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Macroeconomic Effects of Tax Rate and Base Changes: Evidence from Fiscal Consolidations

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Abstract

This paper examines the macroeconomic effects of tax changes during fiscal consolidations. We build a new narrative dataset of tax changes during fiscal consolidation years, containing detailed information on the expected revenue impact, motivation, and announcement and implementation dates of nearly 2,500 tax measures across 10 OECD countries. We analyze the macroeconomic impact of tax changes, distinguishing between tax rate and tax base changes, and further separating between changes in personal income, corporate income, and value added tax. Our results suggest that base broadening during fiscal consolidations leads to smaller output and employment declines compared to rate hikes, even when distinguishing between tax types.

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Contents

1 Introduction

Large fiscal deficits and concerns about debt sustainability in advanced economies in the wake of the Global Financial Crisis have renewed interest in the macroeconomic effects of fiscal policy. Tax-based fiscal consolidations are generally associated with large output declines, but their composition can matter. In particular, policy advice often assumes that measures to broaden the tax base by reducing exemptions and deductions are less harmful to economic activity during austerity. However, differences in the macroeconomic effects of tax rate and base changes have received less attention in the literature.

In this paper we adopt the narrative approach to estimate the macroeconomic effects of tax rate and base changes during fiscal consolidations. Our contribution is two-fold. First, we construct a new dataset of nearly 2,500 exogenous tax changes for 10 OECD countries during fiscal consolidation episodes and estimate multipliers for different tax components. Second, we explicitly distinguish between the macroeconomic impact of tax rate and tax base changes, including for different tax categories. This is important as tax policy changes comprise not only changing rates in specific tax categories, but often setting different thresholds for each rate. Value added taxation (VAT), for example, often includes reduced or zero rates, in addition to the standard rate. Corporate and personal income taxation typically include several tax rates with corresponding thresholds. Clearly, varying the coverage of each rate in the case of the VAT or changing thresholds in the case of income taxes amounts to a change in tax policy that does not involve tax rates.

There is some theoretical support for differences between the economic consequences of tax rate versus base changes. First, base broadening can be less distortionary if it targets agents that are taxed less than the average.[1] This is because base broadening often tends to make taxation across sectors, firms, or activities more homogeneous, contrary to rate increases. This helps re-allocate resources to those projects with the highest pre-tax return, thereby improving economic efficiency and partially offsetting any adverse demand-driven output effects (IMF, 2017; Gale and Samwick, 2017). Second, base broadening may also be perceived as being less salient than rate increases, for example, if agents misperceive their tax liabilities or do not understand their tax schedules (e.g., Chetty et al., 2009; Rees-Jones and Taubinsky, 2016).

[1] Rate changes typically affect all taxpayers, or at least a large number of them. In contrast, base changes are often targeted at certain groups (e.g., specific industries, low-income taxpayers), certain income sources (e.g., capital income), or certain goods (e.g., reduced VAT rates on basic goods).

This can affect taxpayer behavior and mitigate the impact of base changes, in particular when tax systems are complex. Finally, differences in the economic effects may arise from distributional considerations. This is because base broadening often targets higher-income taxpayers, for whom it is easier to smooth temporary income shocks (e.g., Kaplan et al., 2014).

Our key finding is that tax base changes during consolidations appear to have a smaller impact on output and employment than tax rate changes of a similar size. We find a statistically significant one-year cumulative tax rate multiplier of about 1.2, rising to about 1.6 after two years. By contrast, the cumulative tax base multiplier is only 0.3 after one year, and 0.4 after two years, and these estimates are not statistically significant. Our estimates of the tax multiplier are smaller than in the literature because the estimation controls for the effect of base changes when examining the effect of rate changes. These findings are robust to a number of tests, including using alternative controls and estimation methodologies, and using annual instead of quarterly data.

Our paper adds to the existing literature on the economic effect of fiscal consolidations in several other ways. First, our new database extends substantially the information available on the fiscal consolidation episodes identified by Devries et al. (2011) and Alesina et al. (2015, 2017) over the period 1978-2014. We record tax measures in greater detail, distinguishing between tax types (e.g., corporate and personal income taxation, value added tax, excises, social security contributions), and whether they constituted a tax rate or base change. In addition, our new database contains the *ex ante* magnitudes and the details of each tax measure, including the forecast horizon, allowing us to distinguish between the macroeconomic effects of different tax components. Moreover, because we record the exact days of announcement and beginning of implementation of each fiscal consolidation measure, we are able to use quarterly data in our analysis, while the other consolidation datasets are coded at an annual frequency. Finally, we identify exogenous shifts in each tax measure, by recording the motivation of each measure individually. This allows us to distinguish more precisely between measures that were effectively aimed at fiscal consolidation from measures that had a more long-run, or even cyclical focus.

Our work also improves on existing estimates of the macroeconomic impact of tax-based fiscal consolidations by accounting to a greater extent for anticipation effects. By dating individual measures, we are able to distinguish when changes to future tax liabilities became certain (a news shock) from the point in time at which tax liabilities were affected (when disposable income actually changed). Taxpayers are likely to adjust their behavior before tax measures are implemented if they are known in advance. Therefore ignoring these anticipation effects will lead to biased estimates of the impact of tax policy changes.[2] We also control for base changes when estimating the macroeconomic effect of rate changes (and vice versa). This is important because tax policy changes often combine rate changes and base changes, raising questions about the appropriateness of ignoring tax base changes when examining the economic consequences of rate changes (e.g., Kawano and Slemrod, 2016).

Our paper is closely related to studies that identify exogenous tax changes using historical sources to estimate the macroeconomic effects of fiscal policies. We contribute to this literature by further developing the narrative approach by Romer and Romer (2010) for the U.S. for a broad group of countries.[3] Related studies have utilized the narrative approach to examine whether fiscal austerity is expansionary (e.g., Guajardo et al., 2014; Alesina et al., 2015, 2017; Carriere-Swallow et al., 2018). These studies identify the size and direction of exogenous fiscal shocks using cross-country data, distinguishing between tax policy and expenditure measures. However, they do not examine the composition of the tax measures adopted during fiscal consolidations. Our comprehensive analysis and coding of individual tax measures allows us to gain further insight in this dimension.

This work is also related to the wider empirical literature on tax multipliers (e.g., Blanchard and Perotti, 2002; Barro and Redlick, 2011).[4] However, the literature has mostly focused on aggregate tax changes, with relative few papers examining the dynamic effects of specific taxes separately. In this regard, our paper is similar to Riera-Crichton et al. (2016) and Gunter et al. (2017) who use a narrative approach to examine the economic effects of quarterly changes in VAT rates across OECD and developing countries. In reality, there is little reason to expect that the many types of taxes available to governments all have the same impact on

[2]Mertens and Ravn (2012) show that anticipated and unanticipated changes in tax policy can have distinct macroeconomic effects.

[3]Other authors have recently extended it to tax changes in other countries, such as Cloyne (2013) and Cloyne et al. (2018) for the United Kingdom, Uhl (2013) and Gechert et al. (2016) for Germany, Coutinho Pereira and Wemans (2015) for Portugal, Gil et al. (2018) for Spain, Lopes (2015) for Canada, and David and Leigh (2018) for Latin American and Caribbean countries.

[4]Recent contributions to the broader literature on fiscal multipliers include Ramey and Zubairy (2018), Jordà and Taylor (2016), Guajardo et al. (2014), Mertens and Ravn (2014), Nakamura and Steinsson (2014) and Auerbach and Gorodnichenko (2012). Ramey (2011) provides a comprehensive survey of the earlier fiscal multiplier literature.

the economy. In this respect, our paper is similar to Mertens and Ravn (2013), who find that corporate and personal income taxes have different short-term effects on the economy. We contribute by constructing a narrative dataset that covers multiple tax types and rate as well as base changes, allowing us to compare the macroeconomic effects of very specific tax changes.

The rest of the paper proceeds as follows. Section 2 presents the narrative dataset, and section 3 discusses the empirical approach. Section 4 then presents the results for all taxes combined, while section 5 presents the results by tax type. Section 6 concludes.

2 Narrative dataset

2.1 Sample and sources

We start from the narrative dataset constructed by Devries et al. (2011), and later extended by Alesina et al. (2015), which identifies tax and spending changes during years of fiscal consolidation in OECD countries. Since our primary focus is on the effect of tax changes during consolidations, we take these years as our starting point. We then gather the available narrative evidence during these periods to create a comprehensive list of all announced tax policy changes, and code each of the individual tax policy measures involved.

To do this, we rely on information from contemporaneous primary sources including budget documents, reports from the Ministry of Finance and tax authorities, technical reports and notes produced during the legislative procedure, and discussions on tax reforms during parliamentary debates. When necessary, we complement this data with information from secondary sources, such as IMF staff reports, OECD Economic Surveys, Stability and Growth Pact documents, and news articles on tax reform from national newspapers or from the International Bureau of Fiscal Documentation (IBFD). These secondary sources are helpful to fill gaps in the coverage of primary sources, and to create a timeline of the tax measures that were announced throughout the year.

The different political and institutional structures across countries means that we rely on different types of sources depending on the country. For example, we rely heavily on the analysis produced by the Joint Committee on Taxation and the Congressional Budget Office to code tax measures in the U.S., while for Italy we mainly use descriptions of tax policy changes available in reports from the Ministry of Finance, the Economic Bulletin series of the

Bank of Italy, and the annual Stability and Growth Pact documents.

We also check the measures in our dataset against other narrative studies using individual country data, as well as other studies that construct cross-country narrative datasets for specific taxes (e.g., Riera-Crichton et al. (2016) on VAT reforms in OECD countries). We cross-reference our narrative dataset against these studies, in particular regarding the estimated revenue impact and motivation of individual measures, and include a justification in our dataset whenever our judgment differs significantly from theirs. Our narrative dataset covers 10 OECD countries from 1978 to 2014.[5]

2.2 Coding tax measure types

For each individual tax measure, we code the type of tax involved, and whether the measure constituted a rate or base change. To ensure tax types are coded consistently across countries, we follow the tax classification developed by the OECD Revenue Statistics.[6] Using this classification, each tax measure is assigned into a broad tax category, including personal income tax, corporate income tax, social security contributions, payroll tax, property and wealth tax, value added tax, sales tax, and other taxes on goods and services (e.g., excises).

In addition, we classify each measure as a tax rate or a tax base change following the classification proposed in Amaglobeli et al. (2018). A tax rate measure is defined as any change to the main statutory tax rate inclusive of surtax. For example, this would include a change to the standard VAT rate, the top corporate tax rate, or any rate in the personal income tax rate schedule. On the other hand, a tax base measure is any change to the legal definition of the base to which the statutory rate is applied, or any change to provisions that modify the resulting tax liability. This definition encompasses a wide array of tax changes, which depend on the specific tax involved.

For example, in the case of personal income tax, a tax base measure includes any change to personal allowances (including the zero bracket amount, if any), married couple and other specific allowances and deductions, tax credits (e.g., child tax credit), as well as special tax

[5]The countries covered are Australia, Austria, Canada, France, Germany, Italy, Portugal, Spain, the United Kingdom, and the United States. Relative to Devries et al. (2011) and Alesina et al. (2015), we exclude Belgium, Denmark, Finland, Ireland, Japan, the Netherlands and Sweden, since we do not have enough detailed information on all tax changes, especially for earlier years. A partial version of the dataset can be found in Amaglobeli et al. (2018).

[6]We use the 2016 edition of the OECD classification of taxes and interpretative guide, which is available at http://dx.doi.org/10.1787/rev_stats-2017-10-en

treatment of specific types of income (such reduced tax rates on capital income or capital gains) or taxpayers (income tax rates for foreign residents). Importantly, we do not consider statutory inflation adjustments as a tax base measure. These actions, which typically include the adjustment of tax bands, allowances and credits to expected inflation, are often anticipated and simply ensure that the tax burden remains constant in real terms. However, we do consider deviations from a statutory indexation rule that leave tax rates unchanged as a tax base measure. These deviations are unanticipated, and are implemented purposely to increase or reduce future real tax liabilities. For example, this includes the case when personal income tax bands are kept fixed in nominal terms despite having been indexed in previous years, or an increase in excise taxes above inflation.

For corporate income taxes, we identify as tax base measures any changes to corporate tax credits (e.g., R&D or investment credits), depreciation schedules, loss carryforward and carry-back rules, thin capitalization rules, transfer pricing provisions, controlled foreign corporation rules, tax treatment of foreign companies, or reduced corporate tax rates for small businesses or specific industries.

Finally, in the case of value added taxes, we classify as base measures any changes to the reduced or increased VAT rates, the type of goods and services they apply to, and the exemption of certain goods and services from VAT.

Importantly, the definition of tax base changes excludes tax amnesties, as well as pure revenue administration measures, such as changes in minimum VAT thresholds, penalties and fees, or improvements in enforcement and collection mechanisms. It also excludes policy decisions that only change the payment date of certain taxes, as these actions only affect the timing of payment, and not the tax liability itself.

2.3 Size and timing of tax changes

We record the expected revenue impact of each measure using forecasts produced by country authorities when the tax measures were announced. These forecasts are expressed in local currency relative to an unchanged policies baseline, and reflect how each tax measure is expected to change tax revenue starting from the year of announcement. The forecast horizon can range from one to ten years ahead, depending on the country and year. When future tax liabilities are changed in stages, such as when a sequence of tax measures are announced simultaneously but with different implementation dates, we record the revenue impact of each step separately whenever this information is available.

We also record the announcement and implementation dates of each tax measure. We make this distinction to capture different dimensions of tax shocks. Announcement dates capture the moment when changes to future tax liabilities become certain (a news shock). Implementation dates record the point in time at which tax liabilities were affected, which is when disposable income actually changes. In practice, we define announcement dates as the first time the measure was formally proposed by the government, for example through a formal communication of Prime Minister or the Ministry of Finance, or by the introduction of draft legislation to Parliament, such as a draft Budget law. However, in a few cases where the measures were subject to significant alteration during the legislative process, we use the date of signature or promulgation of the final bill as the announcement date. In contrast, the implementation date is the first moment when revenue impacts are non-zero, which often coincides with the beginning of the next fiscal year.

Importantly, we exclude from the analysis actions that extend or renew existing taxes, such as the annual renewal of excise taxes, or extensions of existing tax allowances and credits (e.g., extension of a research tax credit). These renewals are often anticipated long before they are formally presented, so their announcement date is difficult to establish.

2.4 Motivation

Even though our point of departure are the consolidation periods identified by Devries et al. (2011) and Alesina et al. (2015), these periods often include tax measures with non-consolidation motivations, such as significant tax reforms (e.g., the GST reform in Canada in the early 1990s) or measures designed to stimulate a depressed economy. (e.g., the temporary tax incentives adopted in Spain in 2010-11 to counteract a slowdown in economic activity and prop up the housing market). To address this, we code the motivation of each individual tax measure following a similar methodology as Romer and Romer (2010), differentiating between endogenous and exogenous tax changes.

Endogenous tax changes are those taken to offset factors that push growth away from normal. These include measures designed to support the economy when it is or is projected to be below potential, such as a personal income tax cut during a recession, or to mitigate the impact of other macroeconomic shocks, like changes in fuel excises to mitigate the impact of international oil price volatility on retail fuel prices. We also consider tax changes designed to finance a specific increase in government spending as endogenous, such as an increase in the VAT rate to finance job creation programs.

By contrast, exogenous tax changes are those not taken in response to contemporaneous economic shocks. These fall into two broad categories. The first category includes tax changes that were primarily aimed at increasing long-run growth, such as the income tax reform package announced by the Australian government in September 1985. This category also includes tax reforms motivated by a desire to promote competitiveness, protect tax revenue or generally increase the efficiency of the tax system. For example, this was a common motivation of corporate tax reforms such as that announced in the UK in 2010. Second, we also consider as exogenous those measures taken primarily to reduce an existing fiscal deficit or, more broadly, to ensure public debt remains sustainable. Our final dataset includes 2,445 exogenous tax measures, of which 1,585 had a long-run motivation, and 860 had a consolidation motivation.

An issue that arises with deficit reduction measures is that current economic shocks are sometimes part of their motivation. For example, fiscal consolidation measures in Southern European countries during the recent Euro crisis were motivated by existing current account and fiscal imbalances, made worse by the 2008-9 recession, and also by contemporaneous increases in sovereign risk premia. The political cycle also matters for how consolidation is presented to the public: a consolidation package may be framed as a response to inherited budget deficits if the government has recently changed, or as a forced response to current economic shocks if the government is still the same.

In practice, it is often difficult to separate between exogenous "deficit consolidation" measures that are motivated by inherited budget deficits, which reflect past economic shocks, and endogenous "deficit reduction" measures that targeted deficits created by current economic conditions, like a severe recession. Therefore, we adopt the convention set by Devries et al. (2011) of classifying tax measures that aimed to reduce an existing fiscal deficit as exogenous. We test this assumption below using Granger causality tests, and find that tax measures with a consolidation motivation are indeed uncorrelated with past business cycle conditions.

2.5 Example: The UK June 2010 Budget

To illustrate how the narrative dataset looks like, Table (1) shows how we code tax measures from the UK budget, which was presented to Parliament on June 6th, 2010. This was the second budget of 2010, and the first budget of the new Conservative-Liberal Democrats coalition

Table 1: The UK June 2010 Budget (selected measures)

Measure	**Tax**	**Type**	t_0	t_1	t_5	**Ann.**	**Imp.**	**Motivation**
Increase main VAT rate to 20%	VAT	Rate	2.9	12.1	13.5	06/22/10	01/04/11	Consolidation
Decrease CIT rate to 24% over 4 years	CIT	Rate	0	-0.38	-4.1	06/22/10	04/01/11	Long-Run
Lower capital & investment allowances	CIT	Base	0	0	2.7	06/22/10	04/01/12	Long-Run
Increase personal allowance by £1,000	PIT	Base	0	-3.3	-3.8	06/22/10	04/06/11	Long-Run
Increase capital gains tax rate to 28%	PIT	Base	0	0.7	0.9	06/22/10	06/23/10	Long-Run
Increase child tax credit	PIT	Base	0	-1.2	-2.0	06/22/10	04/06/11	Spending-Driven
Total June 2010 Budget			2.8	6.3	7.8			
% GDP			*0.2*	*0.5*	*0.6*			

Note: Estimated revenue impacts are in billions of pound sterling, and by fiscal year, with t_0 representing fiscal year 2010-11, t_1 representing fiscal year 2011-12, and so on. Data is taken from the June 2010 and March 2011 Financial Statement and Budget Reports.

government after the May 2010 general election. The budget had three main themes.

First, it aimed to eliminate the structural fiscal deficit by 2014-15 through additional tax and spending consolidation. The main tax consolidation measure was an increase in the main standard rate of value added tax from 17.5 to 20 per cent from 4 January 2011, which we code as being motivated by deficit reduction. This was expected to raise £2.9 billion in the 2010-11 fiscal year, and about £12.1 billion in the first full year of implementation (one percent of GDP).

The second main theme was a reform of the corporate income tax to promote enterprise and sustainable growth. This reform was carried out by reducing the top corporation tax rate from 28 to 24 percent over four years starting in April 2011. The budget also announced a number of corporate tax base measures, including a decrease in capital (depreciation) and investment allowances from April 2012. This corporate tax reform followed the 2008 reform implemented by the previous Labour government, and was itself extended in the years after, eventually reducing the corporate tax rate to 20 percent by 2015. Notice that although the overall budget planned for deficit reduction, this tax reform was revenue-decreasing in the aggregate. Given it was primarily aimed at increasing investment and job creation in future years, we code these tax measures as having a long-run motivation.

The June 2010 Budget also aimed to promote fairness in direct taxation, which was mainly achieved by raising the personal allowance by £1,000 starting in April 2011, which reduced the tax burden on lower-income taxpayers, and by increasing taxes on higher-income taxpayers, in particular through an increase in the capital gains tax rate. We code this capital gains rate increase as a personal income tax base measure since it changed the tax treatment of capital gains without any change to the main personal income tax rate schedule.

Since these reforms to personal taxation aimed to improve redistribution, we code most of them as long-run measures as well. However, there are exceptions. For example, the government also announced an increase in the child tax credit above inflation, which was to be financed by freezing the child benefit (a social security payment to parents or guardians of children) over three years. Given the specific link between these tax and spending actions, that was explicitly mentioned in the narrative account, we classify the increase in the child tax credit as spending-driven.[7]

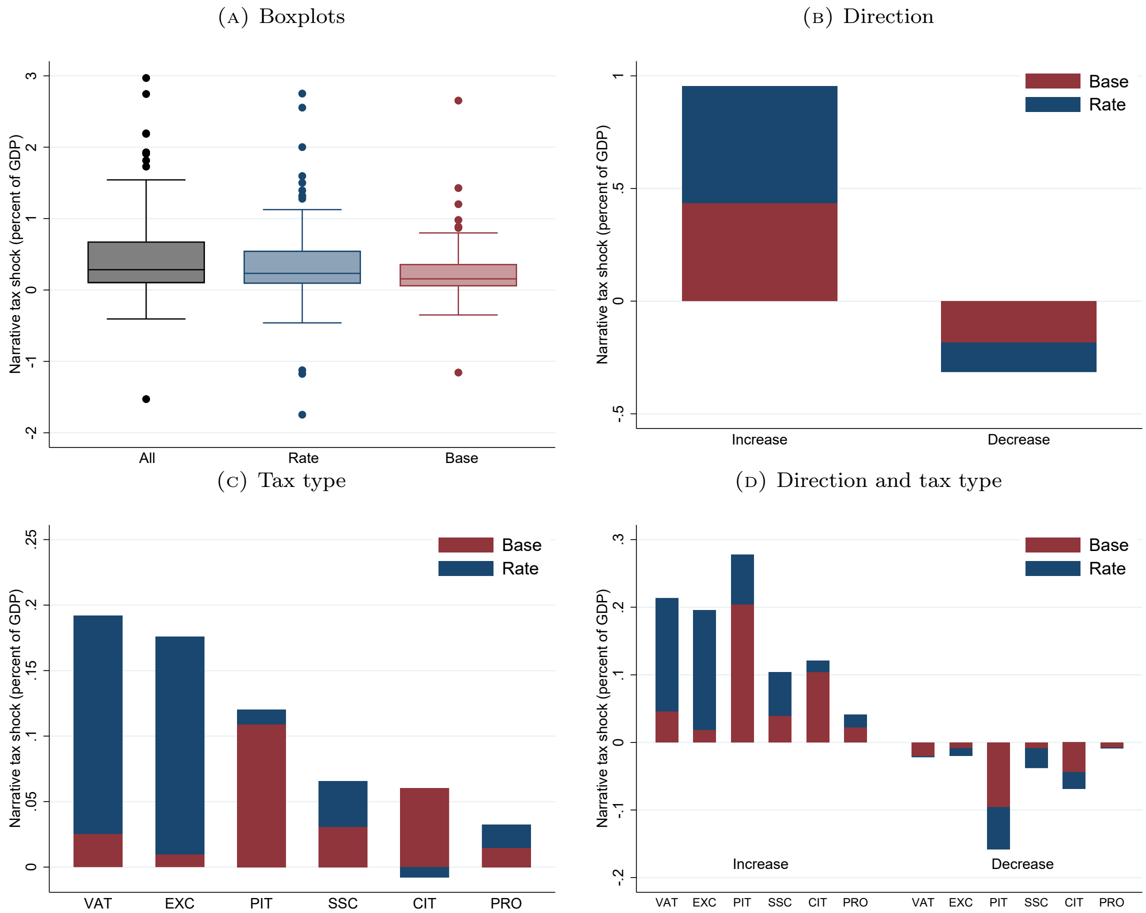
2.6 Descriptive statistics

In this section we document stylized facts from our narrative database. We start by examining the distribution of narrative tax shocks in Figure (1). The top left panel shows box plots of the narrative shocks by type of measure (rate versus base). To ensure that very small tax shocks are not dominating the distribution, we exclude narrative shocks smaller than 0.05 percent of GDP in this panel. A number of interesting patterns emerge. First, large tax consolidations are common, as there are several instances of tax measures worth more than one percent of GDP being announced in the same year. Second, while the composition of tax consolidations is tilted towards rate hikes, base changes still account for nearly 40 percent of tax consolidation efforts.

A third important stylized fact is that consolidations often include both tax increases and decreases. As can be seen from the top right panel of Figure (1), while tax increases tend to dominate, on average, these increases are partially offset by tax decreases. Some consolidation years see base and rate changes of opposite directions, as described above in the case of the June 2010 UK budget. In other years, decreases in taxation have to do with timing (e.g., if fiscal policy was expansionary at the beginning of the year, and turned contractionary towards

[7] In total, we identify 29 tax measures in the June 2010 Budget. Other significant tax measures included a higher threshold for employer social security contributions, a new bank levy, an increase in insurance premium tax, a one-year freeze in council tax, a reduction in the CIT rate for small businesses, and additional PIT base broadening measures.

Figure 1: Distribution of Narrative Tax Shocks during Consolidations



Note: The top left panel shows box plots of narrative shocks by measure type, excluding shocks smaller than 0.05 percent of GDP. The remaining panels use all the narrative shocks, and show the average tax change in consolidation years, separating between measure type, tax type, and the direction of change. Narrative tax shocks are scaled by GDP the year before announcement.

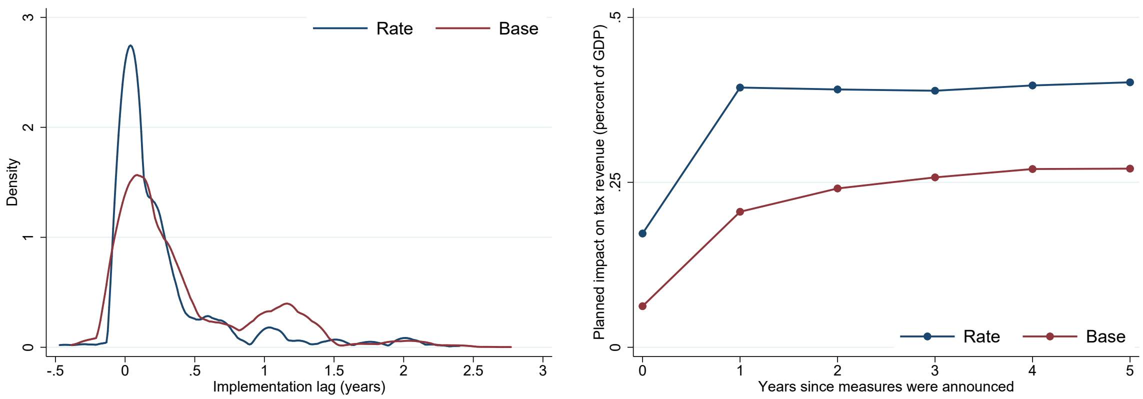
the end). However, on net most years see either base broadening, or rate hikes, or both.

The bottom left panel of Figure (1) shows the average change in taxation in consolidation years, distinguishing between six tax types: value added tax, excises, personal income tax, social security contributions, corporate income tax, and property taxation. As can be seen from the figure, consolidations rely heavily on increases in indirect taxes (both VAT and excises), where tax increases are mostly undertaken via rate hikes. However, consolidations also rely on large changes in direct taxes, especially taxes on labor income. Interestingly, these

FIGURE 2: Planned Revenue Effects of Fiscal Consolidations

(A) Implementation lag

(B) Planned revenue effect



Note: The left panel shows the distribution of implementation lags for rate and base changes, measured in years. The right panel shows the average intended impact of tax shocks on tax revenue. Narrative tax shocks are scaled by GDP the year before announcement.

figures can still hide large and offsetting changes within tax types, as shown in the bottom right panel of Figure (1), particularly in personal income and corporate income taxation. These offsetting changes sometimes represent long-run tax reform packages announced during consolidation years. However, they can also reflect attempts by policymakers to better target consolidation efforts, for example by increasing the incidence of taxes on certain industries (e.g., oil and gas, financial services) or by making them more progressive.

In Figure (2) we examine the planned impact of tax consolidation over time. The left panel shows the distribution of the implementation lag for rate and base measures, defined as the difference between the implementation and announcement dates. The typical tax measure is implemented within one quarter of announcement, with the median implementation lag being 42 days for tax rate changes, and 83 days for tax base changes. This short implementation lag is consistent with consolidation efforts targeting an immediate increase in tax revenue. However, there is a sizable minority of tax base changes that have an implementation lag closer to one year. This primarily reflects measures that were announced in the national budgets not for the fiscal year immediately starting, but for the fiscal year after, often to give taxpayers some time to adjust. A few tax measures have a negative implementation lag, meaning that they implemented retroactively.

The right panel of Figure (2) shows the average planned impact of tax measures on tax revenue, as forecasted by the authorities, separating between rate and base changes. To construct this figure, we use information on the projected impact of tax measures on tax collections taken from narrative sources, which we normalize by GDP the year before announcement. The vast majority of tax measures, both rate and base, aimed to permanently increase or decrease taxation levels. Temporary changes in taxation, which were expected to be reversed at a known future date, constitute less than 10 percent of the measures in our database. We include both temporary and permanent measure in our empirical analysis below, but our results are unchanged when focusing only on permanent measures.

3 Empirical approach

3.1 Predictability of narrative tax changes

We start by examining whether the narrative tax shocks can be predicted by past information. While we cannot test the contemporaneous exogeneity of our measures, we can confirm that the tax changes are unpredictable on the basis of past information. To do so, we first define the narrative tax shock variable as the sum of the intended revenue effects of all tax measures announced in that quarter

$$\text{Tax Shock}_{i,t} = \frac{\text{Intended Year 2 Revenue Effect}_{i,t}}{\text{GDP}_{i,t-1}}$$

To account for implementation lags, we focus on the intended revenue effect in the second fiscal year after the measures were announced (see also Romer and Romer, 2010). We choose a two year horizon since Figure (2) indicates that most of the planned impact of the tax change would have been realized, but our results are unchanged when using other horizons. The revenue forecasts, which are recorded in local currency in our database, are scaled by nominal GDP in the preceding quarter to make these effects comparable across countries and over time.

We perform pairwise Granger causality tests for different narrative tax shocks, estimating the following specification:

$$\text{Tax Shock}_{i,t} = \alpha_i + \delta_t + \gamma(L)x_{i,t-1} + \mu(L)\text{Tax Shock}_{i,t-1} + e_{i,t} \quad (1)$$

where $x_{i,t}$ are predictive variables, and α_i and δ_t are country and time fixed effects, respectively.

Table 2: Granger-causality tests

Regressor:	Output	Inflation	Interest rate	Govt. Debt	Govt. Purchases	Tax
All shocks	1.21	1.17	0.93	1.61	8.47***	0.97
	(0.37)	(0.38)	(0.49)	(0.25)	(0.00)	(0.47)
Motivation						
Consolidation	0.60	1.25	1.22	2.21	3.29*	0.47
	(0.67)	(0.36)	(0.37)	(0.15)	(0.06)	(0.76)
Long-Run	0.43	0.46	0.35	0.89	0.36	0.45
	(0.73)	(0.71)	(0.84)	(0.48)	(0.83)	(0.77)
Measure Type						
Rate	2.38	0.57	1.09	0.64	3.34*	0.70
	(0.13)	(0.69)	(0.42)	(0.65)	(0.06)	(0.61)
Base	0.83	1.73	1.32	1.47	1.51	0.96
	(0.54)	(0.23)	(0.34)	(0.29)	(0.28)	(0.47)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table displays pairwise Granger-causality tests using Equation (1) with four lags. Each entry is the F-statistic for the null hypothesis that the lags of regressor variable are jointly equal to zero, with p-values displayed in brackets. Standard errors are two-way clustered by country and time. * 0.10, ** 0.05, *** 0.01.

We test if our announced narrative tax shocks are predictable by past movements in a range of variables capturing economic and fiscal conditions, including output, inflation, short-term interest rates, public debt ratios, government purchases and tax collections. Each predictor variable is tested one at a time, and we include four lags of both the predictor and the narrative tax shock. We carry out these tests using a quarterly panel dataset of 10 OECD countries from 1975 to 2016. Data on macroeconomic indicators, including output, prices, and interest rates are primarily taken from the OECD Economic Outlook database. A detailed description of data sources is given in the appendix.

The Granger-causality test results are shown in Table (2), where each entry displays the F-statistic for a joint significance test of the coefficients $\gamma(L)$, with p-values given in brackets. The first row tests the predictability of the overall narrative tax shock, while the remaining rows decompose the tax shock by motivation (consolidation or long-run) and measure type (rate or base shocks). Reassuringly, past output fluctuations appear to be uncorrelated with narrative tax shocks, which supports the idea that they are indeed exogenous to contemporaneous business cycles. We also find that the narrative shocks are not predicted by lagged inflation rates or debt ratios. However, we do find that lagged changes in government spending are predictive of narrative tax changes, in particular in the case of consolidation tax rate changes.

To ensure that our results are not confounded by changes in government spending, we include extensive controls for government spending (and other variables) in our baseline specification. We also conduct several robustness tests in section 4.3, and show that spending shocks are unlikely to be confounding our estimates.[8]

It is important to place the results above in the context of recent research that has found narrative fiscal shocks to be predictable by lagged macroeconomic conditions. For example, Jordà and Taylor (2016) find that the annual fiscal shocks constructed by Devries et al. (2011), which combine both tax and government expenditure changes, are forecastable by lagged output growth and debt ratios. Cos and Moral-Benito (2016) find similar results for the fiscal shocks identified by Alesina et al. (2015), which are coded by announcement year rather than implementation year, showing that predictability is not only due to anticipation effects. However, their results also indicate that fiscal shocks are forecastable mostly because of their spending component, while the evidence for predictability is less clear for tax-based fiscal adjustments. This helps explain why we find no evidence that our narrative tax shocks are predictable by lagged growth, inflation or debt ratios. Nevertheless, two other factors are also important. First, we examine the motivation of each individual measure, rather than the motivation of packages of measures as Devries et al. (2011) and Alesina et al. (2015) did. This allows us to identify and drop endogenous tax measures even if they were enacted as part of a group of mostly exogenous measures. Second, we have more precise information on timing of each measure because we record their exact announcement and implementation dates. This also allows us to construct narrative shocks at quarterly frequency, which is likely to lower the correlation between the narrative shocks and lagged macroeconomic indicators compared to using annual data.

3.2 Impulse response functions

We estimate the dynamic response of key macroeconomic variables to different tax shocks by combining the local projections method of Jordà (2005) with an instrumental variables

[8] In the appendix, we also conduct further Granger-causality tests, with Table (A.3) decomposing by tax type, and Table (A.4) testing all regressors jointly. These additional tests find no evidence that the narrative tax shocks are predictable by lagged business cycle or fiscal conditions.

estimator.[9] Our baseline model is

$$\mathrm{Y}_{i,t+h} - \mathrm{Y}_{i,t-1} = \alpha_i + \delta_t + \beta_h \triangle \mathrm{Tax}_{i,t} + \theta_h' \mathbf{X}_{i,t} + \varepsilon_{i,t+h}, \qquad h = 0, 1, ..., H \tag{2}$$

where $\mathrm{Y}_{i,t}$ is the variable of interest, $\mathrm{Tax}_{i,t}$ is the tax-to-GDP ratio, $\mathbf{X}_{i,t}$ is a vector of control variables, α_i and δ_t are country and time fixed effects, respectively, and $\varepsilon_{i,t+h}$ is an error term. We estimate a separate regression for each horizon h, each time instrumenting the change in tax-to-GDP ratio using the relevant narrative tax shock. The baseline specification includes four lags of output growth, inflation, change in government spending, change in tax-to-GDP ratios, debt ratio, and the tax shocks themselves. When estimating the effect of tax rate changes, we instrument the change in tax-to-GDP ratios with the narrative rate shock, and add the narrative base shock as control; to estimate the effect of tax base changes, we do the reverse. Standard errors are two-way clustered by country and time.

Implementation lags As shown previously, there is typically a lag between the announcement and subsequent implementation of tax shocks, with the median implementation lag being 72 days across all exogenous tax measures. In practice, this implementation lag implies that narrative tax shocks are not strongly correlated with observed changes in tax-to-GDP ratios in the quarter of announcement, but are correlated with changes in tax ratios in subsequent quarters. To account for this implementation lag, we define the change in the tax ratio as the difference between taxes four quarters after announcement relative to the quarter before announcement[10]

$$\triangle \mathrm{Tax}_{i,t} = \mathrm{Tax}_{i,t+4} - \mathrm{Tax}_{i,t-1}$$

The coefficients β_h tracing out the response of $\mathrm{Y}_{i,t}$ over time can be interpreted as the effect of an expected increase in taxes worth one percent of GDP over the following four quarters after tax changes are announced.

[9] A similar approach is used by other papers in the literature, including Jordà and Taylor (2016), Mertens and Olea (2018), Fieldhouse et al. (2018), Ramey and Zubairy (2018), and Hebous and Zimmermann (2018). An alternative would be to estimate a narrative VAR, similar to Mertens and Ravn (2013). As discussed in Stock and Watson (2018), under contemporaneous exogeneity of the narrative instruments the identification conditions for local projections and VAR estimators are similar, and the latter is asymptotically more efficiently. However, in practice the local projections estimator is likely to be more robust to misspecification, which is especially important in a panel setting, and also does not require additional assumptions on the ordering of the shocks.

[10] See Fieldhouse et al. (2018) for a similar treatment of implementation lags using a local projections IV estimator.

Identification In Equation (2), the change in the tax ratio is instrumented with our measure of announced narrative tax shocks. Following Stock and Watson (2018), the key assumptions in this framework are that narrative tax shocks should be correlated with observed changes in tax-to-GDP ratios, but uncorrelated with the error term, that is,

$$\mathrm{E}\left[\text{Tax Shock}_{i,t} \times \triangle\text{Tax}_{i,t} \mid \mathbf{X}_{i,t}\right] \neq 0 \qquad \text{(Relevance)}$$

$$\mathrm{E}\left[\text{Tax Shock}_{i,t} \times \varepsilon_{i,t+h} \mid \mathbf{X}_{i,t}\right] = 0 \qquad \text{(Exogeneity)}$$

for all i, t and h. In particular, the exogeneity assumption requires the narrative shocks to be conditionally uncorrelated with contemporaneous macroeconomic shocks or other past or future information contained in the error term. Although our narrative shocks are not predictable by past information, this suggests that controlling for additional variables is important to ensure the exogeneity assumption holds. Moreover, even if controls are not necessary for identification, they can nonetheless improve the estimation efficiency.[11] Therefore, we consider several controls in our baseline specification, including four lags of the narrative tax shock, output growth, inflation, government debt ratio, change in tax revenue and government spending, and the short-term nominal interest rate. We examine the robustness of our key results to different sets of controls, and find that controlling for additional variables does not materially affect our results (see section 4.3 below).

Measurement error and truncation One concern about including the narrative tax shock directly in Equation (2) is that revenue forecasts produced by country authorities may have significant measurement error, or could even be biased (e.g., if there are political pressures to publish "optimistic" forecasts). This measurement error would in turn bias the estimated responses to tax shocks, probably downward. The IV estimator provides a straightforward solution to this problem, since the identification assumption only requires the narrative shocks (and their measurement error) to be uncorrelated with the error term $\varepsilon_{i,t}$. Intuitively, the IV estimator only uses the variation in the narrative shocks that is correlated with observed tax changes, so variation due to measurement error or bias in either variable is discarded.

A second concern is that our narrative tax shocks are only available during years of fiscal consolidation, which means that they could provide a biased estimate of the effect of tax changes

[11] Stock and Watson (2018) discuss conditions under which a contemporaneous exogeneity assumption, conditional on control, is sufficient to identify the model. In general, these conditions are equivalent to requiring the invertibility of the corresponding VAR system, so that the control variables span the space of the unobserved tax shocks.

in non-consolidation years.[12] One way to approach this problem is to consider the recorded narrative shocks as a truncated version of the true narrative shock variable constructed using all years:

$$\text{Tax Shock}_{i,t} = \begin{cases} \text{Tax Shock}^*_{i,t} & \text{if t is a consolidation year} \\ 0 & \text{if t is not a consolidation year} \end{cases}$$

However, a truncated instrument will still identify β_h under a stronger version of the exogeneity assumption, namely

$$\text{Tax Shock}^*_{i,t} \perp\!\!\!\perp \varepsilon_{i,t+h} \mid \mathbf{X}_{i,t} \qquad \text{(Exogeneity*)}$$

Essentially, this new exogeneity assumption requires the untruncated narrative tax shock to be conditionally independent of the error term, rather than just conditional mean independent. This ensures that any function of the tax shocks, such as one that truncates the shocks to be zero in some years, is also conditionally independent of $\varepsilon_{i,t}$.

4 Results for all taxes combined

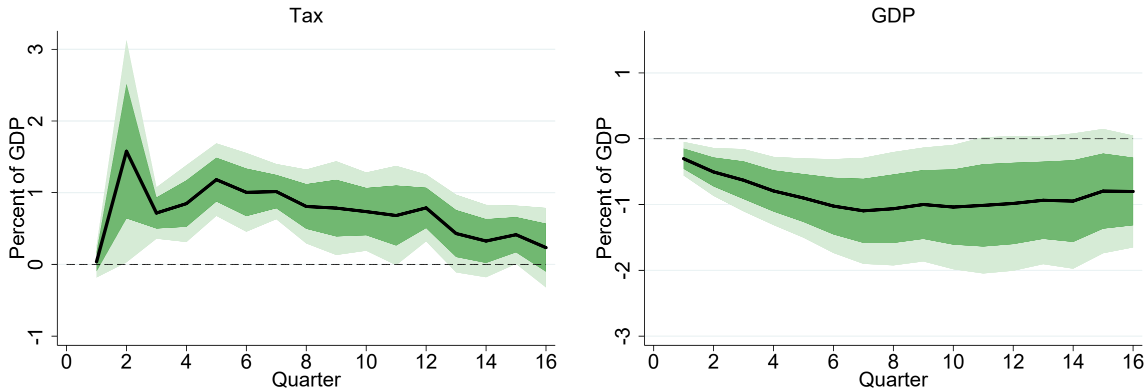
4.1 Dynamic effects of tax changes

We first present our benchmark estimates for the dynamic effects of all tax changes combined. After, we examine the effects of rate and base changes separately. All impulse responses show the response to shock equivalent to a tax increase worth one percent of GDP, and we show results for a forecast horizon of 16 quarters, together with 68 and 90 percent confidence intervals.

All tax types. The response of taxes and output to a narrative tax shock covering all tax types is shown in Figure (3). The left panel shows the response of the tax-to-GDP ratio to the narrative tax shock, the first-stage of our IV estimation. The point estimates are positive throughout, with the response initially building up over time, consistent with the announced intention shown in Figure (2). The improvement in the tax-to-GDP ratio then plateaus, with a point estimate slightly below one, and there is some evidence that the some of the improvement in tax ratios is partially undone after a few years.

[12]For example, see Kilian and Vigfusson (2011) for a similar argument on estimating the elasticity of GDP to oil prices using censored oil shocks.

FIGURE 3: Response of taxes and GDP to a 1 percent of GDP tax increase



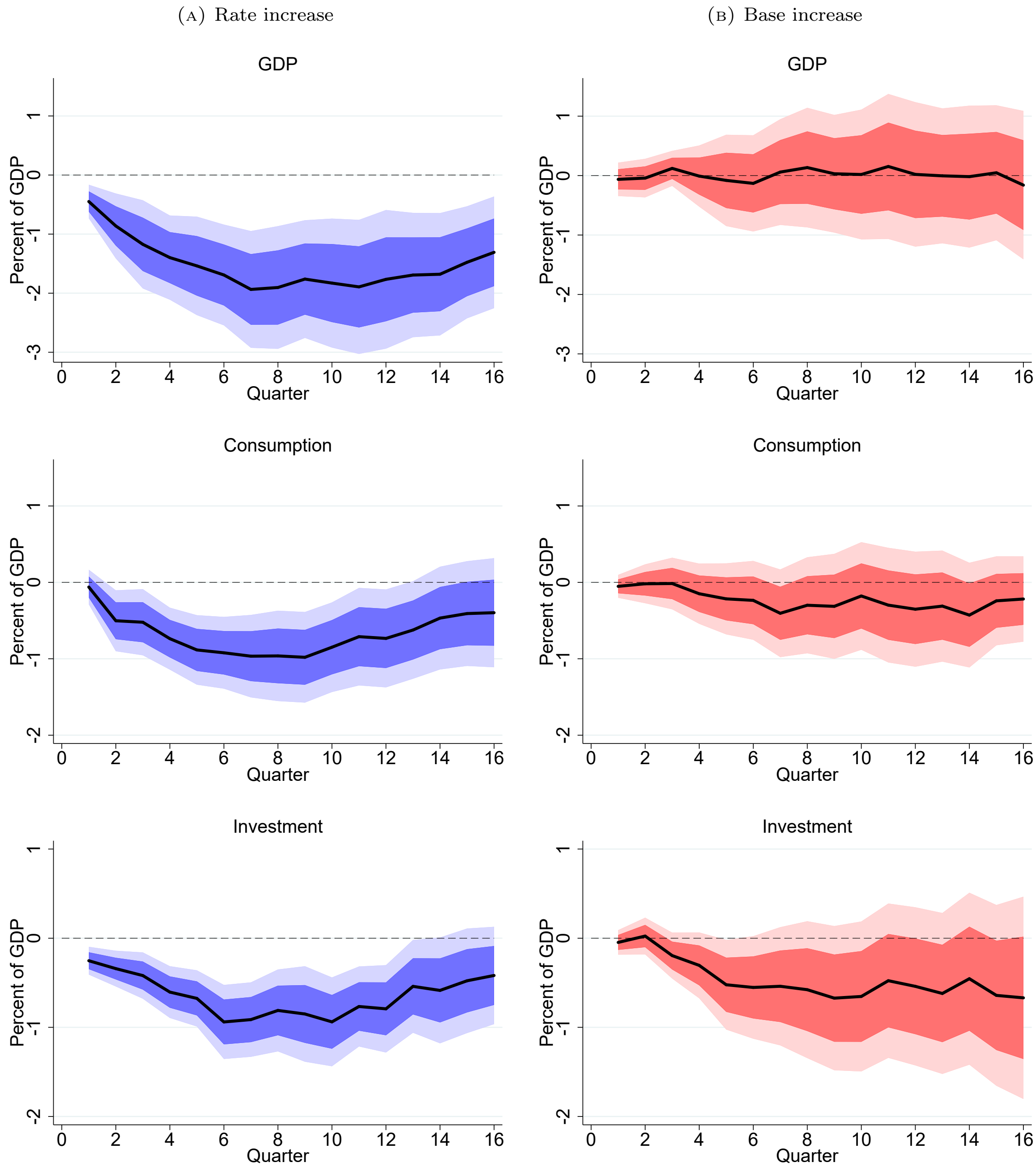
Note: Darker (lighter) areas show 68 percent (90 percent) confidence intervals. Standard errors are two-way clustered by country and time. Estimates show the response to shock equivalent to a 1 percent of GDP tax increase.

The right panel of Figure (3) shows the response of GDP to an improvement of one percent of GDP in the tax-to-GDP ratio, which is instrumented by the narrative tax shock (the second-stage of our IV estimation). Consistent with the findings in other studies (e.g., Romer and Romer, 2010; Cloyne, 2013; Alesina et al., 2015), an increase in tax liabilities during fiscal consolidations results in a short-term decline in output, with an announced increase in taxes of one percent of GDP leading to a decrease in output of about 0.3 percent in the first quarter, and a peak decline of about 1 percent eight quarters after announcement. These results are robust to a battery of checks (see section 4.3 below).

Tax rate and tax base changes. We next turn to the dynamic impact of tax rate versus base changes on output and its components, the focus of our paper. To estimate the impact of tax rate changes, we continue to use Equation (2), but now instrument the change in the tax-to-GDP ratio using the narrative shock for rate changes only. To account for discretionary changes in tax bases that may happen at the same time, we also control for the current and lagged narrative tax base shocks. We estimate the impact of discretionary tax base shocks similarly, this time adding current and lagged narrative tax rate shocks as controls. As before, the estimates shown represent the responses to a 1 percent of GDP tax increase enacted either through rate or base measures.

The left panels of Figure (4) show the responses of GDP and its components to tax rate shocks, while the right panels show the responses of the same variables to tax base shocks.

Figure 4: Response of GDP to a 1 percent of GDP tax rate or base increase



Note: Darker (lighter) areas show 68 percent (90 percent) confidence intervals. Standard errors are two-way clustered by country and time. Estimates show the response to shock equivalent to a 1 percent of GDP tax increase.

The response of GDP is shown in the top tow. A tax rate shock leads to a large decline in GDP, with a decline of about 0.4 percent in the first quarter, and a peak decline of 2 percent after about two years. The impact is persistent, and remains significant even four years after announcement. In contrast, tax base shocks have an impact on GDP that is neither economically nor statistically significant. Importantly, this difference is not driven by base shocks having a smaller impact on tax revenue than tax rate shocks. As Figure (A.1) in the appendix shows, both tax rate and tax base shocks lead to a significant increase in tax revenue. These increases are persistent, but not permanent, as they tend to become smaller and non-significant after 12 quarters.

The remaining panels examine the response of consumption and investment. To ensure that their magnitudes can be directly compared, we scale the impulse responses to have the same units in percent of GDP, by defining the dependent variables as follows:

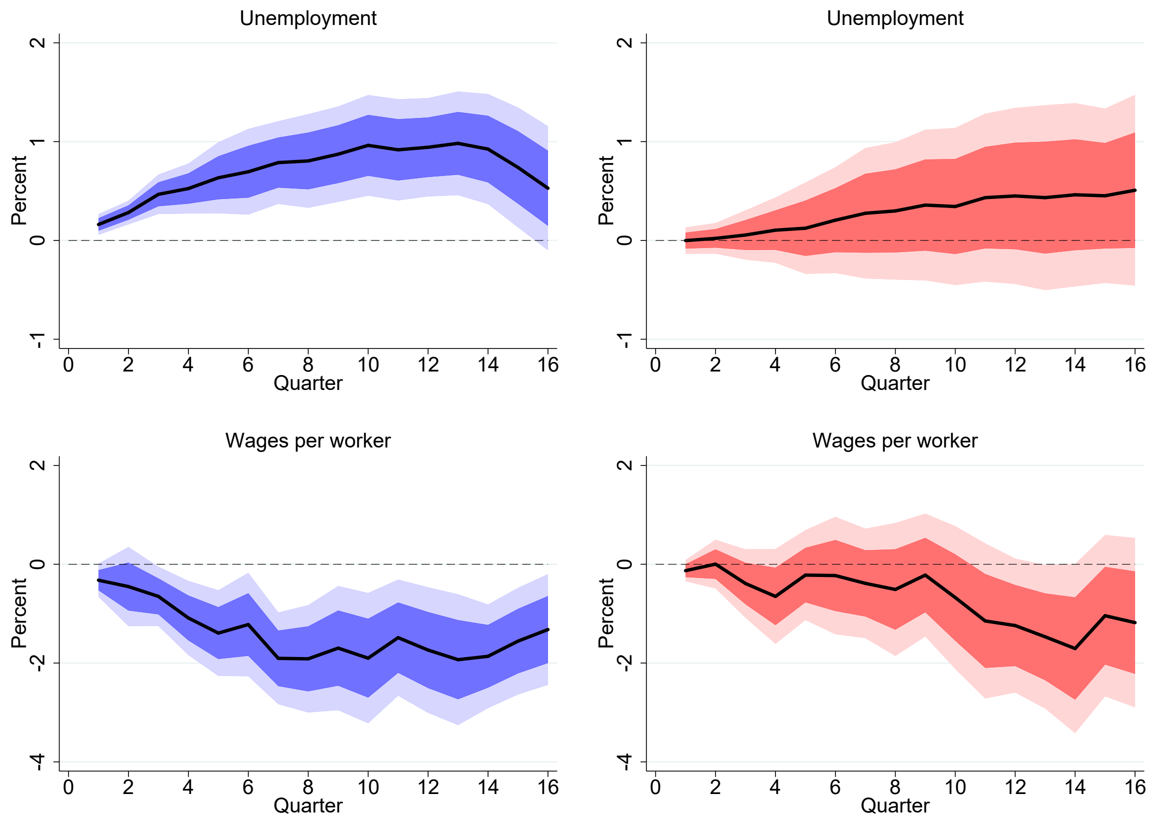
$$\mathrm{Y}_{i,t+h} - \mathrm{Y}_{i,t-1} = \frac{\mathrm{x}_{i,t+h} - \mathrm{x}_{i,t-1}}{\mathrm{GDP}_{i,t-1}}, \quad \mathrm{x} \in \{\mathrm{C, I, NX}\}$$

The second row shows the response of private consumption. After a tax rate shock, private consumption shows a steep decline, decreasing by about 1 percent of GDP after about eight quarters, and then recovering slightly over time. In contrast, the response of private consumption after a tax base shock is more muted, with point estimates indicating a decline of about 0.3 percent of GDP a year after announcement. However, this decline is not statistically significant. A similar picture emerges for private investment (here defined as gross fixed capital formation) in the third row of Figure (4). Investment decreases after a tax rate shock, declining by about 1 percent of GDP after about six quarters, and then partially recovering. However, private investment responds less to a similar size tax increased enacted only through base measures. The point estimates indicate no decline in the first two quarters, and a decline of about 0.5 percent of GDP after about six quarters, although it remains not statistically significant.

Finally, we examine in Figure (5) the responses of labor market indicators to tax rate and tax base increases. The top panels document the response of the unemployment rate to an increase in taxes equivalent to 1 percent of GDP. For tax increases adopted through rate hikes, we observe a significant increase in the unemployment rate, starting at around 0.1 percent in the first quarter, and building up to about 1 percent after three years. However, for increases adopted through base broadening, there is a smaller impact. In this case, there is no initial

FIGURE 5: Response of labor markets to a 1 percent of GDP tax rate or base increase

(A) Rate increase (B) Base increase



Note: Darker (lighter) areas show 68 percent (90 percent) confidence intervals. Standard errors are two-way clustered by country and time. Estimates show the response to shock equivalent to a 1 percent of GDP tax increase.

impact on unemployment, and the point estimate increases slowly to about 0.6 percent in the medium-term. However, these estimates remain statistically insignificant in all quarters.

The bottom row shows the response of average wages per employed worker, and here again the difference between rate and base responses is noticeable. Average wages decline after a rate hike is announced, and the point estimates indicate a statistically significant decline of around 2 percentage points after two years, which appears to be persistent. A similar decline is not observed in the first two years after a similar size base broadening shock is announced, with the point estimates remaining close to zero. There is some evidence of a decline in average wages in the medium-term after a base shock, although this is not statistically significant.[13]

4.2 Tax multipliers

The previous section describes the dynamic impact of tax shocks on the macroeconomy. Alternatively, these impacts can be translated into multipliers, which consider the change in output from tax shocks that increase tax revenue by 1 percent of GDP. We consider two approaches to estimating tax multipliers. The first approach is used extensively in the literature (e.g., Barro and Redlick, 2011), and estimates the contemporaneous multiplier:

$$\triangle^h Y_{i,t+h} = \alpha_i + \delta_t + \beta_h^{\text{Cntp}} \triangle^h \text{Tax}_{i,t+h} + \theta_h' \mathbf{X}_{i,t} + \varepsilon_{i,t+h} \quad (3)$$

This approach regresses the change in output h quarters ahead on the change in the tax-to-GDP ratio over the same horizon. The resulting multiplier is useful because it is directly comparable to other estimates of tax multipliers available in the literature. An alternative approach based on cumulative changes is suggested by Uhlig (2010) and Ramey and Zubairy (2018):

$$\sum_{j=0}^{h} \triangle^j Y_{i,t+j} = \alpha_i + \delta_t + \beta_h^{\text{Cumul}} \sum_{j=0}^{h} \triangle^j \text{Tax}_{i,t+j} + \theta_h' \mathbf{X}_{i,t} + \varepsilon_{i,t+h} \quad (4)$$

Here, the cumulative change in output relative to the quarter before the tax shock is announced is regressed on the cumulative change in the tax ratio. The advantage of this approach is that it takes into account the total change in output and tax revenue following the announcement of a tax shock, independently of their specific paths over time. For example, this could be important if tax shocks have an immediate impact on tax revenue while having a delayed impact on growth, or vice-versa. In both approaches, the change in tax is instrumented with the relevant narrative tax shock, and all baseline controls are included. In all cases, we display the first-stage F-statistic in square brackets. This statistic is typically large, which suggests there is no weak instruments problem.

The estimated tax multipliers are shown in Table (3), which contains two key findings. First, we find that the overall tax multiplier across all types of tax changes is about 0.8 to 0.9 after one year, and about 1 to 1.5 after two years. These magnitudes are consistent with previous literature. For example, Barro and Redlick (2011) and Romer and Romer (2010) find a one-

[13] We have also examined the response of other GDP components (exports, imports) and labor market variables (employment, hours per worker) to tax shocks. These results are available upon request.

Table 3: Tax multipliers

	Contemporaneous			Cumulative		
Horizon:	All	Rate	Base	All	Rate	Base
4 quarters	-0.91**	-1.26**	-0.36	-0.81*	-1.19**	-0.33
	(0.37)	(0.45)	(0.39)	(0.41)	(0.43)	(0.35)
	[48.72]	[37.03]	[29.74]	[53.85]	[34.50]	[41.65]
8 quarters	-1.46	-2.12*	-0.52	-1.06*	-1.55**	-0.40
	(0.86)	(1.01)	(0.74)	(0.49)	(0.49)	(0.44)
	[16.48]	[11.65]	[11.42]	[45.97]	[31.38]	[32.85]
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table displays point estimates for tax multipliers at four and eight quarters. The first stage F-statistic is shown in square brackets. Standard errors (in round brackets) are two-way clustered by country and time. * 0.10, ** 0.05, *** 0.01.

year tax multiplier of just over 1 for the U.S. in the postwar period, and Guajardo et al. (2014) and Alesina et al. (2017) also find impacts of similar magnitude using a narrative panel dataset of OECD countries. However, we find a slightly smaller medium-term tax multiplier compared to the literature. For example, Romer and Romer (2010) and Guajardo et al. (2014) find a two-year tax multiplier closer to 3. Our estimates are closer to the multiplier of 1.3 found by Alesina et al. (2017), although they focus on tax-based consolidations (i.e., fiscal consolidations where more than half of the consolidation effort was adopted through tax changes) rather than tax changes directly.

The second key finding is that multipliers based on tax base changes are consistently lower than those based on tax rate changes. Multipliers for rate changes are larger and statistically significant, starting at about 1.2 after one year, and reaching between 1.5 and 2 after two years, depending on the specification. In contrast, multipliers of tax base changes indicate a more muted impact on output. The point estimates for tax base multipliers are around 0.3 after one year, and rise to about 0.4 or 0.5 after two years. However, these point estimates are not statistically different from zero. The lower tax base multiplier compared to tax rate multipliers also does not depend on the methodology used to compute tax multipliers, since both methodologies deliver a similar result.

Table 4: Robustness checks for tax multipliers

	No controls			Spending shocks			All exogenous		
Horizon:	All	Rate	Base	All	Rate	Base	All	Rate	Base
4 quarters	-0.70	-0.97*	-0.31	-0.86*	-1.20**	-0.33	-0.73*	-1.13**	-0.23
	(0.39)	(0.45)	(0.35)	(0.38)	(0.47)	(0.39)	(0.36)	(0.42)	(0.39)
	[55.43]	[39.24]	[37.94]	[48.01]	[36.59]	[29.24]	[41.55]	[20.57]	[26.77]
	Permanent only			OLS			Annual data		
	All	Rate	Base	All	Rate	Base	All	Rate	Base
4 quarters	-0.93**	-1.29**	-0.37	-1.08***	-1.90***	-0.68	-1.89**	-2.34***	-0.99
	(0.39)	(0.47)	(0.41)	(0.32)	(0.45)	(0.72)	(0.67)	(0.63)	(0.94)
	[47.61]	[36.30]	[28.37]				[19.31]	[17.49]	[8.15]
Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Note: This table displays alternative estimates of the one-year contemporaneous tax multiplier. The first stage F-statistic is shown in square brackets. Standard errors (in round brackets) are two-way clustered by country and time. * 0.10, ** 0.05, *** 0.01.

4.3 Robustness checks

We examine the robustness of our results to alternative control variables, tax shock definitions and estimation methodologies in Table (4). For ease of exposition, we focus on the contemporaneous multiplier one year after announcement, but we find similar results using cumulative multipliers or longer horizons.[14]

The top left of Table (4) considers estimates of tax multipliers excluding all control variables from our baseline specification. The estimated tax rate multiplier is about 1, compared to about 1.3 under our baseline specification with controls. The tax base multiplier is 0.3, essentially unchanged from our baseline results. Overall, the tax multipliers are estimated less precisely, which suggests that including our baseline controls improves the efficiency of the IV estimator.

As discussed in section 3.1, a potential concern with our baseline specification is that rate and base shocks may be associated with different government spending responses. For example, base changes could be correlated with positive spending shocks, which would then explain their

[14] These results are available upon request.

smaller impact on output. Although our baseline specification controls for lagged changes in government spending, this does not necessarily account for spending shocks announced at the same time as tax shocks. Therefore, in the next set of estimates we include the narrative annual government spending shocks constructed by Alesina et al. (2015) as additional controls to our baseline specification. We find that contemporaneous spending shocks do not appear to drive the differences between rate and base multipliers, as the estimated multipliers are essentially unchanged from our baseline results. We also show in Figure (A.1) that observed government purchases react very little to our tax shock measures.

Next, we consider alternative definitions of the tax shocks. The top right estimates of Table (4) use a broader definition of the narrative tax shocks that includes not only shocks with a consolidation motivation, but also shocks with a long-run motivation that were announced during consolidation years. The estimated multipliers are slightly smaller compared to the baseline (e.g., 0.7 using all exogenous tax shocks vs. 0.9 in the baseline), but our key findings remain unchanged. We also redefine the tax shocks to exclude any tax measures that were announced as temporary, meaning that it was known from the start that they would be reversed a few years later. The estimated multipliers are essentially unchanged, consistent with the observation in section 2.6 that most identified tax measures were introduced as permanent changes.

The remaining estimates check if our results are robust to using different methodologies. First, we consider estimating tax multipliers by OLS using the narrative tax shocks directly as the regressors, rather than as an instrument for the change in the tax ratio. The estimated multipliers are slightly larger than our baseline results, suggesting that the OLS estimator may be biased upward. At the same time, the OLS estimator still finds a sizable difference between rate and base multipliers. Figure (A.4) in the appendix shows similar results when looking at the dynamic impact of tax shocks on growth using OLS instead of instrumental variables.

Finally, the estimates on the bottom right of Table (4) are estimated using annual instead of quarterly data.[15] Again, we find a large difference between rate and base multipliers, and somewhat bigger multipliers compared to the baseline. However, when using annual data the first-stage F-statistics are also substantially smaller, which could signal bias in the IV estimator. Figure (A.3) in the appendix examines the annual data responses in more detail,

[15] Annual indicators are taken from the OECD Economic Outlook and Revenue Statistics databases.

and finds that the impact of tax base increases on GDP remains smaller compared to tax rate increases of a similar size, and the estimates are not significantly different from zero. By contrast, tax rate increases lead to a decline in GDP of 1 percent in the first year, and a peak decline slightly over 2 percent in the medium-term. Overall, these results suggest that using quarterly data reduces concerns about weak instrument bias, and improves estimation efficiency.

5 Results for specific taxes

5.1 Changes to the baseline specification

In this section, we estimate the impact of changes in personal income, corporate income, and value added taxes on GDP, separating between tax rate and base increases. We focus on these three tax types because they are the largest contributors to tax revenue in our sample of countries, and quarterly data on revenue collections from each tax type is available.

We start by modifying the baseline specification in Equation (2) to take advantage of the additional information we have for each tax type, namely the statutory tax rate and the effective tax rate. In particular, to estimate the effect of tax rate shocks, we now instrument the change in the statutory tax rate rather than the change in the tax-to-GDP ratio. The former variable has the advantage of being a policy variable that is under the direct control of the government, and that by definition only responds to changes in tax rates. Thus, the baseline specification for estimating the response to rate shocks becomes

$$\mathrm{Y}_{i,t+h} - \mathrm{Y}_{i,t-1} = \alpha_i + \delta_t + \beta_h^{\mathrm{Rate}} \triangle \text{Statutory Tax Rate}_{i,t} + \theta_h' \mathbf{X}_{i,t} + \varepsilon_{i,t+h} \qquad \text{(Rate)}$$

The change in the statutory tax rate is then instrumented with our measure of narrative rate shocks. Conversely, to estimate the effect of tax base changes, we would prefer to focus on a policy indicator that responded only to base changes. Unfortunately, base changes are very diverse, and such an indicator is not available for most countries in our sample.[16] Therefore, we use the effective tax rate, defined as actual tax revenue divided by a measure of the

[16]For example, Devereux et al. (2002) constructs an indicator of the corporate tax base across countries by looking at depreciation and investment allowances. However, this indicator does not capture other base changes that affect tax credits for research or to stimulate employment, transfer pricing regulations, and so forth.

corresponding (potential) tax base, as a reasonable proxy for observed base changes.[17] So the baseline specification for estimating base shocks is

$$\mathrm{Y}_{i,t+h} - \mathrm{Y}_{i,t-1} = \alpha_i + \delta_t + \beta_h^{\text{Base}} \triangle \text{Effective Tax Rate}_{i,t} + \theta_h' \mathbf{X}_{i,t} + \varepsilon_{i,t+h} \qquad \text{(Base)}$$

In both cases, we add to our baseline controls the lagged changes in the statutory and effective tax rates across all three tax types we consider. In addition, to facilitate comparisons across rate and base responses, and with existing estimates in the literature, we scale the estimated impulse responses so that they correspond to a 1 percent increase in the effective tax rate for that particular tax type.

There are several advantages to instrumenting changes in statutory or effective tax rates rather than changes in the tax-to-GDP ratio, as before. First, this approach isolates changes in tax liabilities due to shocks to specific taxes with more precision. In particular, we find larger F-statistics in the first-stage of the IV estimator, which reduces concerns about bias. These first-stage estimates are shown in Figure (A.5) in the appendix. Second, it allows us to estimate the impact of tax shocks with a small or poorly estimated impact on overall tax revenue, which as we will see is particularly important for corporate income taxes. Finally, it produces estimates that are directly comparable with those in other studies.

5.2 Dynamic effects of specific tax changes

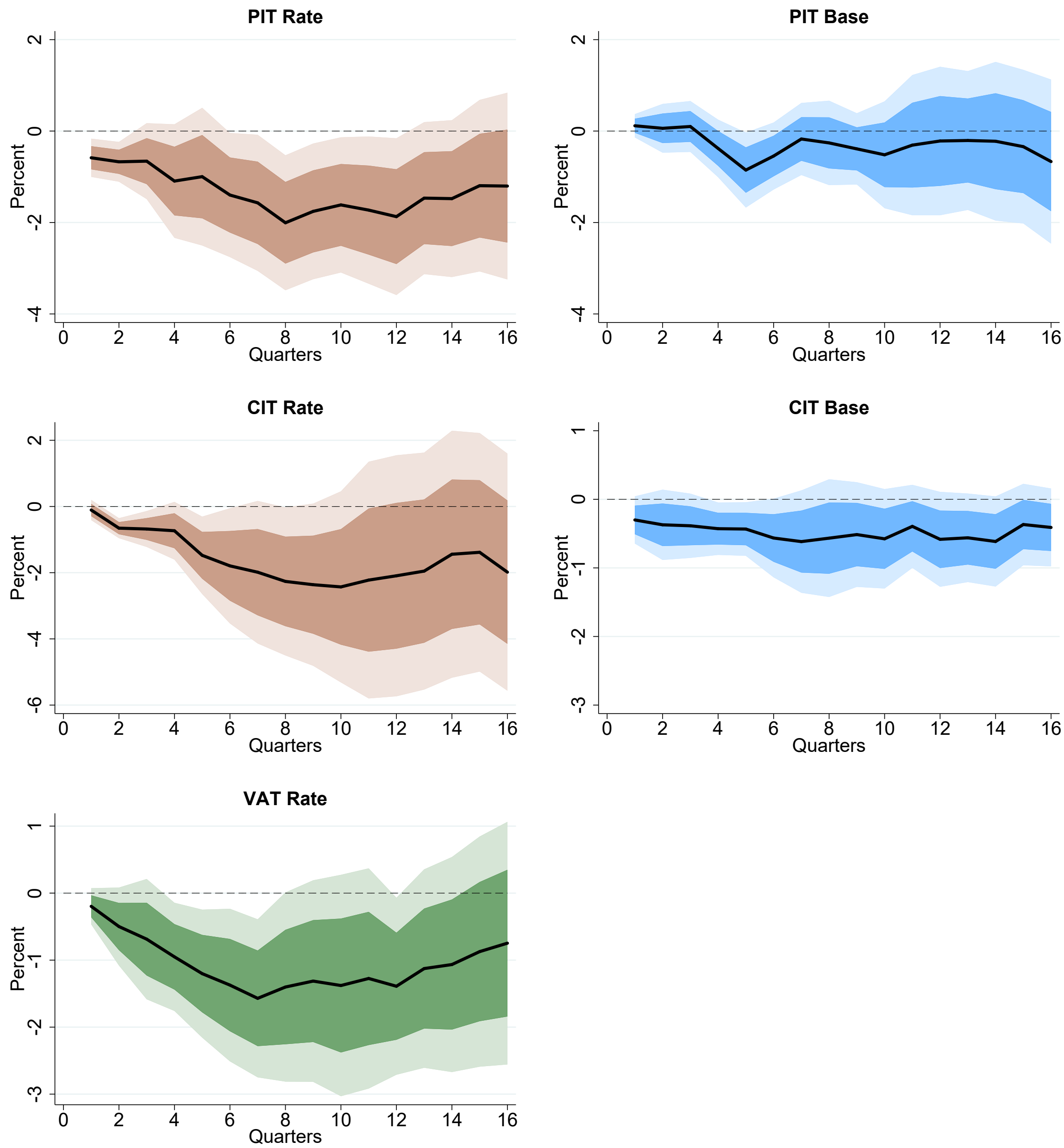
PIT. The estimated responses of output to different tax shocks are shown in Figure (6). The first row looks at personal income taxes (PIT), with the shocks scaled to correspond to a 1 percent increase in the effective PIT rate. We observe that a PIT rate hike is associated with a fall in output on impact, which then builds and peaks at around 2 percent of GDP after about eight quarters. In contrast, a similar size tax increase delivered through PIT base changes has a smaller impact on output, with no impact in the initial quarters, and a small subsequent negative impact, which remains statistically insignificant.

To our knowledge, this is the first attempt to estimate the impact of tax rate and tax base changes separately for specific taxes, so there are no directly comparable estimates in the

[17] As discussed in Appendix A, we construct proxies for the potential tax base using national accounts data, following Mendoza et al. (1994) and Carey and Rabesona (2002). For value added tax, the potential tax base is household consumption net of value added tax. For personal and corporate income tax, we use household personal income and corporate gross operating surplus, respectively.

Figure 6: Response of GDP to rate or base increases for specific taxes

(a) Rate increase (b) Base increase



Note: Darker (lighter) areas show 68 percent (90 percent) confidence intervals. Standard errors are two-way clustered by country and time. Responses to shocks equivalent to a 1 percent increase in the effective tax rate.

existing literature. However, Mertens and Ravn (2013) estimate a similar specification for personal income and corporate tax cuts in the U.S. postwar period, although they do not distinguish between rate and base changes. They find a peak output response of about 1.5 percent of GDP to a 1 percent cut in the effective PIT rate after 4 quarters, and medium-term response of about 1 percent of GDP. These responses are similar in magnitude to the estimates we obtain. Interestingly, they also fall between the responses we estimate for tax rate and tax base changes, which makes sense since their narrative shocks are a combination of personal income tax rate and base changes.

To compare these estimates against our results for all taxes, we also compute cumulative tax multipliers by dividing the cumulative output loss by the cumulative increase in PIT revenue. We find that the one-year cumulative multiplier for a PIT rate shock is 1.3, while the same multiplier for a PIT base shock is zero. At a two year horizon, the PIT rate multiplier increases slightly to 1.5, and the base multiplier also increases to 0.4.

CIT. The second row of Figure (6) examine the response of output to increases in corporate income tax (CIT), with the shocks scaled to represent a 1 percent increase in the effective CIT rate. Similar to the responses to personal income tax rate increases, we see an initial decline in output after a corporate tax rate hike is announced, which builds up and peaks at about 2.2 percent of GDP after about two years. However, the medium-term impact is imprecisely estimated and has a large confidence interval. In contrast, corporate tax base increases have a negative but smaller impact on output, and this impact is typically insignificant in most quarters.

For comparison, Mertens and Ravn (2013) find a permanent increase in output of around 0.5 percent following a 1 percent cut in the effective CIT rate. This is directly comparable against the output response we estimate for a CIT base shock, but smaller than the output response to CIT rate shocks. A possible explanation for this difference lies in the type of CIT shocks they consider. In the U.S., most changes to corporate taxation in the postwar period have involved rate declines and base broadening, policy decisions that have been in part driven by increasing capital mobility across borders and international tax competition. In our case, we focus on rate changes with a consolidation motivation, which are mostly rate increases that went against this international trend for lower corporate tax rates (at least temporarily). Therefore, the impact on output of these rate increases is likely to be larger, as even temporary CIT rate increases will discourage investment, leading to a lower capital stock and persistent output losses. Some evidence this could be the case lies in the estimated impact of CIT shocks on tax revenue, which similar to Mertens and Ravn (2013) is estimated to be close to zero. This also implies that it makes little sense to compute tax multipliers (an issue that also arises in their paper).

VAT. Finally, the bottom row of Figure (6) shows the response of output to an increase in the standard rate of value added tax (VAT), with a shock equivalent to a 1 percent increase in the effective VAT rate. Unlike for personal and corporate income taxes, there are few instances of base changes in value added tax, which implies that it is not possible to estimate the response of output to base changes in value added taxes. A VAT rate hike has a small but negative initial impact on output, which peaks at around 1.8 percent of GDP after seven quarters and then recovers slightly.

The one-year cumulative multiplier for a VAT rate shock is 2.2, and the two-year multiplier is 2.6. These multipliers are in line with those found elsewhere in the literature. For example, Riera-Crichton et al. (2016) finds a one-year multiplier close to 4 for VAT rate shocks, while Gunter et al. (2017) find a two-year VAT rate multiplier of 2.1 in industrial countries. Interestingly, while PIT and VAT rate hikes have a similar impact on output, the larger VAT multiplier is driven by a slower response of VAT collections in the first six quarters compared to quicker pickup in PIT revenue after a PIT rate hike.

6 Conclusion

Our analysis shows that changes in taxes have significant macroeconomic effects, but that these effects can vary with their composition. Using a new narrative dataset of tax changes during fiscal consolidations, we find that tax base changes lead to smaller output and employment declines than tax rate increases, lending support to the common policy advice that base broadening can raise additional revenue while being less detrimental to growth. The estimated cumulative tax rate multiplier is about 1.2 one year after announcement, while the cumulative tax base multiplier at the same horizon is only 0.3 and not statistically significant. These multipliers rise to 1.6 and 0.4, respectively, after two years. We also separate rate and base changes within specific taxes and find similar results, in particular for personal income and corporate income taxes.

The granularity of our narrative tax dataset opens up interesting possibilities for future research. First, one could examine how rate and base multipliers vary with initial conditions such as the state of the economy (e.g., Ramey and Zubairy, 2018) or features of the tax system, such as the initial tax rate or the overall tax structure (e.g., Gunter et al., 2017). In this regard, expanding the dataset to include additional countries and non-consolidation years would be fruitful. Another potential direction would be to classify base measures in more detail, similar to Amaglobeli et al. (2018), and indicate whether theoretically they would be expected to increase tax efficiency or not. This would make it possible to analyze the macroeconomic impact of different types of base measures, and zoom in on those more likely to be beneficial during consolidations.

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A Data

We construct a quarterly dataset for 10 OECD countries from 1975 to 2016, including data on key macroeconomic indicators, tax revenue, statutory tax rates, and potential tax bases by tax type. The data sources are displayed in Table (A.1), while summary statistics are shown in Table (A.2).

Macroeconomic indicators Macroeconomic variables, including GDP, employment, and interest rates, come from the OECD Economic Outlook Nr. 102 (November 2017) and from the OECD.Stat database. For Germany, we use data for West Germany from 1979 to 1990 taken from Destatis, and for the whole of Germany from 1991 onwards. In our regression analysis we control for German reunification by including a dummy which takes value one for Germany in all quarters after 1991Q1, inclusive, and zero otherwise.

Investment is proxied by gross fixed capital formation. Short-term interest rates are the prevailing 90 day or 3-month interbank rates, while long-term interest rates are proxied by the yields on 10-year government bonds. This data comes from the OECD Economic Outlook except for Germany, where we use similar series from the St. Louis FRED database.

Population data is from the UN Population Statistics, which is complemented with data from the WEO (for 2016) and from the Statistisches Jahrbuch der Bundesrepublik Deutschland for West Germany.

Tax data Data on tax revenue by tax type come from national statistics offices and tax authorities. Since cross-country quarterly data for some tax types is limited, we focus on changes in personal income, corporate income, and value added taxes.[18] To ensure the tax revenue series are consistent across countries, we check these series against the annual OECD Tax Revenue dataset. We take care to use only actual tax collections, and avoid data that has been interpolated or imputed from annual figures. However, this also means that our tax revenue panel is unbalanced, as quarterly tax data is not available in earlier years for some countries.

Data on statutory tax rates comes primarily from the OECD Tax Database for personal and

[18]We consider other taxes paid solely by businesses (OECD code 6100) as corporate income taxes. This mainly affects the IRAP (*Imposta regionale sulle attività produttive*) in Italy. While the IRAP started in 1998 as a tax on corporate value added, subsequent reforms have gradually introduced labor cost deductions, bringing the IRAP closer to a corporate income tax.

Table A.1: Data Sources

Variable	Source(s)	Comment
Macro		
GDP	OECD	
Consumption	OECD	
Investment	OECD	
Govt. Purchases	OECD	
Exports and Imports	OECD	
Corporate GOS	National Sources	
Household Income	National Sources	
Interest Rates	OECD EO 102	Except West Germany
Labor Market		
Employment	OECD EO 102	Except West Germany
Unemployment	OECD EO 102	Except West Germany
Labor Force	OECD EO 102	Except West Germany
Hours Worked	OECD EO 102	Except West Germany
Population	UN / WEO	Interpolated Annual data
Wages and Salaries	National Sources	
Fiscal		
Govt. Debt	OECD, National Sources	
Tax Revenue	OECD, National Sources	
Tax Rates	OECD, National Sources, EU Commission	

corporate income taxes, and from the European Commission for value added taxes. When necessary, we extend this information using data from national sources. We convert these datasets to quarterly frequency, taking into account the exact dates of tax rate changes, and adopting the convention that a tax rate changes in a given quarter if it was changed at any time during that quarter.

Tax Bases The VAT base is defined as household consumption expenditure by resident households exclusive of VAT:

$$\text{VAT Base} = \text{Consumption} - \text{VAT revenue}$$

Compared to Mendoza et al. (1994) and Carey and Rabesona (2002), we do not add to the tax base the non-wage component of government purchases, as quarterly data on government wages is not available for some countries in the earlier years. The PIT base is defined as household personal income net of social security contributions

$$\text{PIT Base} = \text{Household gross disposable income} + \text{Current taxes on income and wealth}$$

This definition includes labor income for dependent workers, income from self-employment, income from unincorporated businesses, and capital income paid to households through rents, interest, dividends, and realized capital gains. We exclude social security contributions from the tax base, since these are typically not subject to income tax. Finally, the CIT base is

$$\text{CIT Base} = \text{Gross operating surplus of financial and nonfinancial corporations}$$

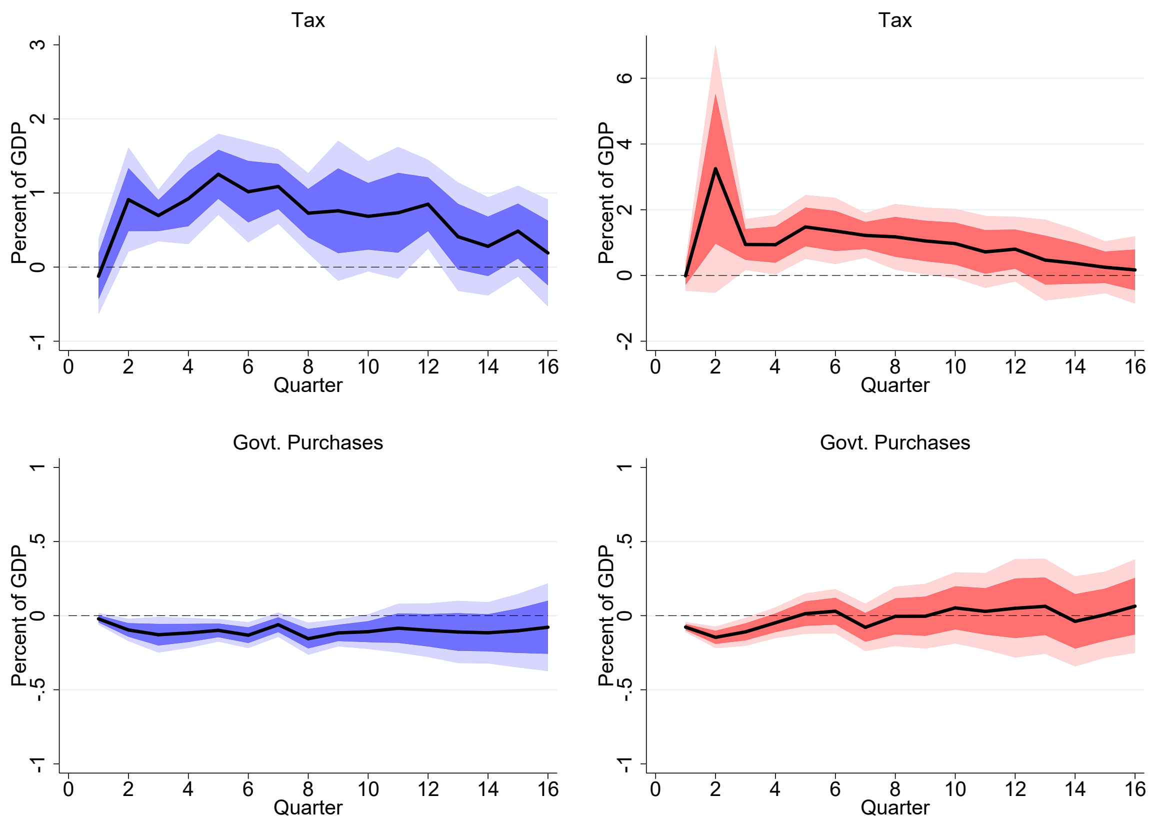
This definition approximates corporate profits before tax and dividend distribution, but has important differences relative to book profits or corporate taxable income. First, we do not subtract consumption of fixed capital from the tax base, even though most CIT systems will include allowances for depreciation. The reasons are that depreciation methods for CIT purposes are often different from those used in the national accounts, the methodologies to calculate depreciation in the national accounts are not harmonized across countries and across time, and measures of depreciation in the corporate sector are missing for some countries and years.

Government Debt Government debt is defined as gross government debt outstanding, measured either at the federal or general government level. For some countries where quarterly debt figures are not available in earlier years, we linearly interpolate the corresponding annual series.

B Other Figures and Tables

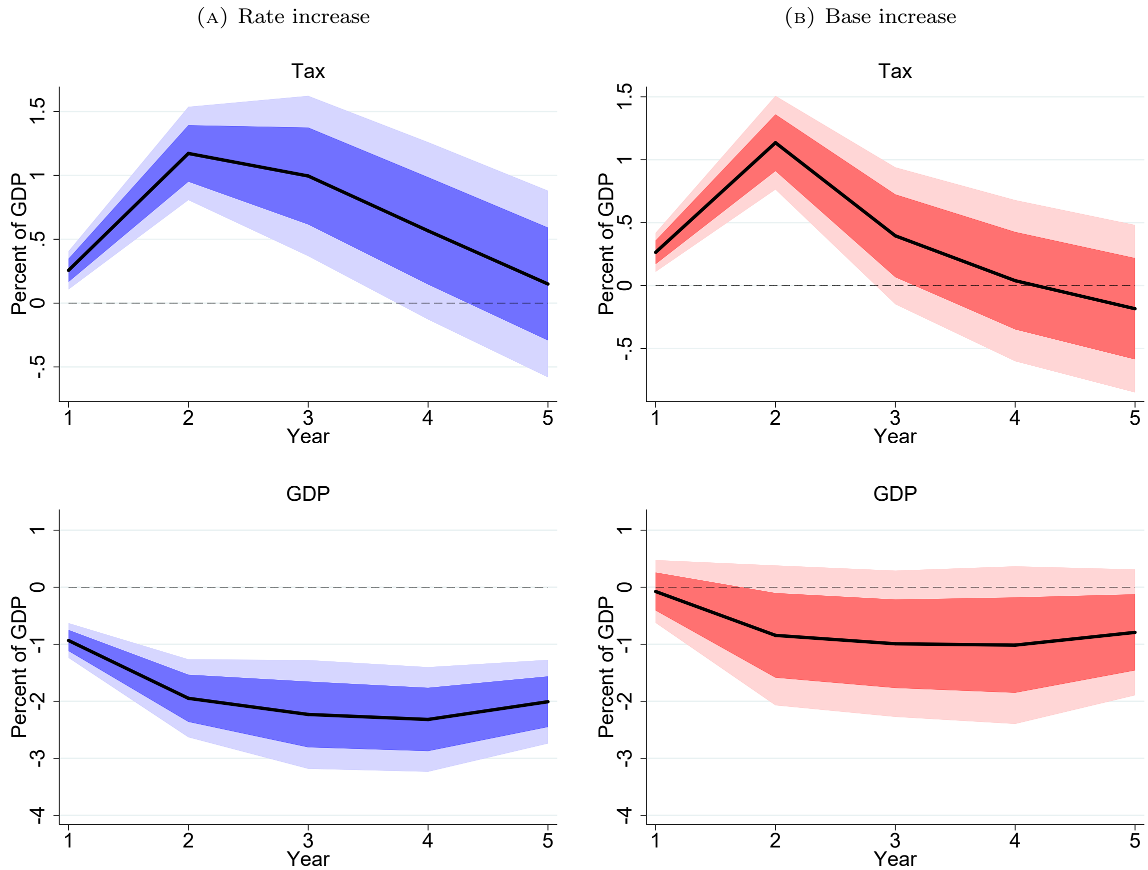
Figure A.1: Fiscal responses to rate or base increases

(a) Rate increase (b) Base increase



Note: Darker (lighter) areas show 68 percent (90 percent) confidence intervals. Standard errors are two-way clustered by country and time. Estimates show the response to shock equivalent to a 1 percent of GDP tax increase.

Figure A.3: Robustness check using annual data

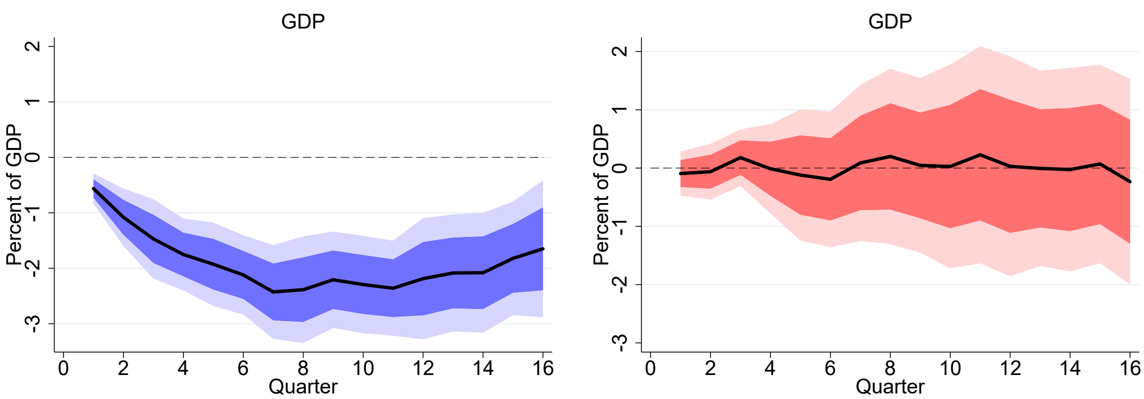


Note: Darker (lighter) areas show 68 percent (90 percent) confidence intervals. Standard errors are clustered by country and year. Estimates show the response to shock equivalent to a 1 percent of GDP tax increase.

FIGURE A.4: Robustness check using OLS estimation

(A) Rate increase

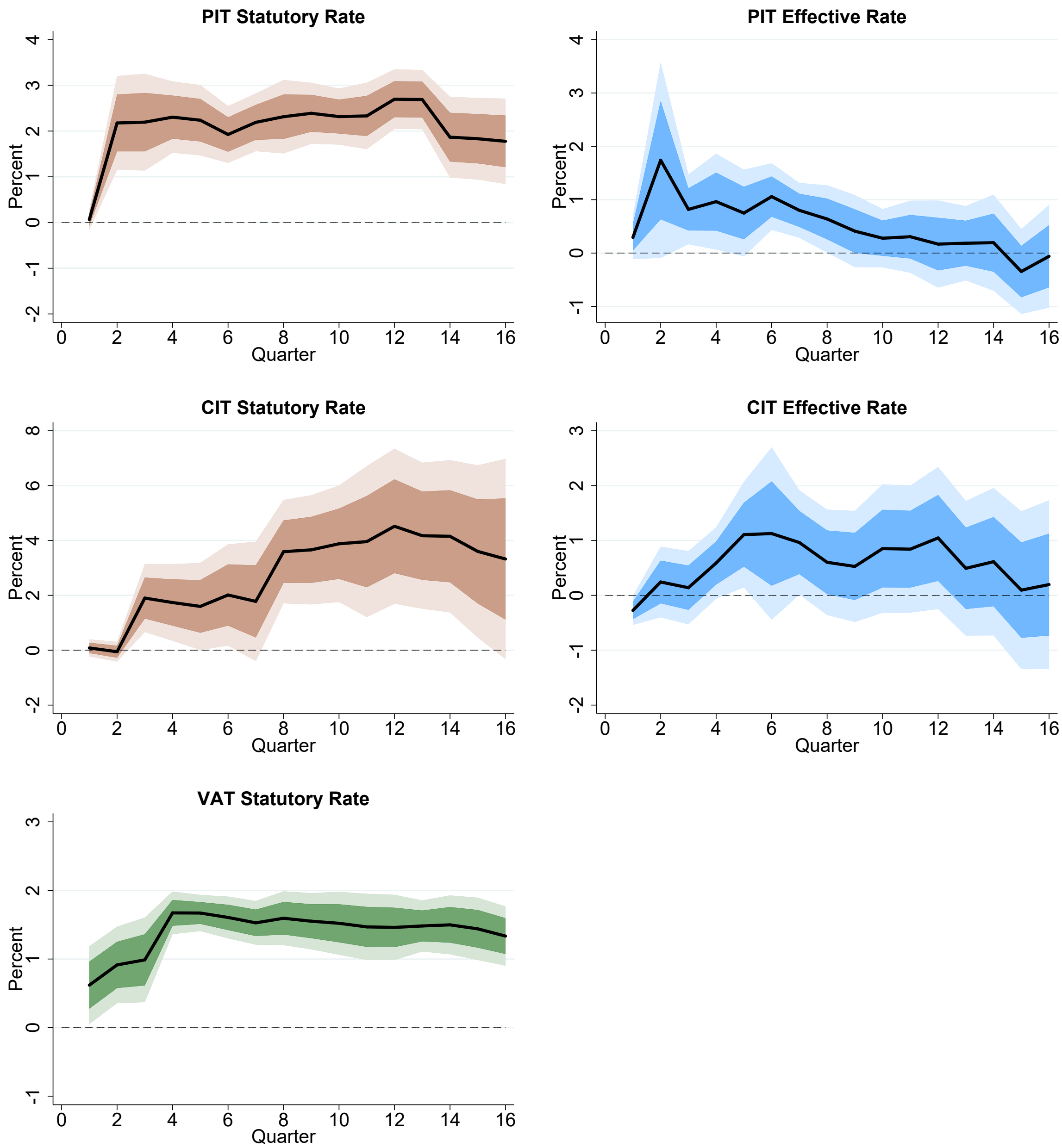
(B) Base increase



Note: Darker (lighter) areas show 68 percent (90 percent) confidence intervals. Standard errors are clustered by country and year. Estimates show the response to shock equivalent to a 1 percent of GDP tax increase.

Figure A.5: First-stage responses for specific taxes

(A) Rate increase (B) Base increase



Note: Darker (lighter) areas show 68 percent (90 percent) confidence intervals. Standard errors are two-way clustered by country and time. Responses to shocks equivalent to a 1 percent increase in the effective tax rate.

Table A.2: Summary Statistics

	Mean	Std. Dev.	10%	25%	50%	75%	90%	Obs
Macro								
GDP p.c.	33,270	7,079	24,721	28,110	32,953	37,893	42,282	1,358
Consumption p.c.	19,086	4,567	13,617	16,125	18,900	21,477	23,223	1,358
Investment p.c.	7,079	1,943	4,794	5,644	6,823	8,304	9,718	1,358
Exports p.c.	9,622	8,265	3,577	5,000	7,690	10,246	17,278	1,358
Imports p.c.	9,262	7,216	3,209	4,971	8,016	10,383	15,733	1,358
Fiscal								
Tax p.c.	8,193	2,232	5,300	6,391	8,150	9,800	11,045	1,358
Government purchases p.c.	6,720	1,175	5,175	5,758	6,857	7,649	8,204	1,358
Government gross debt p.c.	17,250	10,685	5,614	9,380	14,538	22,952	34,064	1,355
Personal Income Tax								
PIT base p.c.	24,101	5,803	16,978	20,080	23,725	27,356	30,403	1,326
PIT revenue p.c.	2,874	1,006	1,474	2,103	2,867	3,591	4,184	1,358
PIT statutory (top) rate (%)	47	10	34	40	48	53	60	1,358
PIT effective rate (%)	12	4	7	10	12	13	16	1,326
Corporate Income Tax								
CIT base p.c.	7,239	2,730	4,274	5,402	6,803	8,541	10,403	1,326
CIT revenue p.c.	841	439	351	514	770	1,060	1,477	1,358
CIT statutory (top) rate (%)	35	10	24	30	34	39	50	1,358
CIT effective rate (%)	12	5	6	8	12	16	20	1,326
Value Added Tax								
VAT base p.c.	17,879	4,553	12,784	15,124	17,239	19,565	22,161	1,358
VAT revenue p.c.	1,553	959	0	879	1,692	2,225	2,687	1,358
VAT statutory (standard) rate (%)	14	8	0	7	17	20	21	1,358
VAT effective rate (%)	12	4	6	9	12	15	17	1,102

Note: This table presents summary statistics for the quarterly sample of 10 OECD countries between 1978 and 2014. Values are in real per capita 2011 PPP dollars, unless otherwise noted.

Table A.3: Granger-causality tests by tax type

Regressor:	Output	Inflation	Interest rate	Govt. Debt	Govt. Purchases	Tax
Personal Income Tax						
Rate	1.86	1.92	1.51	0.19	3.55*	0.67
	(0.20)	(0.19)	(0.28)	(0.94)	(0.05)	(0.63)
Base	0.77	2.93*	1.77	1.86	1.01	0.13
	(0.57)	(0.08)	(0.22)	(0.20)	(0.45)	(0.97)
Corporate Income Tax						
Rate	1.55	0.75	1.32	0.51	1.05	1.55
	(0.27)	(0.58)	(0.33)	(0.73)	(0.43)	(0.27)
Base	1.53	0.82	0.46	2.06	0.40	9.76***
	(0.27)	(0.54)	(0.76)	(0.17)	(0.80)	(0.00)
Value Added Tax						
Rate	1.32	3.57*	1.01	1.10	0.52	0.71
	(0.33)	(0.05)	(0.45)	(0.41)	(0.73)	(0.60)
Country FE	Yes	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes	Yes

Note: The table displays pairwise Granger-causality tests using Equation (1) with four lags. Each entry is the F-statistic for the null hypothesis that the lags of regressor variable are jointly equal to zero, with p-values displayed in brackets. Standard errors are two-way clustered by country and time. * 0.10, ** 0.05, *** 0.01.

Table A.4: Joint Granger-causality tests

		Motivation		Measure Type	
	Total	Consolidation	Long-Run	Rate	Base
2 Lags	1.53	1.26	0.58	1.06	1.40
	(0.12)	(0.25)	(0.86)	(0.40)	(0.17)
4 Lags	1.35	1.27	0.42	0.86	1.39
	(0.14)	(0.19)	(0.99)	(0.66)	(0.12)
Country FE	Yes	Yes	Yes	Yes	Yes
Time FE	Yes	Yes	Yes	Yes	Yes

Note: The table displays joint Granger-causality tests using Equation (1) with two or four lags. Each entry is the F-statistic for the null hypothesis that the lags of the regressor variables are jointly equal to zero, with p-values displayed in brackets. The dependent variables are the narrative tax shocks (indicated by column name), and the regressors include output growth, inflation, interest rate, debt ratio, and the change in tax and government spending ratios. All models also include lags of the narrative tax shock. Standard errors are clustered by time. * 0.10, ** 0.05, *** 0.01.