measurement errors. Mirroring the discussion in the growth literature on the Solow residual, the cyclically-adjusted measure implicitly attributes any change in revenues not associated with the estimated change in the tax base to policymakers' discretionary behavior. This source of measurement error would include, among many others, structural breaks, non-linearities and/or changes in agents' behavior over the business cycle, changes in $\eta$ over time, changes in taxpayers' willingness/possibility to evade taxes, and changes in income distribution.

In sum, there is no good substitute for obtaining data on tax *rates* themselves when it comes to measuring changes in the tax policy instrument. To this effect – and to assess the practical relevance of this measurement issue –we build a novel tax rate dataset and the corresponding cyclically-adjusted revenue measure and compare the implications in terms of the size of the tax multiplier. Given the lack of readily-available data on average marginal individual and/or corporate income tax rates for this group of countries, we focus our efforts on building a new quarterly VAT rate series. Our sample includes 14 industrial countries for the period 1980-2009.[6]

Table 1 summarizes our analysis regarding the *identification* of exogenous fiscal policy shocks (i.e., fiscal policy changes that are not the result of policymakers responding to output fluctuations) as separate from problems related to the *measurement* of tax policy (i.e., finding a tax policy variable under the direct control of the policymaker).

*INSERT TABLE* 1 *HERE*

Our main results can be summarized as follows:

1. *Identification* of tax shocks (i.e., "natural experiment" versus SVAR): Our findings clearly suggest that the "natural experiment" approach is the more accurate strategy to identify exogenous fiscal shocks. As in RR, we show that when using SVAR as our identification strategy, various tax series respond significantly to output fluctuations. These results support previous critiques of the Blanchard-Perotti identification, suggesting that the, presumably, unanticipated changes in fiscal policy (at a one quarter horizon) may actually be anticipated by agents. In contrast, when using exogenous fiscal consolidation episodes as the identification strategy, we show that tax series do not respond to output fluctuations. These findings validate the "natural experiment" approach since tax changes do not seem to be driven by

[6] We exclude the United States from the IMF's 15 country sample because it does not have federal VAT.

policymakers' short-run reactions to GDP fluctuations.

2. *Measurement* of tax policy (i.e., cyclically-adjusted revenues versus tax rates): Our findings strongly support the use of changes in tax rates as a measure of tax policy. Indeed, we show that cyclically-adjusted revenues consistently fail to proxy for changes in tax rates. In fact, in more than 70 percent of the countries, cyclically-adjusted revenue changes are negatively autocorrelated at the quarterly frequency. This would indicate (under the common presumption that this measure proxies for changes in discretionary tax policy) that policymakers' discretionary policy changes are reversed every quarter! Not surprisingly, these measurement differences have important implications for tax multipliers. Multipliers based on tax rate increases are always negative (i.e., tax policy is contractionary). In sharp contrast, tax multipliers based on cyclically-adjusted revenues are close to zero or even positive.

3. Most recent tax studies (e.g., Favero and Giavazzi, 2010; Caldara, 2011; and Ilzetzki, 2011) rely on SVAR and cyclically-adjusted revenue changes (bottom-right cell in Table 1). In line with most of these papers, when using this identification strategy we find that tax multipliers are not negative. In fact, a one unit increase in tax revenues *raises* output by 0.19 on impact and 0.80 after four quarters. When focusing on exogenous fiscal consolidation and cyclically-adjusted revenue changes (top-right cell in Table 1), tax multipliers also tend to be positive (i.e., tax policy is expansionary). When using SVAR and changes in tax rates (bottom-left cell in Table 1), tax multipliers are negative (i.e., tax policy is contractionary). The multiplier is -0.31 on impact and -1.17 after three quarters. When using exogenous fiscal consolidation and tax rate changes (top-left cell in Table 1), tax multipliers are strongly negative. A one unit revenue increase shock reduces output by 0.66 on impact and 3.56 after three quarters. This wide range of results shows the importance of the strategy used to identify exogenous fiscal shocks and, more importantly for our purposes, the measurement of tax policy.

The rest of the paper is organized as follows. Section 2 discusses data and identification. Section 3 computes tax multipliers using exogenous fiscal consolidation episodes (to identify exogenous fiscal shocks) and tax rates (to measure tax policy) and performs numerous robustness tests. Section 4 examines the implications of alternative empirical strategies regarding the identification of tax shocks and measurement of tax policy. Section 5 concludes.

## 2 Data and identification

After introducing our dataset on VAT taxation, this section discusses our main identification strategy.[7] We close this section by presenting our cyclically-adjusted data and comparing it to our data on tax rate changes.

### 2.1 VAT data

Our VAT dataset covers 14 industrial countries at a quarterly frequency for the period 1980-2009.[8] Overall – and as depicted in Figure 1 – we have 42 changes in the VAT rate (32 increases and 10 reductions). Excluding zeros, the median tax rate change is 1 percentage point and the average is 0.7 percentage points. The largest tax rate reduction and tax rate increase are 3.5 and 3 percentage points, respectively. For the same group of countries (though sample periods differ depending on data availability), we also obtained data for VAT revenues (for the purposes, as discussed below, of computing cyclically-adjusted revenues).

### 2.2 Identification of exogenous fiscal consolidation VAT rate changes

Our "narrative-based" empirical strategy relies on the identification of fiscal consolidation episodes carried out in the October 2010 *World Economic Outlook* by the IMF, which covers the period 1980-2009. Examining a variety of documents (including OECD Economic Surveys, IMF Staff Reports, IMF Recent Economic Developments reports, country budget documents, and additional country-specific sources), this IMF study characterizes a fiscal consolidation episode as a situation "in which the government implemented tax hikes, [...] or spending cuts [...] to reduce the budget deficit and put public finances on a more sustainable footing" (IMF, 2010, page 96).

As a first approximation, it would seem natural to view changes in the VAT rate associated with the fiscal consolidation episodes identified by the IMF (2010) as exogenous to the business cycle (i.e., to quarterly changes in GDP in our case) since such tax changes are presumably part of a fiscal package primarily designed to deal with chronic or inherited budget deficits. There are two scenarios, however, in which this exogeneity assumption may not be valid (in which case the

[7] For details on other (standard) data used in this paper, see Appendix 6.1.

[8] See Appendix 6.1 for data sources and sample periods. The countries included are Australia, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, Japan, Portugal, Spain, Sweden, and United Kingdom. We did not include in the analysis other long-standing OECD countries because they are not included in the IMF (2010) study that identifies fiscal consolidation episodes. This IMF study covers 15 countries (the 14 countries listed above and the United States) for the period 1980-2009. We did not include the United States in our sample because it does not have a federal VAT. The VAT rate for Canada corresponds to the federal VAT rate.

underlying identification strategy would not be correct). First, because of potential links between the *current* fiscal balance and tax policy. Specifically, it seems plausible that a sudden (i.e., within the quarter) and large change in the fiscal balance may trigger a change in the VAT rate in the same quarter. This could take the form of a sudden deterioration in the fiscal balance caused by a fall in economic activity that induces an increase in the VAT rate to close the fiscal gap, or a sudden improvement in the fiscal accounts that triggers a reduction in tax rates for political economy considerations. Second, an increase in the VAT rate aimed at offsetting other factor(s) that would tend to move output growth away from normal. These factors may include (i) a cut in other tax rates (e.g., a reduction in the employers' contributions to social security) and/or (ii) additional public spending (e.g., job creation programs).

In light of these two possible scenarios – and following the narrative approach – we further analyze the nature of the 19 VAT rate changes that occurred in the episodes of fiscal consolidation identified by the IMF (hereafter "fiscal consolidation tax rate changes").[9] Bearing in mind the first concern raised above, we evaluate whether the narrative supports the idea of changes in VAT rates triggered by a sudden fiscal deterioration (improvement) resulting from a downturn (upturn) in economic activity, in which case we classify the change as endogenous. Regarding the second type of concern, we evaluate whether the narrative supports the idea of increases in VAT rates intended to offset the expansionary effects of government spending or other tax cuts (not included in our set of controls), in which case we also classify the change as endogenous**.** In contrast, when changes in VAT rates during episodes of fiscal consolidation were motivated by inherited fiscal deficits and/or medium and long term solvency problems, we classify them as exogenous.

Table 2 summarizes the results of our analysis for the 19 fiscal consolidation tax rate changes.[10] The table indicates country name (Column 1), date of implementation of the tax change (Column 2), change (in percentage points) of the VAT rate (Column 3), a brief narrative and a binary classification into endogenous and exogenous (Column 4), Lastly, column 5 shows (only for exogenous fiscal consolidation VAT rate changes) the date of passage of the law mandating the change in the VAT rate, which will be instrumental in evaluating (in Subsection 3.4) the role of expectations in the computation of tax multipliers.

*INSERT TABLE 2 HERE*

[9] More precisely, we match our sample of VAT rate changes to the IMF's sample of fiscal consolidation episodes, and look at the intersection of the two (i.e., fiscal consolidation episodes in which the VAT rate changed).

[10] The detailed narrative underlying Table 2 can be found in Appendix 6.3.

Based on our narrative analysis, Table 2 identifies 75 percent (14 out of 19) of the fiscal consolidation VAT rate changes as exogenous to short-run output fluctuations. Most of the endogenous fiscal consolidation VAT rate changes (4 out of 5) correspond to cases where the VAT rate hike was part of a fiscal consolidation package that was also intended to stimulate economic activity by reducing other taxes, such as social security contributions. In one case (France in 2000), the VAT rate reduction was triggered by the idea that low deficits made it possible to lower existing (and high) tax rates. In Section 3, we will use these 14 exogenous tax rate changes to compute tax multipliers.

### 2.3 Cyclically-adjusted revenues

Figure 2 plots both the VAT rate and VAT cyclically-adjusted revenue changes for each of our 14 countries.[11] The changes in VAT cyclically-adjusted revenues have been computed using expression (3), quarterly VAT revenue, quarterly output growth data, and estimated values of $\eta$ for indirect taxation from van den Noord (2000).[12] Three features are worth noting:

1. While VAT rate changes are discrete in nature and zero most of the time, VAT cyclically-adjusted revenue changes show, by construction, a continuous-variable pattern.

2. The contemporaneous correlation between changes in cyclically-adjusted revenues and the tax rate is very low (0.04) and not significantly different from zero.[13] Even when tax rate changes are not zero, the contemporaneous correlation between changes in cyclically-adjusted revenues and the tax rate is rather low (0.21) and not significantly different from zero. If we restrict our comparison to exogenous fiscal consolidation tax rate changes, the correlation is -0.05 and not significantly different from zero.

3. We complement our analysis by looking at some basic dynamic properties of the cyclically-adjusted measure, which helps us in understanding some inherent weaknesses of this revenue-based measure. Specifically, Figure 2 reports two statistics for each country (see bottom note

[11] Note that Figure 2 has been plotted for the common sample period and hence may not coincide with the sample period in Figure 1.

[12] See Appendix 6.1 for details.

[13] One possible source of low contemporaneous correlation may be differences in timing. Suppose, for example, that revenues are turned over to the government with a one-quarter lag. Then there might be a low correlation between the contemporaneous changes in cyclically-adjusted revenues and the tax rate, but a high correlation between the change in cyclically-adjusted revenues and the lagged change in the tax rate. This conjecture, however, is not warranted as the correlation between cyclically-adjusted revenues and lagged tax rates changes (up to four quarters) ranges between 0.07 and 0.0005.

in each panel). The first is the Ljung-Box test statistic, which tests the null hypothesis that the data are independently distributed. The second is the autocorrelation coefficient ($\beta$) that results from estimating a simple AR1 process (i.e., $\Delta cyclically\text{-}adjusted_t = \alpha + \beta \cdot \Delta cyclically\text{-}adjusted_{t-1}$). The first statistic tells us that in almost 80 percent of the countries (11 out of 14) the changes in cyclically-adjusted tax revenues are not independently distributed over time. Moreover, the second statistic tells us that, in more than 70 percent of the countries (10 out of 14), cyclically-adjusted revenue changes are negatively autocorrelated at the quarterly frequency. This would suggest (under the common presumption that this measure is a proxy for changes in discretionary tax policy) that policymakers' discretionary policy changes are reversed every quarter! Since this is clearly highly implausible, it calls into question the usefulness of cyclically-adjusted revenue changes as a proxy for changes in tax policy.

In Section 4, we will use our full sample of 42 VAT rate changes and our cyclically-adjusted measures in the context of the Blanchard-Perotti identification to argue that, regardless of the identification strategy, cyclically-adjusted revenues are a bad proxy for tax policy.

# 3 The effect of exogenous fiscal consolidation tax rate changes on output

This section studies the effect of exogenous fiscal consolidation VAT rate changes on economic activity. Our two benchmark specifications indicate that tax increases have a strong negative effect on output. We then perform a myriad of robustness tests – alternative error structures, adding time dummies, changing the timing assumption, controlling for other fiscal variables, and taking into account the role of expectations – and conclude that our main results continue to hold in all cases.

## 3.1 Benchmark specifications

We begin our analysis by estimating the effect of tax rate changes on economic activity using, as in RR, the following basic specification:

$$\Delta y_{i,t} = \alpha + \sum_{j=0}^{J} \beta_j \Delta t_{i,t-j} + \sum_{i=1}^{I-1} \beta_i d_i + \mu_{i,t}, \tag{4}$$

where $J = 4$, $I = 14$, $y$ is the logarithm of real GDP (and $\Delta y$ is thus the real GDP growth rate, expressed as the difference in logarithms), $\Delta t$ is the change in tax rate in percentage point terms and represents exogenous fiscal consolidation tax rate changes, $\alpha$ is a constant term, $d$ is a country dummy, and $\mu$ is the error term. In estimating equation (4), we include four lags (i.e., $J = 4$).[14]

This approach should yield unbiased reduced-form impact estimates of tax rate changes on output (see top-left cell in Table 1) given (i) the exogenous nature of tax rate changes and (ii) the tax variable used is a tax instrument (as opposed to a revenue-based one, such as the cyclically-adjusted revenue).[15]

While conceptually appropriate, a drawback of using changes in tax rates as the independent variable in equation (4) is that the estimated coefficients $\beta$ do not correspond to the usual tax multiplier for GDP (i.e., the response of GDP to a unit increase in tax revenues). In other words, the coefficients $\beta$ link the change in GDP to the change in the tax rate and not in tax revenue. To think in terms of standard multipliers, we define the multiplier for time horizon $J$ as [16]

$$Tax\ multiplier\ (J) \equiv \frac{\sum_{j=0}^{J} \Delta Y_j}{\sum_{j=0}^{J} \Delta R_j}. \tag{5}$$

We compute the tax multiplier defined above by exploiting the typical relationship of tax revenues to the tax rate. Let $R$ be real VAT revenue, $Y$ real output, $I$ "implicit" VAT rate (defined as $R/Y$), and $e$ the average relationship between $I$ and $t$ (i.e., $e \equiv I/t$). Following Barro and Redlick (2011, pp. 80-81), the tax multiplier at different time horizons $J$ is then computed as [17]

$$Tax\ multiplier\ (J) = \frac{\omega_J}{e + \omega_J \cdot \overline{I_J}}, \tag{6}$$

where $\omega_J$ represents the cumulative effect on the growth rate of output of a one percentage-point shock in taxes. In the case of specification (4), $\omega_J = \sum_{j=0}^{J} \beta_j$. $\overline{I_J}$ measures the average "implicit"

[14] The selection of four lags balances the need to account for a sufficiently long structure of lags in order to study the effect of fiscal consolidations as well as the need to preserve most of our tax rate changes. Unfortunately, as we move towards longer lag structures, we are forced to drop data points from an already small set of tax rate changes. Having said that, our results for the case of eight and twelve lags remain almost the same as in the four quarter specification. The main difference is that the cumulative multiplier becomes more negative starting in the eight quarter. Figures showing the multipliers for the eight and twelve lag estimations are not shown for the sake of brevity.

[15] Given concerns about the non-normal distribution of tax rate changes (which are zero most of the time) and small sample considerations, standard errors are computed using bootstrap techniques. In particular, standard errors are calculated from the average of 10,000 draws of the coefficient vector from a multivariate normal distribution with mean and variance-covariance matrix equal to the point estimates and variance-covariance matrix of the regression coefficients.

[16] No substantial difference in our results would arise if we discounted by the median real deposit rate.

[17] See Appendix 6.4 for the derivation of the next two expressions.

VAT rate (i.e., $\overline{I_J} \equiv \left(\sum_{j=0}^{J} I_j\right)/J$.[18]

Using a first-order approximation of (6), the standard error of the tax multiplier can be written as

$$Tax\ multiplier_{SE}\ (J) = \frac{e}{\left(e + \omega_J \cdot \overline{I_J}\right)^2}\omega_J^{SE}, \tag{7}$$

where $\omega_J^{SE}$ is the standard error of $\omega_J$.

Figure 3 uses estimates from (4) to illustrate the tax multiplier together with one-standard-error bands. We can see that the multiplier is consistently and significantly negative. In particular, the multiplier is -0.66 on impact and increases (in absolute value) with longer horizons until reaching -3.69 after four quarters.[19]

*INSERT FIGURE 3 HERE*

We now enrich specification (4) by adding four lags of output growth:

$$\Delta y_{i,t} = \alpha + \sum_{j=0}^{J}\beta_j \Delta t_{i,t-j} + \sum_{n=1}^{N}\beta_n \Delta y_{i,t-n} + \sum_{i=1}^{I-1}\beta_i d_i + \mu_{i,t}. \tag{8}$$

where $J = 4$ and $N = 4$. As discussed in RR, including lagged growth helps in controlling for the normal dynamics of output as well as providing a simple way of controlling for other factors affecting output, most likely serially correlated. Using the estimates from (8), Figure 4 shows the corresponding tax multiplier.[20,21] For comparison purposes, this figure also reports the findings from the specification without GDP growth lags (i.e., specification (4)). Results are virtually unchanged both in terms of point estimates and precision.

*INSERT FIGURE 4 HERE*

### 3.2 Robustness tests

We now perform several robustness tests that build upon the specification in equation (8).[22]

[18] We use the sample average values of $e$ (38 percent) and the "implicit" VAT rate (6 percent) for our tax multiplier calculations. While there is some variation across countries, the variability is fairly small. The standard deviations of $e$ and the "implicit" VAT rate are 6 and 2 percent, respectively.

[19] Results are virtually not affected if country dummies are not included. Results are not shown for brevity.

[20] As in the case of regression (4), standard errors in specification (8) are computed using bootstrap techniques.

[21] The impulse response function of a shock in taxes in specification (8) includes both the direct impact of taxes on output (captured by the coefficients $\beta_j$) and the indirect cumulative effect trough the lags of output growth (captured by the coefficients $\beta_n$).

[22] Similar results are obtained if we use the first benchmark specification, given by (4). Results are not shown for the sake of brevity.

### 3.2.1 Alternative error structure

We first assume an alternative error structure. Specifically, Figure 5 shows the results when using Driscoll and Kraay (1998) standard errors, which allow the error term to be serially and cross-sectionally correlated. Our results are not affected.

*INSERT FIGURE 5 HERE*

### 3.2.2 Time dummies

We then allow for time dummies. Figure 6 shows that, relative to our benchmark specification with no time dummies, the impact multiplier is marginally larger (in absolute value) on impact, but remains virtually unchanged afterwards.

*INSERT FIGURE 6 HERE*

### 3.2.3 Timing

We now look at the issue of timing. As is standard in the literature, our benchmark specification – given by (8) – imputes the tax rate change to the quarter in which it was effectively implemented (independently of whether such a tax rate change was implemented at the beginning, middle, or end of the quarter). One may argue, however, that if the tax rate change was, for example, implemented towards the end of the quarter, the impact on output should only be observed in the following quarter. If so, this would bias the multiplier downwards (in absolute value).

Table 2 shows that while more than 70 percent (or 10 out of 14) of exogenous fiscal consolidation tax rate changes were implemented in the very first day of each quarter, more than 20 percent (or 3 out of 14) were implemented in the last month of each quarter.[23] Only one tax rate change was implemented in the second month of the quarter. To account for these instances, Figure 7 shows the results of an alternative measure of tax rate changes in which tax rate changes implemented in the last month of a quarter are imputed to the following quarter.[24] As we see, the results remain virtually unchanged.

*INSERT FIGURE 7 HERE*

[23] In six episodes, the change in the VAT was implemented on January 1, reflecting the start of the new calendar year.

[24] Virtually identical results are obtained if one also imputes the only tax rate change that was implemented in the second month of a quarter to the following quarter.

### 3.2.4 Two-variable SVAR

Lastly – and following RR – we use a natural variation of specification (8): a two-variable pooled SVAR with real GDP growth and exogenous fiscal consolidation tax rate changes. In keeping with the regression specifications on the contemporaneous relationship between tax rate and output, we allow tax rate changes to affect output contemporaneously but not vice-versa. Of course, the construction of a narrative identifying exogenous fiscal consolidation tax rate change makes the use of an SVAR approach trivial. In other words, as long as the narrative is correct, one would not need to run a system of equations and make any assumptions about the contemporaneous relation between exogenous fiscal consolidation tax rate changes and real GDP growth rate in order to identify a fiscal shock.

Having said that, we follow RR and the literature more generally and still estimate an SVAR for several reasons: (i) to provide some empirical evidence that will "validate" our narrative approach for exogenous fiscal consolidation tax rate changes (i.e., analyze whether exogenous fiscal consolidation tax rate changes are, indeed, unrelated to recent past output fluctuations); (ii) to help address potential endogeneity concerns when including other controls variables, such as government spending; and (iii) to compare our exogenous tax measure with other measures of tax policy changes; in particular with the commonly used cyclically-adjusted revenue changes.

We now address the first item (the second will be addressed in Subsection 3.3 and the third in Section 4). Figure 8 shows the tax multiplier when using the two-variable SVAR.[25] We can see that the multiplier is virtually identical to the one obtained using specification (8).[26]

*INSERT FIGURE 8 HERE*

To empirically "validate" our narrative approach, Figure 9 shows the impulse response function of the tax series to a shock in GDP. After a one percent innovation in output, tax rates barely fluctuate between 0.002 and -0.003. The effect is highly statistically insignificant at all time horizons. The $p$-value for the test of the null hypothesis that output does not Granger-cause the tax series is 0.96. This finding strongly supports our conjecture that tax rate changes identified using exogenous fiscal consolidation episodes (i.e., through historical narratives) are, indeed, unrelated to

[25] As in (4) and (8), we use four lags in our estimation of the two-variable SVAR .

[26] For the sake of comparison with Figure 8, we restrict the plot to four quarters (since in the case with the single equation approach the cumulative multiplier beyond period 4 would be constant).

past output fluctuations.

*INSERT FIGURE 9 HERE*

### 3.3 Controlling for changes in other fiscal variables

Following RR, we now control for changes in other fiscal variables such as government spending and other taxes. Government spending, of course, may affect output on its own and hence bias the tax multiplier. For instance, consider an episode of fiscal consolidation where government spending typically falls at the same time that tax rates increase. Omitting the former would bias the tax multiplier downwards, giving the false idea that tax rate hikes are more contractionary than they actually are. The opposite would be true when government spending increases together with tax rate hikes, perhaps in an attempt to offset the potential contractionary effect of tax rate hikes on output. Since the data do not show any systematic contemporaneous relation (at a quarterly frequency) between changes in government spending and tax rates during episodes of fiscal consolidation (the correlation is -0.02 and not statistically different from zero), we do not expect this potential problem to be an issue, but a formal analysis is clearly warranted.

In order to control for government spending, we use a three-variable SVAR specification with four lags of real GDP growth rate, exogenous fiscal consolidation tax rate changes, and government spending changes occurring during periods of fiscal consolidation. In keeping with the regression specifications on the contemporaneous relationship between tax rate and output, we allow tax rate changes to affect output contemporaneously but not viceversa. The same is true for government spending (i.e., government spending can affect output contemporaneously but not viceversa). As is common practice, we also assume no contemporaneous relation between government spending and tax rates.[27]

Figure 10 shows the results of the three-variable SVAR. The tax multiplier remains virtually unchanged after controlling for government spending. This was to be expected to the extent that, as mentioned above, the contemporaneous correlation is essentially zero. While there could still be some dynamic feedback from changes in tax rates to government spending, Figure 10 suggests that

[27] As in Blanchard and Perotti (2002) and Favero and Giavazzi (2007), results remain virtually unchanged when $\alpha_{gt}$ (contemporaneous effect of government spending on tax rates) and $\alpha_{tg}$ (contemporaneous effect of tax rates on government spending) are estimated using, for example, two dynamic least squares dummy variable regressions with four lags of tax rates and four lags of government spending in each regression and including these estimates, one at a time, in the three-variable SVAR analysis ($\alpha_{gt} = 0.01$ ($t = 1.06$) and $\alpha_{tg} = 0.21$ ($t = 1.10$)). Results are not shown for the sake of brevity.

this not the case either.[28]

*INSERT FIGURE* 10 *HERE*

With respect to changes in other taxes, we now control for corporate and individual income tax rates. Given the lack of readily-available data on average marginal income and corporate tax rates for the 14 countries used in this study, we use top marginal corporate and individual income tax rates. To this effect, we extend our benchmark two-variable SVAR to a four-variable SVAR specification with the real GDP growth rate and exogenous fiscal consolidation tax rate changes (value-added, corporate, and income).[29] Figure 11 shows the tax multiplier with and without these additional tax rates. The impact effect remains virtually unchanged and the cumulative multiplier is also virtually the same.

In sum, we conclude that our benchmark results remain essentially unchanged after controlling for other relevant fiscal variables.

*INSERT FIGURE* 11 *HERE*

### 3.4 The role of expectations

In deriving our measure of tax multipliers, the date of tax changes corresponds to the date in which they were actually implemented. Expectations, however, could also matter. As discussed in RR and Alesina, Favero, and Giavazzi (2012), announcements may lead to anticipated effects of tax changes.

To explore this issue, Figure 12 shows the density function for the number of days between passage and implementation of exogenous fiscal consolidation tax rate changes. The lag between passage and implementation is relatively short, with a median and average lag of about one and three months, respectively.

*INSERT FIGURE* 12 *HERE*

In order to control for possible anticipated effects arising from announcements, we follow RR's strategy of adding to our previous specification, given by equation (8), a "news" term and its

[28] If we included all changes in government spending instead of only those associated with fiscal consolidation episodes, the tax multiplier would remain virtually unchanged. These figures are not reported for the sake of brevity.

[29] As before, we allow tax rate changes to affect output contemporaneously but not the other way around. We include four lags of all variables. We also assume that the contemporaneous relations between the different tax rate changes in the matrix of endogenous variables are zero. Our results barely change when using estimates from a least squares dummy variable model. These plots are not shown for brevity's sake.

corresponding lags. Formally,

$$\Delta y_{i,t} = \alpha + \sum_{j=0}^{J} \beta_j \Delta t_{i,t-j} + \sum_{m=0}^{J} \beta_m \Delta News_{i,t-m} + \sum_{n=1}^{N} \beta_n \Delta y_{i,t-n} + \sum_{i=1}^{I-1} \beta_i d_i + \mu_{i,t}, \tag{9}$$

where $J = 4$, $N = 4$, $I = 14$, and the variable "News" represents the expected change of the tax rate at the time of the official passage of the law. As in the case of the actual tax change, we also include four lags for the "News" term. Figure 13 then shows the response of output to the passage of the law and to the implementation of the law (i.e., the tax multiplier that we have been reporting above).

Interestingly, Figure 13 shows that, at the time of the passing of the law, output increases. Most importantly for our purposes, the contractionary effect of tax increases in response to the implementation of the law remains valid even after we control for fiscal news.

We conclude that news about tax changes (proxied by the passage of tax laws) seem to lead to intertemporal substitution effects in the sense that output increases in response to the news even though it falls when the law is actually implemented. Our estimates of the tax multiplier itself, however, remain robust.

*INSERT FIGURE* 13 *HERE*

# 4 Comparison with alternative empirical strategies

This section examines the implications of using alternative empirical strategies for the identification of exogenous fiscal shocks and tax policy measurement. In terms of Table 1, notice that the analysis above has dealt exclusively with the top-left cell. This section deals with the cases captured by the other three cells. For comparison purposes, the plots in this section also report the findings from the two-variable SVAR specification with exogenous fiscal consolidation tax rate changes illustrated in Figure 8.[30]

## 4.1 Tax rate and Blanchard-Perotti

We first analyze the implications of using the VAT rate, a tax instrument, and the Blanchard-Perotti identification strategy (bottom-left cell in Table 1). In other words, we use all tax rate changes – not only those associated with exogenous fiscal consolidation episodes – and follow the structure

[30] We use as our benchmark the SVAR specification in order to have a valid comparison because all of the results in this section are derived in the context of an SVAR.

proposed by Blanchard and Perotti (2002) to identify fiscal shocks.[31] Figure 14, panels A and B, displays the results. Panel A shows that the time profile of the multiplier is quite similar to our benchmark, but considerably smaller (in absolute value). In particular, the impact effect, while still contractionary, is halved (i.e., falls in absolute value from 0.66 to 0.31) and is borderline significant. The maximum contractionary effect is reduced to about a third, from 3.56 to 1.29 (in absolute value). Panel B indicates that tax rate changes tend to respond significantly to output changes, especially after four quarters. The $p$-value for the test of the null hypothesis that a positive shock in output does not reduce the tax rate in the fourth quarter is 0.015.

*INSERT FIGURE* 14 *HERE*

One could argue that the difference in tax multipliers in Figure 14, Panel A, may reflect the asymmetric impact of tax rate changes depending on whether they are tax cuts or hikes. Since all of our exogenous fiscal consolidation tax rate changes are tax hikes, we now look at the case in which we use *all positive* tax rate changes and follow the Blanchard-Perotti identification scheme.[32] Panels C and D in Figure 14 display the results. Panel C confirms the findings in Panel A; that is, the time profile of the multiplier is similar to our benchmark but its size (in absolute value) is much smaller.

### 4.2 Cyclically-adjusted revenues and narrative

We now address the case captured by the top-right cell in Table 1. In other words, we use VAT cyclically-adjusted revenue changes as the tax instrument and the exogenous fiscal consolidation tax rate changes as our identification strategy.[33] Since we only include cyclically-adjusted revenue changes for those quarters with exogenous fiscal consolidation tax rate changes, the exercise truly compares alternative tax policy measures.[34] Panels A and B in Figure 15 display the results. Panel A shows a profile for the tax multiplier very different from the one we have obtained so far. In particular, the impact multiplier is very small (borderline zero) – in line with the results in Perotti (2004), Favero and Giavazzi (2010), Caldara (2011), and Ilzetzki (2011) – and expansionary after

[31] Recall that our VAT rate sample includes 42 tax rate changes.

[32] Recall that our sample includes 32 tax rate increases.

[33] See Appendix 6.4 for the computation of the multiplier and its standard deviation.

[34] Similar results are obtained including cyclically-adjusted revenue changes for the year (as opposed to just the quarter) with exogenous fiscal consolidation tax rate changes. In this case, the tax multiplier is much smaller (in absolute value). Results are not shown for the sake of brevity.

two quarters.[35] Interestingly, and in line with our previous results, Panel B seems to empirically "validate" the use of exogenous fiscal consolidation as a means of identifying exogenous fiscal shocks. The $p$-value for the test of the null hypothesis that output does not granger-cause changes in tax rates is 0.75.

*INSERT FIGURE* 15 *HERE*

### 4.3 Cyclically-adjusted revenues and Blanchard-Perotti

Lastly, we focus on VAT cyclically-adjusted revenue changes and the Blanchard-Perotti identification strategy (bottom-right cell in Table 1). As discussed in the Introduction, this strategy is by far the most commonly used in the literature. Figure 16, panels A and B, display the results. Panel A provides evidence that (as in panel A in Figure 15) the impact effect on output is not only non-contractionary, but, in fact, expansionary! A unit shock increase in VAT revenue collection increases output by 0.19 on impact and about 0.80 after a year.

*INSERT FIGURE* 16 *HERE*

Furthermore, when using this revenue-based measure together with the Blanchard-Perotti identification strategy, our results indicate that cyclically-adjusted revenue changes tend to respond to output changes (Panel B). This cast severe doubts on the validity of Blanchard-Perotti as a tool for identifying fiscal shocks.

### 4.4 Implications of our findings

This section's findings clearly support our concerns regarding the use of both cyclically-adjusted revenue measures to determine changes in tax policy as well as the use of econometric structural assumptions to identify exogenous fiscal shocks.

In terms of *identification*, our empirical evidence favors the use of "natural experiments" as a more accurate way of assessing fiscal policy changes not resulting from policymakers' response to output fluctuations (see Figure 14, panel B for tax rates and panel B in Figures 15 and 16 for cyclically-adjusted revenues).

[35] A notable exception is Mertens and Ravn (2014), who find large tax multipliers using the narrative approach for tax changes in the United States.

Regarding the (much less explored) issue of *measurement* of tax policy, we conclude (complementing our findings from Subsection 2.3) that there is really no good substitute for data on tax rates. Our main point – the importance of using tax rates to measure tax policy – is, in fact, independent of the particular identification strategy, as it applies equally to the analysis of fiscal shocks identified through narratives à la RR and SVAR identification à la Blanchard-Perotti. The critical importance of this issue is reflected in the very different tax multipliers that result. Tax rate increases are always contractionary (Figure 14, Panels A and C). Tax multipliers based on cyclically-adjusted revenues, however, are either very small or actually positive (Panel A in Figures 15 and 16), suggesting that tax increases actually raise output. In other words, not using tax rates may lead to a very misleading value of the tax multiplier.

# 5 Conclusions

Our paper contributes to the empirical literature on tax multipliers. Specifically, we first revisit the issue of the identification of exogenous fiscal policy shocks (i.e., fiscal policy changes that are not the result of policymakers responding to output fluctuations). Our findings support Romer and Romer's (2010) narrative approach as a more convincing strategy than SVAR to truly identify exogenous fiscal shocks. We then explore the (rarely addressed) implications of tax policy measurement (i.e., obtaining a tax policy variable that is under the direct control of policymakers). For this purpose, we built a novel value-added tax rate dataset and the associated cyclically-adjusted revenues at a quarterly frequency for 14 industrial countries for the period 1980-2009. Our results strongly support the use of tax rates as a true measure of tax policy as opposed to widely-used revenue-based measures, such as cyclically-adjusted revenues. We conclude that the latter is a poor proxy for changes in tax rates since it shows a very erratic pattern (on many occasions even implying that the presumably discretionary policy changes are reversed every quarter!). Our main point – the importance of using tax rates as a proxy for tax policy – is, however, independent of the particular identification strategy, as it applies equally to the analysis of fiscal shocks identified through narratives à la Romer-Romer as well as to SVAR identification. Tax rate increases are always contractionary. However, tax multipliers based on cyclically-adjusted revenues are, as suggested by some papers in this area, close to zero or even positive.

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# 6 Appendices

## 6.1 Data definitions and sources

### 6.1.1 Gross domestic product and government spending

We constructed quarterly seasonally-adjusted real measures of gross domestic product and government spending using data from the following sources: Australia (Australian Bureau of Statistics), Belgium (Banque Nationale de Belgique), Canada (Statistics Canada), Denmark (IFS, IMF), Finland (Statistics Finland), France (I.N.S.E.E.), Germany (IFS, IMF), Ireland (Central Statistics Office), Italy (IFS, IMF), Japan (Cabinet Office), Portugal (IFS, IMF), Spain 1980-2009 (IFS, IMF), Sweden (IFS, IMF), and United Kingdom (Office for National Statistics).

Ireland is the only case for which we were not able to gather data on output originally collected on a quarterly basis since the introduction of the VAT, and hence our data, start only in 1997:Q1.[36]

### 6.1.2 Personal income tax rate

Highest marginal personal income tax rate. World Development Indicators (WDI-World Bank) and World Tax Database (University of Michigan, Ross School of Business), with updates from local sources and international consulting firms. Data period covers 1980-2013.

### 6.1.3 Corporate income tax rate

Maximum corporate income tax rate. WDI-World Bank and World Tax Database, with updates from local sources and international consulting firms. Data period covers 1980-2013.

[36] As discussed in detail in Ilzetzki, Mendoza, and Vegh (2013), it is common for "quarterly data" to be built based on interpolating annual data as opposed to collecting data on a quarterly basis.

#### 6.1.4 VAT rate

Belgium, Denmark, France, Germany, Ireland, Italy, Sweden, and United Kingdom introduced value-added taxation before 1980, so our sample for these countries includes the full 1980-2009 period. The six remaining countries implemented VAT taxation later: Australia (introduced in 2000:Q3), Canada (1991:Q1), Finland (1994:Q3), Japan (1989:Q1), and Portugal and Spain (1986:Q1). As a result, the sample for these countries runs from the quarter of implementation to 1980.

VAT rate series for Belgium, Denmark, Finland, France, Germany, Ireland, Italy, Portugal, Spain, Sweden, and United Kingdom were obtained from "VAT rates applied in the member states of the European Community," European Commission, Taxation and Customs Union, 2009. Data for Japan from Beyer (2000) and the TMF group. Data for Canada from Canada Revenue Agency; data for Australia from Australian Taxation Office.

#### 6.1.5 VAT revenue

Quarterly data for VAT revenue for Belgium, Denmark, Finland, France, Germany, Ireland, Italy, Portugal, Spain, Sweden, and United Kingdom were obtained from Eurostat. Data for Canada are from Statistics Canada, Government Finance Statistics Series; data for Japan from Bank of Japan. For Australia, VAT tax revenue data were not available on a quarterly basis. As a proxy, we used tax revenues on production and imports from the Australian Bureau of Statistics, Income Component Tables.

Sample periods start as follows: United Kingdon (1990:Q1), Belgium, Canada, Denmark, France, Germany, and Italy (1991:Q1), Japan and Sweden (1993:Q1), Finland (1994:Q3), Portugal (1991:Q1), Ireland (1999:Q1), Spain (2000:Q1), and Australia (2000:Q3). In all cases, the sample period ends in 2009:Q4.

### 6.2 Cyclically-adjusted VAT revenue measure

We use equation (3) to compute cyclically-adjusted VAT revenues, with values of $\eta$ for indirect taxation computed by van den Noord (2000).[37] [38] The average elasticity for the countries included in the sample is 0.9. The individual values are as follows: Australia (0.4), Belgium (0.9), Canada (0.7), Denmark (1.6), Finland (0.9), France (0.7), Germany (1), Ireland (0.5), Italy (1.3), Japan (0.5), Portugal (0.6), Spain (1.2), Sweden (0.9), and United Kingdom (1.1). The sample periods are the same as for tax revenues.

### 6.3 Tax narrative

#### 6.3.1 Belgium

**Change in VAT rate (in percentage-points): 2 (from 17% to 19%). Date of implementation: January 1, 1983. Date of passage: October 10, 1983. Exogenous.**

The government that took office following the general elections of November 8, 1981 was confronted with a declining GDP, a large external current account deficit, a soaring public sector deficit (which rose from 12% of GDP in 1980 to 16% of GDP in 1981), sharply rising unemployment, and

[37] To calculate this elasticity, van den Noord (2000) assumes that the relevant tax base fluctuates in proportion with private consumption. The elasticity then corresponds to the output elasticity of consumption, which is estimated for each country using semi-annual data on private consumption and output for the period 1985-1998.

[38] Similar results are obtained if we assumed that $\eta = 1$ for all countries (as in Giorno, Richardson, Roseveare, and van den Noord, 1995). Results are not shown for brevity.

accelerating inflation. Various policy measures were adopted that improved the external balance and corporate profitability. The fiscal deficit, however, remained at 16% in 1982 and the public debt reached 127% of GDP. In early 1982, the government decided to take supplementary measures aimed at reducing such deficit including the increase of the VAT rate from 17% to 19%. Sources: IMF Archives: SM/84/279, SM/83/255, SM/84/271.

**Change in VAT rate (in percentage-points): 0.5 (from 19% to 19.5%). Date of implementation: April 1, 1992. Date of passage: March 1, 1992. Exogenous.**

Belgium's general government deficit peaked at over 16 percent of GNP in 1981. As a result, fiscal policy for the rest of the decade first focused on stabilizing and then reducing the public debt ratio. Belgium's 1992 VAT increase was motivated by an interest in harmonizing VAT rates with other members of the European community and reducing deficits toward the 3% Maastricht treaty limit. Sources: "Belgium to align its VAT, duty Laws with rest of EC." *Wall Street Journal* [Brussels], March 17, 1992, p. 2. IMF Archives: SM/92/206, SM/92/222, and SM/93/253.

**Change in VAT rate (in percentage-points): 1 (from 19.5% to 20.5%). Date of implementation: January 1, 1994. Endogenous.**

Belgium experienced recessionary conditions in 1993. Although Belgium's fiscal policy had focused on reducing deficits to the 3% of GDP level required by the Maastricht treaty, the recession's negative effect on tax revenues largely offset the impact of these fiscal measures. To restore competitiveness and stimulate employment, the Parliament reduced employers' contributions to social security, which were funded by the VAT hike. Sources: Greiff, Peter R. "Belgium Is unlikely to meet target for its budget deficit this year." *Wall Street Journal* [Brussels], January 12, 1994, p. 2. IMF Archives: SM/93/253.

**Change in VAT rate (in percentage-points): 0.5 (from 20.5% to 21%). Date of implementation: January 1, 1996. Date of passage: October 1, 1995. Exogenous.**

Belgium's VAT increase in 1996 took place in the context of modest economic growth. The VAT increase was part of the fiscal plan to continue medium-term deficit reduction in agreement with the Maastricht limit of 3% of GDP. Sources: Asert, Raf C. "Belgium put on austerity diet." The Independent, October 4, 1995, p. 14. IMF Archives: SM/96/31.

### 6.3.2 France

**Change in VAT rate (in percentage-points): 2 (from 18.6% to 20.6%). Date of implementation: August 1, 1995. Endogenous.**

An important motive for the 1995 VAT increase was deficit reduction and efforts to comply with Maastricht treaty requirements. However, the VAT increase was put in place partly to pay for a job creation program and was offset by other changes "seen as emergency measures to combat a "calamitous" economic situation." (Dejevsky, 1995). Sources: Dejevsky, Mary. "French pay higher taxes for jobs plan." *The Independent*, June 23, 1995, p. 13. IMF Archives: SM/95/251 and SM/95/266.

**Change in VAT rate (in percentage-points): -1 (from 20.6% to 19.6%). Date of implementation: April 1, 2000. Endogenous.**

In 2000, France was in its third straight year of sustained output growth with low inflation and enjoyed a strong fiscal situation. Low deficits presented an opportunity for France to lower its tax burden, which was the highest in the Euro region. This tax reduction was motivated by pressure to lower taxes in the face of fiscal surpluses. Sources: IMF Public Information Notice No. 00/95. IMF Concludes Article IV consultation with France. November 13, 2000. IMF Staff Country Report No.

00/147. France: 2000 Article IV Consultation – Staff Report; Supplement to the Staff Report; and Public Information Notice Following Board Consultation. November 2000.

### 6.3.3 Germany

**Change in VAT rate (in percentage-points): 1 (from 11% to 12%). Date of implementation: July 1, 1983. Endogenous.**

Germany experienced recessionary conditions in early 1982. At end-1982 and early 1983 signs of renewed strengthening of demand were appearing. On the fiscal front, the fiscal deficit moved from approximate balance in 1970 to a deficit of 3.9% of GDP in 1982. Fiscal policy in 1983 combined consolidation efforts, on the one hand, with measures to stimulate private and public investment, on the other. Measures involved limiting the rise in expenditure for certain social benefits and VAT tax increases to reduce the deficit, as well as the reduction in business taxes to increase investment. Sources: IMF Archives: SM/83/154 and SM/84/152.

**Change in VAT rate (in percentage-points): 1 (from 14% to 15%). Date of implementation: January 1, 1993. Date of passage: February 1, 1992. Exogenous.**

German reunification took place in 1990. Afterwards, the country ran large fiscal deficits due to reunification costs including income support and developing a market economy in the former East Germany. The 1993 VAT increase was intended to address the deficits resulting from reunification. The general fiscal deficit reached 2.8% of GDP in 1992 and was expected to rise above 4 percent of GDP in 1993. European Union rate harmonization also motivated the increase. Sources: "Anywhere But Germany." *The Wall Street Journal* (Brussels) January 25, 1993, p. 10. IMF Archives: SM/93/136 and SM/94/213.

**Change in VAT rate (in percentage-points): 1 (from 15% to 16%). Date of implementation: April 1, 1998. Date of passage: December 1, 1997. Exogenous.**

The resulting increase in social expenditures and declining tax revenue exacerbated the large budget deficits that followed reunification. The economy began to recover in 1997 due to increased demand for its exports, with real GDP growth reaching $2\frac{1}{4}$ percent. Economic priorities included lowering deficits to the 3% level required by the Maastricht treaty. The VAT increased mainly for this goal. Sources: "Kohl Faces tax rises and welfare cuts to meet Euro date." *The Times* (London), April 21, 1997, p. 15. IMF Archives: SM/98/209. IMF Public Information Notice No. 98/72.

**Change in VAT rate (in percentage-points): 3 (from 16% to 19%). Date of implementation: January 1, 2007. Date of passage: June 16, 2006. Exogenous.**

After several years of slow growth, Germany was experiencing a gradual upswing. Despite strengthening GDP growth, investment, and employment, Germany's economy struggled due to high-cost entitlement programs and concerns over deficits, which were above 3% of GDP through 2004 and contributed to rising debt-to-GDP ratios projected at 67.5 percent in 2006. This tax increase was enacted to lower the fiscal deficits. Sources: Atkins, Raph. "Germany's gamble with hefty VAT pays off." *Financial Times* (London) January 31, 2007, p. 9. IMF Public Information Notice No. 06/141. IMF Country Report No. 06/438.

### 6.3.4 Ireland

**Change in VAT rate (in percentage-points): 0.5 (from 21% to 21.5%). Date of implementation: December 1, 2008. Date of passage: October 14, 2008. Exogenous.**

Ireland's "Celtic Tiger" growth period ended with the onset of the global financial crisis. Ireland's fiscal imbalance developed during the boom years. "Well before the crisis hit, public finances

had developed serious structural weaknesses. The facts are well known. In the boom years, personal income tax rates were lowered and expenditure grew rapidly (at about the highest pace among OECD economies). Buoyant property-related revenues (stamp duties, VAT, and capital-related taxes) masked the growing structural deficit, which reached $12\frac{1}{2}$ percent of GDP in 2008" (IMF, 2009). With the fiscal deficit about to hit 15% of GDP in 2008, tax increases and spending cuts, each totaling about 1 percent of GDP, were announced with the 2009 budget. Sources: IMF. Country Report No. 09/195. IMF. Public Information Notice No. 10/86. "Indirect Taxes" Sunday Business Post (Cork) October 19, 2008.

### 6.3.5 Italy

**Change in VAT rate (in percentage-points): 1 (from 19% to 20%). Date of implementation: October 1, 1997. Date of passage: September 1, 1997. Exogenous.**

After a slowdown of the economy in 1996, economic growth resumed in the second quarter of 1997 as private consumption increased. Italy's primary fiscal goal in 1997 was ensuring entry into the European Monetary Union as one of the founding members. Italy's fiscal situation had improved significantly from the 1980s, when it suffered chronically large budget deficits. The primary balance first recorded a surplus in 1991 and the surplus was estimated at 6.7 percent of GDP in 1997. The debt-to-GDP ratio was still high at 122 percent of GDP but was declining at record rates. Despite earlier plans to reach the Maastricht deficit criterion of 3% of GDP in 1998, in late 1996 the Italian government decided to increase the pace of fiscal consolidation with additional revenue measures such as an increase in VAT. Sources: Blitz, James. "Prodi budget aims to avert welfare clash." *Financial Times* (London, UK) September 29, 1997, p. 2. IMF Archives: SM/97/85 and SM/98/50.

### 6.3.6 Japan

**Change in VAT rate (in percentage-points): 2 (from 3% to 5%). Date of implementation: April 1, 1997. Date of passage: June 25, 1996. Exogenous.**

Japan's asset price bubble collapsed in 1990. Afterwards, GDP growth averaged only 1.5% annually for eight years as domestic demand stalled. Japan attempted to stimulate the economy repeatedly during the early 1990s with fiscal stimulus and monetary policy. As a result of fiscal stimulus, central government deficits had increased to 4.2% of GDP by 1996. As a result, the policy focus shifted toward deficit reduction and a return to a stable fiscal position. The 1997 budget aimed at reducing the structural deficit to 2% of GDP. Revenue measures included the increase of the VAT by 2 percentage points to 5%. Sources: Bartlett, Bruce. "Economic miseries of VAT tax creep." Washington Times (Washington, DC) January 13, 1997, p. A13. "Japan's consumption tax increases to 5% from 3%," *Wall Street Journal* (New York), March 13, 1997, p. A11. IMF. Public Information Notice No. 97/19. IMF Archives: SM/98/191.

### 6.3.7 Portugal

**Change in VAT rate (in percentage-points): 2 (from 17% to 19%). Date of implementation: June 5, 2002. Date of passage: May 5, 2002. Exogenous.**

Portugal's Stability Program aimed at a balanced structural budget by 2004. The high and chronic deficits since early 1990s forced the government in 2002 to take several measures (including an increase in the standard VAT rate) to correct the deficit. Sources: "Curbing expenditure is a delicate balancing act: Heavy reliance on exports brings a chill wind when global markets take a

turn downwards." *Financial Times* (London) June 3, 2002, p. 1. IMF. Public Information Notices: No. 00/99, No. 02/48, and No. 03/48.

**Change in VAT rate (in percentage-points): 2 (from 19% to 21%). Date of implementation: July 1, 2005. Date of passage: June 1, 2005. Exogenous.**

Rising private sector indebtedness, low bank capitalization, and declining competitiveness resulted in a slowdown beginning in 2001 and a crippling recession between 2002 and 2004. Despite expectations of real GDP growth of only $\frac{1}{2}$ percent in 2005 and 1 $\frac{1}{4}$ percent in 2006, Portugal's fiscal strategy focused on lowering the budget deficit, which had risen to 6.1% of GDP. The chief measures were a VAT rate increase and freeze on public sector wages. "The government, reluctant to cut other spending, defended the increase in the VAT rate from 19 to 21 percent by arguing it had to raise taxes due to European Community pressure for quick improvement on the deficit front" (Frasquilho, 2005). Sources: Frasquilho, Miguel. "The Socratic Method." *The Wall Street Journal* (Brussels) July 21, 2005, p. 9. IMF. Country Report No. 05/375. IMF. Public Information Notice No. 05/147.

### 6.3.8 Spain

**Change in VAT rate (in percentage-points): 1 (from 12% to 13%). Date of implementation: January 1, 1992. Date of passage: December 30, 1991. Exogenous.**

Spain's economic growth decelerated in 1990 and stagnated in 1991 in response to the sluggish international environment. The fiscal deficit increased from about 3% in 1989 to 5% in 1991, in large part due to the effects of automatic stabilizers. In order to adjust fiscal deficits to the Maastricht Treaty levels of no greater than 3%, the Spanish authorities reacted to the fiscal deterioration by reducing unemployment compensation benefits and increasing the VAT on two occasions in 1992. Sources: IMF Archives: SM/92/145, SM/94/10, and SM/94/10 supplement.

**Change in VAT rate (in percentage-points): 2 (from 13% to 15%). Date of implementation: August 1, 1992. Date of passage: July 13, 1992. Exogenous.**

Rationale is the same as in the previous case.

**Change in VAT rate (in percentage-points): 1 (from 15% to 16%). Date of implementation: January 1, 1995. Date of passage: December 31, 1994. Exogenous.**

Deficits grew from 2.8% to 7.5% of GDP between 1989 and 1993, due to both the recession and lax expenditures policy. In July 1994, the Spanish government submitted a Convergence Plan to the European Council that set out targets for deficit reduction. Most of the reduction was to come from expenditure reduction, including a number of labor market reforms involving limits on public sector wage increases and reforms to unemployment compensation. Sources: IMF Archives: SM/93/262, SM/96/72, and SM/96/77.

### 6.3.9 Sweden

**Change in VAT rate (in percentage-points): 1.95 (from 21.51% to 23.46%). Date of implementation: January 1, 1983. Endogenous.**

In the early 1980s Sweden suffered from a twin deficit problem of large current account and fiscal deficits. These problems developed after the oil price shocks in the mid-1970s. Rising wages and loss of competitiveness in key industries, combined with labor market rigidities, and subsidies for deteriorating industries resulted in slow adjustment. A new government took office in late 1982 after general elections in September. The government pursued a crisis program aimed at increasing production and raising full employment (OECD, 1984). The program raised the VAT to 23.46%

in 1983 to cover the cost of restoring benefits cut by the previous government (i.e., countercyclical action). Sources: Apple, R. W. Jr. "Social Democrats favored in Sweden." *The New York Times* (New York) September 19, 1982, p. A17. Feder, Barnaby J. "Swedes Rankled By Taxes." *The New York Times* (New York) April 25, 1983, p. D6. IMF Archives. SM/84/179. OECD. Economic Surveys. Sweden. February 1984.

### 6.4 Computation of tax multiplier and standard error

The derivation of equation (6) is as follows. First, let us define $\Delta X_J \equiv X_J - X_0$ and $\overline{X_J} \equiv \left(\sum_{j=0}^{J} X_j\right)/J$ where the variable $X$ may be $Y$, $R$, $I$, $t$. Notice that $\Delta Y_J/\Delta R_J = (\Delta Y_J/\overline{Y_J})/\left(\Delta R_J/\overline{Y_J}\right)$. From equation (4), $\Delta Y_J/\overline{Y_J} = \omega_J \Delta t$. Therefore, $\Delta Y_J/\Delta R_J = \omega_J \Delta t/\left(\Delta R_J/\overline{Y_J}\right)$. Since $R \equiv I \cdot Y$, then $\Delta R_J \approx \overline{Y_J} \cdot \Delta I_J + \overline{I_J} \cdot \Delta Y_J$. Further, given that $e \equiv I/t$ is taken as a constant and since $\Delta t_J = \Delta t$, then $\Delta I_J = e \cdot \Delta t$. Hence, $\Delta Y_J/\Delta R_J = \omega_J \Delta t/\left(\Delta I_J + \overline{I_J} \cdot \omega_J \Delta t\right) = \omega_J/\left(e + \overline{I_J} \cdot \omega_J\right)$. In our empirical calculations we proxy $\overline{I_J}$ with the sample average.

The derivation of equation (7) is as follows. From (6), $Tax\ multiplier = f(x)$, where $f(x) = x/(e + x \cdot \overline{I_J})$. Using a first-order approximation, we obtain $Tax\ multiplier\ (J) = f(\omega_J) + f'(\omega_J)(x - \omega_J) = f(\omega_J) + f'(\omega_J)\, x - f'(\omega_J)\, \omega_J$. Hence, $Var[Tax\ multiplier\ (J)] = [f'(\omega_J)]^2 Var[x]$. Evaluating $f(x)$ at $x = \omega_J$, $Var[Tax\ multiplier\ (J)] = [f'(\omega_J)]^2 Var[\omega_J]$. Hence, $Tax\ multiplier_{SE}(J) = f'(\omega_J) \cdot \omega_J^{SE}$. Using (6), it follows that $Tax\ multiplier_{SE}\ (J) = [e/\left(e + \omega_J \cdot \overline{I_J}\right)^2] \cdot \omega_J^{SE}$.

We now show that for the case of cyclically-adjusted revenues ($ca$), the corresponding expressions are given by

$$
\begin{aligned}
Tax\ multiplier(J) &\equiv \left(\sum_{j=0}^{J} \Delta Y_j\right) / \left(\sum_{j=0}^{J} \Delta R_j\right) = \frac{\upsilon_J}{(1 + \upsilon_J \cdot \eta)\,\overline{I_J}}, \\
Tax\ multiplier_{SE}(J) &= \frac{\overline{I_J}}{\left((1 + \upsilon_J \cdot \eta)\,\overline{I_J}\right)^2} \upsilon_J^{SE},
\end{aligned}
$$

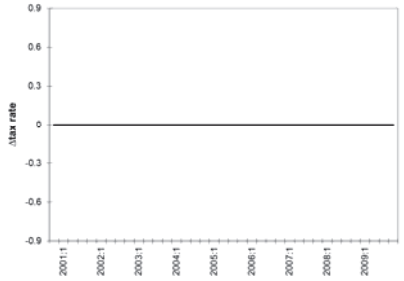
where $\upsilon_J$ represents the cumulative effect on the growth rate of output of a one percentage-point shock in cyclically-adjusted revenues.

The proof of the first expression is as follows. We start by defining $\Delta X_J \equiv X_J - X_0$ and $\overline{X_J} \equiv \left(\sum_{j=0}^{J} X_j\right)/J$ where the variable $X$ may be $Y$, $R$, $I$, $ca$. Notice that $\Delta Y_J/\Delta R_J = (\Delta Y_J/\overline{Y_J})/\left(\Delta R_J/\overline{Y_J}\right)$. From an equation like (4) with $\Delta ca$ (instead of $\Delta t$) as regressors, $\Delta Y_J/\overline{Y_J} = \upsilon_J \Delta ca$. Therefore, $\Delta Y_J/\Delta R_J = \upsilon_J \Delta ca/\left(\Delta R_J/\overline{Y_J}\right)$. Since $R \equiv I \cdot Y$, then $\Delta R_J \approx \overline{Y_J} \cdot \Delta I_J + \overline{I_J} \cdot \Delta Y_J$. Therefore, $\Delta Y_J/\Delta R_J = \upsilon_J \Delta ca/\left(\Delta I_J + \overline{I_J} \cdot \Delta Y_J/\overline{Y_J}\right)$. From (3) and since $\Delta ca_J = \Delta ca$ and $\Delta Y_J/\overline{Y_J} = \upsilon_J \Delta ca$, $\Delta I_J/\overline{I_J} \approx \Delta ca - (1 - \eta)\left(\Delta Y_J/\overline{Y_J}\right) = (1 - (1 - \eta)\,\upsilon_J)\,\Delta ca$. Using the latter expression, it is easy to show that $\Delta Y_J/\Delta R_J = \upsilon_J/(1 + \upsilon_J \cdot \eta)\,\overline{I_J}$. In our empirical calculations we proxy $\overline{I_J}$ with the sample average.

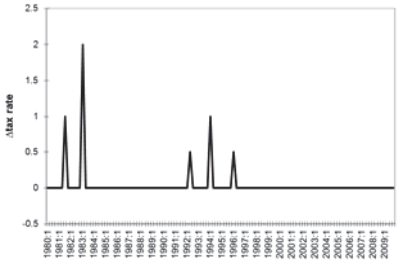
To derive the second expression above, notice that we can write the first expression as $Tax\ multiplier\ (J) = f(x)$, where $f(x) = x/[(1 + x \cdot \eta)\,\overline{I_J}]$. Using a first order approximation, we obtain $Tax\ multiplier\ (J) = f(\upsilon_J) + f'(\upsilon_J)(x - \upsilon_J) = f(\upsilon_J) + f'(\upsilon_J)\, x - f'(\upsilon_J)\, \upsilon_J$. Hence, $Var[Tax\ multiplier\ (J)] = [f'(\upsilon_J)]^2 Var[x]$. Evaluating $f(x)$ at $x = \upsilon_J$, then $Var[Tax\ multiplier\ (J)] = [f'(\upsilon_J)]^2 Var[\upsilon_J]$. Hence, $Tax\ multiplier_{SE}\ (J) = f'(\upsilon_J) \cdot \upsilon_J^{SE}$. Finally, $Tax\ multiplier_{SE}\ (J) = \left\{\overline{I_J}/[\left((1 + \upsilon_J \cdot \eta)\,\overline{I_J}\right)^2]\right\} \upsilon_J^{SE}$.

# Figure 1. Tax rate changes

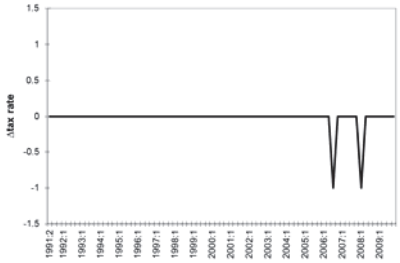
Panel A. Australia



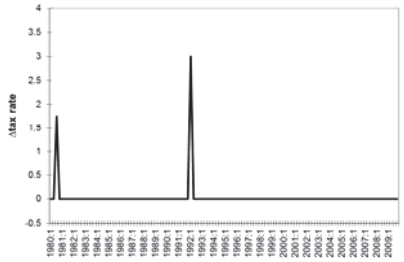

Panel B. Belgium



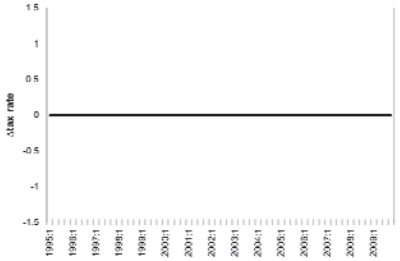

Panel C. Canada



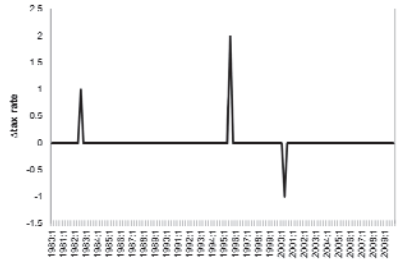

Panel D. Denmark



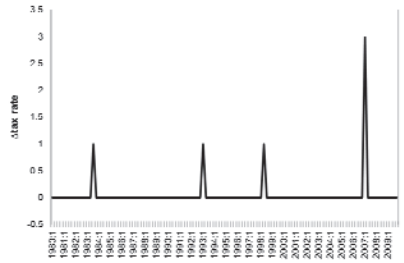

Panel E. Finland



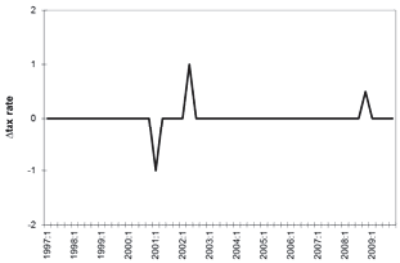

Panel F. France



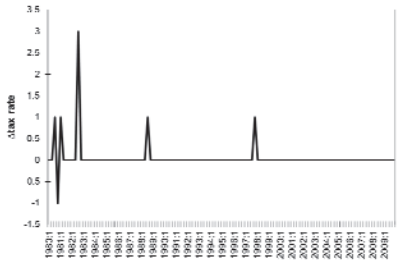

Panel G. Germany



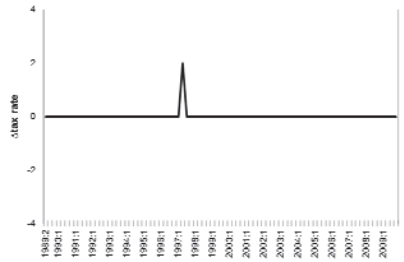

Panel H. Ireland



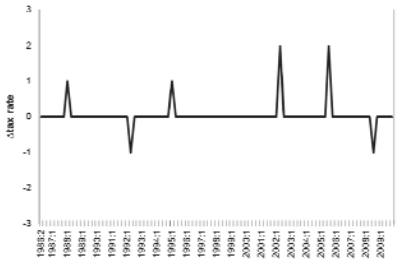

Panel I. Italy



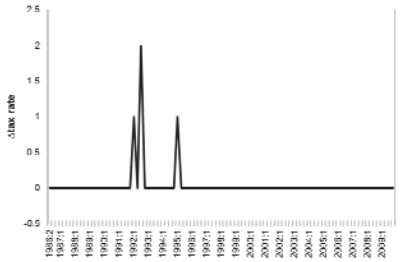

Panel J. Japan



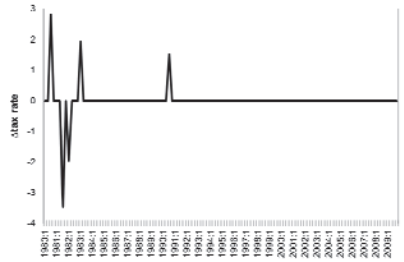

Panel K. Portugal



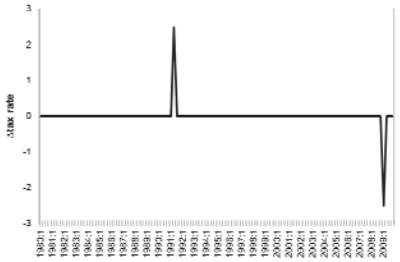

Panel L. Spain




Panel M. Sweden

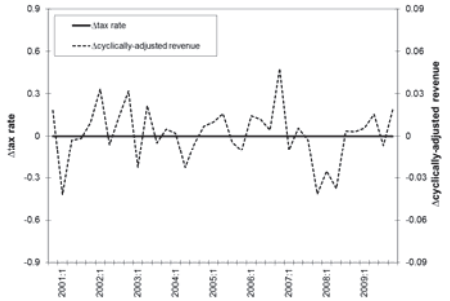



Panel N. United Kingdom



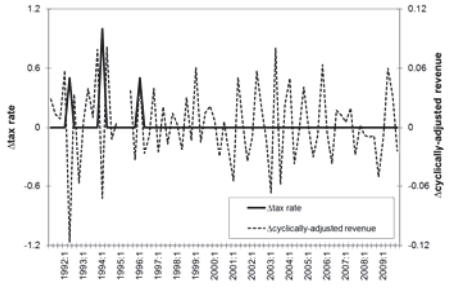
# Figure 2. Tax rate changes vs. cyclically-adjusted revenue changes (CA)

Panel A. Australia



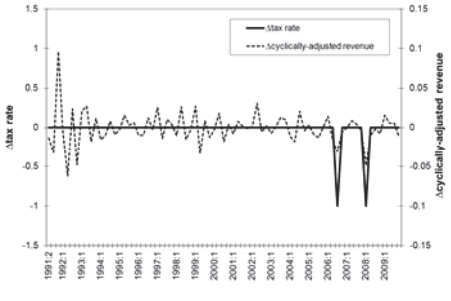

Note: Ljung-Box test (CA) = 0.02, Autocorrelation coeff. (CA) = 0.02

Panel B. Belgium



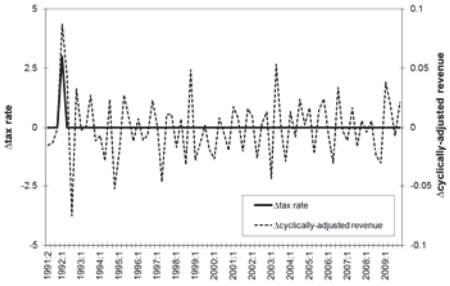

Note: Ljung-Box test (CA) = 7.02***, Autocorrelation coeff. (CA) = -0.34***

Panel C. Canada



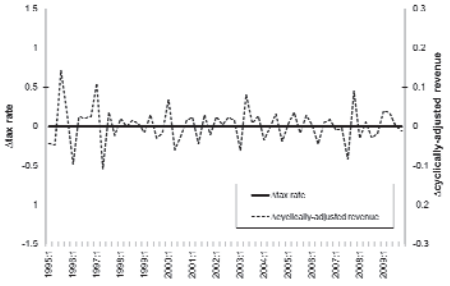

Note: Ljung-Box test (CA) = 6.46**, Autocorrelation coeff. (CA) = -0.29**

Panel D. Denmark



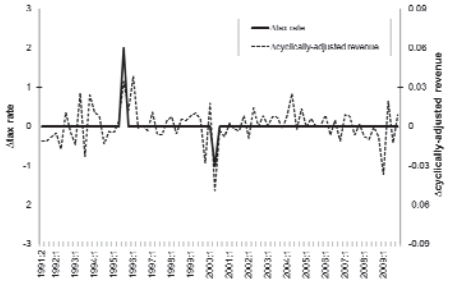

Note: Ljung-Box test (CA) = 5.48**, Autocorrelation coeff. (CA) = -0.27*

Panel E. Finland



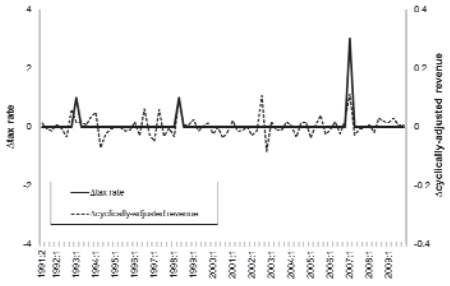

Note: Ljung-Box test (CA) = 9.58***, Autocorrelation coeff. (CA) = -0.39**

Panel F. France



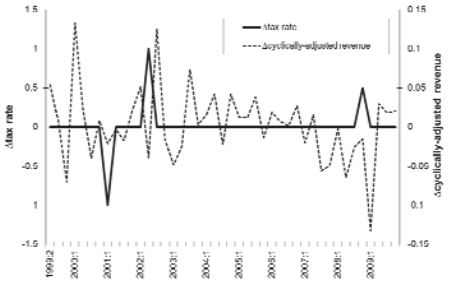

Note: Ljung-Box test (CA) = 2.24, Autocorrelation coeff. (CA) = -0.17

Panel G. Germany



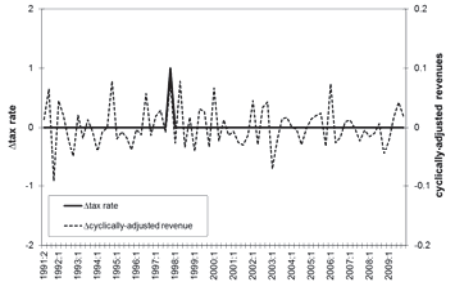

Note: Ljung-Box test (CA) = 6.62**, Autocorrelation coeff. (CA) = -0.29**

Panel H. Ireland



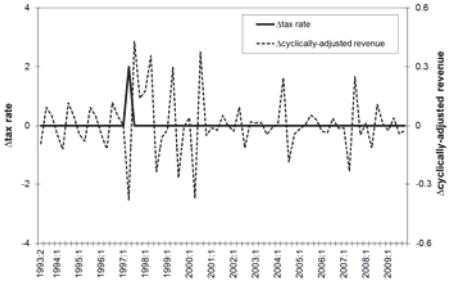

Note: Ljung-Box test (CA) = 1.02, Autocorrelation coeff. (CA) = -0.15

Panel I. Italy



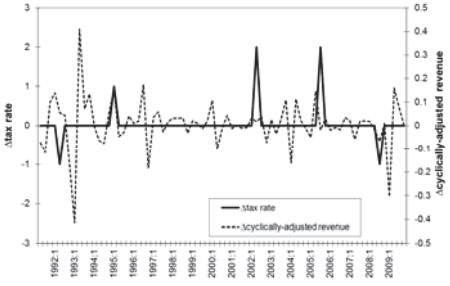

Note: Ljung-Box test (CA) = 8.22***, Autocorrelation coeff. (CA) = -0.33***

Panel J. Japan



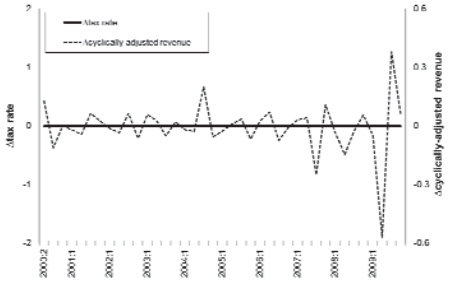

Note: Ljung-Box test (CA) = 8.89***, Autocorrelation coeff. (CA) = -0.36**

Panel K. Portugal



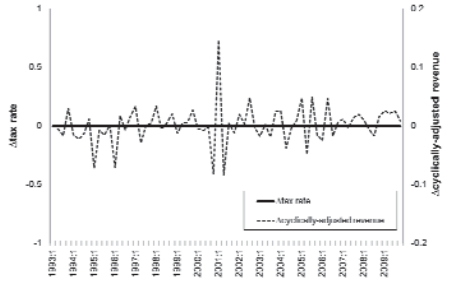

Note: Ljung-Box test (CA) = 3.65*, Autocorrelation coeff. (CA) = -0.22

Panel L. Spain



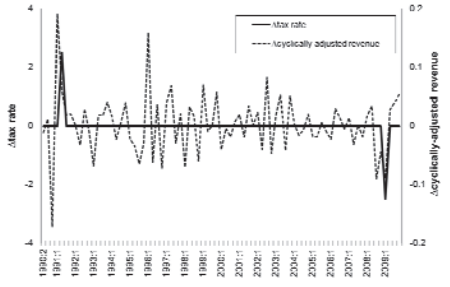

Note: Ljung-Box test (CA) = 5.66**, Autocorrelation coeff. (CA) = -0.37*

Panel M. Sweden



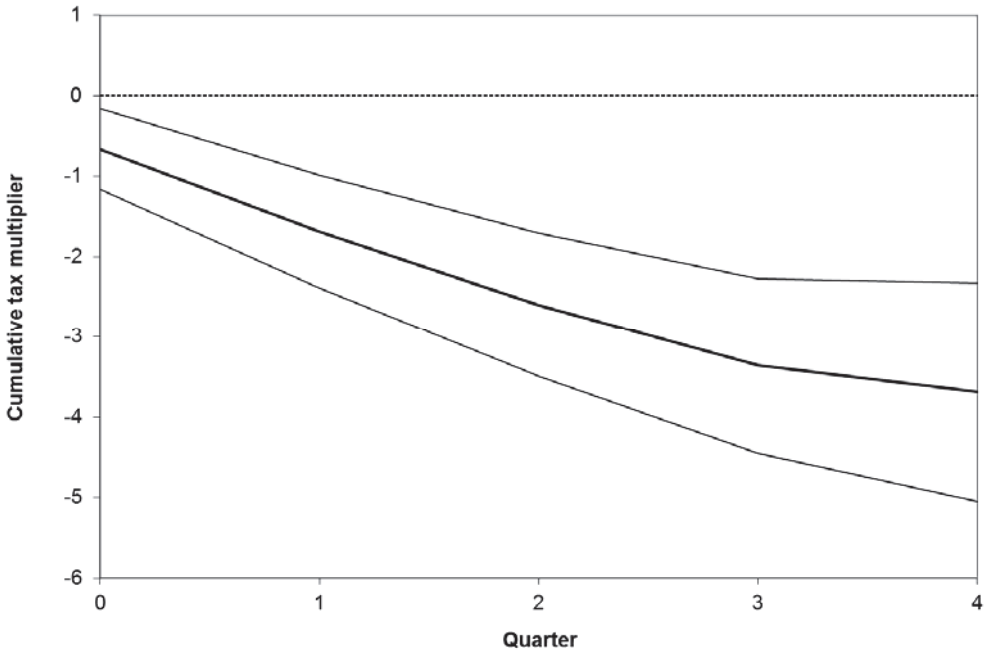

Note: Ljung-Box test (CA) = 14.22***, Autocorrelation coeff. (CA) = -0.45***

Panel N. United Kingdom



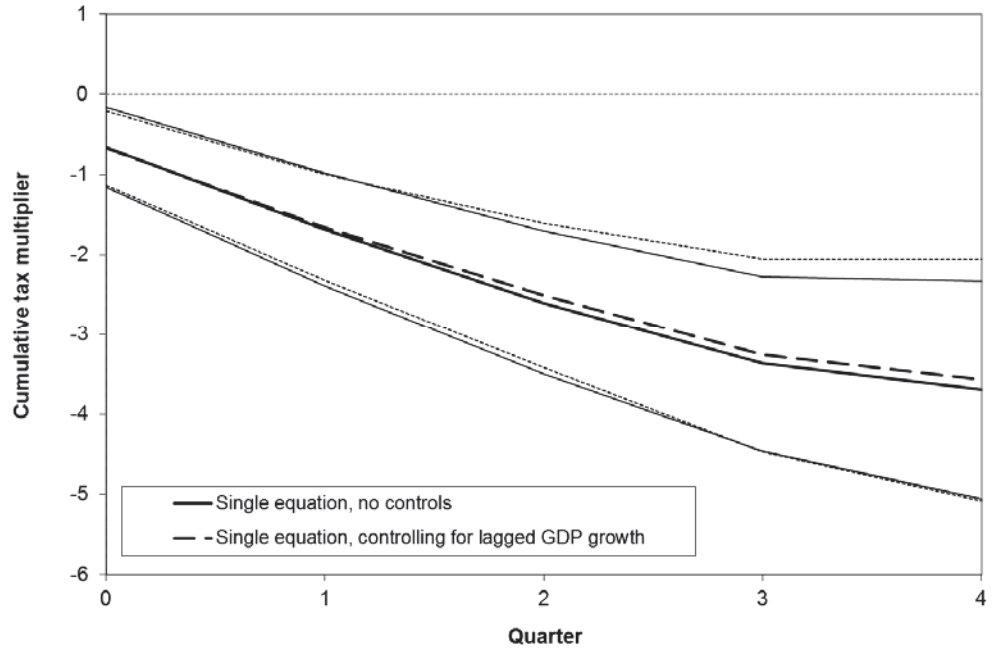

Note: Ljung-Box test (CA) = 7.46***, Autocorrelation coeff. (CA) = -0.31*

**Figure 3. Cumulative tax multiplier: Exogenous fiscal consolidation tax rate shock, single equation, no controls**



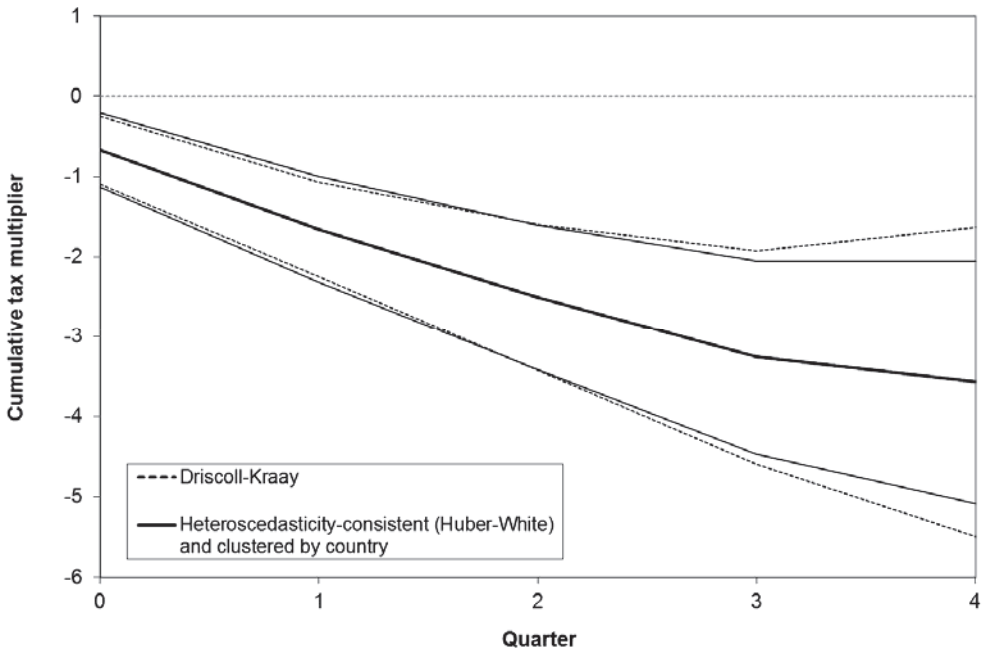

Notes: Country fixed effect panel regression. Standard errors are bootstrapped, heteroscedasticity-consistent (Huber-White), and clustered by country. Observations: 1304.

**Figure 4. Cumulative tax multiplier: Exogenous fiscal consolidation tax rate shock, single equation, controls vs. no controls**



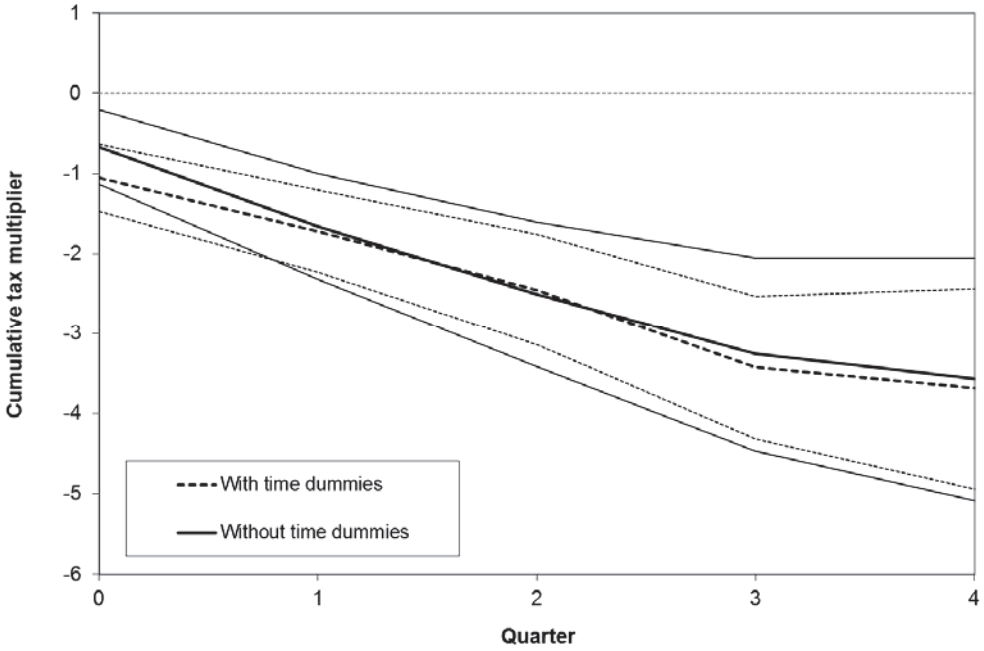

Notes: Country fixed effect panel regression. Standard errors are bootstrapped, heteroscedasticity-consistent (Huber-White), and clustered by country. Observations: 1304 (no controls), 1288 (controlling for lagged GDP growth).

**Figure 5. Cumulative tax multiplier: Exogenous fiscal consolidation tax rate shock, single equation with controls, alternative structure of errors**



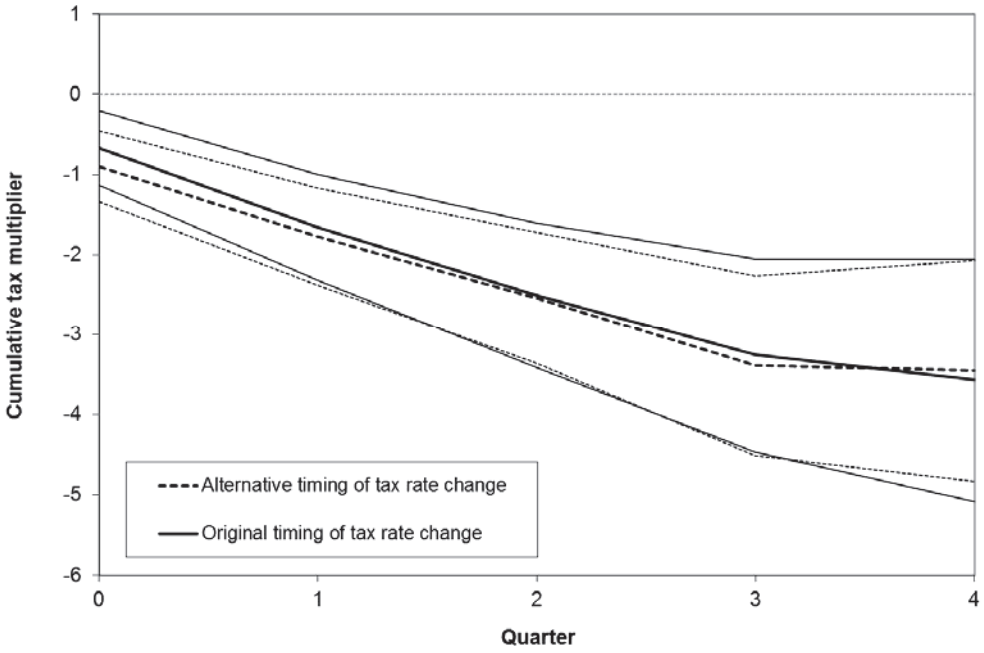

Notes: Country fixed effect panel regression. Standard errors are (i) solid lines: bootstrapped, heteroscedasticity-consistent (Huber-White), and clustered by country, and (ii) dashed lines: bootstrapped, cross-sectional (spatial) and temporal dependence robust (Driscoll-Kraay). Observations: 1288.

**Figure 6. Cumulative tax multiplier: Exogenous fiscal consolidation tax rate shock, single equation with controls, with and without time dummies**



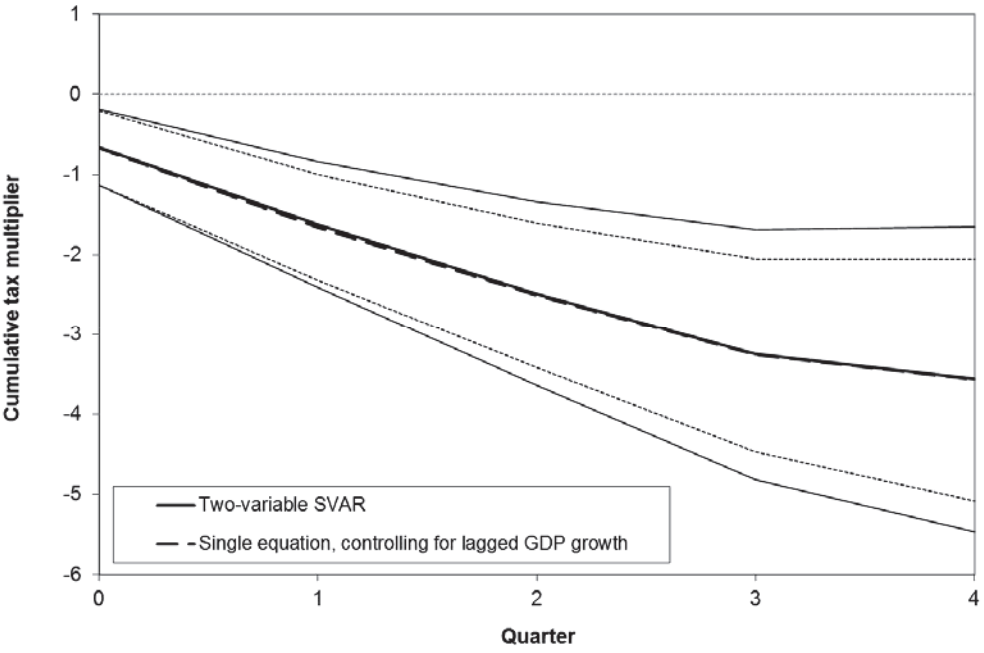

Notes: Country fixed effect panel regression. Standard errors are bootstrapped, heteroscedasticity-consistent (Huber-White), and clustered by country. Observations: 1288.

**Figure 7. Cumulative tax multiplier: Exogenous fiscal consolidation tax rate shock, single equation with controls, alternative timing of tax rate change**



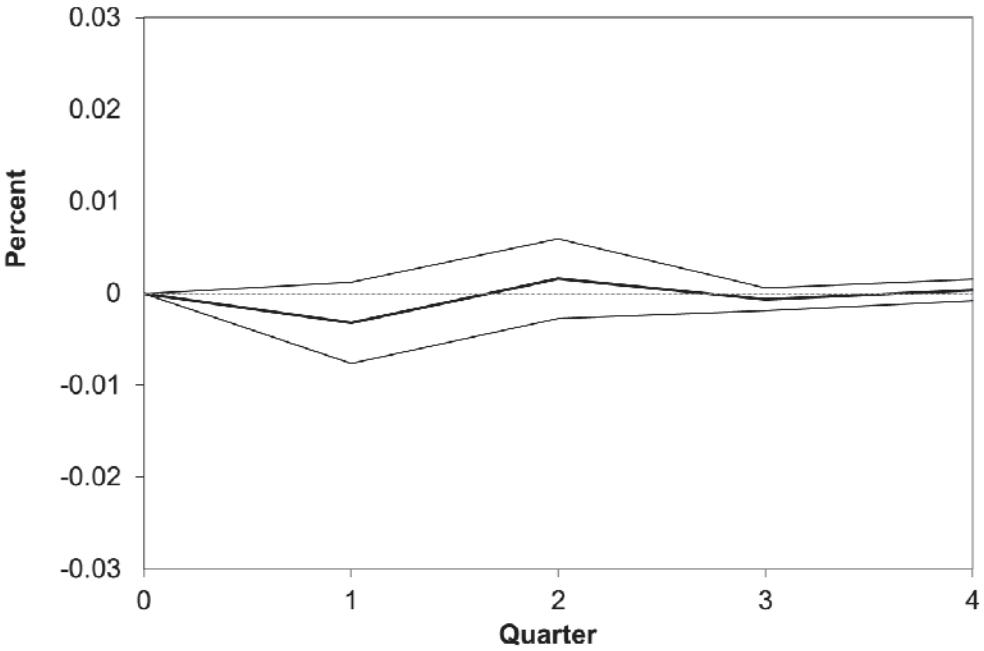

Notes: Country fixed effect panel regression. Standard errors are bootstrapped, heteroscedasticity-consistent (Huber-White), and clustered by country. Observations: 1288.

**Figure 8. Cumulative tax multiplier: Exogenous fiscal consolidation tax rate shock, single equation with controls vs. two-variable SVAR**



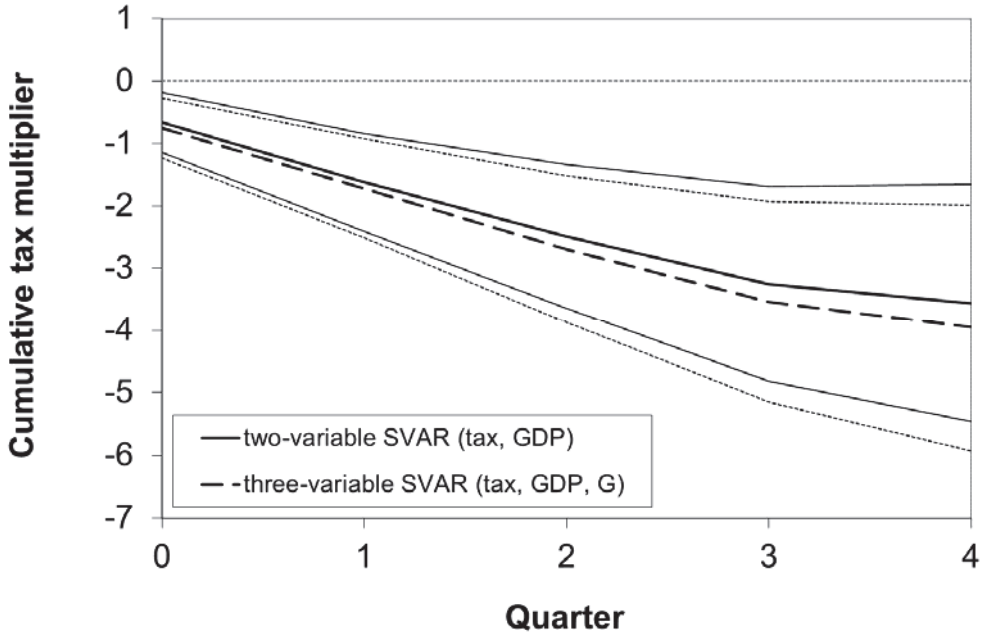

Notes: The two variables in SVAR are exogenous fiscal consolidation tax rate change and GDP growth. We assume no contemporaneous effect of output on tax rate change. Observations: 1288 (controlling for lagged GDP growth), 1284 (SVAR).

**Figure 9. Response of exogenous fiscal consolidation tax rate to GDP shock: One percent impulse response function, two-variable SVAR**



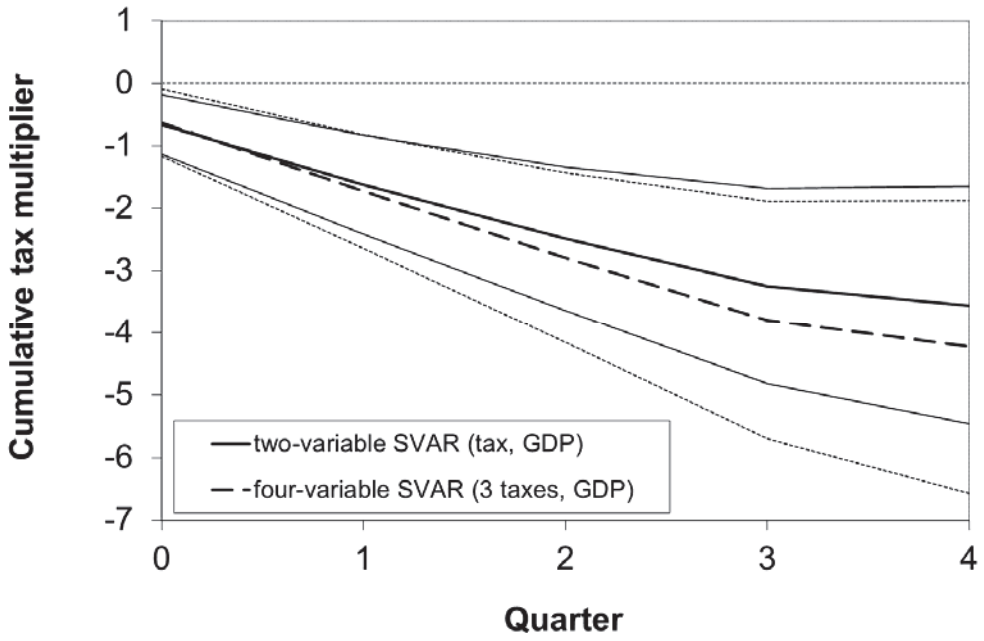

Notes: The two variables in SVAR are exogenous fiscal consolidation tax rate change and GDP growth. We assume no contemporaneous effect of output on tax rate change. Observations: 1284.

**Figure 10. Cumulative tax multiplier: Exogenous fiscal consolidation tax rate shock, two-variable vs. three-variable SVAR**




Notes: The two-variable SVAR includes exogenous fiscal consolidation tax rate changes and GDP growth. The three-variable SVAR includes exogenous fiscal consolidation tax rate changes, fiscal consolidation government expenditure growth, and GDP growth. We assume no contemporaneous effect of output on fiscal variables. We also assume no contemporaneous correlation between tax rate changes and changes in government expenditure. Observations: 1284 (two-variable SVAR), 1063 (three-variable SVAR).

**Figure 11. Cumulative tax multiplier: Exogenous fiscal consolidation tax rate shock, two-variable vs. four-variable SVAR**




Notes: The two-variable SVAR includes exogenous fiscal consolidation VAT rate changes and GDP growth. The four-variable SVAR includes exogenous fiscal consolidation tax rates changes (value-added, top individual income, and top corporate income) and GDP growth. Observations: 1284 (two-variable SVAR), 975 (four-variable SVAR).

**Figure 12. Density function of days between passage and implementation of exogenous fiscal fiscal consolidation tax rate shock**

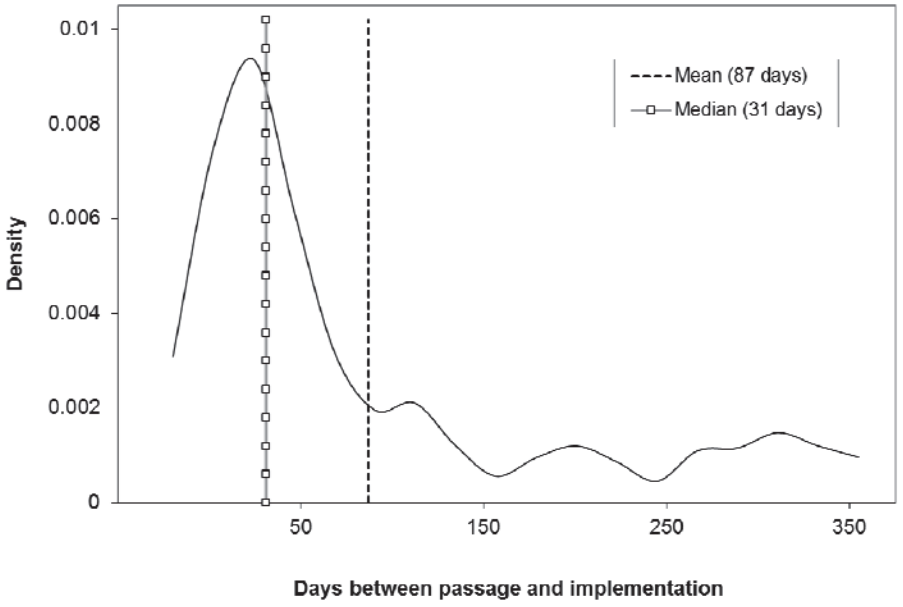
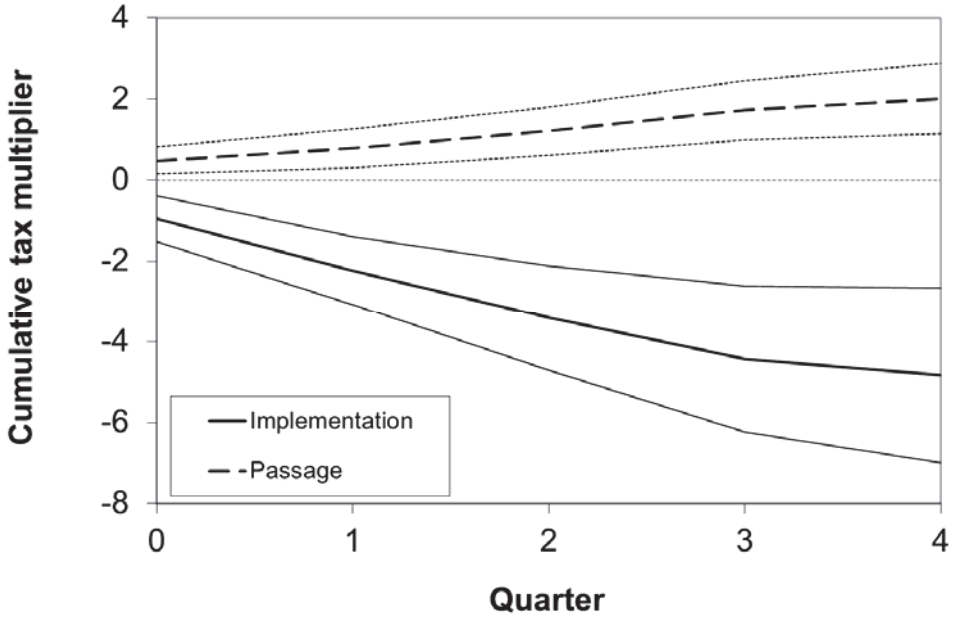


**Figure 13. Cumulative tax multiplier: Exogenous fiscal consolidation tax rate shock, single equation with controls, controlling for the announcement date**

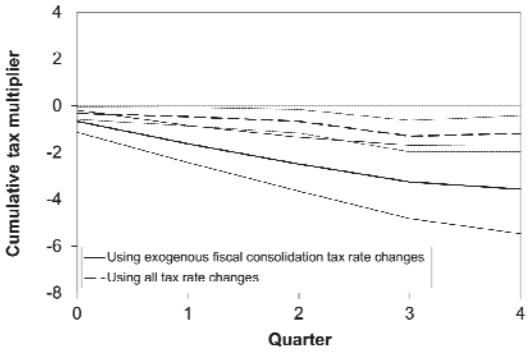



Notes: Country fixed effect panel regression. Standard errors are bootstrapped, heteroscedasticity-consistent (Huber-White), and clustered by country. Observations: 1288.

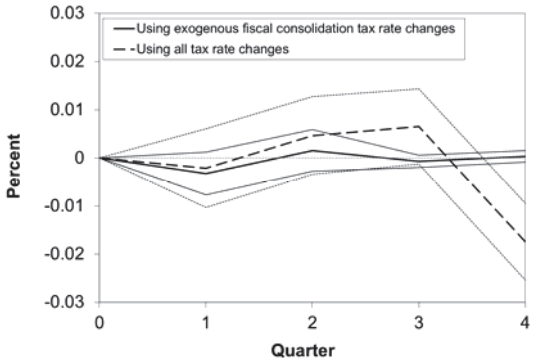
**Figure 14. Cumulative tax multiplier and one percent impulse response functions, two-variable SVAR, all and positive tax rate changes vs. exogenous fiscal consolidation tax rate changes**

**Using all tax rate changes**

Panel A. Cumulative tax multiplier

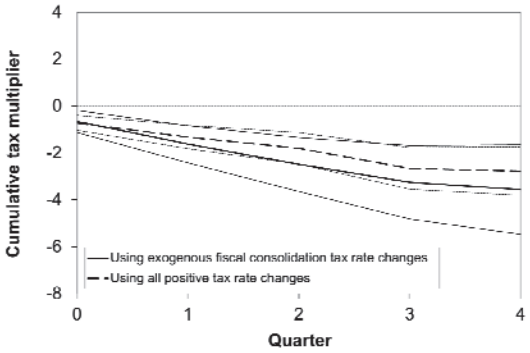



Panel B. Response of tax to GDP

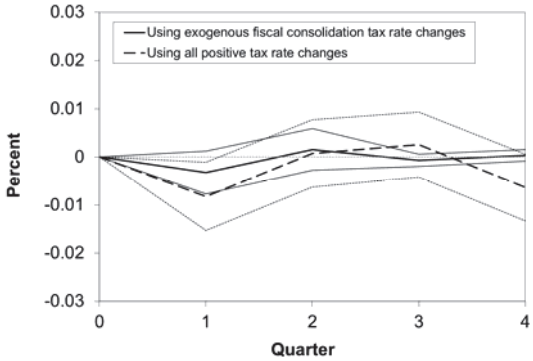



**Using all positive tax rate changes**

Panel C. Cumulative tax multiplier



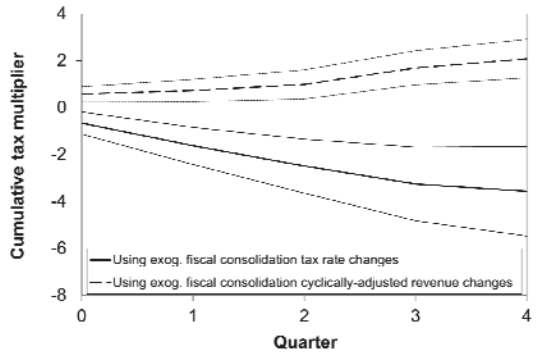

Panel D. Response of tax to GDP



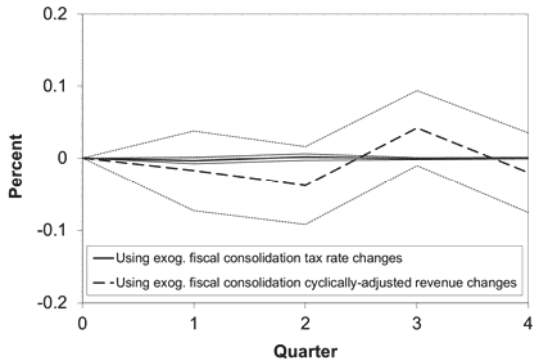

Notes: Panels A and C show the cumulative tax multiplier and panels B and D the one percent impulse response of tax to a GDP shock. We assume no contemporaneous effect of output on fiscal variables. Observations: 1284.

## Figure 15. Cumulative tax multiplier and one percent impulse response functions, two-variable SVAR, exogenous fiscal consolidation cyclically-adjusted revenue changes vs. exogenous fiscal consolidation tax rate changes

Panel A. Cumulative tax multiplier



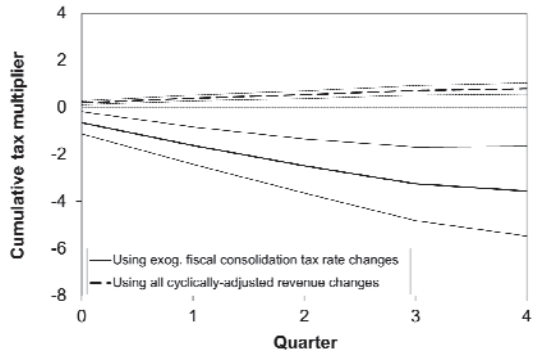

Panel B. Response of tax to GDP



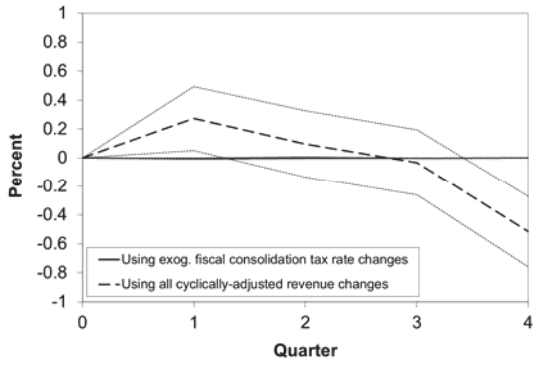

Notes: Panel A shows the cumulative tax multiplier and panel B the one percent impulse response of tax to a GDP shock. We assume no contemporaneous effect of output on fiscal variables. Observations: 856.

## Figure 16. Cumulative tax multiplier and one percent impulse response functions, two-variable SVAR, cyclically-adjusted revenue changes vs. exogenous fiscal consolidation tax rate changes

Panel A. Cumulative tax multiplier




Panel B. Response of tax to GDP




Notes: Panel A shows the cumulative tax multiplier and panel B the one percent impulse response of tax to a GDP shock. We assume no contemporaneous effect of output on fiscal variables. Observations: 856.

## Table 1. Identification of exogenous fiscal shocks vs. measurement of tax policy

| | | **Measurement of tax policy** (Finding a tax policy variable under the direct control of the policymaker) | |
|---|---|---|---|
| | | Tax rate | Cyclically-adjusted revenue |
| **Identification of exogenous fiscal shocks** (Fiscal policy changes that are not the result of policymakers responding to output fluctuations) | "Natural experiment" (à la Romer and Romer, 2010) | • No endogeneity.<br>• No measurement error. | • No endogeneity.<br>• Possible measurement error, especially due to changes in non-policy factors. |
| | Structural VAR (à la Blanchard and Perotti, 2002) | • Endogeneity problems if tax changes are forward-looking/anticipated.<br>• No measurement error. | • Endogeneity problems if tax changes are forward-looking/anticipated.<br>• Possible measurement error, especially due to changes in non-policy factors. |

## Table 2. Tax narratives

| Country | Date of implementation of VAT change | Δ VAT rate (in percentage-points) | Brief narrative | Date of passage of VAT change (for exogenous cases only) |
|---|---|---|---|---|
| (1) | (2) | (3) | (4) | (5) |
| Belgium | Jan. 1, 1983 | 2 (17% to 19%) | Exogenous. Fiscal deficit rose from 12% of GDP in 1980 to 16% of GDP in 1981. Public debt reached 127% of GDP in 1982. | Oct. 10, 1983 |
| Belgium | April 1, 1992 | 0.5 (19% to 19.5%) | Exogenous. Belgium's general government deficit peaked at over 16% of GNP in 1981. As a result, fiscal policy for the rest of the decade first focused on stabilizing and then reducing the public debt ratio. | March 1, 1992 |
| Belgium | Jan. 1, 1994 | 1 (19.5% to 20.5%) | Endogenous. A reduction in employers' contributions to social security to restore competitiveness and stimulate employment was funded by a VAT hike. | |
| Belgium | Jan. 1, 1996 | 0.5 (20.5% to 21%) | Exogenous. The VAT increase was part of a medium-term fiscal plan to reduce the deficit to the Maastricht limit of 3% of GDP. | Oct. 1, 1995 |
| France | Aug. 1, 1995 | 2 (18.6% to 20.6%) | Endogenous. The VAT increase was put in place partly to pay for a job creation program and was offset by other changes "seen as emergency measures to combat a "calamitous" economic situation." (Dejevsky, 1995). | |
| France | April 1, 2000 | -1 (20.6% to 19.6%) | Endogenous. The VAT reduction was motivated by pressure to lower taxes in the face of fiscal surpluses in the context of a booming economy. | |
| Germany | July 1, 1983 | 1 (11% to 12%) | Endogenous. Fiscal policy in 1983 was a combination of consolidation efforts and measures to stimulate the economy. | |
| Germany | Jan. 1, 1993 | 1 (14% to 15%) | Exogenous. The 1993 VAT increase was intended to address the deficit resulting from the 1990 reunification. | Feb. 1, 1992 |
| Germany | April 1, 1998 | 1 (15% to 16%) | Exogenous. Increasing social expenditure and declining tax revenue exacerbated the large budget deficits run following reunification (which were above the Maastricht treaty). The VAT was increased to address this situation. | Dec. 1, 1997 |
| Germany | Jan. 1, 2007 | 3 (16% to 19%) | Exogenous. High-cost entitlement programs contributed to rising debt-to-GDP ratio, projected at 67.5% in 2006. The increase in VAT was meant to improve the fiscal situation. | June 16, 2006 |
| Ireland | Dec. 1, 2008 | 0.5 (21% to 21.5%) | Exogenous. "Well before the crisis hit, public finances had developed serious structural weaknesses" (IMF, 2009). With the fiscal deficit projected to reach 15% of GDP in 2008, the government increased taxes and cut spending. | Oct. 14, 2008 |
| Italy | Oct. 1, 1997 | 1 (19% to 20%) | Exogenous. To ensure entry into the EMU -- and with the fiscal deficit for 1997 projected at 6.7% of GDP and the debt-ratio still at 122% of GDP -- the Italian government decided to increase the pace of fiscal consolidation with additional revenue measures such as an increase in VAT. | Sept. 1, 1997 |
| Japan | April 1, 1997 | 2 (3% to 5%) | Exogenous. As a result of fiscal stimulus in the early 1990s, the government deficit had increased to 4.2% of GDP by 1996. In 1997, policy shifted toward deficit reduction and returning to a stable fiscal position, which included an increase in the VAT. | June 25, 1996 |
| Portugal | June 5, 2002 | 2 (17% to 19%) | Exogenous. Portugal's Stability Program carried a commitment to a balanced structural budget by 2004. The high and chronic fiscal deficits since the early 1990s forced the government to take several measures to correct the deficit in 2002. | May 5, 2002 |
| Portugal | July 1, 2005 | 2 (19% to 21%) | Exogenous. "The government, reluctant to cut other spending, defended the increase in the VAT rate from 19% to 21% by arguing it had to raise taxes due to European Community pressure for quick improvement on the deficit front" (Frasquilho, 2005). | June 1, 2005 |
| Spain | Jan. 1, 1992 | 1 (12% to 13%) | Exogenous. In order to adjust fiscal deficits to Maastricht Treaty levels, the Spanish authorities reacted to the fiscal deterioration of 1990 and 1991 by reducing unemployment compensation benefits and increasing the VAT on two occasions in 1992. | Dec. 30, 1991 |
| Spain | Aug. 1, 1992 | 2 (13% to 15%) | Exogenous. Same rationale as previous case. | July 13, 1992 |
| Spain | Jan. 1, 1995 | 1 (15% to 16%) | Exogenous. Deficits increased from 2.8% to 7.5% of GDP between 1989 and 1993 due to the recession of the early 1990s and a lax expenditures policy. In 1994 the Spanish government introduced several fiscal reforms to reduce the deficit. | Dec. 31, 1994 |
| Sweden | Jan. 1, 1983 | 1.95 (21.51% to 23.46%) | Endogenous. The new government that took office in 1982 pursued a program aimed at increasing production and raising full employment (OECD, 1984). As part of this program, the VAT was increased to 23.46% in 1983 to cover the cost of restoring benefits cut by the previous government. | |

Note: See Appendix 6.3 for full tax narratives.