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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. We also provide complementary evidence using Romer and Romer (2010) and Barro and Redlick (2011) data for the United States. 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.

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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. These stimulus packages were aimed at increasing aggregate demand and hence counteracting the recessionary environment. More recently, large fiscal deficits and concerns about debt sustainability in industrial countries, particularly in Europe and the United States, have shifted the tone of the discussion from stimulus to fiscal adjustment. Since then – and following the seminal paper of Giavazzi and Pagano (1990) – there has also been a revival of studies analyzing whether fiscal adjustments might be neutral, or even expansionary, especially when driven by spending cuts.[1]

As a result of the policy interest first in fiscal stimulus and later in fiscal consolidation, there has been an explosion in both the theoretical and empirical literature on fiscal multipliers, both on the spending and the taxation side. The theoretical literature has delivered a wide range of fiscal multipliers depending upon preferences, technology, productivity of government spending, degree of tax distortion, price stickiness, underutilization of resources (i.e., the current state of the economy), the extent to which the monetary policy "leans against the wind," and debt sustainability concerns.[2] Depending on the nature of the experiment at hand, the multiplier can be as low as -2.5 (i.e., contractionary) or as expansionary as 4.

On the other hand, the empirical literature has estimated government spending multipliers that range from -2.3 (in highly indebted countries in Ilzetzki, Mendoza, and Vegh, 2010) to 3.6 (during recessions in Auerbach and Gorodnichenko, 2011).[3] The evidence on tax multipliers –defined as the response of output to an increase in taxes– also shows wide variation, ranging from 0.3 (in open economies in Ilzetzki, 2011) to -5 (in Mountford and Uhlig, 2009).[4]

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

[2] See, for example, Baxter and King (1993), Aiyagari, Christiano, and Eichenbaum (1992), Burnisde, Eichenbaum, and Fischer (2004), Galí, López-Salido, and Vallés (2007), Cogan, Cwik, Taylor and Wieland (2010), Christiano, Eichenbaum, and Rebelo (2011), and Woodford (2011).

[3] See, for example, Barro (1981), Ramey and Shapiro (1998), Blanchard and Perotti (2002), Perotti (2004), Cavallo (2005), Beetsma, Giuliodori, Klaasen (2008), Hall (2009), Mountford and Uhlig (2009), Cogan, Cwik, Taylor, and Wieland (2010), Fisher and Peters (2010), Gordon and Krenn (2010), Auerbach and Gorodnichenko (2011), Barro and Redlick (2011), Ilzetzki (2011), Ilzetzki, Mendoza and Vegh (2010), and Ramey (2011).

[4] See, for example, Perotti (2004), Mountford and Uhlig (2009), Romer and Romer (2010), Barro and Redlick (2011), Ilzetzki (2011), and Romer and Romer (2012).

The empirical literature has faced some major methodological obstacles, mainly related to potential endogeneity problems that may critically affect the reliability of existing estimates. The most important disagreement in the empirical literature lies on the identification of fiscal shocks (i.e., changes in fiscal policy variables that are not directly or indirectly related to output changes). Endogeneity problems arise mainly because of two reasons. First, changes in some fiscal variables, particularly tax revenues, are heavily influenced by output fluctuations. Indeed, tax revenues constitute a policy *outcome* (as opposed to a policy *instrument*) that endogenously responds to the business cycle, increasing during booms and falling in recessions as the tax base (be it income or consumption) positively moves with output. The source of this endogeneity is thus related to the difficulty of correctly identifying the change in discretionary tax policy or policy instrument (such as tax rates), as opposed to a policy outcome (such as tax revenues).

A second source of endogeneity arises because policymakers often adjust fiscal policy in response to output fluctuations. In fact – and based on Keynesian considerations – policymakers should engage in expansionary fiscal policy precisely when output is low. Therefore, when measuring the effect of fiscal policy changes on economic activity, the researcher must control for the possibility that such changes are taking place as a response to output fluctuations.

What methodologies have been used in the empirical literature to overcome these endogeneity problems? Two main approaches have been pursued when it comes to estimating fiscal multipliers. Following Blanchard and Perotti (2002), the first approach is based on the structural vector autoregression (SVAR) method in combination with quarterly data (Perotti, 2004; Favero and Giavazzi, 2007; Mountford and Uhlig, 2009; Favero and Giavazzi, 2010; Gordon and Krenn, 2010; Ilzetzki, Mendoza, and Vegh, 2010; Ilzetzki, 2011). These authors identify fiscal shocks using information about fiscal institutions. On the expenditure side, their identifying assumption relies on the dynamics between fiscal policy and output. In particular, they assume that government spending requires at least one quarter to respond to news about the state of the economy. On the taxation side, they use the output elasticity of tax revenues in order to differentiate "discretionary" changes in taxation (also referred to as changes in cyclically-adjusted revenues) from those driven by fluctuations in economic activity. Under these assumptions, these authors argue that the use of quarterly data solves the endogeneity problems mentioned above. A key advantage of the SVAR is its ease of implementation and data availability. Indeed, many of these studies analyze the size of the multiplier not only in the United States and other industrial economies, but also in developing countries. This strategy has been criticized on the basis that most changes in government spending and taxes are actually forward-looking/anticipated by agents, which is at the root of the structural identification (Hansen and Sargent, 1991; Ramey and Shapiro, 1998; Leeper, Walker, and Yang, 2008; Ramey, 2011).

The second methodology used to overcome identification problems is related to the "natural experiment" approach. Since Barro (1981), this approach has been used to identify fiscal policy changes not triggered by fluctuations in current economic conditions. On the spending side, studies have typically focused on particular spending categories, such as military buildups, the least likely to respond to output fluctuations (Barro, 1981; Ramey and Shapiro, 1998; Hall, 2009; Fisher and Peters, 2010; Ramey, 2011; Barro and Redlick, 2011).[5]

The evidence of the "natural experiment" approach on the taxation side is scarce. Romer and Romer (2010), hereafter RR, use the narrative record, such as presidential speeches and congressional reports, to identify, on an individual basis, the nature of legislated United States federal tax changes spanning from 1945 to 2007. These authors identify exogenous (to the business cycle) tax changes; either because they are passed for long-run growth reasons or involve increases seeking to reduce an inherited budget deficit. Regarding long-run growth tax changes, 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. An example of such a deficit

[5] This strategy has two drawbacks. From an identification point of view, it is possible that military buildups have other effects on the economy (e.g., via patriotism, price controls, and rationing) apart from the effects on government spending (Ramey, 2011). Moreover, most of the evidence derives from the United States experiences during World War II and the Korean War. Large wars have been relatively infrequent after 1955 in the United States, whereas military spending is fairly small outside the United States, limiting the extent to which this methodology can be applied in other countries of the world. Furthermore, in the few cases in which military spending is important, the fact that wars are often waged on domestic soil makes it impossible to distinguish the effects of the war itself from the effects of military spending.

driven tax change is the Clinton tax increase in the Omnibus Budget Reconciliation Act of 1993. Policymakers raised taxes not because they felt the economy was overheated and needed to be restrained, but because they felt it was prudent fiscal policy and might increase long-run growth." Their main tax change measure is the planned change in tax receipts, assessed during the prior legislative process. They find that long-run growth tax changes have important effects on output; a one unit revenue increase shock decreases output up to $3. In contrast, deficit-driven tax hikes have a weak positive effect on GDP. While RR caution about the estimates' precision, they suggest that this difference across exogenous motivations might reflect that augmenting taxes to reduce an inherited deficit may be less costly than other tax increases. In a similar vein, Barro and Redlick (2011), hereafter BR, use United States average annual marginal income tax rates from individual federal and state income taxes as well as social security payroll taxes for the period 1913-2006 and find a sizable negative effect of tax rate increases on economic activity: a one unit revenue increase shock reduces output by $1.1.

Our paper contributes to the literature on tax multipliers. As Table 1 summarizes, our analysis disentangles the controversy regarding the *identification of exogenous fiscal policy shocks* (i.e., fiscal policy changes that are not the result of policymakers responding to output fluctuations) from problems related to the *measurement of tax policy* (i.e., finding a tax policy variable under the direct control of the policymaker). We believe that these two issues, though related, are distinct and should be addressed accordingly.

To address the issue of *identification*, we revisit the merits of the SVAR versus the "natural experiment" approach. For the latter strategy we use – as in Favero, Giavazzi, and Perego (2011) – action-based episodes of fiscal consolidations developed by the IMF (2010). The IMF study analyzes a variety of documents for 15 industrial countries including OECD Economic Surveys, IMF Staff Reports, IMF Recent Economic Developments reports, country budget documents, and additional country-specific sources. They define fiscal consolidation 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." As in RR, the action-based approach identifies – through the narrative record – policy actions motivated by deficit reduction. "If policymakers are not motivated by the state of the economy, the resulting actions should not be systematically correlated with prospective economic conditions. As a result, they are legitimate actions to use to estimate the output effects of tax changes" (RR, page 770). Moreover, because accidental correlations are always a possibility, we include a number of checks and controls. In particular, as in RR, we evaluate to what extent fluctuations in economic activity are good predictors of (i.e., Granger cause) changes in taxes.

INSERT TABLE 1 HERE

To address the (much less explored) issue of *measurement of tax policy*, 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. As discussed in Kaminsky, Reinhart, and Vegh (2004) and Vegh and Vuletin (2012), policymakers do control and legislate on tax rates. In contrast, tax revenues are a policy outcome not under the policymaker's direct control. To fix ideas, define tax revenues (R) as follows:

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

The policymaker controls $TAX\ RATE$ but not $TAX\ BASE$ and, consequently, does not control R.[6] Alternatively, we can rewrite (1) as:

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

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

The standard macroeconomic measure of discretionary policy tax change is the change in the so-called cyclically-adjusted revenues (Giavazzi and Pagano, 1990; Alesina and Perotti, 1997; Blanchard and Perotti, 2002; Alesina and Ardana, 2010; Favero and Giavazzi, 2010; Perotti, 2011; Ilzetzki, 2011). The cyclically-adjusted revenue change is typically calculated

[6] This concern is particular to tax policy, since the level and changes in public consumption represent, by construction, the appropriate policy instrument on the spending side.

as:

$$\Delta \textit{cyclically-adjusted}_t = r_t - r_{t-1} - \eta \left(y_t - y_{t-1} \right), \tag{3}$$

where y is the log of output (Y) and η the historical average tax revenue elasticity of output. The first two terms on the right-hand side of (3) capture 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. This approach thus uses the percentage change in GDP together with the tax revenue-GDP elasticity. In principle, this cyclically-adjusted measure offers an intuitive way of dealing with the fact that part of tax revenues, in particular the tax base, moves endogenously with the business cycle. The idea is, of course, that once tax revenues are cyclically-adjusted, changes in the fiscal variable will reflect the discretionary action of policymakers. Indeed, assuming that $\textit{tax base}_t = \eta \cdot y_t$ it follows that $\Delta \textit{cyclically-adjusted}_t = \Delta \textit{tax rate}_t$.[7,8] In other words, cyclically-adjusted revenue changes seem to capture discretionary changes in tax policy; i.e. changes in tax rates.

While appealing at first, revenue-based measures of tax policy, such as those cyclically-adjusted, suffer from important measurement errors. Mirroring the discussion in the growth literature about 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' behavior. This source of measurement error would include technical/calibration issues regarding, for example, the stability of η over time (or equivalently, the stability of the ratio tax base to GDP). Moreover, there are further conceptual issues related to the influence of non-policy factors including, among others, changes in willingness/possibility to evade taxes, agents' behavior (either structural breaks or over the cycle), and ability to bribe. Thus, to the extent that measurement errors are not random in nature but depend on output fluctuations, these problems will introduce measurement biases.

In sum, there is really no good substitute for obtaining data on tax *rates* themselves when it comes to measuring changes in the tax policy instrument. In order to asses the relevance of

[7] Assuming that $\textit{tax base}_t = \eta \cdot y_t$ implies that η is the tax base-GDP elasticity.

[8] What follows is the proof of this result. Replacing (2) in (3) we obtain $\Delta \textit{cyclically-adjusted}_t = \Delta \textit{tax rate}_t + \Delta \textit{tax base}_t - \eta \cdot \Delta y_t$. From $\textit{tax base}_t = \eta \cdot y_t$, it is straightforward that $\Delta \textit{tax base}_t = \eta \cdot \Delta y_t$. Considering the latter result and $\Delta \textit{cyclically-adjusted}_t = \Delta \textit{tax rate}_t + \Delta \textit{tax base}_t - \eta \cdot \Delta y_t$, we obtain $\Delta \textit{cyclically-adjusted}_t = \Delta \textit{tax rate}_t$.

this measurement problem, 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. The list of countries comprises Australia, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, Japan, Portugal, Spain, Sweden, and United Kingdom.

Our main results can be summarized as follows:

1. "Natural experiment" vs. SVAR: Our findings favors RR's in that the "natural experiment" approach seems to be a more convincing strategy to truly identify exogenous fiscal shocks.[9] As in RR, we show that when using SVAR as our identification strategy, alternative tax series respond promptly and significantly to output fluctuations. These results support previous critiques about the SVAR identification strategy, suggesting that the alleged unanticipated (at a one quarter horizon) changes in fiscal policy are actually forward-looking/anticipated by agents. In contrast, when focusing on fiscal consolidation episodes, alternative tax series barely move following movements in output. These findings validate the "natural experiment" approach since tax changes do not seem to be driven by policymakers' short-run reactions to GDP fluctuations.

2. Cyclically-adjusted revenues vs. tax rate: Our findings strongly support the use of changes in tax rates as a true measure of tax policy instrument. The correlation between cyclically-adjusted revenue and tax rate changes is very low, namely 0.05. The alleged discretionary tax policy proxy is a poor approximation to changes in tax policy. Indeed, while the data comprises 49 changes in tax rates, the cyclically-adjusted revenue measure identifies 900 changes. This measure thus over-counts discretionary changes in tax policy by a factor of 17. Even when focusing on observations where tax rate changes are not zero, the correlation between changes in cyclically-adjusted revenues and tax rates is rather low, 0.26, and we cannot reject the null hypothesis of independence at a 23 percent level of confidence. If we further restrict our comparison to fiscal consolidation episodes such correlation is 0.09, and we cannot reject the null hypothesis of independence at a 77 percent level of confidence.

[9] As in RR, we obviously do not use the term "exogenous" either in the strict econometric sense or to mean that the changes have no economic causes. An equally appropiate terminology would be "valid" and "invalid", rather than "exogenous" and "endogenous."

Not surprisingly, these measurement issues have important implications for tax multipliers. Our main point – the importance of using tax rates to measure tax policy instrument – is, however, independent of the particular identification strategy: it applies identically to the analysis of fiscal shocks identified through the narratives à la RR as well as when using SVAR. Tax rate increases are always contractionary. However, tax multipliers based on cyclically-adjusted revenues are, at worst, neutral or even expansionary!

3. Most up-to-date tax studies (Perotti, 2004; Favero and Giavazzi, 2010; Caldara, 2011; 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 strategy, we find that tax multipliers are not contractionary. Indeed, we find that a one unit revenue increase shock increases output by \$0.18 on impact and \$0.78 after three quarters. When focusing on fiscal consolidation episodes and cyclically-adjusted revenue changes (top-right cell in Table 1), tax multipliers are neutral. When using SVAR and changes in tax rates (bottom-left cell in Table 1), tax multipliers are contractionary. A one unit revenue increase shock reduces output by \$0.37 on impact and \$1.32 after three quarters. Regarding fiscal consolidation episodes and changes in tax rates (top-left cell in Table 1), tax multipliers are strongly contractionary. A one unit revenue increase shock reduces output by \$1.02 on impact and \$2.76 after three quarters. This wide range of results show the importance of the strategy used to identify exogenous fiscal shocks as well as the measurement of tax policy.

4. Borrowing average marginal individual income tax rates data from BR and revenue-based ones from RR for the United States, we also provide complementary evidence regarding the importance of appropriately measuring tax policy. Focusing on fiscal consolidation episodes, we show that the tax multiplier is basically neutral in terms of GDP, in line with RR's main tax series implications. Using cyclically-adjusted revenue changes also generates neutral tax multipliers. In sharp contrast, with average marginal individual income tax rates, tax multipliers are strongly contractionary in the medium and long term.

The rest of the paper is organized as follows. Section 2 discusses how to measure tax policy and briefly elaborates on some of the practical pros and cons of focusing on the value-added tax. Section 3 presents the tax dataset and analyzes the differences between the tax rate and the cyclically-adjusted revenue series. Section 4 computes tax multipliers using fiscal consolidation episodes (to identify exogenous fiscal shocks) and tax rates (to measure the tax policy). Using alternative econometric specifications and several control variables, we find robust evidence on the contractionary effects of value-added tax rate increases. We also show, not surprisingly, that changes in private consumption seem to drive GDP fluctuations. Section 5 examines the implications of alternative empirical strategies regarding the identification of fiscal shocks and measurement of tax policy. Section 6 provides complementary evidence on the importance of appropriately measuring the tax policy using BR and RR data for the United States. Final thoughts are presented in Section 7.

2 Measuring tax policy

When analyzing the business cycle properties of spending policy, most papers use government spending or government consumption. These fiscal variables represent the overall policy instrument on the spending side. In contrast, tax policy does not rely on a single tax rate associated with a single activity. Governments typically resort to many different taxes, including, among others, individual and corporate income, social security contributions, property, goods and services as well as taxes on trade and financial transactions. Many of these taxes, especially personal income taxes, have several brackets and an intricate system of deductions. These features complicate the extent to which researchers can unequivocally assess the stance and changes in tax policy. Up to now, most papers relying on tax rates have studied the United States while typically focusing on individual income taxes as well as social security contributions. BR use United States average annual marginal individual income tax rates from federal and state taxes as well as social security payroll taxes for the period 1913-2006. Romer and Romer (2012) analyze the evolution of individual marginal tax rates as well as corporate tax rates in the United States for the interwar period 1919-1941. In terms of the cyclicality of taxation policy at the annual frequency, Vegh and Vuletin (2012) use the top marginal rates on individual and corporate taxes as well as value-added tax rates for 62 countries for the period 1960-2009. No approach is completely satisfactory and, most likely, given the intricacies of the taxation system, none will ever be. That said, the profession seems to be moving in the right direction by devoting significant efforts to gather new datasets, allowing both researchers' and policymakers' better understanding of tax instruments (such as tax rates) behavior and effect, as opposed to tax outcomes (such as cyclically-adjusted revenues).

The main practical advantage of the VAT rate is that it consists of a single standard rate.[10] On the contrary, personal income taxes have several rates for different income brackets and an intricate system of deductions. The single rate allows the researcher to clearly assess the stance of tax policy. As discussed in great detail in BR, changes in the average marginal individual tax rates (AMITR) may be triggered by shifts in the underlying distribution of marginal tax rates in a manner correlated with differences in labor-supply elasticities (e.g., the 1948 U.S. tax cut). Moreover, increases in the AMITR, such as the one observed in the U.S. from 1971 to 1978, may reflect the shift of households into higher brackets due to high inflation in the context of an unindexed tax system. This concern seems to be particularly relevant in the case of the developing world as well as industrial countries with a long history of moderate/high and persistent levels of inflation, such as Greece, Italy, Portugal, and Spain. A second identification advantage of the VAT relates to the lag between the change in tax legislation and the household learning about it. As pointed out by BR, information regarding changes in tax rates, tax brackets, and deductions in the AMITR are arguably gradually learned by households throughout the year. This is indeed the main reason why BR use annual frequency data. In contrast, changes in VAT rate are arguably internalized promptly by households, since consumption is performed on a more continuos and frequent manner.

3 Data

In this paper, we study the macroeconomic effects of tax policy in 14 industrial countries for the period 1980-2009.[11] Given the absence of a readily-available series of average marginal individual and/or corporate income tax rates for this group of countries, we focus our efforts on building a new quarterly value-added tax (VAT) rate series. For comparison purposes,

[10] We should note that while countries usually have a reduced value-added rate, it typically applies to particular goods, such as some food categories and child and elderly care.

[11] The countries are Australia, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, Japan, Portugal, Spain, Sweden, and United Kingdom.

we also construct the corresponding VAT cyclically-adjusted revenue measure at a quarterly frequency.[12] Naturally, since changes in VAT rates could be reinforced or compensated by changes in other fiscal variables, we also control for changes in public expenditure as well as those in individual or corporate tax rates. For the latter, we use top marginal tax rates. Our analysis also includes macroeconomic variables such as output, consumption, investment, exports, and imports; all of them at a quarterly frequency.

Deficit-driven/fiscal consolidation episodes are identified by the October 2010 IMF World Economic Outlook. It covers the period 1980-2009 for 15 industrial countries.[13] According to the IMF World Economic Outlook, a fiscal consolidation episode is 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." As in RR, this action-based approach identifies – through the narrative record – policy actions motivated by deficit reduction. For this purpose, the IMF study examines a variety of documents including OECD Economic Surveys, IMF Staff Reports, IMF Recent Economic Developments reports, country budget documents, and additional country-specific sources.

We now describe the VAT rate data collected. Figure 1 shows the change in VAT rate for each of the 14 countries in the sample. We divide those changes into two categories: those occurring during episodes of fiscal consolidation versus those that do not. Overall, we have 49 changes in VAT rate; 36 are tax rate increases and 13 are tax rate decreases. Excluding zeros, the median tax rate change is 1 percentage point; while the average tax rate change is 0.7 percentage points. The highest tax rate reduction and tax rate increase are -12 and 5 percentage points, respectively. As expected, all fiscal consolidation tax rate changes (i.e., tax rate changes taking place during periods of fiscal consolidations) are positive, totaling 21 changes. About 60 percent (21 out of 36) tax rate increases are associated with episodes of fiscal consolidation. Moreover, there is a fair degree of variation in the distribution of tax rate changes across countries and time.

Figure 2 displays both VAT rate and VAT cyclically-adjusted revenue changes. The latter proves to be a poor approximation to changes in tax policy (i.e., tax rates). It tends to

[12] See Appendix 8 for a description of VAT tax rate data sources and details on the VAT cyclically-adjusted revenue measure calculation.

[13] The countries are Australia, Belgium, Canada, Denmark, Finland, France, Germany, Ireland, Italy, Japan, Portugal, Spain, Sweden, United Kingdom, and United States.

strongly over-count discretionary changes in tax policy (by about a factor of 17). While the data comprises 49 changes in the tax rate, the cyclically-adjusted revenue measure identifies 900 changes. For this reason, the contemporaneous correlation between changes in cyclically-adjusted revenues and tax rate is very low, namely 0.05.[14] Even when tax rate changes are not zero, the contemporaneous correlation between changes in cyclically-adjusted revenues and tax rate is quite moderate, 0.26, preventing us from reject the null hypothesis of independence at a 23 percent level of confidence. If we further restrict our comparison to fiscal consolidation episodes, such correlation is 0.09, and we cannot reject the null hypothesis of independence at 77 percent level of confidence.

4 The effects of fiscal consolidation tax rate changes on output

In this section, we study the effect of fiscal consolidation VAT rate changes on economic activity using quarterly data. We start by estimating this relationship in three progressively more elaborate ways. We find that VAT rate (hereafter tax rate) increases have strong negative effects on output. In section 4.1, we analyze how various components of GDP, such as consumption and investment respond to tax rate changes. Predictably, consumption seems to be the most sensitive component. We conclude our analysis with some robustness tests, where we control for other fiscal variables changes (section 4.2). Our main findings are strongly robust to these considerations.

We begin our analysis by estimating the effect of tax rate changes on economic activity using 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 Δy is the real GDP growth rate, Δt represents fiscal consolidation tax rate change, and d is country dummy. In estimating equation (4), we include four lags (i.e., $J = 4$).[15] This

[14] One possible source of low correlation may be differences in timing. Suppose, for example, that revenues are turned over to the government with a 1-quarter lag. Then there might be low correlation between the change in cyclically-adjusted revenues and the change in the tax rate, but a high correlation between the change in cyclically-adjusted and the lagged change in the tax rate. This suspicion, however, is not warranted as the correlation between cyclically-adjusted revenues and lagged tax rates changes range (within a 1-year period) between 0.07 and 0.005.

[15] Our findings remain robust to the inclusion of further lags.

approach should yield unbiased reduced-form impact estimates of tax rate changes on output (see top-left cell in Table 1). Considering the intrinsic nature of fiscal consolidation episodes, tax rate increases bear, in principle, no relation to the current or prospective state of the economy, but rather to inherited budget deficits. Therefore, the estimation of (4) is based on exogenous fiscal shocks. Moreover, any issues in terms of measurement errors are also disregarded, since the tax variable used is a tax instrument, as opposed to a revenue-based one, such as the cyclically-adjusted revenue.

While conceptually appropriate, a key inconvenient of using changes in tax rates is that the estimated coefficients β do not correspond to the usual tax multiplier for GDP. The results link the change in GDP to the change in the tax rate and not in tax revenue *per se*. We compute the tax multiplier (i.e., the response of GDP to a unit increase in tax revenue) by exploiting the typical relationship of tax revenue to tax rate. Let R be real VAT revenue, Y real output, T "implicit" VAT rate (defined as R/Y), and e the average relationship between T and t (i.e., $e = T/t$). Following BR (pages 80-81), the tax multiplier is then given by[16]

$$Tax\ multiplier \equiv \frac{\Delta Y}{\Delta R} = \frac{\beta}{e + \beta T}. \tag{5}$$

Using a Taylor approximation of (5), it follows that the standard error of the tax multiplier equals[17]

$$Tax\ multiplier_{SE} = \frac{e}{(e + \beta T)^2}\beta_{SE}, \tag{6}$$

where β_{SE} is the standard error of β. Figure 3 uses estimates from (4) to illustrate the tax multiplier, defined as the effect of a unit shock increase in VAT revenue collection on output together with the one-standard-error bands. This figure shows that the effect is consistently

[16] What follows is the proof of this result. $\frac{\Delta Y}{\Delta R} = \frac{\Delta Y/Y}{\Delta R/Y}$. From equation (4), we know that $\Delta Y/Y = \beta \Delta t$. Therefore, $\frac{\Delta Y}{\Delta R} = \frac{\beta \Delta t}{\Delta R/Y}$. Since $R \equiv T \cdot Y$, then $\Delta R = \Delta T \cdot Y + \Delta Y \cdot T$. Therefore, $\frac{\Delta Y}{\Delta R} = \frac{\beta \Delta t}{\Delta T + \beta \Delta t \cdot T}$. e captures the historical relationship between T and t (i.e., $e = T/t$). Then, $\Delta T = e \cdot \Delta t$. Finally, $\frac{\Delta Y}{\Delta R} = \frac{\beta \Delta t}{\Delta T + \beta \Delta t \cdot T} = \frac{\beta}{e + \beta T}$.

[17] The proof is as follows. From (5) $Tax\ multiplier = f(x)$, where $f(x) = \frac{x}{e + xT}$. Using a Taylor first order approximation, we obtain $Tax\ multiplier = f(\beta) + f'(\beta)(x - \beta) = f(\beta) + f'(\beta)x - f'(\beta)\beta$. Applying the variance to both sides of the equation, we obtain $Var(Tax\ multiplier) = 0 + [f'(\beta)]^2 Var(x) - 0 = [f'(\beta)]^2 Var(x)$. Evaluating $f(x)$ at $x = \beta$, then, $Var(Tax\ multiplier) \approx [f'(\beta)]^2 Var(\beta)$. Applying square roots to both sides of the equation, it follows that $Tax\ multiplier_{SE} = f'(\beta)\beta_{SE}$. Finally, $Tax\ multiplier_{SE} = \frac{e}{(e + \beta T)^2}\beta_{SE}$.

and significantly negative. In particular, the impact effect represents a fall in output of \$1.02 ($t = -2.36$). The subsequent effect on output is downwards for the next three quarters before slightly rebounding after a year. The maximum effect is reached after three quarters, with a fall in output of \$2.70 ($t = -2.96$).

INSERT FIGURE 3 *HERE*

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

$$\Delta y_{i,t} = \alpha + \sum_{j=0}^{J} \beta_j \Delta t_{i,t-j} + \sum_{n=1}^{N} \beta_n \Delta y_{i,t-n} + \sum_{i=1}^{I-1} \beta_i d_i + \mu_{i,t}. \tag{7}$$

That is to say, $J = 4$ and $N = 4$. As discussed in RR, including this key control variable could help in several ways. In particular, it helps control for the normal dynamics of output while, simultaneously, providing a simple way of controlling for other factors affecting output, most likely serially correlated. Using the estimates from (7), Figure 4 shows the tax multiplier when controlling for lagged output growth.[18] For comparison purposes, this figure also reports the findings from the specification without GDP growth lags. Results are virtually unchanged both in terms of point estimates and precision as well as the temporal profile depicted.

INSERT FIGURE 4 *HERE*

Last, but certainly not least, we present the results of a two-variable SVAR. As in (4) and (4) we use four lags. Figure 5 displays the tax multiplier when using the two-variable SVAR. The temporal profile of the tax multiplier is similar to the one obtained using specification (7). However, the estimated maximum effect falls from \$2.70 ($t = -2.96$) to \$2.76 ($t = -2.13$).

INSERT FIGURE 5 *HERE*

Figure 6 presents the following impulse response functions. Panel A shows the response of the tax series to a one percent tax shock. The dynamic is very short lived; it basically

[18] The estimated impact of tax rate changes is now a dynamic multiplier accounting for the implied changes in the path of lagged GDP growth. The coefficient standard errors of regression (7) are calculated by taking 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.

vanishes after the innovation. Panels B and C display output and the tax series response to a one percent GDP shock. Panel B shows that output shocks are, as expected, serially correlated. In sharp contrast, the tax series virtually does not respond to an output shock (Panel C). After a one percent innovation on output, tax rates barely fluctuate between 0.003 ($t = 0.61$) and -0.005 ($t = -0.99$). 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 0.81. This finding strongly supports our conjecture that tax rate changes identified using fiscal consolidation episodes (i.e., through historical narratives) are, indeed, unrelated to past output fluctuations.

INSERT FIGURE 6 *HERE*

In sum, evidence drawn from VAT rate changes associated with fiscal consolidation episodes strongly support the idea that tax rate increases have large negative effects on economic activity. Indeed, a $1 increase in revenue collection reduces output by $1.02 on impact. The subsequent effect on output is downwards for the next three quarters before rebounding slightly after a year. The maximum effect implies a fall in output of around $2.70.

4.1 Components of output and the transmission mechanism

This section analyzes the extent to which an increase in fiscal consolidation VAT rate actually reduces GDP components. In principle, VAT rate changes would most clearly affect the consumption-saving margin. Because exports are generally zero-rated (and VAT refunded or offset against other taxes), it is sometimes argued that changes in the VAT rate could also alter the domestic-export margin. Rising VAT rates could increase firms' incentives to export while reducing their supply to the domestic market. Additionally, increases in VAT rates may also deter imports. Indeed, this kind of rationale is at the core of some organizations' missions, such as the American Manufacturing Trade Action Coalition (AMTAC). They support a revision of United States tax law to eliminate the disadvantage to domestic manufacturers caused by foreign border-adjusted taxes such as the value-added tax.

Figure 7 shows the tax multiplier for consumption, investment, exports, and imports. An increase in VAT rates seems to reduce investment (panel B), exports (panel C) as well as imports (panel B). However, consumption (panel A) is the macroeconomic component that responds the most to a change in tax rates. The impact effect represents a fall in consumption of \$1.56 ($t = -1.84$). The subsequent effect on consumption is downwards; the maximum effect implies a fall in consumption of \$3.03 ($t = -4.08$).

INSERT FIGURE 7 HERE

4.2 Controlling for other fiscal variables changes

Like RR, we now control for changes in other fiscal variables. First, we analyze government spending and, later, we focus on some other taxes. Government expenditure can have a large impact on output. If during episodes of fiscal consolidation, government spending typically decreased at the same time that tax rate increases, omitting the former would bias the tax multiplier downwards, giving the false idea that tax rate hikes are more contractionary than they actually are. On the other hand, if during episodes of fiscal consolidation government spending typically increased to compensate to a certain extent for the alleged negative effect of tax rate increases, omitting the former would bias the tax multiplier towards zero, giving the false impression that tax rate hikes are not that contractionary. The data do not seem to support any systematic contemporaneous relation (at a quarterly frequency) between changes in government expenditure and tax rates during episodes of fiscal consolidation.[19] Naturally, changes in government expenditure and tax rates could also be non perfectly syncronized and still affect the tax multiplier. Indeed, we cannot reject at the 10 percent level of significance that government expenditure decreases after a quarter of a fiscal consolidation tax rate change.

Figure 8 shows the results.[20] Panel A shows that the tax multiplier remains unchanged after controlling for government expenditure. Panel B also confirms –as Figure 6 panel C– that tax rates barely fluctuate after a one percent innovation on output. In Figure 8, panel C displays the government expenditure multiplier, only with spending changes occurring during periods of fiscal consolidation. The size of the spending multiplier is in line with some previous

[19]Such correlation is -0.02, preventing the rejection of the null hypothesis about independence at a 68 percent level of confidence.

[20]For comparison purposes, we assumed as it is common practice in the literature that $\alpha_{gt} = 0$ and $\alpha_{tg} = 0$ in the matrix of contemporaneous relations between endogenous variables. Our results are almost identical if we used estimates of α_{gt} and α_{tg} using a LSDV model. These figures are not reported for brevity.

studies. A one unit shock to government expenditure increases output by \$0.68 ($t = 3.40$) on impact and by \$1.15 ($t = 1.99$) after a year. However, focusing on spending changes occurring during fiscal consolidations episodes does not fully insulate government spending from responding to GDP shocks (potential endogeneity problems). Unlike the case of tax rate changes, where it is fairly straightforward, if time consuming, to assess the nature of a legislated tax change, we cannot gauge the nature of total government spending fluctuations. Indeed, as discussed in section 1, this is the main reason why a significant part of the literature has focused on spending categories, such as military buildups, since these are the least likely to respond to output fluctuations. Figure 8, panel D supports this presumption. A one percent increase in GDP triggers an increase in government expenditure, especially after three quarters. The p-value for the test of the null hypothesis that output does not granger-cause the spending series is 0.01.[21]

INSERT FIGURE 8 *HERE*

We now control for changes in other taxes; namely corporate and individual income tax rates. Given the absence of a readily available dataset on average marginal income tax rate for the 14 countries used in this study, we use top marginal corporate and individual income tax rates. Figure 9 shows the VAT multiplier with and without these additional tax rates.[22] The impact effect weakens from \$1.02 ($t = -2.36$) to \$0.89 ($t = -1.59$). The estimated maximum effect falls from \$2.76 ($t = -2.13$) to \$4.09 ($t = -1.76$). Broadly speaking, results are robust and VAT tax increases reduce output.

INSERT FIGURE 9 *HERE*

[21] If we included all government expenditure changes instead of only those associated to fiscal consolidation episodes, the tax multiplier would remain virtually unchanged. On the other hand, the government multiplier would decrease because of the apparent short-run countercyclical response of spending to output fluctuations. These figures are not reported for the sake of brevity.

[22] We assumed that the contemporaneous relations between the different tax rate changes in the matrix of endogenous variables are zero. Our results are almost identical if we used estimates using a LSDV model. These figures are not reported for brevity.

5 Comparison with alternative empirical strategies

This section examines the implications of using alternative empirical strategies for exogenous fiscal shocks identification and tax policy measurement. For comparison purposes, figures also report findings from the specification on fiscal consolidation tax rate changes (Figure 5 and Figure 6, panel C).

We first analyze the implications of using the VAT rate, a proper 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 fiscal consolidation episodes – and allow for the structural assumption proposed by Blanchard and Perotti (2002) to identify fiscal shocks.[23] Figure 10, panels A and B, display the results. Panel A shows that the intertemporal profile of the multiplier is quite similar, yet strongly biased towards zero. The impact effect is reduced by almost 200 percent. While always contractionary, this effect changes from \$1.02 ($t = -2.36$) to \$0.37 ($t = -1.43$). The estimated maximum contractionary effect essentially halves, from \$2.76 ($t = -2.13$) to \$1.32 ($t = -1.97$). Panel B shows that tax rate changes tend to respond significantly to output changes, especially after two quarters. We cannot reject the hypothesis that the fourth lag is negative (p-value = 0.99).

One could also think that the difference in tax multipliers shown in Figure 10, panel A, may reflect that the impact of tax rate changes are asymmetric depending on whether they are tax cuts or tax hikes. Since all our fiscal consolidation tax rate changes are positive (i.e., increases in tax rates), we also provide evidence when using all *positive* tax rate changes and allowing for the structural assumption proposed by Blanchard and Perotti (2002) to identify fiscal shocks.[24] Figure 10, panel C and D, displays the results. Panel C confirms the findings of Panel A to the effect that the intertemporal profile of the multiplier is contractionary, yet strongly biased towards zero. The impact and estimated maximum contractionary effects are reduced by almost 7 times. While always contractionary, this effect falls from \$1.02 ($t = -2.36$) to \$0.14 ($t = -1.66$). The estimated maximum contractionary effect falls from \$2.76 ($t = -2.13$) to \$0.36 ($t = -1.71$).

[23] Remember that our sample includes 49 tax rate changes, 21 of which are associated with fiscal consolidation episodes.

[24] Recall that our sample includes 36 tax rate increases, 21 of which are associated with fiscal consolidation episodes.

We now focus on a revenue-based measure, aiming at assessing policymaker's discretionary changes (i.e., the VAT cyclically-adjusted revenue), as well as fiscal consolidation episodes to identify exogenous fiscal shocks (top-right cell in Table 1). In other words, we adopt a commonly-used proxy for the tax policy instrument as well as a "natural experiment" approach in order to identify changes in fiscal policy not driven by output fluctuations. Figure 10, panels E and F, displays those results.[25] Panel E shows that the general profile of this multiplier is completely different from that obtained with tax rates. In particular, the tax multiplier seems to be neutral with respect to GDP, as in Perotti (2004), Favero and Giavazzi (2010), Caldara (2011), and Ilzetzki (2011). Interestingly, and in line with our previous results, Panel F seems to validate the use of fiscal consolidation episodes 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 0.89. These findings support not only concerns regarding the use of cyclically-adjusted revenue measures to asses changes in tax policy instruments, but also the use of true tax instruments, such as the tax rates.

Lastly, we focus on VAT cyclically-adjusted revenues and the Blanchard-Perotti identification strategy (bottom-right cell in Table 1). As discussed in Section 1, this strategy is by far the most commonly used in the literature. Figure 10, panels G and H, displays our results. Panel G proves that this multiplier is not only non-contractionary, but, in fact, fairly expansionary! A unit shock increase in VAT revenue collection increases output by \$0.18 ($t = 2.19$) on impact, and about \$0.78 ($t = 3.40$) after three quarters. Panel H shows that

[25] Considering (3) we show that when using cyclically-adjusted revenues, $Tax\ multiplier \equiv \frac{\Delta Y}{\Delta R} = \frac{\beta}{(1+\eta\beta)T}$ and $Tax\ multiplier_{SE} = \frac{T}{((1+\eta\beta)T)^2}\beta_{SE}$. What follows is the proof of these results. First, we show that $Tax\ multiplier \equiv \frac{\Delta Y}{\Delta R} = \frac{\beta}{(1+\eta\beta)T}$. $\frac{\Delta Y}{\Delta R} = \frac{\Delta Y/Y}{\Delta R/Y}$. From the regression equation when using cyclically-adjusted revenue changes, we know that $\Delta Y/Y = \beta\Delta ca$; where $\Delta ca \equiv \Delta cyclically\text{-}adjusted$ as in equation (3). Therefore, $\frac{\Delta Y}{\Delta R} = \frac{\beta\Delta ca}{\Delta R/Y}$. Since $R \equiv T \cdot Y$ (i.e., T is the "implicit" VAT rate), then $\Delta R = \Delta T \cdot Y + \Delta Y \cdot T$. Therefore, $\frac{\Delta Y}{\Delta R} = \frac{\beta\Delta ca}{\Delta T + \beta\Delta ca \cdot T}$. From (3), $\Delta T/T = \Delta ca - (1+\eta)(\Delta Y/\Delta Y) = (1-(1-\eta)\beta)\Delta ca$. Therefore, $\frac{\Delta Y}{\Delta R} = \frac{\beta\Delta ca}{(1-(1-\eta)\beta)\Delta ca \cdot T + \beta\Delta ca \cdot T} = \frac{\beta}{(1+\eta\beta)T}$. Second, we show that $Tax\ multiplier_{SE} = \frac{T}{((1+\eta\beta)T)^2}\beta_{SE}$. From $Tax\ multiplier \equiv \frac{\Delta Y}{\Delta R} = \frac{\beta}{(1+\eta\beta)T}$; $Tax\ multiplier = f(x)$, where $f(x) = \frac{x}{(1+\eta x)T}$. Using a Taylor first order approximation, we obtain $Tax\ multiplier = f(\beta) + f'(\beta)(x-\beta) = f(\beta) + f'(\beta)x - f'(\beta)\beta$. Applying the variance to both sides of the equation, we obtain $Var(Tax\ multiplier) = 0 + [f'(\beta)]^2 Var(x) - 0 = [f'(\beta)]^2 Var(x)$. Evaluating $f(x)$ at $x = \beta$, then, $Var(Tax\ multiplier) \approx [f'(\beta)]^2 Var(\beta)$. Applying square roots to both sides of the equation, it follows that $Tax\ multiplier_{SE} = f'(\beta)\beta_{SE}$. Finally, $Tax\ multiplier_{SE} = \frac{T}{((1+\eta\beta)T)^2}\beta_{SE}$.

cyclically-adjusted revenue changes tend to respond positively to output changes in the short-term while negatively after three quarters. We cannot reject the hypothesis that the first and fourth lags are positive (p-value = 0.93) and negative (p-value = 0.98), respectively. These findings support our concerns regarding the validity of econometric structural assumptions as a way to identify fiscal shocks.

To sum up, these findings support our concerns on both the use of cyclically-adjusted revenue measures to asses changes in tax policy instruments as well as the exclusive use of econometric structural assumptions to identify exogenous fiscal shocks. On the *identification* front, our empirical evidence favors the use of "natural experiments" as a more legitimate manner of assessing fiscal policy changes not resulting from policymakers' response to output fluctuations. Our results also support Perotti (2011), who shows that once narrative information from RR is incorporated into SVAR estimations, tax multipliers are more contractionary than standard SVAR estimates. In our sample, fiscal consolidation episodes are associated, both for tax rates and cyclically-adjusted series, with more contractionary tax multipliers. Compare Panels A and C for tax rates in Figure 10, and Panels E and G for cyclically-adjusted revenues.

In terms of the (much less explored) issue of *measurement of the tax policy instrument*, we conclude that there is really no good substitute for data on tax rates themselves. Our main point – the importance of using tax rates to measure tax policy instrument – is, however, independent of the particular identification strategy: it applies equally to the analysis of fiscal shocks identified through the narratives à la RR as well as when using SVAR. Tax rate increases are always contractionary (Figure 10, panels A and C). However, tax multipliers based on cyclically-adjusted revenues are, at worst, neutral (Figure 10, panel E) or even expansionary (Figure 10, panel G). Our findings also do not support the claim that tax multipliers are neutral (Perotti, 2004; Favero and Giavazzi, 2010; Caldara, 2011; Ilzetzki, 2011) when using the appropriate tax policy instrument.

6 Evidence from Romer-Romer and Barro-Redlick data

Using BR and RR income tax data for the United States, this section provides complementary evidence regarding the importance of appropriately measuring the tax policy instrument. BR use average annual marginal income tax rates from individual United States federal and state income taxes as well as those from social security payroll. On the other hand, as discussed later, BR use a revenue-based measure which in fact suffers from forward-looking endogeneity.

6.1 Barro-Redlick tax rate

We first use the BR measure of United States average annual marginal income tax rates from individual federal and state income taxes as well as social security payroll taxes (AMITR). Figure 11 shows the change in this tax measure. We categorize these changes depending on whether they occurred during episodes of fiscal consolidation or not. Overall, we have 24 changes in AMITR; 15 being tax rate increases and 9 tax rate decreases. Excluding zeros, the median tax rate change is 0.1 percent. The highest tax rate reduction is -2.3 percent in 1987, whereas the maximum tax rate increase is 2 percent in 1981. All fiscal consolidation tax rate changes (i.e., tax rate changes that occurred during periods of fiscal consolidations) are positive; adding up to a total of 9. The median fiscal consolidation tax rate change is 0.7 percent. About 60 percent of tax rate increases (9 out of 15) are associated with episodes of fiscal consolidations. Most of these fiscal consolidation tax rate changes occurred during the early 80s and mid 90s.

In Figure 12, Panel A shows the cumulative response of GDP to a fiscal consolidation BR tax rate shock.[26] While the effect on impact is slightly positive, in the medium and long term, the effect of an individual income tax rate increase is to reduce output. BR also find a sizable negative effect of tax rate increases on economic activity. As opposed to the immediate contractionary reaction observed for increases in VAT rates (Figure 5), this output delayed contractionary response to increases in individual income tax rates is consistent with our conjecture of agents asymmetrically learning and responding to different tax type changes. As suggested by BR, information regarding changes in tax rates, tax brackets, and deductions in the AMITR are gradually learned by households throughout the year. Therefore, it is no surprise that output reaction is initially muted, becoming stronger over time as agents learn about it. On the other hand, changes in VAT rates are, arguably, promptly internalized by households, since consumption is an activity performed on a more continuos and frequent manner.

[26] For comparability, we include – as in RR – 12 lags in the SVAR estimations.

Figure 12, Panel B, shows that the effect of output changes on income tax rates is insignificant. The null hypothesis test p-value that output does not granger-cause the tax series is 0.36. As in Sections 4 and 5, this finding strongly supports our conjecture that tax rate changes identified using fiscal consolidation episodes are, indeed, unrelated to past output fluctuations.

6.2 Cyclically-adjusted revenues

We now focus our attention on changes in the cyclically-adjusted revenue measure.[27] Figure 13 displays both the BR tax rate and cyclically-adjusted revenue changes. The latter proves to be a poor approximation to changes in tax policy (i.e., tax rates). As in the case of the VAT, changes in cyclically-adjusted revenues tend to strongly over-count discretionary shifts in tax policy (by a factor of about 3.5). While the data comprises 24 changes in the individual income tax rates, the cyclically-adjusted revenue measure identifies more than a hundred. Thus, the correlation between changes in cyclically-adjusted revenues and tax rate is rather low, namely 0.08. Even when focusing on observations where tax rate changes are not zero, the correlation between changes in cyclically-adjusted revenues and tax rate is rather low, 0.17. Moreover, we cannot reject the null hypothesis of independence at a 40 percent level of confidence. If we further restrict our comparison to fiscal consolidation episodes, such correlation is 0.1, and we cannot reject the null hypothesis of independence at a 65 percent level of confidence.

In line with our findings for the VAT (Figure 10, Panel C), the output response to a cyclically-adjusted revenue shock is strongly biased towards zero (Figure 12, Panel C). This neutrality result contradicts the one obtained using BR tax rates. Also in line with our previous findings, the null hypothesis test p-value that output does not granger-cause the tax series is 0.89 (Figure 12, Panel D).

6.3 Romer-Romer tax measure

Last, we turn to the measure used by RR. As opposed to the actual change in revenues occurred after the implementation of a tax change, they measure tax policy using the planned change in tax receipts, assessed during the prior legislative process. One would think, then, that

[27] This measure is obtained from RR's paper.

this measure could avoid the contemporaneous feedback from GDP (BR, 2011). However, those planned changes in tax receipts are based on forecasts, including the impact of the proposed legislative change on economic activity.[28] The difference between their measure of planned change in tax receipts and the ex-post one lies in the measurement errors in income forecasts (Perotti, 2011). In other words, their measure of tax policy is also revenue-based, but on predicted rather than current revenues. It thus suffers from forward-looking-reverse causality from future economic activity. One must bear in mind that RR find that deficit-driven tax increases have a weak positive effect on GDP. While they caution about the estimates' precision, they suggest that this difference might reflect that increasing taxes for fiscal consolidation purposes may be less costly than other tax increases.

Figure 14 presents RR measure together with BR tax rate. While we reject the null hypothesis of independence between the BR tax rate and RR changes at the one percent level of confidence, the correlation is quite moderate, namely 0.35. Out of 15 tax rate increases, RR only capture 8 tax hikes. Similarly, out of 8 tax rate decreases, RR only identify 4 tax cuts. In other words, these two measures are quite related to each other, yet they are far from being in sync. Moreover, supporting our conjecture about RR being a revenue-based tax measure, we can reject the null hypothesis of independence between RR and cyclically-adjusted revenues at the one percent level of confidence (see Figure 15). In contrast, we cannot reject the null hypothesis about independence between the BR tax rate and cyclically-adjusted revenue changes.

Indeed, using RR, we also find that fiscal consolidation tax changes do not have an effect on economic activity (Figure 12, Panel E).[29] This neutrality result contradicts that obtained using BR tax rates, yet is more similar to the one using cyclically-adjusted revenues (Figure 12, Panel E).

To sum up, the evidence resulting from tax rates and revenue-based measures for the United States also supports our claim about the importance of using a tax policy instrument under the direct control of the policymaker, as opposed to a revenue-based performance inherently contaminated by tax base fluctuations and measurement errors. In line with our

[28] They gather changes in fiscal receipts associated to legislated tax changes from different sources, including annual Economic Reports of the President, which is written by the Council of Economic Advisers.

[29] We also find that output changes do not trigger changes in the RR tax policy measure (Figure 12, panel F). The null hypothesis test p-value that output does not granger cause the tax series is 0.56.

VAT findings, we observe that tax rates increases are contractionary. Our evidence suggests that RR's weak positive/neutral effect of fiscal consolidation tax increases does not seem to support their claim that tax increases for fiscal consolidation purposes may be less costly than others. This seems to be the result of not appropriately measuring the tax policy instrument. Alternatively, using BR tax rates, we find that fiscal consolidation tax rate increases reduce output.

7 Conclusions

Our paper contributes to the empirical literature on tax multipliers. On the one hand, we revisit the discussion regarding 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 RR's in that the narrative approach seems to be a more convincing strategy than SVAR to truly identify exogenous fiscal shocks. We also show that once narrative information is incorporated into SVAR estimations, tax multipliers are more contractionary than standard SVAR estimates (Perotti, 2011).

On the other hand, we explore the less discussed implications about the measurement of the tax policy (i.e., changes in tax policy variable under the direct control of the policymaker). For this purpose, we built a novel value-added tax rate dataset and the associated cyclically-adjusted revenue measure at the quarterly frequency for 14 industrial countries for the period 1980-2009. We also complement our analysis using BR and RR income tax data for the United States. Our results strongly support the use of tax rates as a true measure of tax policy instrument as opposed to widely-used revenue-based measures, such as cyclically-adjusted revenues. The latter tend to severely over-count alleged discretionary changes in tax policy and is a poor proxy for changes in tax rates. Our main point – the importance of using tax rates to measure tax policy instrument – is, however, independent of the particular identification strategy: it applies identically to the analysis of fiscal shocks identified through the narratives à la RR as well as when using SVAR. Tax rate increases are always contractionary. However, tax multipliers based on cyclically-adjusted revenues are, at worst, neutral or even expansionary!

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8 Appendix of data

8.1 Gross domestic product and government expenditure

We constructed quarterly seasonally-adjusted real measures of gross domestic product and government expenditure.

8.2 Personal income tax rate

Maximum marginal personal income tax rate.

8.3 Corporate income tax rate

Maximum corporate income tax rate.

8.4 VAT rate

Standard VAT rate.

8.5 VAT revenue

VAT Revenue.

8.6 Cyclically-adjusted VAT revenue measure

As described by equation (3), cyclically-adjusted revenue change is calculated as follows:

$$\Delta \textit{cyclically-adjusted}_t = r_t - r_{t-1} - \eta \left(y_t - y_{t-1} \right), \tag{8}$$

where r is the log of VAT tax revenues, y the log of output, and η the historical average tax revenue elasticity of output. We use values of η for indirect taxation calculated by Paul van den Noord (2000).[30] The average elasticity for the countries included in the sample is 0.9. What follows is the elasticity estimated by these authors for each country: Australia (0.4), Austria (0.5), Belgium (0.9), Canada (0.7), Denmark (1.6), Finland (0.9), France (0.7), Germany (1), Greece (0.8), Ireland (0.5), Italy (1.3), Japan (0.5), Netherlands (0.7), New Zealand (1.2), Norway (1.6), Portugal (0.6), Spain (1.2), Sweden (0.9), and United Kingdom (1.1).

8.7 Episodes of fiscal consolidation

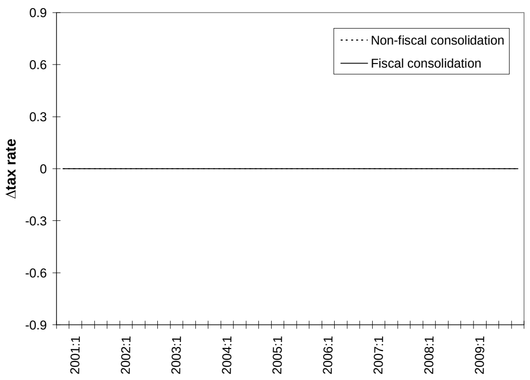
Episodes of fiscal consolidation are taken from "Will it hurt? Macroeconomic effects of fiscal consolidation," World Economic Outlook, IMF, 2010. We use their action-based approach episodes. They calculated these episodes for the period 1980-2009.[31]

[30] Similar results are obtained if we assumed that $\eta = 1$. Results are not shown for brevity.

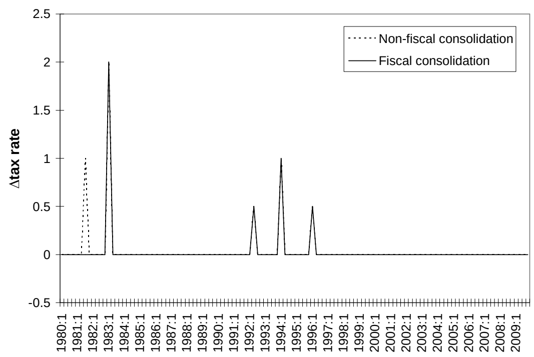
[31] The value-added tax rate reductions of France in 2000 (from 20.6 to 19.6) and Ireland in 1984 (from 35 to 23) were not considered a fiscal consolidation tax rate change because, based on Recent Economic Development and other staff reports from the IMF, those changes were driven by long-run economic growth concerns. An identical treatment was applied when Romer-Romer and Barro-Redlick tax series are associated with tax reductions in 1981 and 1988, respectively.

Figure 1. Tax rate changes. Fiscal consolidation vs. non-fiscal consolidation.

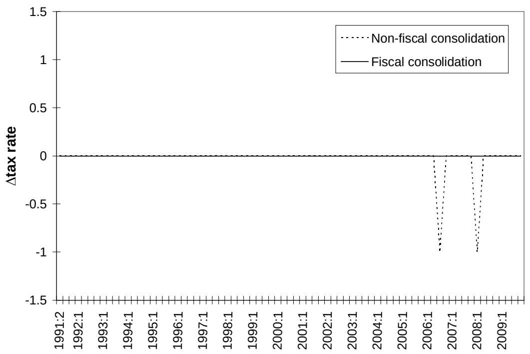
Panel A. Australia



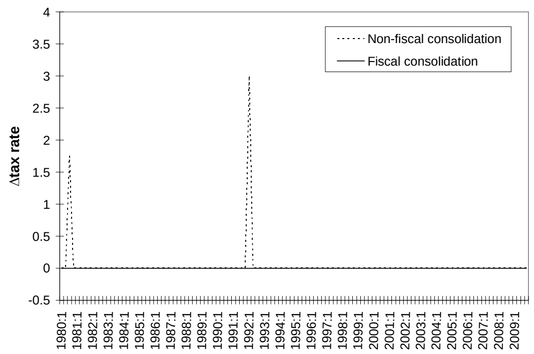
Panel B. Belgium



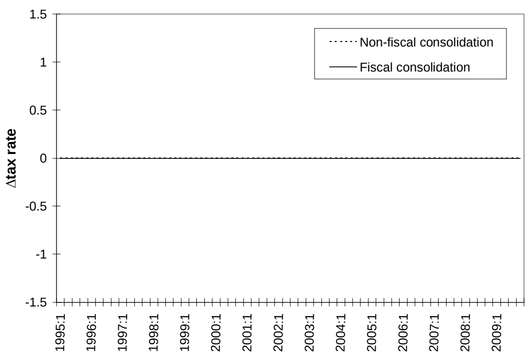
Panel C. Canada



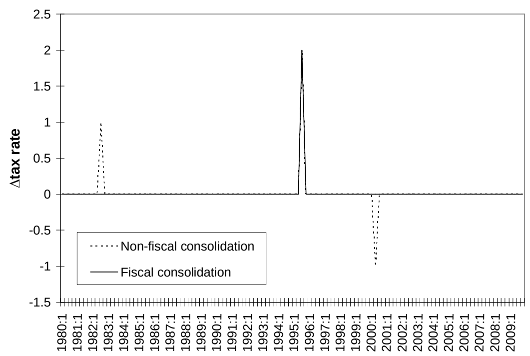
Panel D. Denmark



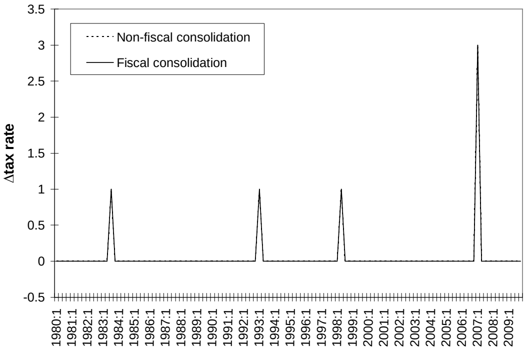
Panel E. Finland



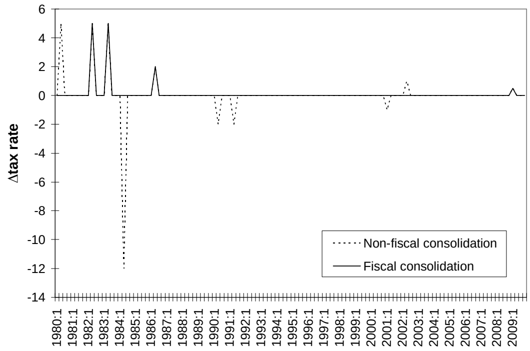
Panel F. France



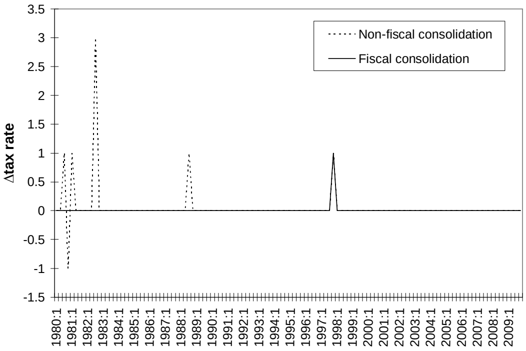
Panel G. Germany



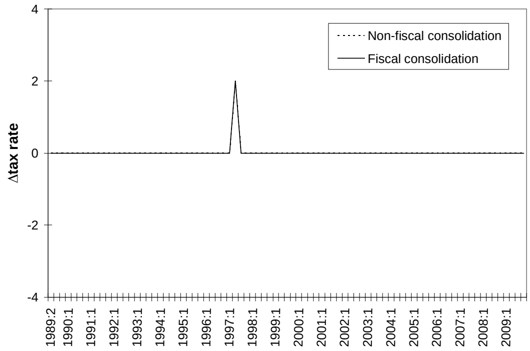
Panel H. Ireland



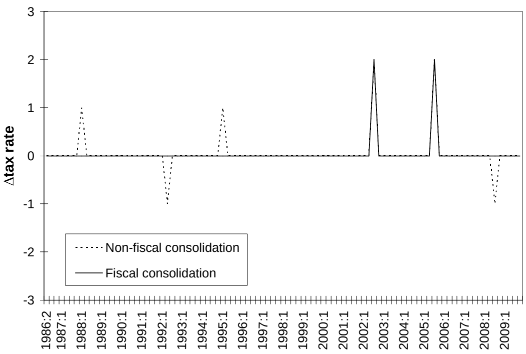
Panel I. Italy



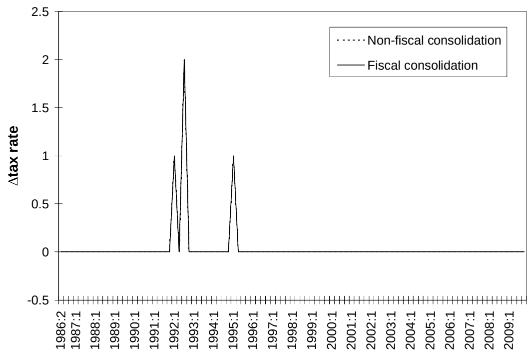
Panel J. Japan



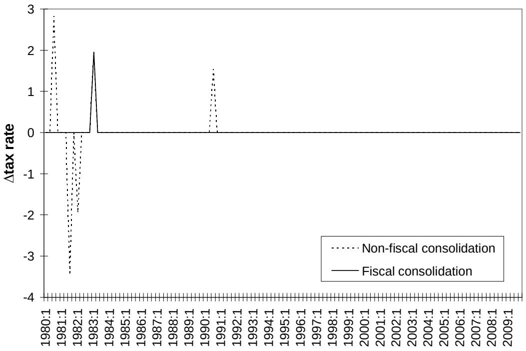
Panel K. Portugal



Panel L. Spain



Panel M. Sweden



Panel N. United Kingdom

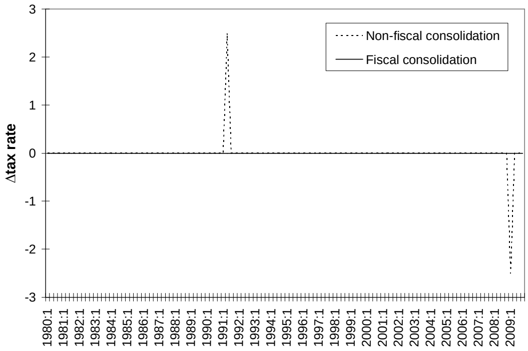
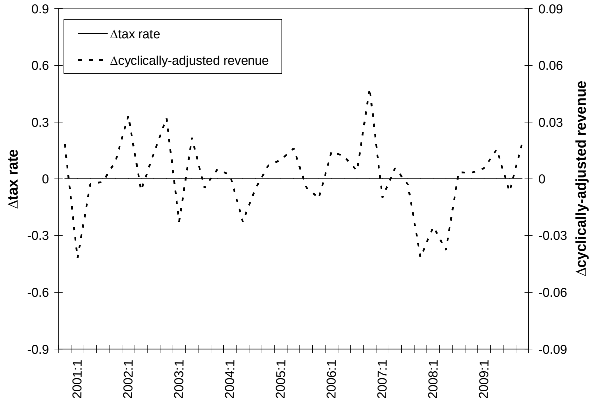
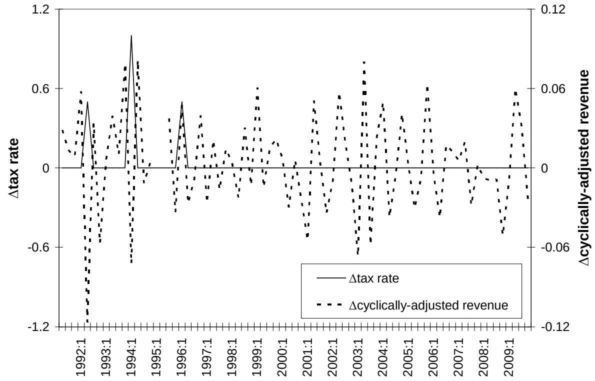


Figure 2. Tax rate vs. cyclically-adjusted revenue changes.

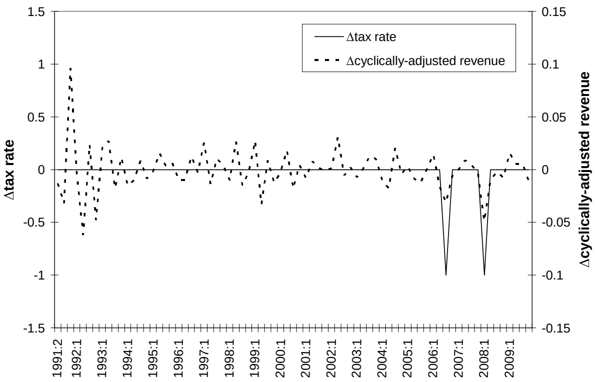
Panel A. Australia



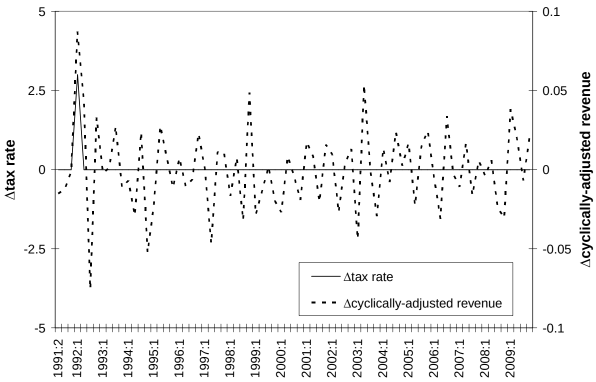
Panel B. Belgium



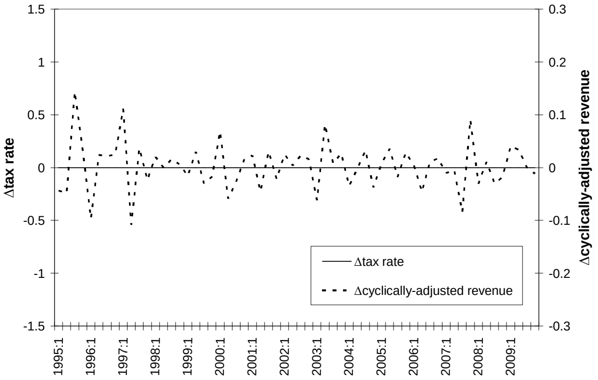
Panel C. Canada



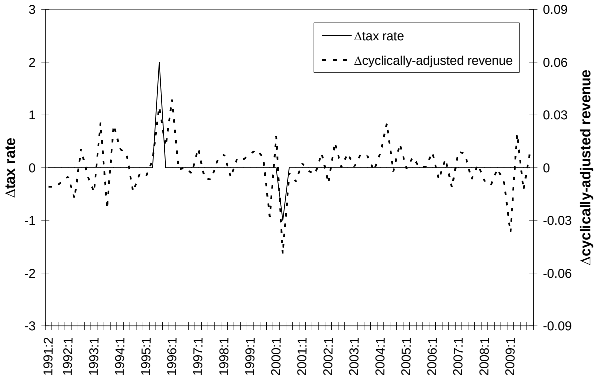
Panel D. Denmark



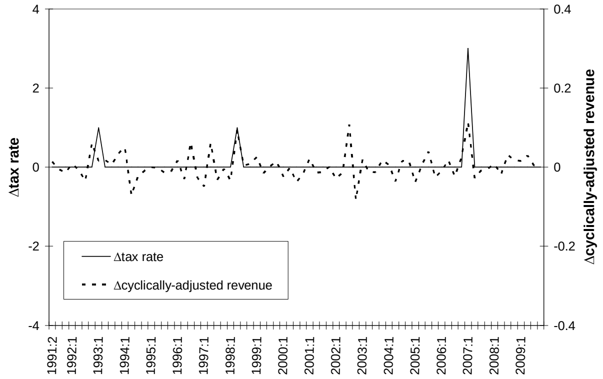
Panel E. Finland



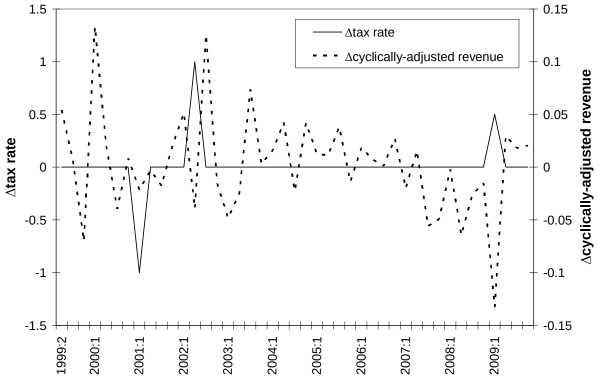
Panel F. France



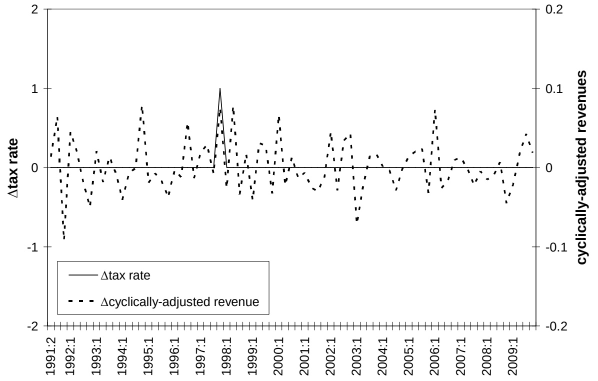
Panel G. Germany



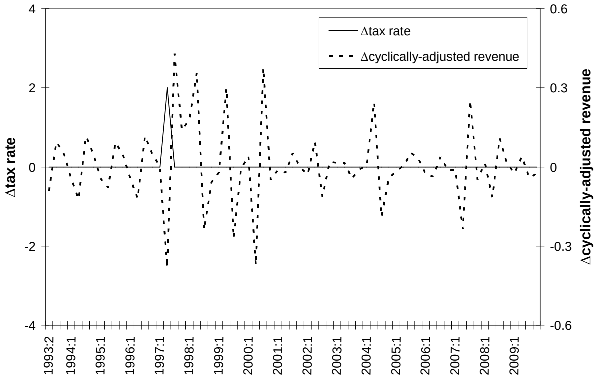
Panel H. Ireland



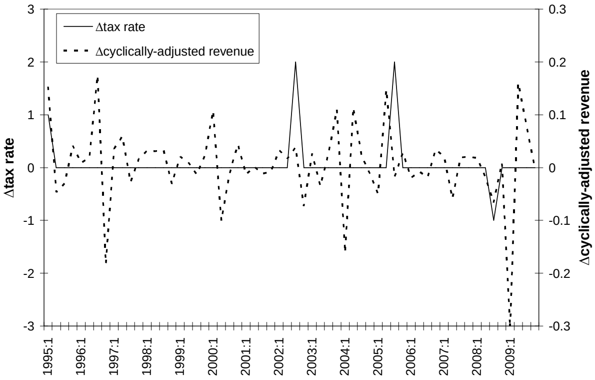
Panel I. Italy



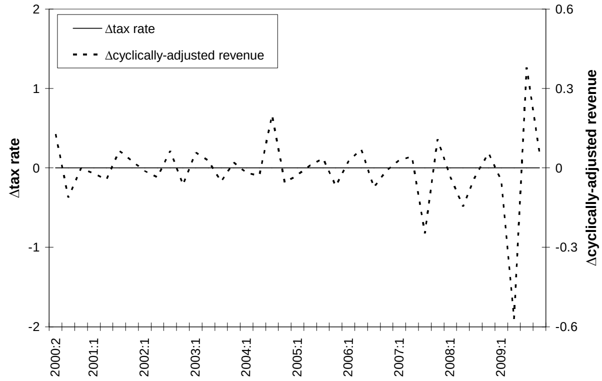
Panel J. Japan



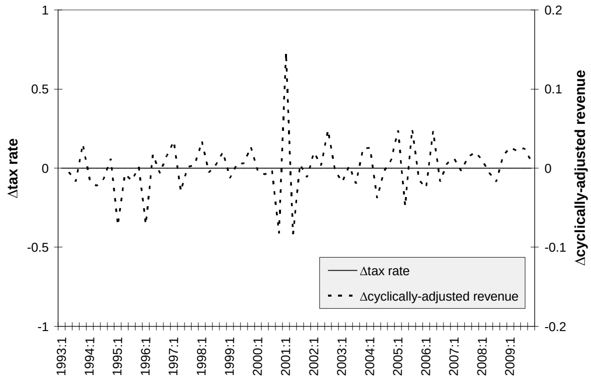
Panel K. Portugal



Panel L. Spain



Panel M. Sweden



Panel N. United Kingdom

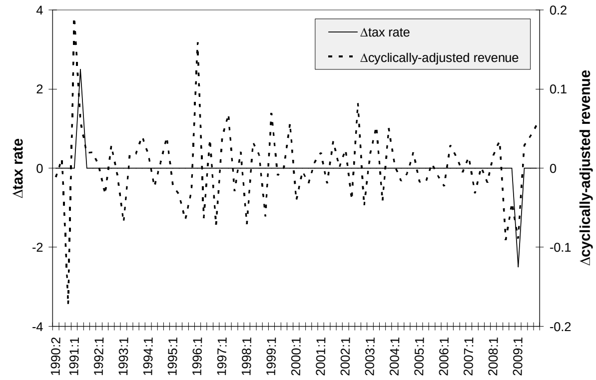


Figure 3. Cumulative tax multiplier. Fiscal consolidation tax rate shock. Single equation, no controls.

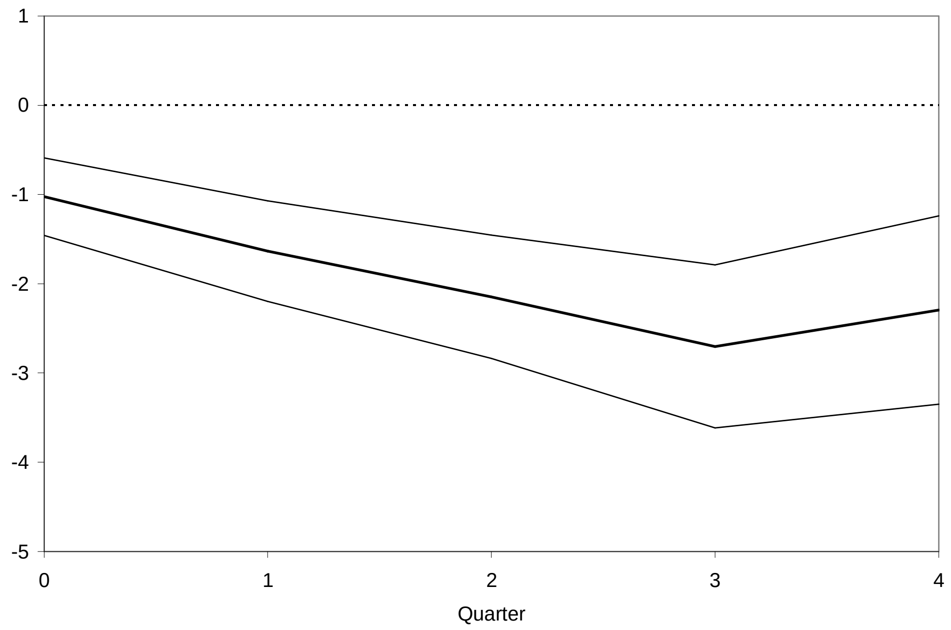


Figure 4. Cumulative tax multiplier. Fiscal consolidation tax rate shock. Single equation: controls vs. no controls.

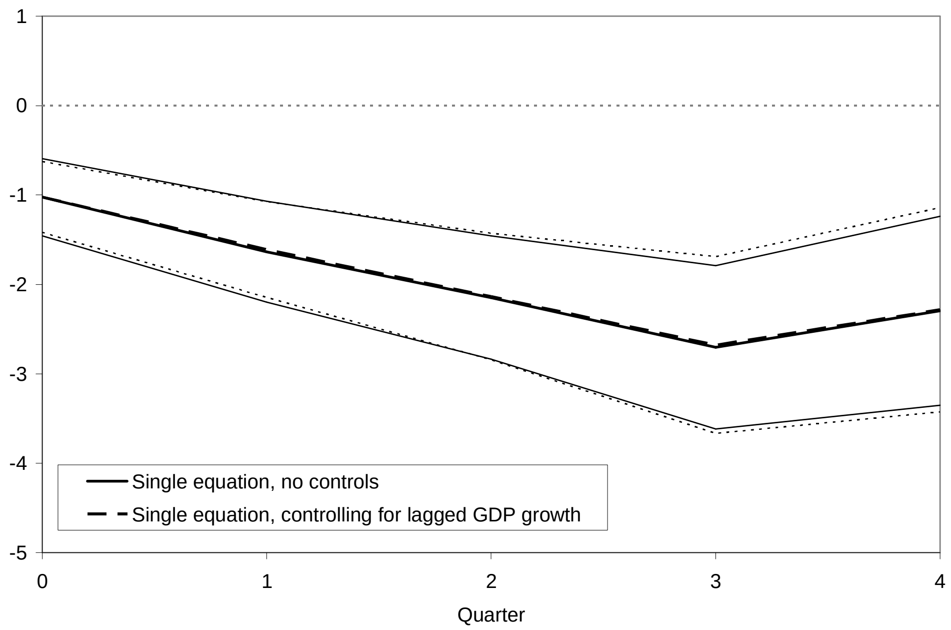
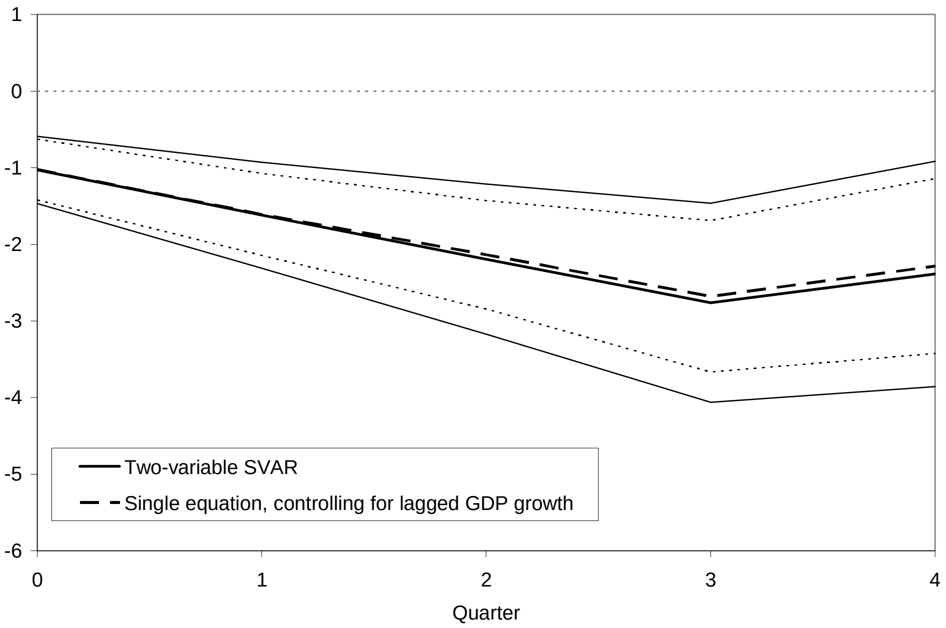


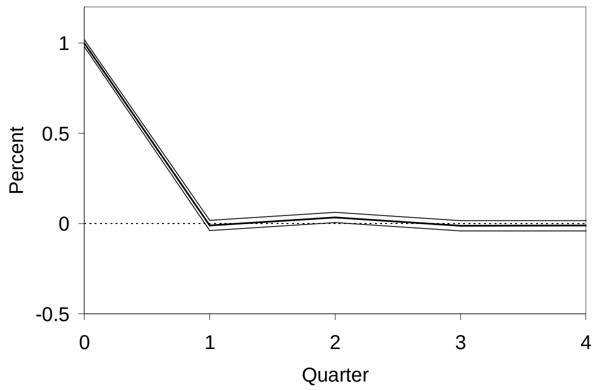
Figure 5. Cumulative tax multiplier. Fiscal consolidation tax rate shock. Single equation (with controls) vs. two-variable SVAR.



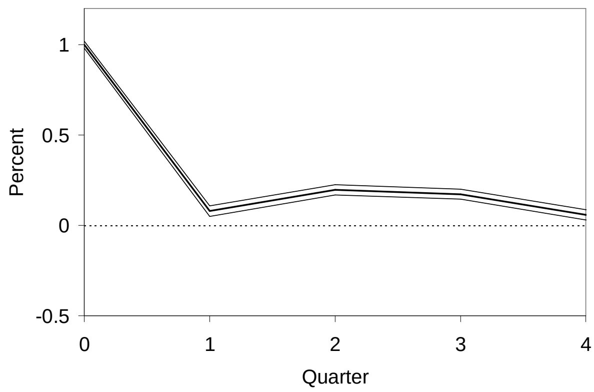
Note: The two variables in SVAR are fiscal consolidation tax rate change and GDP growth.

Figure 6. Results of a two-variable SVAR. One percent impulse response functions. Fiscal consolidation tax rate changes and GDP growth.

Panel A. Response of tax to tax



Panel B. Response of GDP to GDP



Panel C. Response of tax to GDP

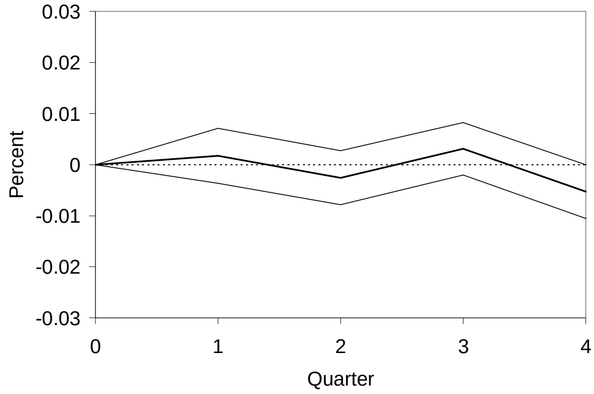
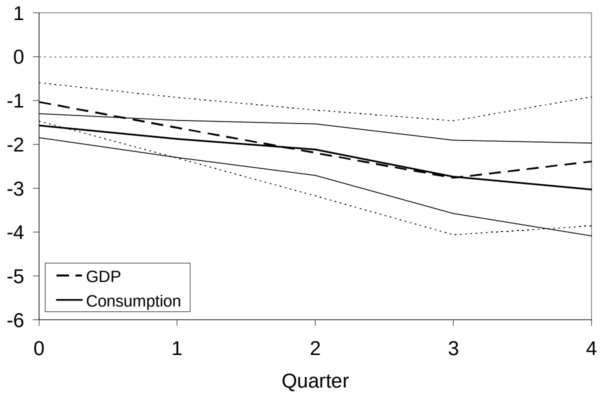
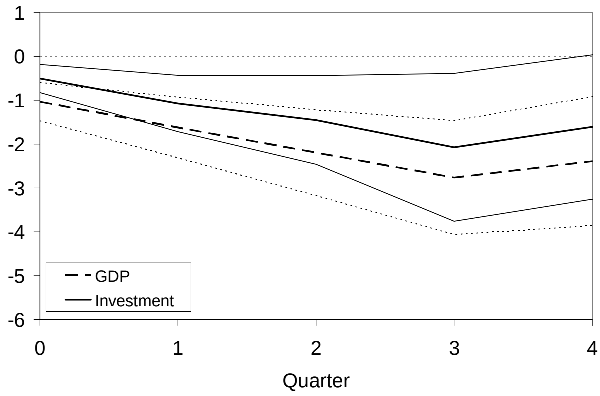


Figure 7. Cumulative tax multiplier for consumption, investment, exports, and imports. Fiscal consolidation tax rate shock. Two-variable SVAR.

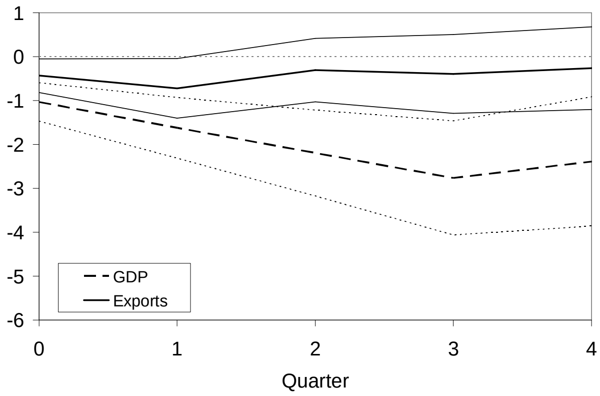
Panel A. Cumulative tax multiplier for consumption



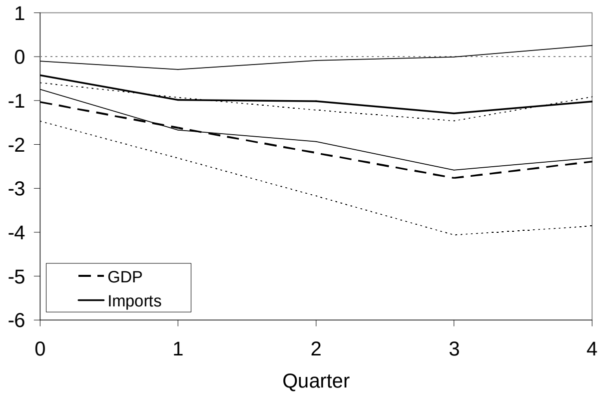
Panel B. Cumulative tax multiplier for investment



Panel C. Cumulative tax multiplier for exports

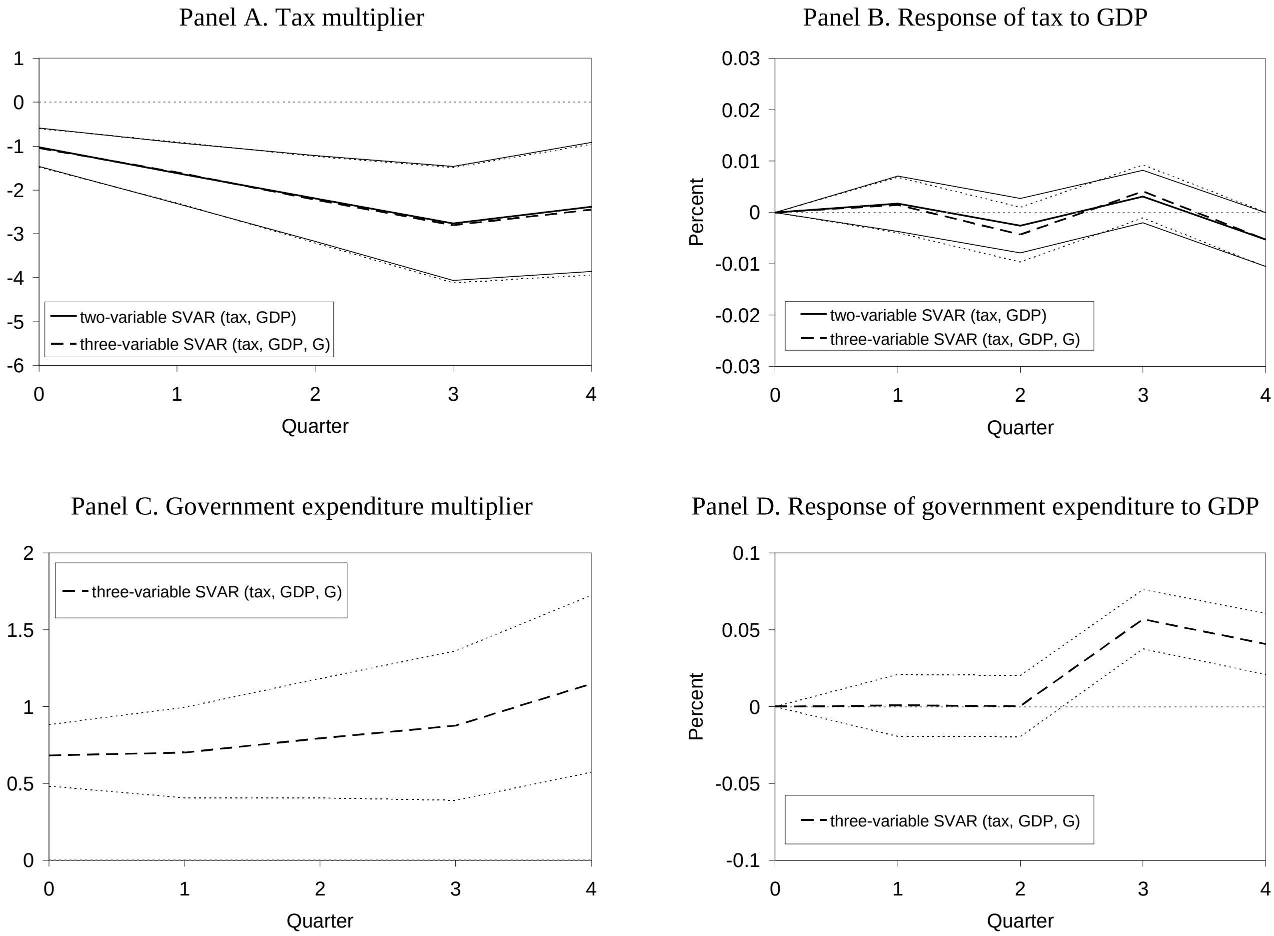


Panel B. Cumulative tax multiplier for imports



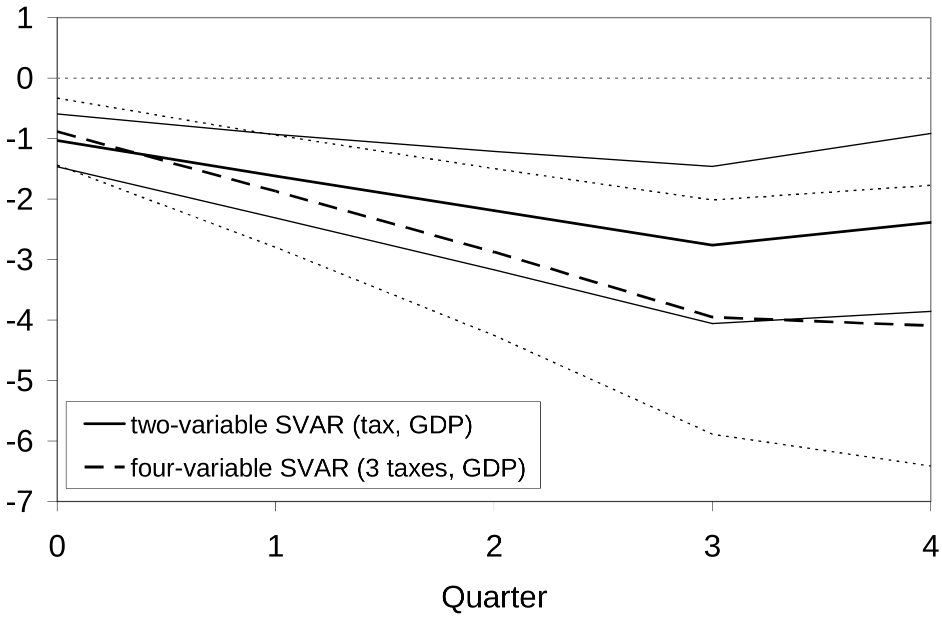
Note: The two variables in SVAR are fiscal consolidation tax rate changes and the GDP component growth.

Figure 8. Cumulative tax and government expenditure multipliers, and one percent impulse response functions. Fiscal consolidation tax rate and government expenditure changes. Two-variable vs. three-variable SVAR.



Note: The two variables in two-variable SVAR are fiscal consolidation tax rate changes and GDP growth. The three variables in three-variable SVAR are fiscal consolidation tax rate changes, fiscal consolidation government expenditure growth, and GDP growth. Panels A and C show the cumulative tax and government expenditure multipliers. Panels B and D show the impulse response functions of tax rates and government expenditure to a one percent GDP shock.

Figure 9. Cumulative VAT tax multiplier. Fiscal consolidation tax rate shock. Two-variable vs. four-variable SVAR.

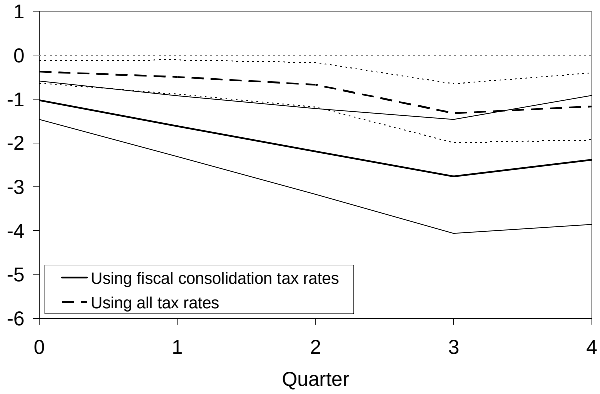


Note: The two variables in the two-variable SVAR are fiscal consolidation VAT tax rate changes and GDP growth. The four-variable SVAR include fiscal consolidation tax rates changes (VAT, top individual income, top corporate income) and GDP growth.

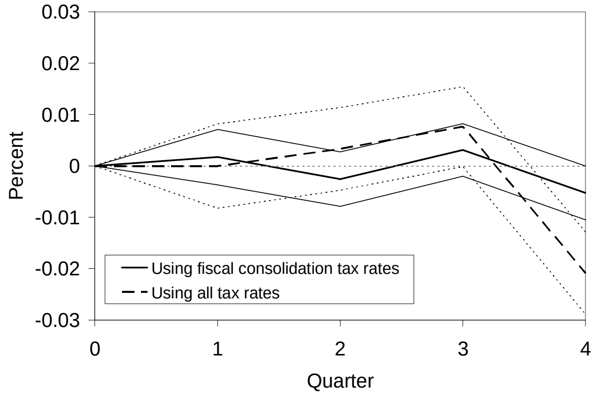
Figure 10. Cumulative tax multiplier and one percent impulse response functions. Alternative empirical strategies. Two-variable SVAR.

Using all tax rate changes

Panel A. Cumulative tax multiplier

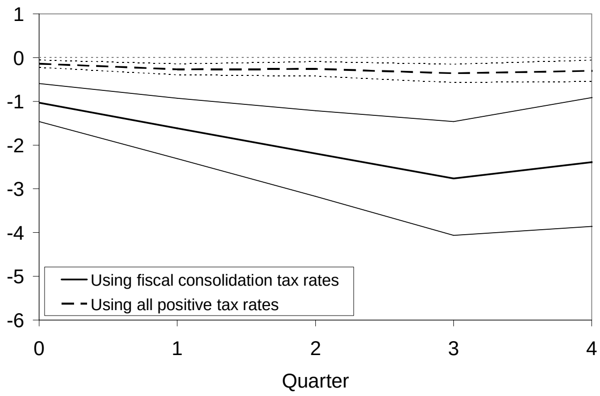


Panel B. Response of tax to GDP

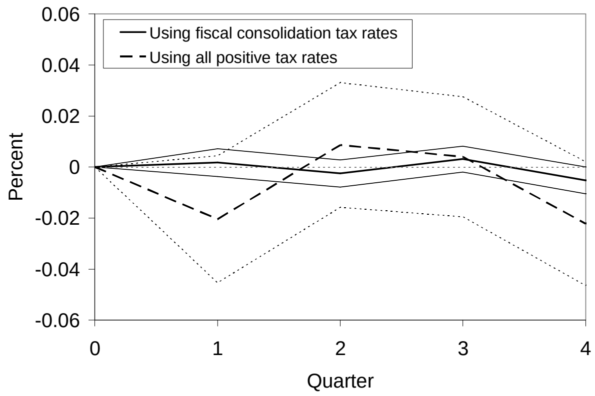


Using all positive tax rate changes

Panel C. Cumulative tax multiplier

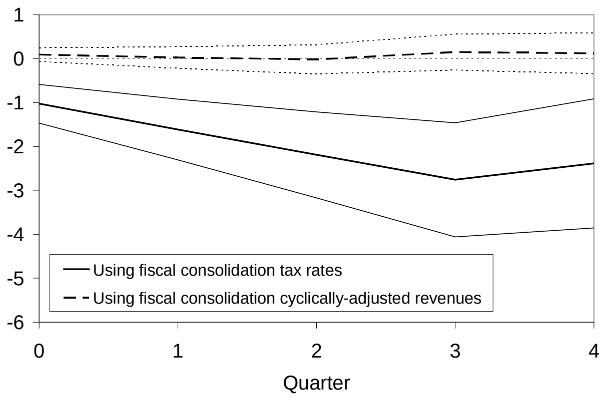


Panel D. Response of tax to GDP

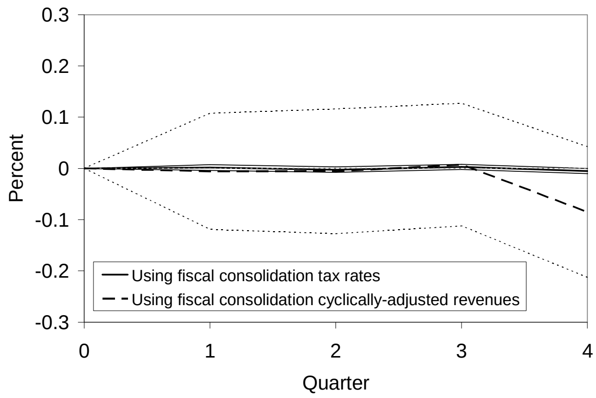


Using fiscal consolidation cyclically-adjusted revenue changes

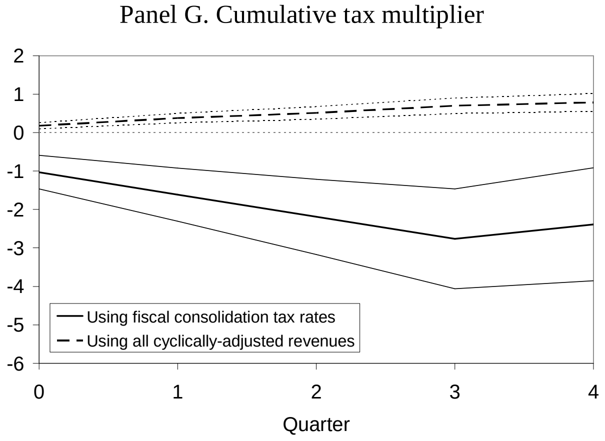
Panel E. Cumulative tax multiplier

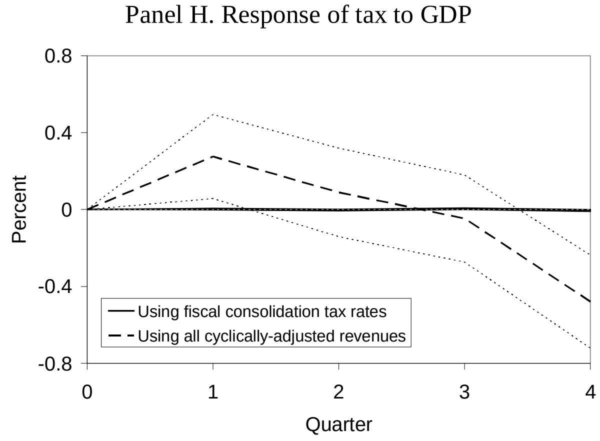


Panel F. Response of tax to GDP



Using all cyclically-adjusted revenue changes





Panels A, C, and E show the cumulative estimated impact of a tax increase of one-unit on GDP using alternative tax measures. Panels B, D, and F show the estimated impact of a GDP increase of one percent on alternative tax measures.

Figure 11. Barro-Redlick tax rate changes. Fiscal consolidation vs. non-fiscal consolidation.

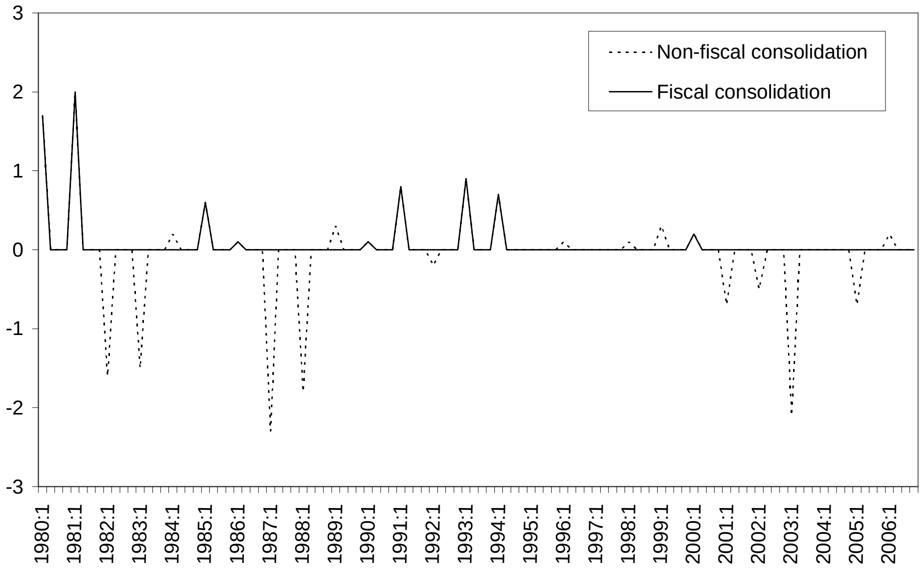
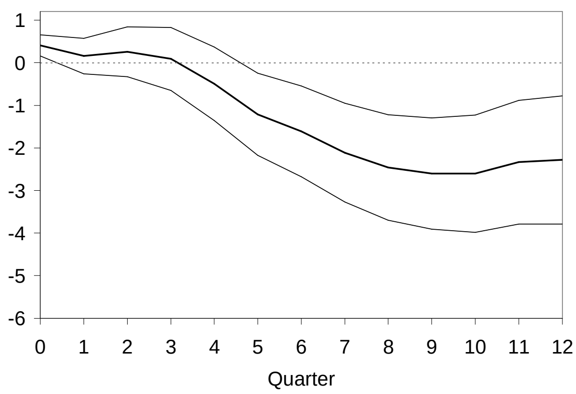


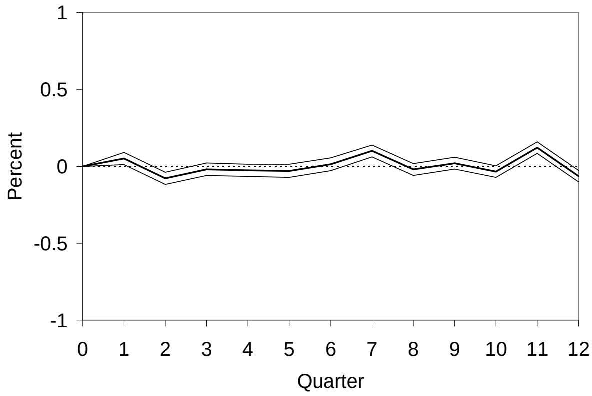
Figure 12. One percent impulse response functions. Fiscal consolidation tax shock. Alternative tax variables. United States. Two-variable SVAR.

Using Barro-Redlick tax rate changes

Panel A. Cumulative response of GDP to tax

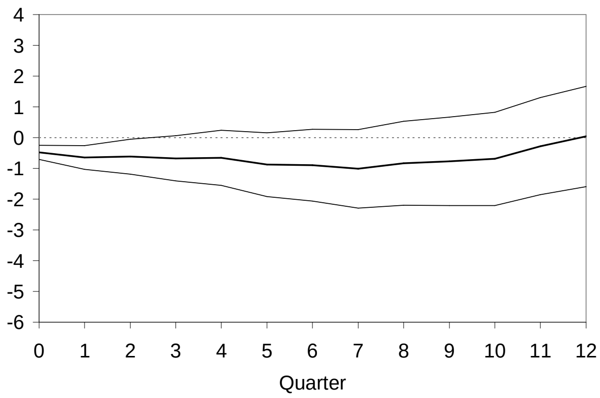


Panel B. Response of tax to GDP

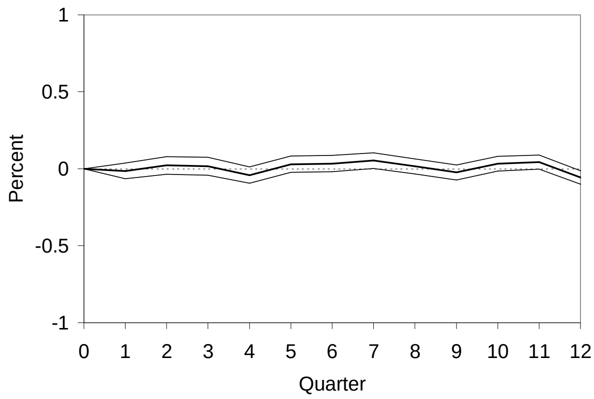


Using cyclically-adjusted revenue changes

Panel C. Cumulative response of GDP to tax

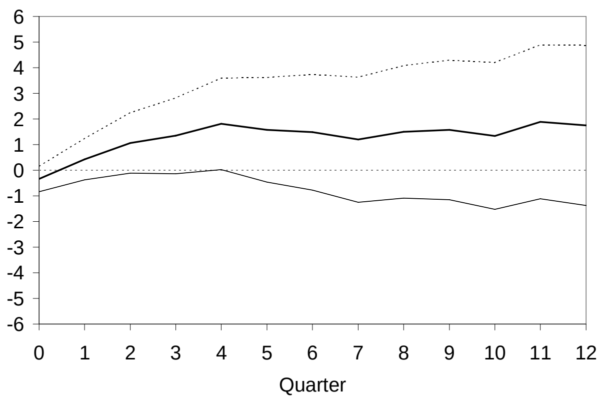


Panel D. Response of tax to GDP



Using Romer-Romer revenue changes

Panel E. Cumulative response of GDP to tax



Panel F. Response of tax to GDP

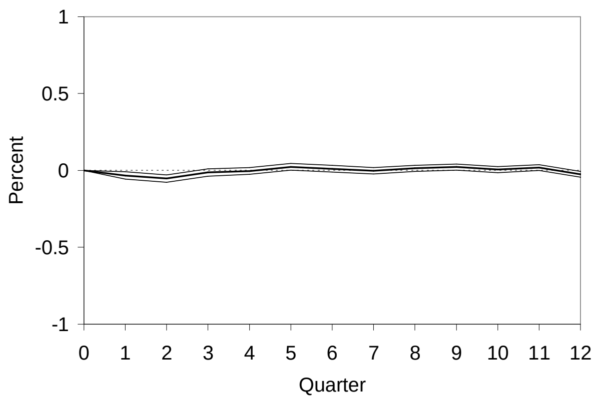


Figure 13. Barro-Redlick tax rate vs. cyclically-adjusted revenue changes.

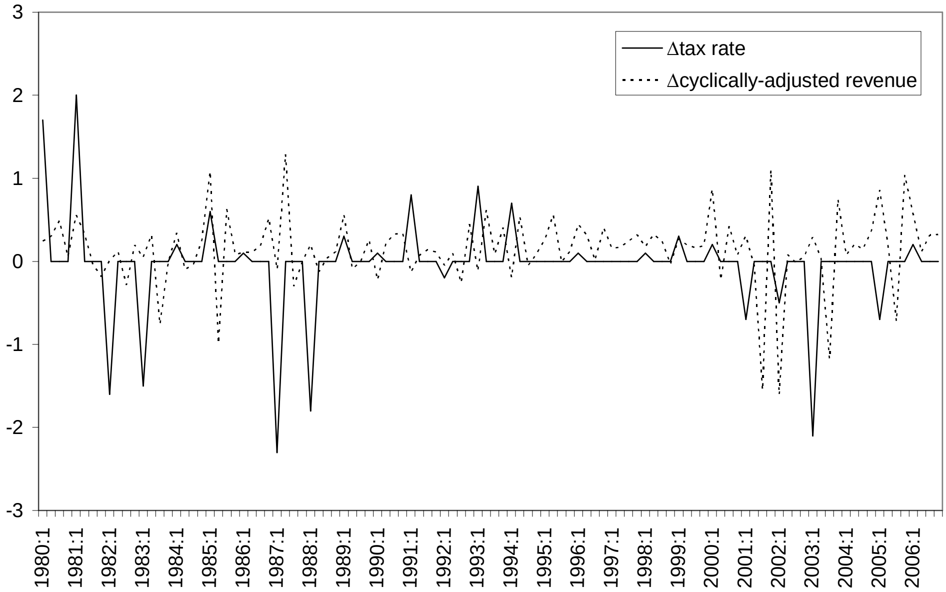


Figure 14. Barro-Redlick tax rate vs. Romer-Romer changes.

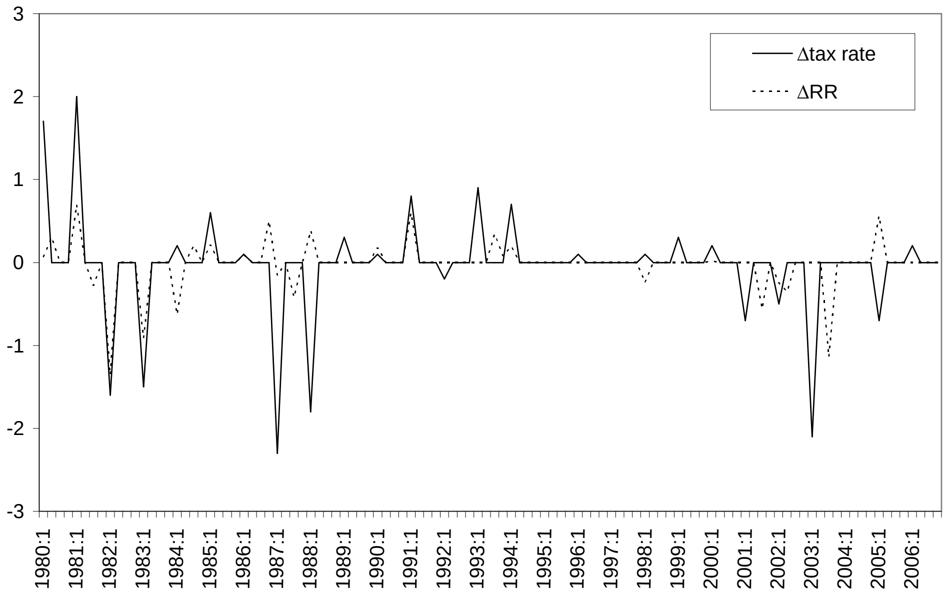


Figure 15. Cyclically-adjusted revenue vs. Romer-Romer changes.

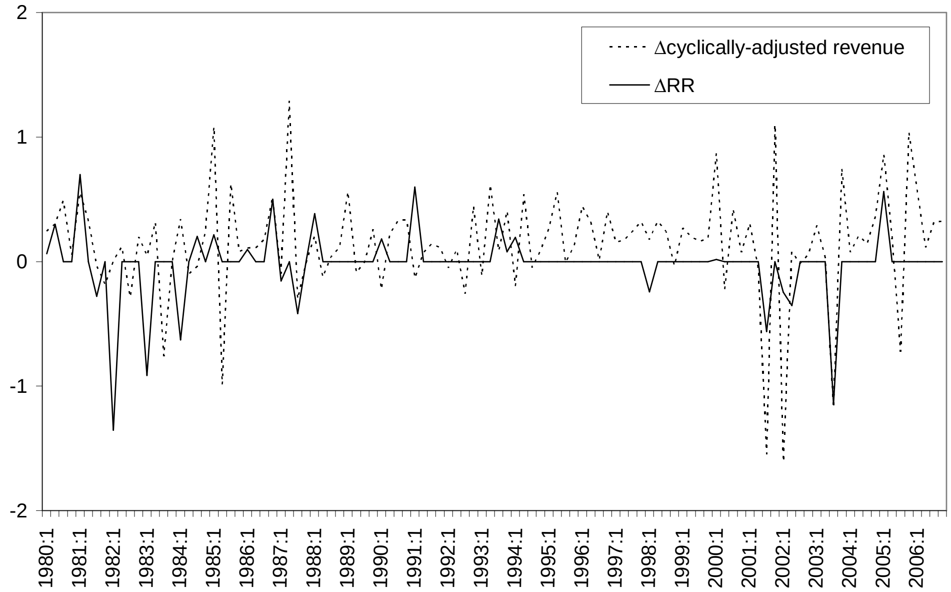


Table 1. Identification of exogenous fiscal shocks vs. measurement of tax policy

<table>
<tr><td colspan="2" rowspan="2"></td><td colspan="2">Measurement of tax policy
(Finding a tax policy variable under the direct control of the policymaker)</td></tr>
<tr><td>Tax rate</td><td>Cyclically-adjusted revenue</td></tr>
<tr><td rowspan="2">Identification of exogenous fiscal shocks
(Fiscal policy changes that are not the result of policymakers responding to output fluctuations)</td><td>"Natural experiment"
(à la Romer and Romer, 2010)</td><td>• No endogeneity.
• No measurement error.</td><td>• No endogeneity.
• Possible measurement error, especially due to changes in non-policy factors.</td></tr>
<tr><td>Structural VAR
(à la Blanchard and Perotti, 2002)</td><td>• Endogeneity problems if tax changes are forward-looking/anticipated.
• No measurement error.</td><td>• Endogeneity problems if tax changes are forward-looking/anticipated.
• Possible measurement error, especially due changes in to non-policy factors.</td></tr>
</table>