The Composition Effects of Tax-Based Consolidations on Income Inequality

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Abstract

We analyse the effects of tax-based consolidations on income inequality, output and labour market conditions for a sample of 16 OECD countries over the period 1978-2012 employing a panel vector autoregressive methodology. We find that tax-based consolidations reduce income inequality, but at the cost of weaker economic activity. However, tax composition does matter. We show that indirect taxes reduce income inequality by more than direct taxes, possibly due to the operation of a positive labour supply channel. Among indirect taxes, value added and sale taxes are the most successful tool for policymakers to balance efficiency and equity. Finally, we show that tax-based consolidations reduce disposable income inequality via a decrease in market income disparities and an increase in government redistribution respectively in countries with a weaker and a stronger preference for redistribution.

Keywords: Income distribution; Tax-based consolidation; Fiscal consolidation; Labour force participation; Tax composition.
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1. Introduction

Following the recent build-up of large public debt stocks, consolidating public finances has become a priority for several governments in developed countries. In the Euro Area (EA), for instance, the primary balance has increased from -3.8% of GDP in 2009 to 0.1% in 2015. Fiscal policy in the United Kingdom and the United States has followed a similar path, with the primary deficit ratio shrinking respectively from 9.4% to 2.8% and from 11.2% to 1.7% during the same period.[1]

Many have raised concerns that the recent wave of fiscal consolidations may exacerbate high and rising income and wealth disparities, with adverse consequences for long-term economic growth. Indeed, a vast strand of the literature argues that inequality may compromise economic growth through a number of channels. First, it creates political instability, which may discourage investments (Alesina and Perotti (1996); Berg and Ostry (2011)). Second, in a highly unequal society, the majority of people are not in condition to save or to invest in education, which reduces investments and the accumulation of human capital (Perotti (1996); Galor and Moav (2004); Aghion et al. (1999); OECD (2014); OECD (2015b)). Some recent studies have also argued that income inequality played a major role in the recent financial crisis, by creating the conditions for low interest rates, which have ultimately fuelled a rapid rise in debt accumulation among lower and middle-income class agents (Fitoussi and Saraceno (2009); Kumhof et al. (2015)). In this light, policy-makers have now become more concerned about the consequences for inequality of their policy actions.

In this context, the aim of our analysis is twofold. First, we assess the effects of tax-based consolidations (i.e. consolidations in which tax hikes are larger than spending cuts) on disposable income inequality, output and labour market conditions. Second, we investigate the composition effects, distinguishing between direct and indirect taxes and their main sub-components. Although we are aware of the centrality of both income and wealth inequality in the debate, we focus on income inequality due to limited time series data availability about wealth inequality. Moreover, as income directly impacts living standards, we believe that rising income inequality has played a more prominent role in fuelling the recent wave of discontent in large fractions of the population that policy-makers are now trying to address. Finally, we only analyse tax-based consolidations since, as we will see below, this is the area with most disagreement in the literature.

Fiscal consolidation can affect income inequality directly, due to changes in government redistribution, and indirectly, through its effects on agents' labour supply decisions as well as on firms' labour demand and on the profits of capital owners. More generally, a fiscal retrenchment is likely to have an impact on inequality due to the response of the economy. For instance, Ball et al. (2013) argue that fiscal adjustments reduce output and increase unemployment. This decreases the wage share, which in turn tends to increase inequality due to the relatively higher share of wage income in lower-income

[1] This data is from the IMF 2016 World Economic Outlook.

groups. Bastagli et al. (2012) suggest that the tendency of employers to hoard high-skilled workers, who usually have higher income levels, could be another factor potentially raising inequality at times of fiscal restraint. However, these hypotheses do not consider the possibility that the inequality impact of fiscal consolidations may depend on which particular measure is implemented. As an example, while a cut in social spending may well increase income inequality, raising direct taxes on richer agents may decrease it.

The empirical literature on the effects of fiscal consolidations on income inequality has been limited in scope and has provided mixed evidence. Ball et al. (2013), Woo et al. (2013) and Agnello and Sousa (2014) all start from the same action-based consolidations dataset of Devries et al. (2011), which contains information about spending cuts and tax hikes during fiscal consolidation episodes in 17 OECD countries between 1978 and 2009.[2] But they employ different methodologies: Ball et al. (2013) use local projections, Woo et al. (2013) adopt seemingly unrelated regressions (henceforth SUR) and fixed effects, and Agnello and Sousa (2014) use SUR. All these contributions distinguish between tax-based and spending-based consolidation episodes, where a tax-based consolidation is defined as having tax hikes larger than spending cuts, and vice versa for spending-based consolidations. While they all conclude that spending-based consolidations increase income inequality, a consensus on the effects of tax-based consolidations has not been reached. In fact, Ball et al. (2013), Woo et al. (2013), and Agnello and Sousa (2014) find, respectively, significant positive, insignificant and significant negative effects of tax-based consolidations on income inequality.

Besides disagreeing on the inequality effects of tax-based consolidations, these contributions limit the analysis to the effects of changes in overall taxation and neglect the composition effects, their actual incidence, and the level of progressivity of different taxes. An attempt to empirically study the impact of changes in the composition of fiscal policy on inequality in a multivariate framework has been done by Martínez-Vázquez et al. (2012) and Muinelo-Gallo and Roca-Sagalés (2013). However, neither of those contribution identifies exogenous fiscal policy changes and hence do not address the endogenous relationship between fiscal policy on one side and growth and inequality on the other side, which could bias their results.

Our analysis contributes to the existing literature in two aspects. First, most of the empirical works on the effects of fiscal consolidations are carried out in a single-equation setup and assesses the impacts either on growth (Guajardo et al. (2014), and (Alesina et al., 2015b)) or on inequality (Ball et al. (2013), Woo et al. (2013), and Agnello and Sousa (2014)). On the contrary, we use a multi-equation setup to take into account both the direct effects of changes in fiscal policy on inequality, output and labour market variables and the indirect (feedback) effects among them. Second, the literature limits the analysis to the effects of overall tax changes and neglects potential composition

[2]For an extensive discussion about the Devries et al. (2011) dataset we refer the reader to Section 2.

effects (Ball et al. (2013), Woo et al. (2013), and Agnello and Sousa (2014)). Our paper fills this gap in the literature by analysing also the effects of specific tax instruments.

Our sample includes 16 OECD countries during the period 1978-2012. To identify episodes of fiscal consolidations, we use the action-based datasets of Devries et al. (2011) and Alesina et al. (2015b). These two databases exclusively consider those consolidation episodes aiming solely at reducing the government deficit, and not at stabilizing economic activity. This approach allows us to identify only exogenous fiscal shocks and hence limit the potential endogeneity between fiscal variables and GDP. Next, we make use of the OECD tax revenues database in order to quantify changes in specific taxes during fiscal consolidation years. In this way, we are able to pin down which particular instrument was used during each consolidation episode and analyse its effects. As proxies for income inequality, we use both the market and the disposable income Gini index as well as top income shares.[3] For the estimation, we rely on a panel vector auto regressive (PVAR) methodology. This allows us to take into account both the direct effects of fiscal policy on income inequality and the potential feedback effects through the response of output and the labour market.

Our results can be summarized as follows. First, tax-based consolidations significantly lower income inequality. However, lower inequality comes at the cost of weaker economic activity. Second, we show that using different tax instruments matters. In particular, we find that indirect taxes reduce income inequality by more than direct taxes. This result, which seems to contradict the main literature, can be explained by the operation of a positive labour supply channel: higher indirect taxes decrease the amount of goods that households can buy given a certain income and hence create incentives for agents voluntarily out of the labour force to start searching for a job. This in turn promotes labour force participation, especially from women, fosters employment, and reduces income inequality. Among direct taxes, personal taxes significantly decrease inequality, whereas corporate taxes and social security contribution do not. Finally, we show that the channels through which inequality is reduced may depend on country-specific factors. Namely, we find that in countries where governments have lower preferences for redistribution, tax-based consolidations decrease disposable income inequality through a reduction in market income disparities. On the other hand, in countries where governments have higher preferences for redistribution, the reduction in inequality is achieved through an increase in redistribution. Overall, our findings suggest that the design of the fiscal consolidation does matter and that labour supply channels play a major role.

[3]The Gini index measures the extent to which the distribution of income (or consumption expenditure) among individuals deviates from a perfectly equal distribution. It ranges between 0 (which represents perfect equality) and 100 (which represents perfect inequality). It is usually estimated from survey data and it can be based on market income as well as disposable income. The Gini index of market income is calculated on income before taxes and transfers. The Gini index of disposable income – sometimes referred to as after-tax Gini index or net Gini index – is calculated on income after taxes and transfers. On the other hand, top income shares are estimated from tax filing data and are based on market incomes. They are often used as proxies for the concentration of incomes in the right tail of the income distribution.

The remainder of the paper is structured as follows. Section 2 presents the dataset and explains the empirical methodology. Section 3 contains our baseline results on the overall effects of tax-based consolidations and reports several robustness checks. Section 4 focuses on the composition of tax-based consolidations and disentangles the specific effects of each single tax instrument. In Section 5 we examine whether the government's preferences for redistribution shape the effects of tax-based consolidation. Section 6 concludes.

2. Dataset and Methodology

2.1. Dataset and descriptive statistics

Our empirical analysis covers 16 OECD countries between 1978 and 2012 at the annual frequency. To identify exogenous tax shocks we start from the action-based dataset compiled by Devries et al. (2011). Devries et al. (2011) made use of official policy records to gather real-time data about estimated changes in tax revenues and public spending resulting from consolidation measures decided in 17 OECD countries during the 1978-2009 period. The peculiarity of this dataset is that it only selects those consolidation episodes having the sole objective of reducing the budget deficit and that are therefore exogenous to GDP, labour market variables or inequality developments.[4] Next, we use another action-based dataset compiled by Alesina et al. (2015a), which identifies additional fiscal consolidation episodes in 11 countries between 2010 and 2013, using the same method of Devries et al. (2011). By relying on the so-called narrative approach to identify episodes of fiscal consolidation we limit potential endogeneity between the response of fiscal variables and the GDP that could bias the analysis.[5]

After merging the two action-based datasets of Devries et al. (2011) and Alesina et al. (2015a), we have an unbalanced panel with data on consolidation episodes occurred in Austria, Belgium, Denmark, France, Germany, Ireland, Italy, Portugal, Spain, the United Kingdom and the United States over the

[4]Examples of such episodes may include consolidations that are caused by the operation of fiscal rules (such as for instance the Golden Rule in Germany), the presence of limit to public debt (such as the debt ceiling in the US) or by commitments to reduce public debt taken in a supranational context (as it was the case for several Euopean countries in the lead up to the creation of the Eurozone).

[5]Another approach to identify episodes of fiscal adjustments, which we define the statistical approach, simply considers periods of large changes in tax revenues and government spending. These two approaches yield very different results when analysing the effects of fiscal policy. For instance, Guajardo et al. (2014) analyse the growth impact of fiscal consolidations using first the action-based dataset compiled by Devries et al. (2011) and then a dataset compiled by Alesina and Ardagna (2013), who use the statistical approach. Estimates based on the former suggest that fiscal consolidation has contractionary effects. On the contrary, estimates based on the latter find expansionary effects (for similar findings see also Afonso and Jalles (2014)). The reason is that the statistical approach (i) tends to classify periods in which the budget balance improved simply due to favourable economic conditions as periods of fiscal consolidation, and (ii) it gives less weight to unsuccessful episodes of fiscal consolidation (i.e. when the government does not succeed in consolidating the budget). Admittedly, the narrative approach may also have some drawbacks. First, it largely relies on subjective judgements to identify exogenous fiscal consolidations and quantify their magnitude. Second, it may not eliminate completely the endogeneity between fiscal policy and growth, if the debt level correlates with changes in output. As it will be clear later, we try to address the first point by combining data from the action-based datasets with real ex-post fiscal data.

1978-2013 period, and in Australia, Canada, Finland, Japan, the Netherlands and Sweden over the 1978-2009 period. In carrying out the analysis, we restricted the sample to the period between 1978 and 2012, since data on the Gini index was not available for 2013. Following Alesina et al. (2015b), we have decided to exclude the Netherlands from the sample, as for the Netherlands the consolidation episodes identified by Devries et al. (2011) can be predicted using their own past values and lagged values of output growth. Therefore, these can not be considered as exogenous shocks and are not valid instruments for fiscal policy changes.[6]

The consolidations episodes in our dataset differ significantly in their size and nature. We classify them into four categories: (i) any kind of consolidation episode, (ii) spending-based consolidations (i.e. when spending cuts are larger than tax hikes), (iii) tax-based consolidations (i.e. when tax hikes are larger than spending cuts), and (iv) large tax-based consolidations (i.e. the upper quintile of tax-based consolidations). Over the 1978-2012 period, we identify 175 consolidation episodes, of which 103 are spending-based, 69 tax-based and 14 large tax-based.[7] Since tax hikes and spending cuts are likely to affect income inequality differently, including episodes of spending-based consolidations in the analysis may confound the results on the effects of tax changes. Hence, we mainly focus on tax-based consolidations. We use data on spending-based and large tax-based consolidations respectively for a robustness check and an extension.

Finally, we match our tax-based consolidation episodes with data on changes of actual tax revenues, expressed as a percentage of GDP, obtained from the OECD Revenue Statistics database. Our identification strategy is summarized in Appendix A. In Table 1, we show mean values of tax hikes and spending cuts identified through the narrative approach and the corresponding ex-post changes in tax revenues for the four different categories of consolidation episodes discussed above.

Table 1 leads us to an interesting consideration. The ex-post change in tax revenues during tax-based consolidations (0.6% of GDP) was lower on average than the government real-time estimate (0.7% of GDP). Even more, we count 14 tax-based consolidation episodes which resulted either in a no change or in a decrease of revenues. Potential explanations may be that in some cases policy-makers have relied on over-optimistic assumptions concerning the response of the business cycle and of the tax base to changes in the tax policy or have backtracked from their original plans. We will come back on this issue when performing robustness checks.

Besides distinguishing between spending cuts and tax hikes, the action-based dataset of Devries et al. (2011) does not always provide additional information about the composition of each consolidation episode. To gather data about the different tax instruments used by governments, we again rely on the OECD Revenue Statistics database. Particularly, we focus on two broad categories: direct and indirect

[6]For more information we refer the reader to Alesina et al. (2015b) and Jordà and Taylor (2016).

[7]In three instances tax hikes were exactly equal to spending cuts. Hence, we do not classify them as either tax-based or spending-based consolidations.

Table 1: Mean values of consolidation episodes during 1978-2012 (% of GDP)

	Narrative approach		Realization	
	Tax	**Spending**	**Tax**	**Obs.**
Full sample	0.15	-0.21	0.13	545
Consolidation	0.47	-0.67	0.47	175
Spending-based	0.29	-0.96	0.38	103
Tax-based	0.73	-0.24	0.59	69
Large tax-based	1.83	-0.67	0.93	14

Notes: Narrative approach refers to the real-time consolidation episodes identified by Devries et al. (2011) (for the 1978-2009 period) and Alesina et al. (2015a) (for the 2010-2013 period). Realization refers to tax data provided by the OECD Revenue Statistics database. Tax and spending refer respectively to changes in total tax revenues and general government spending. The spending-based sample comprises episodes in which spending cuts, as identified through the narrative approach, were larger than tax hikes, and vice versa for the tax-based sample. The large tax-based sample comprises the upper quintile of those episodes in which tax hikes were larger than spending cuts. All numbers are expressed as averages.

taxes.[8] Direct taxes include (i) personal taxes, (ii) corporate taxes, (iii) social security contributions (henceforth SSC) and (iv) payroll taxes. Indirect taxes include: (i) general taxes (henceforth GT), such as value added and sales taxes, (ii) taxes on specific goods and services (henceforth SGS), and (iii) taxes on the use of goods and services (henceforth UGS). For illustrative purposes, a flow-chart of the composition of direct and indirect taxes is depicted in Appendix B. Due to the marginal change of payroll and UGS tax revenues during tax-based consolidations we exclude them from the analysis.

Table 2 presents descriptive statistics of the change in the different tax instruments during (i) the full sample, (ii) tax-based consolidation and (iii) large tax-based consolidation episodes.

We notice that governments typically relied the most on direct taxes, and particularly personal taxes, to consolidate the budget. Among indirect taxes, GT were by far the most used instrument.

Table 3 shows the correlation coefficients among the changes in revenues generated from the different tax instruments during tax-based consolidations. As expected, direct and indirect taxes display a positive correlation with total taxes (respectively 0.82 and 0.56). Surprisingly, however, the correlation between them is rather low (0.05). This suggests that in most cases governments resorted either to direct or indirect taxes to consolidate the budget. Moreover, personal and corporate taxes and SSC also display very low correlations among each other. In this light, our aim to assess the composition effects of tax-based consolidations gains even more relevance.

[8]Property taxes show only small changes (relative to direct and indirect taxes) during tax-based consolidations. As a result we do not further investigate this tax component in our analysis.

Table 2: Mean changes of different tax instruments, 1978-2012

	Full sample	Tax-based	Large tax-based
Total	0.13	0.59	0.93
Direct	0.09	0.40	0.44
Indirect	0.02	0.16	0.38
Personal	0.01	0.20	0.15
Corporate	0.02	0.07	0.07
SSC	0.05	0.10	0.15
GT	0.05	0.12	0.20
SGS	-0.04	0.03	0.16

Notes: The tax-based sample comprises consolidation episodes in which tax hikes, as identified through the narrative approach, were larger than spending cuts. The large tax-based sample comprises the upper quintile of those episodes in which tax hikes were larger than spending cuts, according to the narrative approach. For a precise definition of the different tax categories refer to Appendix B.
Source: OECD Revenue Statistics database and authors' own calculations.

Table 3: Correlation of changes in tax revenues stemming from different instruments during tax-based consolidation years

	Total	Direct	Indirect	Personal	Corporate	SSC	GT	SGS
Total	1.00	0.82	0.56	0.52	0.45	0.46	0.35	0.16
Direct		1.00	0.05	0.73	0.53	0.35	0.07	-0.09
Indirect			1.00	-0.13	0.05	0.36	0.64	0.29
Personal				1.00	0.01	0.02	-0.05	-0.13
Corporate					1.00	-0.07	-0.07	0.13
SSC						1.00	0.17	0.03
GT							1.00	-0.35
SGS								1.00

Notes: SSC, GT and SGS stand for, respectively, social security contributions, general taxes and taxes on specific goods and services.
Source: OECD Revenue Statistics database and authors' own calculations.

Along with the series of tax shocks, we also collect other variables in our dataset. A standard measure of inequality in the empirical literature is the Gini index. The net Gini index is calculated on disposable income (i.e. after government taxes and transfers), while the market Gini index is calculated

on market income (i.e. before government taxes and transfers). We collect the net and market Gini indexes from the Standardized World Income Inequality Database (SWIID), which provides the most comparable series across countries. For data on the labour force participation and the unemployment rates, as well as for real per capita GDP, we rely on the OECD 2015 Economic Outlook. The labour force participation rate is defined as the percentage of the population aged between 15 and 64 years which is either employed or unemployed. Similarly, the unemployment rate is the share of jobless people between 15 and 64 years who are available to work and are actively seeking employment, expressed as a percentage of the labour force.[9]

To carry out some extensions and robustness checks, we collect additional data. As alternative inequality measures, we employ the share of income belonging to the richest 0.01%, 0.01-1%, and 1-10% individuals. Top income share data is constructed from tax filing and is compiled by the World Wealth and Income Database. Due to 20 missing values, we linearly interpolated top income share variables. To construct alternative tax shock variables we retrieve data on the standard VAT rate (or equivalent) for all the countries in our sample except the United States (the reason being that consumption taxes there are set by the states rather than the federal government). For European countries, we use information contained in Taxation and Directorate-General (2016). For Australia, Canada and Japan we use information available on their respective government's websites. To identify episodes of systemic banking crises, we use the dummy variable created by Laeven and Valencia (2012). Moreover, we obtain: (i) government consumption as a percentage of GDP, (ii) government spending in cash benefits as a percentage of GDP, (iii) consumer price inflation rate, (iv) employment rate defined as employed people as share of total population aged between 15 and 64 years, (v) average hours worked per employed individual, (vi) GDP per hour worked, from the OECD National Account database. Finally, we collect (i) imports and exports as a percentage of GDP, (ii) the trade balance as a percentage of GDP, (iii) gross fixed capital formation as a percentage of GDP, (iv) gross savings as a percentage of GDP from the World Bank's World Development Indicator database.

2.2. Methodology

For the econometric analysis we make use of PVAR models. By adopting a multi-equation methodology, we account for potential interactions among variables that might be otherwise overlooked within a single-equation framework.[10]

Given that our dataset is at the annual frequency, we estimate the VAR model in a panel format by pooling together observations for all the countries considered. This approach implies imposing

[9]The only exception is Austria, for which data for the 15-64 age group is not available before 1994. For this country, we use labour force participation and unemployment rates among all age groups.

[10]Several contributions in the literature employ the VAR methodology to estimate the macroeconomic effects of fiscal policy shocks and identify these through the narrative approach. For references, see Ramey (2011), Guajardo et al. (2014), and Alesina et al. (2015b).

cross-country homogeneity on the relationships among the endogenous variables. To take into account cross-country heterogeneity, we follow Beetsma and Giuliodori (2011) and include in the regressions a country-specific constant term and a country-specific linear time trend.[11] Additionally, we include time-fixed effects to control for common factors. Below, we show that our main results are robust to the inclusion of alternative deterministic components.

Our PVAR takes the following standard form:

$$A_0 y_{i,t} = A_1 y_{i,t-1} + A_2 y_{i,t-2} + \alpha_i + \delta_t + \tau_{it} + \epsilon_{i,t} \tag{1}$$

where the sub-indexes (i, t) refer respectively to country and time, $y_{i,t}$ is the vector of endogenous variables, $A's$ are the coefficient matrices, and α_i, δ_t and τ_{it} denote respectively country-fixed effects, time-fixed effects and country-specific linear time trends. Finally, $\epsilon_{i,t}$ is a vector of error terms, which are assumed to be serially uncorrelated. The baseline PVAR model includes five variables, namely the tax shock (as a percentage of GDP), the real per capita GDP (in logs), the disposable Gini index (in units), the unemployment rate and the labour force participation rate (both in percentage points).[12] Following a standard practice in the VAR literature on the macro effects of fiscal policy at the yearly frequency and consistently with the Akaike and Schwarz information criteria, we opt for a baseline specification containing two lags of the endogenous variables. After adjustments, and due to some missing data, we have a total of 479 observations.

To construct our tax shock variables, we first create a dummy variable d_t^1 taking value 1 in years where governments implement a tax-based consolidation and 0 otherwise. Second, we interact this dummy variable with the first difference of tax revenues as a percentage of GDP. We repeat this step for both total tax revenues and for each tax instrument. To sum up, our tax shock variables are constructed according to the following formula:

$$X_{i,t}^{j,1} = d_t^1 \Delta t_{i,t}^j \tag{2}$$

where $t_{i,t}^j$ denote revenues, as a share of GDP, stemming from tax instrument j, and Δ is the first difference operator.

At this point, it is worth making a consideration about the construction of the tax shocks. Admittedly, changes to the tax policy may influence the tax base as well as the business cycle, which in turn affect the level of revenues. Hence, relying on ex-post changes in tax revenues is an imperfect way to proxy tax policy changes. To use actual tax rates may appear as a better approach. However, actual

[11] We include linear trends since real GDP, the Gini index and the labour force participation all display a trending behaviour.

[12] Through time, many countries in our sample switched to a continuous survey to estimate the labour force participation and unemployment rates. This has generally resulted in an increase in labour force participation and a decrease in unemployment. To check whether this might bear an influence on our results, we estimate also the model including a dummy variable taking values 0 and 1 respectively in the years before and after the adoption of the continuous survey. The estimates derived from this extended specification did not significantly differ from those of our baseline specification, giving us confidence on the robustness of our results.

changes in tax rates are less frequent than commonly thought. When governments engineer tax policy changes, they often intervene on the tax base. This is especially true for direct taxes. For instance, tax credits, exemptions, or deductions are often introduced or removed, thus affecting the base. In the case of the personal income tax, nominal thresholds defining the different brackets can also be changed, and even not indexing them to the price level amounts to a change in the tax policy.[13] In the case of indirect taxes, different rates apply to different type of goods, and when governments modify the rates they may also decide to shift goods between different categories. In conclusion, proxying changes in the tax policy with tax rates only carries the risk of overlooking important policy actions. Lacking a quantifiable measure for changes in the tax base, we believe that using ex-post revenues is the most suited approach to address our research question. Although imperfect, since they are influenced by the behavior of economic agents and the response of the cycle, tax revenues capture governmental intervention on the base as well as the rate of taxes.

Given the characteristics of the action-based dataset, which identifies consolidation episodes that are motivated by the sole objective of reducing the budget deficit, the most natural way to identify the PVAR in equation (1) is to use a Cholesky decomposition. This strategy is particularly convenient when one of the variables is exogenous to the others, as in our case. By ordering our tax variable ordered first, we impose this to not be contemporaneously affected by GDP, the Gini index, the unemployment rate or the labour force participation rate. On the other hand, we allow these variables to be contemporaneously affected by the tax shock and by each other, thus capturing all potential feedback effects. Moreover, an important advantage of using the Cholesky decomposition is that the order of the variables after the shock does not matter (we refer the reader toChristiano et al. (1999) for a theoretical explanation).

In the following sections, we estimate impulse response functions (hereafter IRFs) to tax shocks over a 10-year horizon and construct confidence intervals as ± 1.645 standard errors (equivalent to a 90% confidence level) around the mean response.[14] To compute standard errors we use Monte Carlo methods with 1,000 replications. The GDP response is measured in percentage change, while the response of the Gini index is in units and the response of unemployment and labour force participation is in percentage points.

[13] For instance, in 1990 the Belgian government generated higher revenues equalling 0.3% of GDP by respectively broadening the base of the corporate income tax and not applying the indexation of the personal income tax parameters (Devries et al. (2011)).

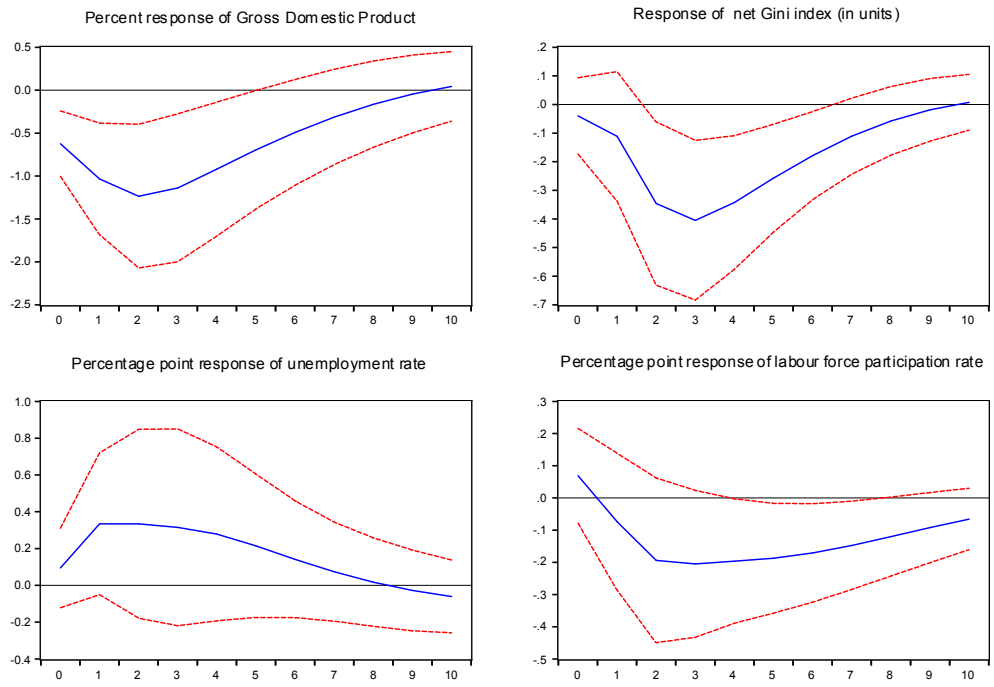
[14] Alternative approaches to construct confidence intervals are also accepted in the literature (for instance, Ball et al. (2013) use ± 1 standard errors), but we opt for a more conservative approach.

3. Overall effects of tax-based consolidations

3.1. Baseline results

In this section, we present our baseline results, namely the effects of change in overall taxation. Figure 1 shows the response to a 1% of GDP increase in total tax revenues during tax-based consolidations of real GDP, the Gini index, the unemployment rate and the labour force participation rate. The IRF of the Gini index is not statically different from 0 in the short run, but it turns negative and significant in the medium term (in the order of -0.4 after 3 years, and -0.3 after 5 years). In absolute value, the 0.4 drop in the Gini index after 3 years is equal to about a tenth of its sample standard deviation, whereas the cumulative change over the 11-year horizon is equivalent to a 6.4% reduction relative to the sample mean. Hence, our estimates suggest that tax-based consolidations have statistically significant and economically meaningful effects in decreasing disposable income inequality.

Figure 1: IRF to a 1% of GDP tax-based consolidation shock



Note: The central solid blue line represents the response to a 1% tax shock, the solid red lines represent the 90% confidence intervals.

However, higher equality is achieved at the cost of a slack in the economy. Looking at the other variables, a 1% of GDP increase in tax revenues has significant, although relatively mild, negative short-to-medium run effects on real GDP. Output decreases by 0.6% on impact, by 1% after 1 year, 1.1% after 3 years, and 0.7% after 5 years. The effect becomes insignificant in the long run. The response

of labour force participation is not significant on impact and in the short run, while it is negative and significant in the medium run (-0.2 percentage points after 5 years), which can be interpreted as a lagged consequence of the decrease in real economic activity. Finally, unemployment marginally increases in the short-to-medium term, but the coefficients are not statistically different from zero.

To better understand what drives our results, we also estimate the model using the market instead of the disposable income Gini index and we find similar responses.[15] This suggests that the decrease in inequality observed following tax-based consolidations is due to a reduction of market income disparities, rather than an increase in the amount of government redistribution. A possible explanation for this may be that capital owners, who normally are the richer agents in the economy, suffer the most from the drop in real activity. For instance, in countries characterized by a high degree of worker's protection , capital owners might find it too expensive to shed off labour and might decide to forgo income instead, especially if they expect the recession to be temporary. This would be consistent with the insignificant response of the unemployment rate that we estimate.

We also estimate the IRFs to large tax-based consolidations only (that is the upper quintile). To construct the respective tax shock variable, we replace d_t^1 in Equation 2 with d_t^2, a dummy variable taking value 1 in years of large tax-based consolidations and 0 otherwise. We present the results in Table 4, where bold numbers indicate significance at the 90% confidence interval.

The only qualitative difference concerns the impact response of the labour force participation rate, which is positive (+0.6) and significant during large consolidation episodes. A possible reason underlying the more sizeable response of the labour force participation during large tax-based consolidations is the larger government's reliance on indirect taxes during these episodes. That – as we will show later in the analysis – may create positive labour supply incentives. We will come back to this issue in Section 4, when we discuss the composition effects of tax-based consolidations. Concerning the other variables, the estimates are qualitatively similar but quantitatively larger than in the baseline specification. This suggests that there could be some non-linearities in the effects of tax hikes, with larger increases having relatively larger effects.

Overall, our IRFs analysis points to a medium-term trade-off between income equity and economic efficiency. This is in line with what was found by Agnello and Sousa (2014), who employ the SUR framework and find that tax-based consolidations have beneficial effects for income equality, which however disappear within two years.

3.2. Robustness checks

To verify the validity of our results we carry out a number of robustness checks on our baseline model. We present the tables with the results in Appendix C. To ease comparison, in Panel (a) of each

[15] We present these results later in the robustness check.

Table 4: The effects of large tax-based consolidations - Response to a 1% of GDP tax shock

	Impact	1y	3y	5y	10y
GDP	**-1.39**	**-2.95**	**-3.28**	**-2.57**	-0.40
Net GINI	**-0.27**	**-0.58**	**-0.95**	**-0.68**	-0.10
Unemployment	**0.64**	**1.65**	**1.77**	**1.35**	0.13
Participation	**0.62**	0.11	-0.36	**-0.53**	**-0.32**

Notes: The table reports the response on impact and after 1, 3, 5 and 10 years to a 1% of GDP increase in total taxes revenues during large tax-based consolidations. Bold numbers indicate significance at the 10% confidence level.

table we report estimates from our baseline specification.

In Table C1, we estimate the response to a 1% of GDP tax revenues increase during different consolidation episodes. First, a criticism that might be raised is that our analysis focuses only on tax-based consolidations, while governments normally implement a mix of tax and spending measures to rein in public finances. In Panel (b) and Panel (c) of Table C1 we report estimates to a total tax revenue shock during both any consolidation years and spending-based consolidation years. The estimated coefficients suggest that the contemporaneous presence of spending cuts and tax hikes might confound the results concerning the effects of tax shocks on the economy. This is why we exclusively consider tax-based consolidation years in our baseline. Second, we address the potential concern that anticipation effects may bias our results. To this end, we use information contained in Alesina et al. (2015b) and Alesina et al. (2015a) to identify unanticipated tax-based consolidations (i.e. decided at year t for implementation in the same year) and we estimate relevant IRFs. The results (Panel (d)) are qualitatively similar to those of our baseline.[16] Third, we show that our results are not influenced by those consolidation episodes that resulted in a decrease in revenues by estimating IRFs to a 1% of GDP increase in taxes during only those tax-based consolidations that generated higher revenues (Panel (e)).

In Table C2, we report IRFs obtained using alternative tax shock variables. To control for potential endogeneity in the response of tax revenues to the business cycle, we estimate the model using cyclically-adjusted revenues and find results very similar to our baseline (Panel (b)).[17] As a further check, and

[16] Admittedly, although not statistically different from our baseline, the response of the net Gini index becomes insignificant. We believe this owes to the larger increase in unemployment, which partly erodes the beneficial effects of higher taxes for income equality. Moreover, by focusing on unanticipated tax-based consolidations only, we substantially reduce the number of consolidation episodes, namely from 69 to 43.

[17] Cyclically-adjusted tax revenues are computed according to the following formula (see OECD (2015a)):

$$t_t^{ad,i} = t_t^i (y_t^n / y_t)^{\varepsilon^i} \tag{3}$$

where $t_t^{ad,i}$ and t_t^i respectively stand for cyclically and not cyclically-adjusted tax revenues stemming from tax instrument i; y_t^n is potential per capita output (derived from the IMF output gap measure); y_t is real per capita output and ε_i refer to the elasticity of tax instrument i. Elasticities are taken from the OECD Economic Outlook database inventories (OECD (2015a)).

in order to more easily compare our results with those of Woo et al. (2013) and Agnello and Sousa (2014), we also estimate the model employing the original real-time data collected by Devries et al. (2011) and Alesina et al. (2015a). We do so both for the sample of all tax-based consolidations (Panel (c)), and for the two samples of tax-based consolidations that resulted respectively in an increase and a decrease in tax revenues (Panels (d) and (e)). The response of the Gini index is qualitatively is in line, although quantitatively smaller in absolute value, to our baseline estimation for all samples except that of tax-based consolidations that resulted in a decrease in tax revenues. Interestingly, the negative response of GDP is more pronounced for those consolidations that resulted in an increase rather than for those that resulted in a decrease in revenues. This weakens the hypothesis that some consolidations caused revenues to shrink due to their contractionary effects on the economy (otherwise we would likely observe a stronger negative response of output). Instead, the hypothesis that in some cases policy makers have backtracked from their original plans seems more plausible.[18] Therefore, including those episodes might introduce a downward bias (in absolute value) in the estimation when using real time data.

Next, we observe that tax-based consolidations occurred over a period spanning one or more consecutive years (see Figure A2 in Appendix A). This could potentially introduce a bias in the estimation. To understand why, consider a consolidation cycle spanning from period t to $t+1$. If the consolidation of period $t+1$ was decided by the government in the same period, after observing the outcome of the consolidation at t, our results would be biased due to reverse causality. To circumvent this problem, Ball et al. (2013) employ a dummy taking value 1 in the first year of the tax-based consolidation cycle and 0 otherwise as shock variable. However, their approach has two main drawbacks. First, it treats all consolidation cycles as if they were equal in size and length. Second, it unnecessarily sacrifices a large number of observations. An alternative approach is to simply exclude all consolidation years that might suffer from reverse causality issues. To do so we used information contained in Alesina et al. (2015b) and Alesina et al. (2015a) in order to construct a shock variable which takes value 0 in all years of tax-based consolidations that were (i) unanticipated (i.e. decided at year t for implementation in the same year), (ii) part of a multi-year consolidation cycle, and (iii) not the first year of such cycle.[19] Next, we estimate the model using both a dummy variable à la Ball et al. (2013) and our alternative shock variable. We show results in C3 In both cases the IRFs are qualitatively similar to our baseline, although as expected some quantitative differences emerge when using the dummy à la Ball et al. (2013). We conclude that our baseline results do not suffer from a reverse causality bias.

[18] Indeed, for 6 of the 14 episodes that resulted in lower revenues the decrease was smaller than 0.2 percentage points.

[19] The shock variable is constructed according to the following formula:

$$X_{i,t}^{j,3} = d_t^1(1 - d_t^u(1 - d_t^f))\Delta t_{i,t}^j \tag{4}$$

where d_t^u and d_t^f are two dummy variables taking value 1 in every year of unanticipated tax-based consolidations and in each first year of a tax-based consolidation cycle 0 otherwise respectively.

We also check the robustness of our baseline results to the inclusion of different deterministic components (Table C4) and the usage of local projections as an alternative estimation method (Table C5).[20] When estimating IRFs from local projections we employ both our standard shock variable (Panels (b) and (c) for results with and without control variables) and the dummy à la Ball et al. (2013), so as to directly compare their results with ours (Panel (d)). In all cases, the IRFs obtained using local projections are qualitatively similar to those generated by the PVAR methodology. Furthermore, in Table C6 we show that our results are not driven by particular groups of countries, time periods, or type of shocks. More specifically, we repeat the estimation by excluding from the sample, in turn, (i) the period following the global financial crisis (2008-2012), (ii) non-EU countries, (iii) shocks occurring during, or 1 or 2 years after, systemic banking crises, and (iv) shock outliers. We also run the baseline regression by dropping one country at a time (Figure C1).

Next, we show that our results are robust to using alternative endogenous variables and to the inclusion of several control variables. First, we estimate the model employing GDP per hour worked, average hours worked by employed individuals and the employment rate, instead of the GDP, unemployment and participation (Table C7). This exercise confirms the validity of our baseline results and suggests that the decline of real economic activity observed following tax-based consolidations is due to a drop in productivity. Second, we verify that our results are not biased by the omission of variables commonly used in the literature as a proxy for the degree of a country openness (import plus exports as a percentage of GDP), of the progressivity of its tax system (the ratio of direct-to-indirect tax revenues) and other macroeconomic conditions (Tables C8 and C9).

Finally, we check whether our baseline result, pointing out that tax-based consolidations reduce income inequality, is robust to different measures of inequality. To this purpose, we use top income shares as proxies for income inequality. However, this robustness check is not without caveats. First, these variables are based on market income, that is before taxes and transfers, rather than disposable incomes. Second, due to data availability, the analysis is limited to 10 countries of our sample (Aus-

[20] To estimate IRFs directly from local projections we employ the original specification ofJordà (2005) and augment it with the correction proposed by Teulings and Zubanov (2014). Omitting such correction would leave the model misspecified and thus introduce a bias. To understand this point, consider a country i featuring only one fiscal policy shock at $t = 2$. When estimating an IRF(k) using the Jordà (2005) specification, the estimator for $k = 1$ will be biased, since for $t = 1$ y_2 is already affected by the shock but this does not appear among the regressors. Hence, after including the Teulings and Zubanov (2014) correction, we estimate the following equation (with $k = 1, .., 11$, being the time horizon):

$$y^j_{i,t+k} = c + \sum_{l=1}^{2} \beta^{j,k}_l y^j_{i,t-l} + \gamma^{j,k} X_{i,t} + \sum_{l=1}^{k} \theta^{j,k}_l X_{i,t+l} + \alpha_i + \delta_t + \sum_{l=1}^{2} \varphi^{j,k}_l Z^j_{i,t-l} + \tau_{it} + \epsilon_{i,t} \quad (5)$$

where $y^j_{i,t}$ denotes, in turn, the log of real GDP per capita, the Gini coefficient, the unemployment and the labour force participation rate; $s_{i,t}$ denotes the shock variable (either the change in total tax revenues during tax-based consolidation years or a dummy variable taking value 1 for the first year of a consolidation cycle); the term $\sum_{l=1}^{k} \theta^{j,k}_l X_{i,t+l}$ represents the Teulings and Zubanov (2014) correction; $Z^j_{i,t-l}$ is a vector of the other endogenous variables used as controls; as in the PVAR specification, α_i, δ_t, τ_{it} denote, respectively, country-fixed effects, time-fixed effects and country-specific trends. To obtain the IRFs and construct confidence bands, we use respectively the estimated $\gamma^{j,k}$ coefficients and $\pm$ 1.645 cross-section heteroskedasticity robust standard errors.

tralia, Canada, Denmark, France, Germany, Italy, Japan, Portugal, Sweden and the US).[21] Bearing in mind these data availability limitations, we present the main results in Table C10 (Panel (c), (d) and (e)). For better comparability, we also report estimates obtained using the market income Gini index as inequality measure (Panel (b)). Our estimates indicate that both the richest 0.01% and 0.01-1% richest individuals significantly lose from tax-based consolidations (after one year the reduction in their income share relative to the sample mean is respectively equivalent to about 13% and 3.8%).[22] On the other hand, we do not find significant effects for the income share of the 1-10% richest agents. Overall, this exercise confirms the intuition that tax-based consolidations reduce inequality due to negative effects on the market income of capital owners (who are the individuals in the right tail of the income distribution).

4. Composition effects of tax-based consolidations

4.1. Main results

In the previous section, we have analysed the effects of changes in overall taxes during fiscal consolidation episodes. In what follows we disentangle the effects of single tax instruments. As a first step, we estimate IRFs to a 1% (of GDP) shock in direct and indirect taxes. Results are presented in Table 5.

Unexpectedly, direct taxes do not have significant positive effects in reducing disposable income inequality, while indirect taxation does improve income distribution. A 1% increase in indirect taxes significantly reduces the Gini index over the short-to-medium term. After 1 year, the Gini index decreases on average by 0.6. After 3 years the decline is equal to 1, while after 5 years it is again 0.6. Regardless of the instrument used by governments, a tax shock has always a negative and significant effect on real GDP on impact. However, this effect is stronger and more persistent in the case of indirect taxes. Consistently with the large and persistent decline in output, the unemployment rate significantly increases in the medium term in the case of indirect taxes. More surprisingly, instead, the labour force participation rate increases by 0.4 percentage points on impact after an indirect tax shock. On the other hand, neither the unemployment rate nor the participation rate display significant reactions to a direct tax shock.

[21] Top income share data for Spain are also available. However, we exclude this country since it seems to introduce a bias in the results.

[22] This is confirmed when we use average incomes rather than shares, which highlights that the richest individuals do not only lose in relative terms, but also in absolute terms. We also notice that when we use top income shares, inequality seems to decrease faster than when we use the Gini index. This might be due to the fact that top income shares are estimated based on yearly data, whereas the Gini index provided by the SWIID is constructed through imputation, with the original data being available only at 3 to 5-year intervals.

Table 5: Composition effects of tax-based consolidations

	Impact	1y	3y	5y	10y
a) Total taxes (baseline)					
GDP	**-0.62**	**-1.03**	**-1.14**	**-0.70**	0.04
Net Gini	-0.04	-0.11	**-0.40**	**-0.26**	0.01
Unemployment	0.09	0.33	0.31	0.21	-0.06
Participation	0.07	-0.07	-0.20	**-0.19**	-0.07
b) Direct taxes					
GDP	**-0.58**	-0.79	-0.98	-0.49	0.16
Net Gini	0.04	0.09	-0.30	-0.22	0.03
Unemployment	0.09	0.26	0.10	0.03	-0.12
Participation	0.01	-0.14	-0.26	-0.18	-0.03
c) Indirect taxes					
GDP	**-0.90**	**-1.80**	**-2.24**	**-1.90**	-0.39
Net Gini	-0.28	**-0.67**	**-0.99**	**-0.64**	-0.07
Unemployment	0.17	0.67	**1.14**	**0.94**	0.14
Participation	**0.36**	0.21	-0.09	-0.31	**-0.23**

Notes: The table reports the response to a 1% of GDP tax shock respectively in total taxes, direct taxes and indirect taxes. Bold numbers indicate significance at the 10% confidence level.

In general, these estimates suggest that our baseline results are mainly driven by indirect taxes.[23] The estimated impact of indirect tax hikes on real GDP are supported by recent theoretical works. In analysing fiscal policy rules in a new Keynesian model with labour market frictions, Gehrke (2014) finds that multipliers are large for indirect taxes, such as consumption taxes, while they are small for direct taxes, such as labour taxes. As regards the impact on inequality, instead, the literature generally assumes indirect taxes to be regressive. The reasoning goes as follows: compared to high-income households, low-income agents normally spend a larger fraction of their income on consumption goods. Hence, by increasing the price of the consumption basket, indirect taxes are expected to increase inequality. Following this argument, however, in order to find a direct positive effect of indirect taxes on inequality a measure of consumption-based inequality should be used. Unfortunately, scarce data availability prevents us to investigate further the validity of this line of thought.

[23] Our results are only partly in line with what found by Muinelo-Gallo and Roca-Sagalés (2013), who, however, analyse the effects of budget-neutral changes in fiscal policy rather than fiscal consolidations. More specifically, they adopt a system of simultaneous equations to account for the interdependence between inequality, growth and fiscal policy in a panel of 21 OECD countries between 1972 and 2006. This approach allows them to model (i) gross inequality as a determinant of fiscal policy, (ii) fiscal policy as affecting both growth and net inequality and (iii) net inequality as being endogenous to growth and vice versa. Still, their model has some limitations. For instance, while net inequality is endogenised, gross inequality is assumed to be exogenous, whereas it may well be endogenous to both growth and fiscal policy. Furthermore, the model neglects potential dynamic effects. Muinelo-Gallo and Roca-Sagalés (2013) conclude that an increase in direct taxes has a negative impact on both net inequality and growth. Instead, increasing indirect taxes does not seem to have significant effects.

Instead, we find that indirect taxes reduce income inequality. We believe that this outcome may be partly due to the operation of a positive labour supply channel. In fact, contrary to the main literature, our results suggest that indirect taxes create incentives for agents to more actively participate in the labour market, due to a negative income effect. This effect is likely to be stronger for low-income agents as they spend a larger fraction of their income on consumption goods and hence are relatively more affected by an increase in indirect taxes.

To shed more light on the labour market effects of indirect taxes, we estimate four additional 6-variable PVARs (we report results in Table D1 in Appendix D). In the first one, we add the inflation rate. This goes up by 1.3 percentage points both at impact and after one year, thus confirming that the price of the consumption basket does increase following an indirect tax hike. Next, we include the log of hours worked per person employed. Hours worked significantly decline by 1% one year after the shock, while the impact response of labour force participation is still positive and significant. These may seem like contrasting results. However, the timing is important: participation increases immediately after the shock, whereas hours worked decline with a 1-year lag. Moreover, the number of hours worked depends both on individuals' labour supply decision and firms labour demand, which makes it difficult to establish what causes its decline. In the first stages of a recession in an economy characterized by rigid labour markets, firms may find it more convenient to first reduce the number of hours worked and only after lay off workers, which is what we observe. As far as income inequality is concerned, the increase in labour force participation and the reduction in average hours worked should generally support the result that inequality declines following an indirect tax shock, as more individuals are likely to work and the agents that were already employed work less.

As a further extension, we distinguish between women and men labour force participation rates. In accordance with the several contributions emphasizing higher participation elasticities for women (see for instance Bargain et al. (2011)), we find a significant impact response of 0.5 percentage points for women and an insignificant one of 0.3 percentage points for men. Finally, in order to check that higher participation is actually reflected in higher employment, we estimate the model including employment rates as percentages of the population, rather than the labour force, as it is commonly done. Women employment increases 0.5 percentage points on impact, whereas the change in men employment is not significant. In the medium term, as the depth of the recession gets larger, both male and female employment rates significantly decrease. Importantly, however, the magnitude of the declines is similar for women and men, which means that the initial relative employment gain for women is not reversed.[24] These findings confirm our earlier hypothesis about the existence of a positive labour supply channel and further suggest that by boosting women participation and employment indirect taxes might also

[24]It would be interesting to also distinguish between hours worked by women and men. Unfortunately scarce data availability does not permit this type of analysis.

reduce gender inequality.[25]

Before narrowing the analysis further down, we perform a number of robustness checks. Instead of presenting results in the main text, we show the relevant tables in Appendix E. First, we address a possible criticism regarding our methodology. Introducing only one shock at a time (i.e. either direct or indirect taxes) might lead to neglect potential interaction effects among the different tax instruments. Although the low correlation between direct and indirect tax shocks during tax-based consolidation years (see Table 3) makes this line of argument implausible, we check whether our results remain valid once including both shocks simultaneously. The new estimates (reported in Table E1) highlight the robustness of our results to this new specification. Further, we check whether our results are driven by a particular country and estimate the model excluding one country at a time.[26] We conclude that our results are robust.

Finally, we estimate the model using the market income Gini index and the top income shares as inequality measures. IRFs are presented in Table E2 (for direct taxes) and E3 (for indirect taxes). The results are broadly in line with what found earlier. Direct taxes significantly reduce the share of income of the very rich agents (the top 0.01%), by 0.1 percentage points at impact and after one year.[27] On the other hand, indirect taxes do not significantly reduce the share of the top 0.01% income earners, but do have some short-term significant negative effects on the share of income of the richest 0.01-1% and 1-10% individuals.

4.2. Direct taxes

In this section, we examine more in detail the effects of direct taxes. In particular, we focus on the impact of personal taxes, corporate taxes and SSC. Our analysis so far has suggested that direct taxes have only a significant negative impact effect on GDP. However, it may be that different instruments have different effects. For instance, personal taxes are generally thought to be more progressive – and therefore more redistributive – than SSC. This is all the more true in those countries where SSC are directly used to finance the future pensions or where governments call for a cap on the maximum taxable income for SSC. On the other hand, corporate taxes may have ambiguous effects on income inequality, as capital income owners may shift the tax burden on wage earners (see Bastagli et al. (2012) for a discussion).

[25] An alternative, but not mutually excluding, channel may be working through the effects of indirect taxes on inequality via capital income and labour demand. The fall in GDP and the rise in unemployment might affect high-skilled workers and capital owners particularly strongly, thereby lowering their market incomes more than for low-income households. This would also lead to a reduction in measured inequality.

[26] Figures available upon request.

[27] At this respect we notice that given the likely capacity of high-income earners to shift income over time and country (see also Atkinson et al. (2011)), this result could also be due to increased tax avoidance practices, rather than actual changes in income of the top earners.

Table 6 shows the estimates to a 1% of GDP shock in personal taxes, corporate taxes and SSC.[28] To ease the comparison, we also display the baseline results with total direct taxes in Panel (a). Results in Table 6 point out that only personal taxes have some significant effects on the Gini index. More specifically, following a 1% of GDP increase in personal taxes, the Gini index decreases by 0.7 after 3 years and by 0.4 after 5 years. Real activity also drops in the short-term, with GDP declining by 0.7% on impact and 1.2% after 1 year. On the other hand, neither the unemployment rate nor the labour force participation rate are significantly affected.

Table 6: Composition effects of tax-based consolidations - Direct taxes

	Impact	**1y**	**3y**	**5y**	**10y**
a) Direct taxes					
GDP	**-0.58**	-0.79	-0.98	-0.49	0.16
Net Gini	0.04	0.09	-0.30	-0.22	0.03
Unemployment	0.09	0.26	0.10	0.03	-0.12
Participation	0.01	-0.14	-0.26	-0.18	-0.03
b) Personal taxes					
GDP	**-0.73**	**-1.18**	-1.19	-0.71	-0.05
Net Gini	-0.10	-0.14	**-0.69**	**-0.39**	0.02
Unemployment	0.19	0.38	0.00	0.05	-0.01
Participation	-0.11	-0.15	-0.26	-0.21	-0.07
c) Corporate taxes					
GDP	-0.25	0.93	1.16	1.20	0.47
Net Gini	0.23	0.33	0.03	0.02	0.06
Unemployment	-0.29	-0.40	-0.44	-0.40	-0.04
Participation	-0.02	-0.24	-0.27	-0.04	0.06
d) Social security contributions					
GDP	-0.75	-1.25	-1.35	-0.70	0.09
Net Gini	0.16	0.38	0.16	0.01	0.01
Unemployment	0.31	0.74	-0.03	-0.21	-0.24
Participation	0.20	-0.18	-0.19	-0.07	0.08

Notes: The table reports the response to a 1% of GDP tax shock in different components of direct taxes. Bold numbers indicate significance at 10% level.

Since personal income taxes, which constitute the bulk of personal tax revenues, are progressive in all the countries of our sample we would expect them to have significant redistributive effects. However, when we estimate the model using the market Gini index, we find results similar to the

[28] These results are robust to different PVAR specifications in which several shocks enter at the same time (estimates available upon request). This comes as no surprise given the small correlation coefficients between SSC, personal and corporate taxes (see Table 3).

original specification with the net Gini index, which suggests that the reduction in inequality is achieved thanks to decreasing market income disparities, rather than an increase in government redistribution. This result is confirmed when we use top income shares as measures of inequality.[29] There are several factors that could determine the extent of the redistributional effects of higher personal tax revenues. For instance, a higher marginal top personal income tax rate should increase redistribution. On the contrary, if the government reduces personal allowances or tax credits the system is likely to become less progressive. Hence, whether personal taxes should have redistributive effects largely depends on which particular measure is introduced.[30]

An increase in corporate taxes and SSC does not cause a significant reaction in any of the variables considered. Since capital profit earners have normally higher incomes than wage earners, in principle one would expect that corporate taxes should reduce inequality. However, recent empirical evidence suggests that in advanced economies capital profit earners manage to shift between 45% and 75% of the corporate tax burden to the employees' wages (Bastagli et al. (2012)). This would explain the rather muted response in the Gini index.[31]

4.3. Indirect taxes

Indirect taxes comprise several instruments. We focus on GT and SGS taxes. The former are levied on most goods. Therefore, GT hikes should induce a generalized increase in the price of the representative items composing the agents' consumption basket. Instead, the latter are normally levied on goods – such as gambling, gasoline, alcohol and tobacco – on which low-income households spend a large fraction of their income. Interestingly, during years of tax-based consolidations governments relied on SGS taxes only to a small extent (refer to Table 2). Conversely, SGS taxes made up for almost half of the overall increase in indirect taxes during large tax-based consolidations. This suggests that governments prefer using such instruments only in extreme circumstances.

Concerning the potential impact of GT and SGS taxes on income inequality, we do not have a particular prior. Both of them are expected to increase consumption inequality. For what concerns income inequality, instead, the potential effects are more ambiguous and likely to depend on the contemporaneous responses of real economic activity and labour market variables. On the one hand, higher GT and SGS taxes decrease the marginal return of labour. Hence, agents might respond by substituting away labour for more leisure time (i.e. substitution effect). On the other hand, since

[29] Results are available upon request.

[30] Using Tax Policy Center data on the top marginal income tax rate, we checked that this was increased and decreased an equal number of times (namely 6) during tax-based consolidation years in the countries of our sample. This suggests that increasing personal tax revenues does not necessarily imply making the tax system more redistributive.

[31] Our results are in line with Martínez-Vázquez et al. (2012), who analyse how changes in tax revenues affect inequality in a panel of 150 countries over the period 1970-2009 using the Generalized Method of Moments estimation. They find that personal income taxes have a significant negative effect on income inequality. The effects of corporate taxes in reducing inequality, instead, are estimated to be weaker in more open economies. Differently from our results, SSC are found to be positively associated with income inequality.

agents' real income decreases, they could respond by supplying more labour (i.e. income effect). Our previous findings suggest that overall the income effect may dominate the substitution effect. In addition, if labour demand is more elastic for higher and middle than for lower-income earners, a composition effect could occur. Hence, we now investigate whether these results hold for both the sub-components of indirect taxes.

Estimates for GT and SGS tax shocks are presented in Table 7 (Panel (b) and (c)). In the medium term, GT significantly lower inequality, with the Gini index decreasing 0.6, 1.1 and 0.7 after respectively 1, 3 and 5 years. At the same time, GT produce a statistically significant decline in real economic activity only after 5 years. The response of labour participation is positive and significant on impact and gets even stronger after 1 year. Therefore, the income effect seems to dominate the substitution effect.

To provide further empirical evidence backing these results we estimate responses to a 1 percentage point increase in either the standard VAT rate or the goods and service tax (GST) rate during tax-based consolidation years.[32] We report IRFs in Table 8 below. The estimates confirm what already emerged above. A 1 percentage point increase in the standard GT rate significantly raises the labour force participation rate by 0.2 percentage points both at impact and 1 year after the shock, while it significantly reduces the net Gini coefficient by 0.1 after 6 years. All in all, our results seem to suggest that GT might be a successful tool for policy-makers to consolidate the budget, while at the same time improving equity and limiting the detrimental effects on economic activity.

To summarise, the underlying mechanism that we have in mind works as follows: a hike in GT pushes up inflation and hence decreases households' real income. The income loss creates higher incentives for agents voluntarily out of the labour force to search for a job. In turn, as agents join the labour force, their probability of becoming employed increases. In OECD countries we believe the agents that voluntarily decide to stay out of the labour force to be mostly married women in low- and high-, but not mid-income, households. Provided that participation elasticities are stronger in low-income households, this could explain the observed improvement in income distribution.

Turning to SGS taxes (Panel (c)), the estimates we obtain are more difficult to rationalize. SGS taxes have larger and more immediate effects in reducing income inequality. The Gini index significantly decreases by 0.8 on impact and it keeps declining over a 5-year horizon. The response of real economic activity is strongly negative and significant over the short-to-medium term. Parallel to the deep contraction, the unemployment rate significantly increases by around 2 percentage points in the

[32]Several countries introduced a GT tax only after the beginning of our sample (this is the case for Australia (2000), Canada (1991), Finland (1994), Japan (1989), Portugal (1986) and Spain (1986)). Moreover, in the US GST rates are decided at the State level. Hence we exclude this country from this analysis. In total, we count 15 instances of tax-based consolidations that resulted in a change in the standard GT rate in the 15 countries of our sample excluding the US. Of these 15 episodes we decided to exclude 1, Ireland in 1984, since in that occasion the government drastically overhauled the VAT system (that is, it decreased the standard rate by 12 percentage points but it also cut the number of reduced rates from 5 to 2, which could confound the effects of the change in the standard rate).

Table 7: Composition effects of tax-based consolidations - Indirect taxes

	Impact	1y	3y	5y	10y
a) Indirect taxes					
GDP	**-0.90**	**-1.80**	**-2.24**	**-1.90**	-0.39
Net Gini	-0.28	**-0.67**	**-0.99**	**-0.64**	-0.07
Unemployment	0.17	0.67	**1.14**	**0.94**	0.14
Participation	**0.36**	0.21	-0.09	-0.31	**-0.23**
b) GT (VAT and sales)					
GDP	-0.63	-0.77	-1.74	**-1.97**	-0.87
Net Gini	-0.15	**-0.62**	**-1.08**	**-0.72**	-0.14
Unemployment	-0.07	0.01	0.68	0.83	0.37
Participation	**0.41**	**0.57**	0.41	-0.06	**-0.24**
c) SGS taxes					
GDP	-0.82	**-3.66**	**-4.14**	-2.82	0.20
Net Gini	**-0.78**	**-1.14**	**-1.43**	**-0.91**	0.04
Unemployment	0.53	**2.08**	**2.24**	1.56	-0.14
Participation	0.42	0.02	-0.92	**-0.92**	-0.34

Notes: The table reports the response to a 1% of GDP tax shock in different components of indirect taxes. Bold numbers indicate significance at the 10% confidence level.

short run, while the labour force participation rate significantly decreases after 5 years. We expect that these two dynamics should exacerbate inequality. Hence, the only possible channel explaining the observed decline in the Gini index is the contraction in real economic activity. This could be explained by deep recessions hitting high-income agents more strongly than low-income agents, thereby causing a decrease in disposable income inequality.[33]

Table 8: Additional results on GT taxes

	Impact	1y	3y	5y	10y
a) GT rate shock					
GDP	**-0.44**	-0.40	-0.64	-0.58	-0.17
Net GINI	-0.09	-0.10	-0.19	-0.15	-0.03
Unemployment	0.20	0.27	0.39	**0.32**	0.06
Participation	**0.23**	**0.22**	-0.02	-0.08	-0.06

Notes: The table reports the response to a 1 percentage point increase in the standard GT rate. Bold numbers indicate significance at the 10% confidence level.

[33]Our results concerning consumption taxes are not in line with Martínez-Vázquez et al. (2012), who find that GT and SGS have, respectively, positive and not significant effects on inequality.

5. Do governments' preferences for redistribution matter?

So far, we have shown that tax-based consolidations lower income inequality mainly through a reduction in market income disparities, rather than an increase in government redistribution. This was perhaps surprising since higher tax revenues are generally thought to be associated with higher redistribution. However, whether tax-based consolidations increase or decrease government redistribution might depend on the particular measures adopted by governments to raise revenues and on the operation of automatic stabilizers, such as unemployment benefits. In turn, these are likely to differ among the countries in our sample according to political and cultural preferences. So far, we have estimated the average effects of tax-based consolidations on income inequality in the full country sample. We now extend the analysis by investigating whether the final outcomes, as well as the channels through which tax-based consolidations impact inequality, differ across countries depending on the government's preference for redistribution.

Measuring preferences for redistribution in not straightforward. The empirical literature has mostly relied on the ratio of direct-to-indirect tax revenues as a measure of progressivity (see for instance Woo et al. (2013)). However, as we discussed above, higher direct tax revenues do not necessarily imply a more progressive system. Furthermore, the direct-to-indirect tax ratio does not account for the expenditure side of fiscal policy. We believe that in order to capture the government's preferences for redistribution, the difference between the market income and the disposable income Gini index (henceforth the redistribution Gini index) is better suited. In fact, this can be interpreted as a measure of the reduction of disposable income inequality that is achieved through taxes and transfers, with higher values indicating more redistribution.

Table F1 in Appendix F reports country averages and the sample median of the redistribution Gini index as well as the ratio of direct-to-indirect tax revenues. Interestingly, we notice that countries with relatively high scores in standard inequality measures, such as Australia, Canada and the United States, have direct-to-indirect tax ratios above median. On the contrary, countries with relatively low inequality scores, such as Finland, France and Germany, have ratios below median. The correlation between the redistribution Gini index and the ratio of direct-to-indirect tax revenues is even negative, at -0.36, underscoring the importance of choosing the right measure to proxy preferences for redistribution.

We now split countries in two groups, depending on whether their sample average redistribution Gini index is below or above sample median.[34] In Table F2 in Appendix F, we show the average values of tax shocks in tax-based consolidation years for the two groups of countries. Interestingly, tax-based consolidations were more frequent among low-redistribution countries. Furthermore, these countries

[34]It could be argued that government preferences for redistribution vary over time and so does the redistribution Gini index. However, we are interested in relative country scores, and these do not change over time.

relied more on direct taxes, and in particular personal taxes, than high-redistribution ones. This further reinforces our observation that for determining progressivity and redistribution it is important not only which tax instrument is used but also how it is used.

In what follows, we estimate both our 5-variable baseline model with the net Gini index and a 6-variable PVAR featuring both the market income and the redistribution Gini index. A possible criticism to this approach is that the redistribution index is a measure artificially constructed from the market Gini index. We also estimated two 5-variable PVAR with only, respectively, the market and redistribution Gini index and the results did not change. We present IRFs to a 1% GDP increase of total tax revenues in Table 9 below.[35]

Table 9: The effects of tax-based consolidations in low versus high redistribution countries

	Impact	**1y**	**3y**	**5y**	**10y**
a) Total - redistribution below median (Net Gini)					
GDP	-0.40	-0.66	-0.70	-0.40	0.04
Net Gini	-0.06	-0.09	**-0.41**	-0.21	0.07
Unemployment	-0.06	0.19	0.01	-0.10	-0.09
Participation	0.05	-0.10	-0.15	-0.13	-0.04
b) Total - redistribution above median (Net Gini)					
GDP	**-0.79**	-1.28	-1.77	-1.45	-0.11
Net Gini	**-0.22**	**-0.56**	**-0.65**	**-0.50**	-0.16
Unemployment	0.28	0.56	**1.02**	**0.86**	0.16
Participation	0.13	0.17	-0.26	**-0.34**	-0.21
c) Total - redistribution below median (Market and Redistribution Gini)					
GDP	**-0.44**	**-0.74**	-0.79	-0.45	0.12
Market Gini	-0.12	-0.27	**-0.74**	-0.28	0.18
Redistribution Gini	-0.05	-0.16	**-0.32**	-0.10	0.11
Unemployment	-0.07	0.17	0.00	-0.08	-0.12
Participation	0.07	-0.09	-0.16	-0.17	-0.02
d) Total - redistribution above median (Market and Redistribution Gini)					
GDP	**-0.90**	-1.44	-1.47	-1.16	-0.14
Market Gini	0.00	0.26	-0.03	0.14	0.03
Redistribution Gini	0.27	**0.89**	**0.69**	**0.65**	0.15
Unemployment	0.28	0.56	0.90	0.73	0.15
Participation	0.05	0.09	-0.29	**-0.36**	-0.22

Notes: The table reports the response to a 1% of GDP tax shock respectively in total taxes. Redistribution Gini stands for the difference between the market and the disposable income Gini index. The analysis addresses separately countries characterized by high and low preference for redistribution. Bold numbers indicate significance at the 10% confidence level.

[35]Results for direct and indirect taxes are available upon request.

When looking at the model with only the net Gini index, the results point again to the usual trade-off between equity and efficiency. In countries with a higher preference for redistribution, GDP significantly drops on impact; the response of the unemployment rate is positive and significant in the medium term, whereas the reaction of the net Gini index is negative and significant already on impact and is persistent in the medium term. In countries characterized by a low preference for redistribution, instead, a 1% of GDP increase in total taxes affects the net Gini index only marginally and has significant effects only 5-year after the consolidation. The response of all the other variables is not significant.

We now attempt to disentangle the channels determining the final inequality outcome by looking at the IRFs derived from the model including both the market income and the redistribution Gini index. Interestingly, in low-redistribution countries tax-based consolidation seems to induce a reduction in market income disparities, with the market Gini index always negative and significant, decreasing by 0.7 3 years after the shock. However, such decline is partially offset by a reduction in government redistribution, which is proxied by the redistribution Gini index. This is negative and significant, at -0.3, after 3 years. A possible explanation for this result is that either the tax system becomes less progressive or government transfers decline.

The picture is very different when we look at high-redistribution countries. We now observe a significant increase in the redistribution Gini index. This goes up by 0.9, 0.7 and 0.6 respectively, 1, 3 and 5 years after the shock. The market income Gini, instead, is generally positive but not significant. This indicates that, in high-redistribution countries, the observed reduction in inequality comes from an increase in government redistribution, rather than a reduction in market income disparities. The different responses of the market and the redistribution Gini indexes in countries with a lower and a higher preference for redistribution is confirmed when we analyse the effects of direct and indirect taxes.[36]

Our findings point to the existence of different country-specific channels through which tax-based consolidations reduce disposable income inequality. These do not seem to depend only on the different tax instruments used. Other factors, such as the actual incidence of the particular measures adopted and the government spending in transfers, may also be important in shaping the response of agents and in determining the change of government redistribution following a tax-based consolidation. By distinguishing between different tax instruments, our study constitutes a first attempt to understand how important the design of a tax-based consolidation is for determining its effects on income inequality. However, in order to better understand the channels through which different tax instruments impact income inequality, more efforts should be done to disentangle the way in which such instruments are used.

[36]Results are available upon request.

6. Conclusions and further extensions

In this paper, we use PVAR models to estimate the composition effects of tax-based consolidations on disposable income inequality in 16 OECD countries during the period 1978-2012. Although the main focus is on inequality, we also consider the impact on real output and the labour market, which might be potential channels through which taxation affects inequality. The results suggest that tax-based consolidations reduce both market and disposable income inequality, but at the cost of short- to medium-run detrimental effects on output.

This trade-off between equity and efficiency results to be smaller for some indirect taxes. More precisely, general indirect taxes (e.g. VAT) have the largest effects in reducing income inequality with mild short-term contractionary effects on economic activity. The underlying reason behind this result may be that, by causing an increase in the price of the consumption basket, hikes in general indirect taxes affect low-income households particularly strongly, since they generally have a higher marginal propensity to consume. The increase in prices induces those agents who, before the tax hike, were voluntarily inactive to start searching for a job. Higher participation rates increase the probability of being employed and ultimately reduce income inequality. Among direct taxes, only personal taxes increase equity. Moreover, these do not have significant negative effects on labour force participation.

In general, our results suggest that incentives on labour market participation represent an important channel through which different tax instruments may affect income inequality. For this reason, the PVAR methodology is appropriate in decomposing some general equilibrium effects that determine the response of income inequality to tax-based consolidations.

We also assess whether the government's preferences for redistribution play a role. We find that in countries where the government gives a relatively high weight to redistribution tax-based consolidations have larger effects in decreasing income inequality. Moreover, in these countries, lower inequality is achieved through an increase in government redistribution rather than a decrease in market income disparities, as it is instead the case for countries with a lower preference for redistribution. This underscores the importance of further decomposing the channels through which tax-based consolidations bring about reductions in inequality. Investigating how different institutional settings might affect the reaction of inequality to tax-based consolidations would be a step in that direction. Some empirical works point out that changes in income inequality over the last decades were mainly related to structural changes in the labour market (e.g. the rapidly growing use of temporary work agencies and fixed-term contracts, the elimination of the wage-compressing indexation mechanism, the decline in unionisation and the erosion of minimum wage).[37]

In terms of policy advice, we find that governments should take into consideration the effects that

[37] Jaumotte and Osorio Buitron (2015) analyse the role played by labour market institutions in 20 advanced economies between 1980 and 2010, while Jappelli and Pistaferri (2010) provide an analysis for Italy over the period 1980-2006. They both conclude that the erosion of labour market institutions is associated with the rise of income inequality.

different tax instruments may have on economic activity and inequality though labour market channels and choose those tax instruments, such as the VAT tax, which minimise the trade-off between equality and efficiency. Moreover, our results point out that there is not a one-fit-all solution and the final effect of tax-based consolidation may depend on country-specific characteristics.

A possible criticism to our analysis is that we only look at agents' aggregate behaviours. We acknowledge that different groups of population may react differently to taxation and hence we partially address this issue when we analyse the response of labour force participation for men and women separately. However, agents' heterogeneity should be further taken into account and, provided that disaggregated data are available for a large number of countries, a useful extension of this paper would be to disentangle the effects of taxation for different groups of agents.

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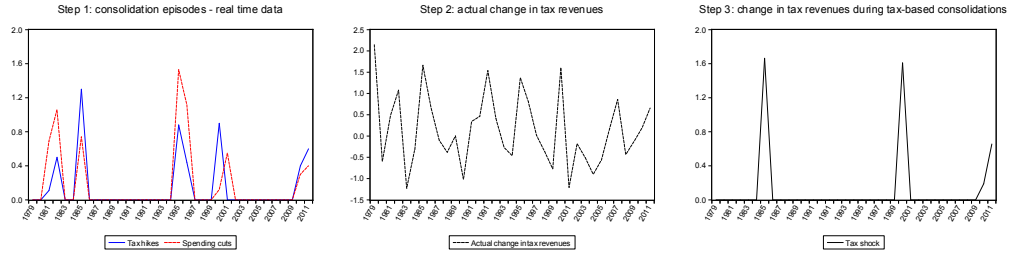
Appendix

Appendix A. Identification strategy

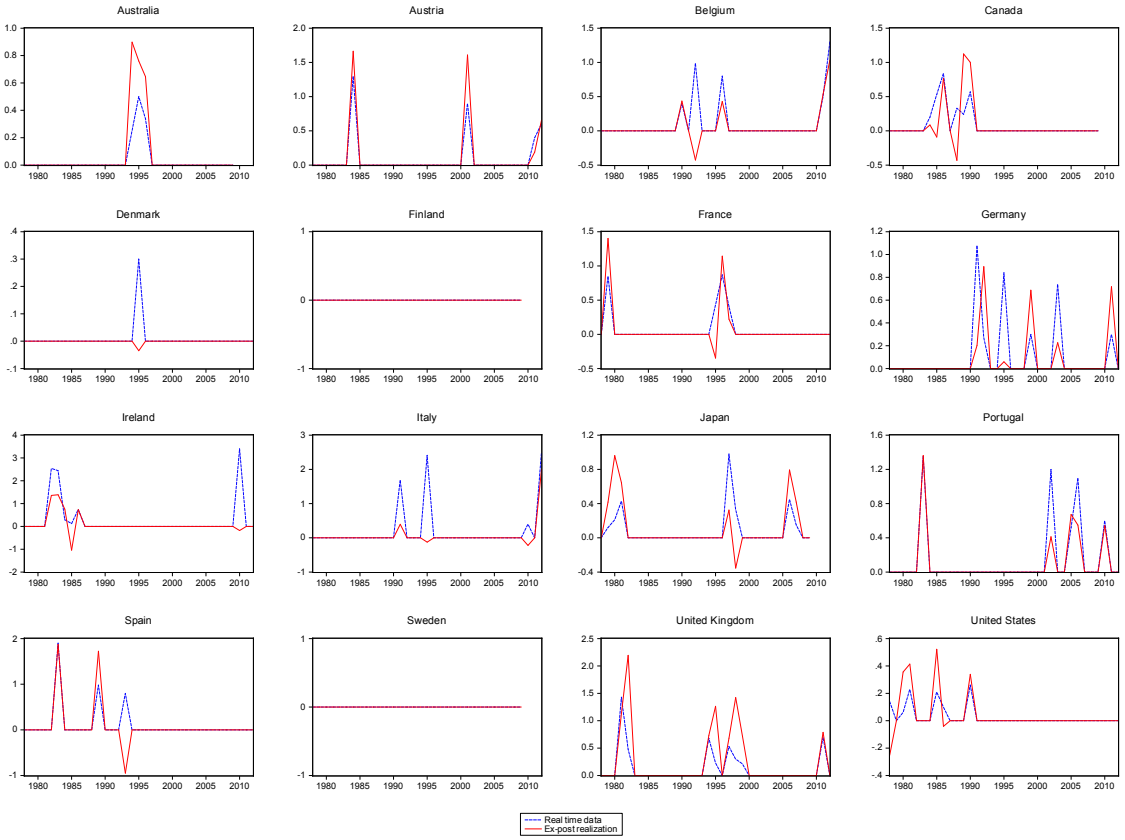
Figure A1 illustrates how we construct our tax shocks, taking the case of Austria as an example. We start from the action-based consolidation datasets compiled by Alesina et al. (2015b) and Alesina et al. (2015a) (step 1). Next, we consider changes in total tax revenues as recorded by the OECD Revenue Statistics database (step 2). Finally, we select years in which tax hikes, as identified through the narrative approach, were larger than spending cuts (i.e. tax-based consolidation years). Ex-post realized changes of tax revenues during those years constitute our shocks (step 3). In Figure A2 we show our (total) tax shocks (red solid line) compare to the values reported in the action-based datasets (blue dotted line) for all countries in our sample.

Figure A1: Construction of the tax shocks



Source: Alesina et al. (2015b), Alesina et al. (2015a), OECD Revenue Statistics database and authors' own calculations.

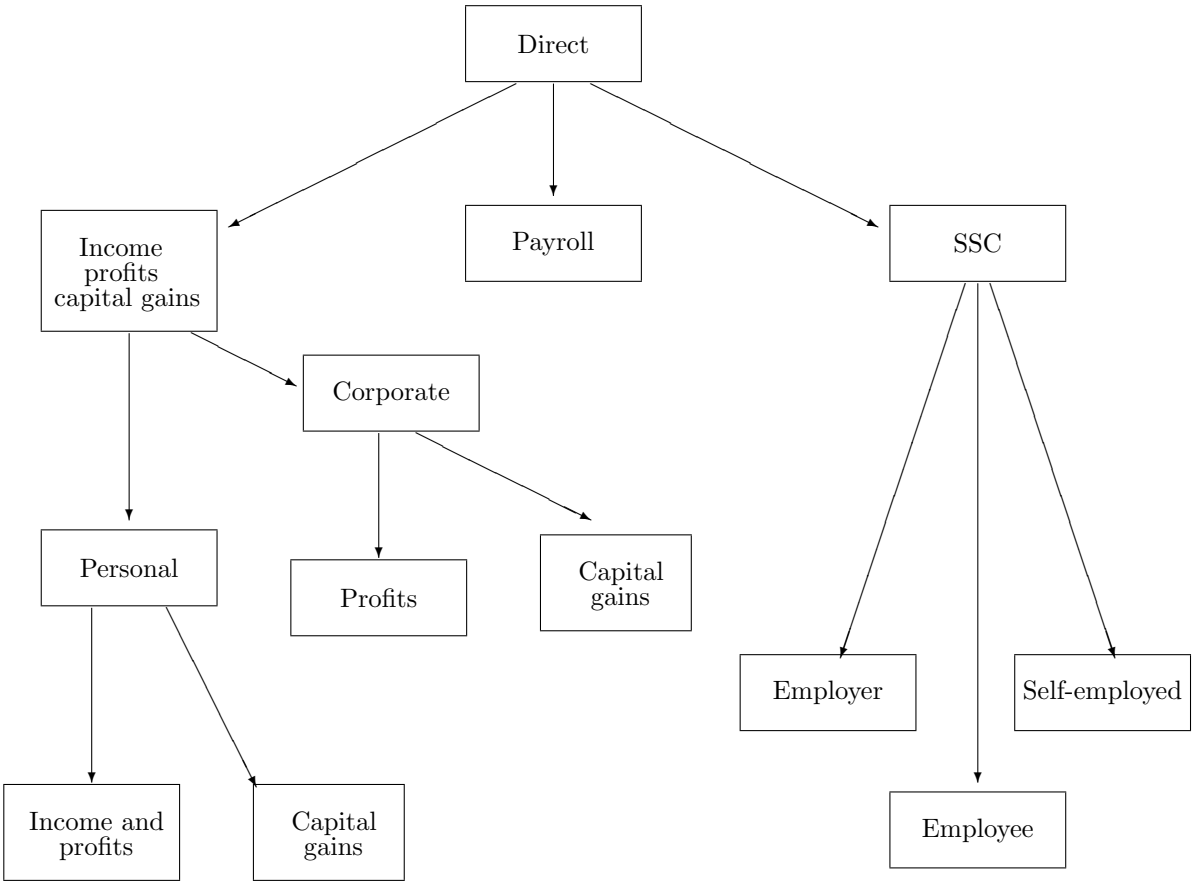
Figure A2: Changes in tax revenues during tax-based consolidation years (1978-2012)



Source: Alesina et al. (2015b), Alesina et al. (2015a), OECD Revenue Statistics database and authors' own calculations.

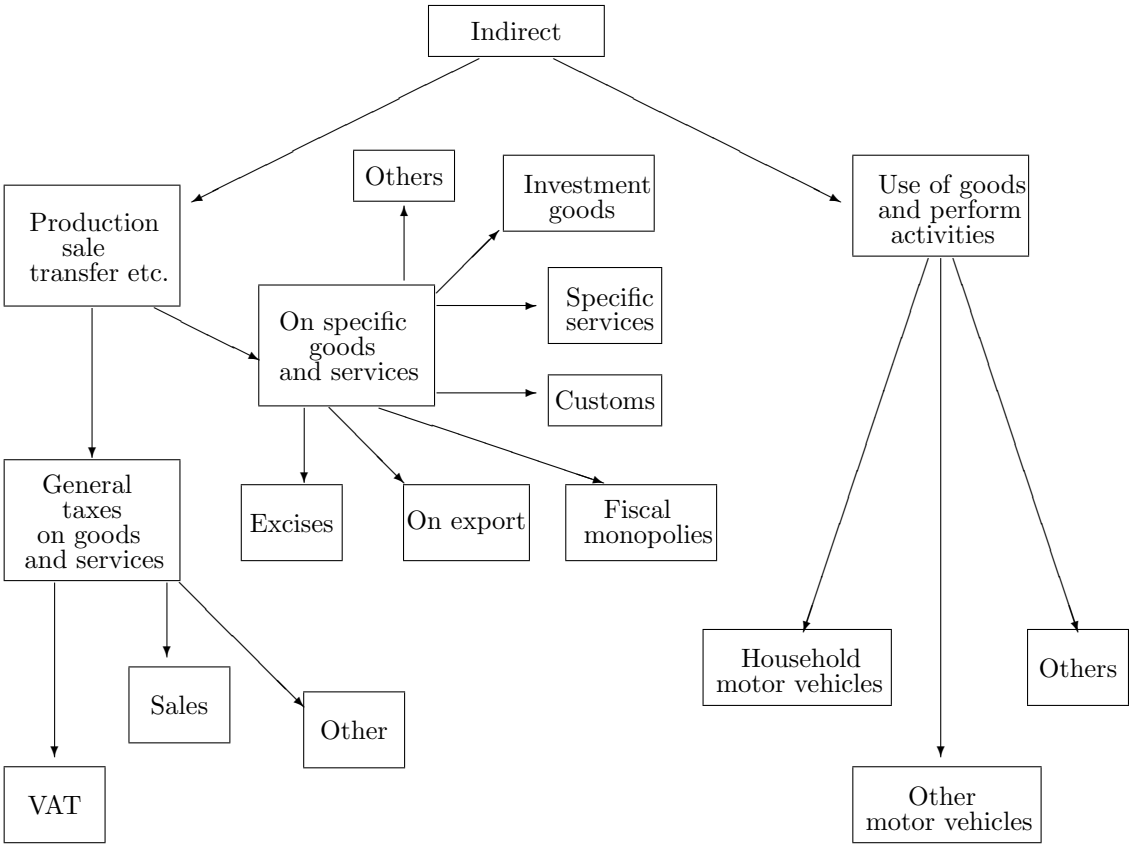
Appendix B. Breakdown of tax instruments

Figure B1: Direct taxes



Note: Direct taxes are generally defined as to include (i) taxes on income, profits and capital gains, (ii) social security contributions and (iii) taxes on payroll and workforce. The breakdown of such tax categories presented above follows the OECD classification method. For more information refer to the OECD Interpretative guide and methodology.

Figure B2: Breakdown of indirect taxes



Note: Indirect taxes are generally defined as taxes on goods and services. The breakdown presented above follows the OECD classification method. For more information refer to the OECD Interpretative guide and methodology.

Appendix C. Robustness checks on baseline regression

Table C1: Type of consolidation

	Impact	**1y**	**3y**	**5y**	**10y**
a) Tax-based consolidation (baseline)					
GDP	**-0.62**	**-1.03**	**-1.14**	**-0.70**	0.04
Net Gini	-0.04	-0.11	**-0.40**	**-0.26**	0.01
Unemployment	0.09	0.33	0.31	0.21	-0.06
Participation	0.07	-0.07	-0.20	**-0.19**	-0.07
b) Any consolidation					
GDP	**-0.39**	**-0.62**	**-0.53**	-0.22	0.05
Net Gini	**0.16**	**0.25**	0.14	0.06	-0.01
Unemployment	0.03	0.19	0.24	0.07	-0.09
Participation	0.01	-0.09	-0.11	-0.05	0.02
c) Spending-based consolidation					
GDP	-0.21	-0.38	-0.04	0.11	0.06
Net Gini	**0.25**	**0.43**	**0.41**	**0.21**	-0.01
Unemployment	-0.01	0.13	0.15	-0.06	-0.09
Participation	-0.03	-0.10	-0.04	0.04	0.06
d) Unanticipated tax-based consolidation					
GDP	**-0.80**	**-1.39**	**-1.63**	**-0.93**	0.10
Net Gini	-0.01	-0.08	-0.32	-0.19	0.01
Unemployment	0.11	0.43	0.58	0.36	-0.13
Participation	0.06	-0.14	**-0.32**	**-0.26**	-0.06
e) Tax-based consolidation with positive realization					
GDP	**-0.75**	**-1.43**	**-1.69**	**-1.12**	-0.06
Net Gini	-0.03	-0.06	**-0.36**	**-0.29**	-0.02
Unemployment	0.20	**0.56**	**0.59**	0.39	-0.07
Participation	0.12	-0.05	-0.23	**-0.23**	-0.08

Notes: The table reports the response to a 1% of GDP tax shock in total taxes. Bold numbers indicate significance at the 10% confidence level. The spending-based sample comprises episodes in which spending cuts, as identified through the narrative approach, were larger than tax hikes, and vice versa for the tax-based sample. The unanticipated tax-based sample comprises tax-based episodes in which unanticipated tax hikes, announced during the same year of implementation, were larger than anticipated tax hikes (that is announced in years preceding the implementation year), according to the accounts of Alesina et al. (2015b) and Alesina et al. (2015a). Tax-based with positive realizations comprises tax-based episodes in which the ex-post realization involved positive changes in the overall level of total tax revenues.

Table C2: Alternative shock variables

	Impact	1y	3y	5y	10y
a) Actual tax revenues (baseline)					
GDP	**-0.62**	**-1.03**	**-1.14**	**-0.70**	0.04
Net Gini	-0.04	-0.11	**-0.40**	**-0.26**	0.01
Unemployment	0.09	0.33	0.31	0.21	-0.06
Participation	0.07	-0.07	-0.20	**-0.19**	-0.07
b) Cyclically adjusted tax revenues					
GDP	**-0.58**	**-0.90**	**-0.97**	-0.76	-0.31
Net Gini	-0.04	-0.12	**-0.35**	**-0.22**	-0.03
Unemployment	0.14	**0.38**	0.31	0.20	-0.02
Participation	0.07	-0.08	-0.21	**-0.20**	**-0.11**
c) Real-time estimates					
GDP	**-0.60**	**-1.31**	**-1.66**	**-1.18**	-0.15
Net Gini	**-0.12**	-0.04	-0.07	-0.10	-0.05
Unemployment	0.14	**0.50**	**0.68**	**0.47**	-0.08
Participation	0.11	0.03	-0.13	-0.15	-0.04
d) Real-time estimates - positive ex-post realization					
GDP	**-0.62**	**-1.45**	**-1.93**	**-1.48**	-0.29
Net Gini	**-0.18**	-0.13	-0.25	**-0.23**	-0.06
Unemployment	0.17	**0.65**	**0.74**	**0.58**	-0.02
Participation	**0.31**	0.19	-0.08	-0.16	-0.08
e) Real-time estimate - negative ex-post realization					
GDP	**-0.61**	**-1.18**	**-1.38**	-0.84	0.00
Net GINI	-0.03	0.08	0.33	0.20	-0.04
Unemployment	0.08	0.30	**0.87**	0.53	-0.15
Participation	-0.17	-0.22	-0.20	-0.15	0.01

Notes: Panels (a), (b) and (c) report the response to a 1% of GDP shock in total taxes, using alternative tax revenue data (respectively ex-post actual revenues, cyclically adjusted ex-post revenues and real-time estimates). Panels (d) and (e) report the response to a 1% of GDP shock in total taxes using real-time estimates during tax-based consolidation years that resulted in, respectively, an increase and a decrease in actual tax revenues (measured by ex-post data). Bold numbers indicate significance at the 10% confidence level.
Cyclically-adjusted tax revenues are computed according to Equation 3.

Table C3: Reverse causality issues

	Impact	**1y**	**3y**	**5y**	**10y**
a) All tax-based consolidation years (baseline)					
GDP	**-0.62**	**-1.03**	**-1.14**	**-0.70**	0.04
Net Gini	-0.04	-0.11	**-0.40**	**-0.26**	0.01
Unemployment	0.09	0.33	0.31	0.21	-0.06
Participation	0.07	-0.07	-0.20	**-0.19**	-0.07
b) Tax-based consolidations excluding years of potential reverse causality					
GDP	**-0.54**	**-1.03**	-0.84	-0.44	0.11
Net GINI	-0.02	-0.09	**-0.42**	**-0.27**	0.02
Unemployment	0.12	0.37	0.20	0.08	-0.06
Participation	0.12	-0.11	-0.20	-0.16	-0.05
e) Dummy for first year of tax-based consolidation cycle					
GDP	**-0.55**	**-1.35**	**-1.95**	**-1.40**	-0.18
Net GINI	**-0.17**	-0.13	-0.18	-0.16	-0.05
Unemployment	**0.25**	**0.59**	**0.93**	**0.67**	-0.05
Participation	0.12	-0.01	-0.17	**-0.23**	-0.08

Notes: Panels (a) and (b) report the response to a 1% of GDP shock in total taxes during, respectively, all tax-based consolidation years and all tax-based consolidation years except those when the consolidation was (i) unanticipated (i.e. decided at year t for implementation in the same year), (ii) part of a multi-year consolidation cycle, and (iii) not the first year of such cycle. Panel (c) reports the response to a tax-based consolidation cycle. This is estimated using a dummy variable taking value 1 for the first year of a tax-based consolidation cycle and 0 otherwise. Bold numbers indicate significance at the 10% confidence level.
The shock variable used to estimate the IRFs reported in Panel (b) is constructed according to Equation 19.

Table C4: Alternative deterministic components

	Impact	**1y**	**3y**	**5y**	**10y**
a) Country and time fixed effects, country-specific linear trends (baseline)					
GDP	**-0.62**	**-1.03**	**-1.14**	**-0.70**	0.04
Net Gini	-0.04	-0.11	**-0.40**	**-0.26**	0.01
Unemployment	0.09	0.33	0.31	0.21	-0.06
Participation	0.07	-0.07	-0.20	**-0.19**	-0.07
b) Country and time fixed effects, no country-specific trends					
GDP	**-0.69**	**-1.21**	**-1.62**	**-1.35**	-0.67
Net Gini	-0.04	-0.10	**-0.36**	-0.22	0.08
Unemployment	0.14	**0.47**	**0.61**	0.47	0.03
Participation	0.03	-0.19	**-0.46**	**-0.48**	**-0.37**
c) First differences, country and time fixed effects, no trends					
GDP	**-0.53**	**-0.85**	-1.04	-0.95	-0.92
Net Gini	0.05	0.05	-0.25	-0.27	-0.27
Unemployment	0.15	0.29	0.30	0.26	0.23
Participation	0.02	-0.18	**-0.44**	**-0.46**	**-0.45**

Notes: The table reports the response to a 1% of GDP tax shock in total taxes. Bold numbers indicate significance at the 10% confidence level.

Table C5: Estimation from local projections

	Impact	**1y**	**3y**	**5y**	**10y**
a) PVAR (baseline)					
GDP	**-0.62**	**-1.03**	**-1.14**	**-0.70**	0.04
Net GINI	-0.04	-0.11	**-0.40**	**-0.26**	0.01
Unemployment	0.09	0.33	0.31	0.21	-0.06
Participation	0.07	-0.07	-0.20	**-0.19**	-0.07
b) local projections - with no control variables					
GDP	**-0.43**	-0.60	**-1.29**	**-1.51**	-0.83
Net GINI	-0.08	-0.19	**-0.60**	**-0.33**	0.06
Unemployment	0.12	0.23	0.38	**0.85**	0.19
Participation	0.11	-0.14	**-0.41**	-0.31	-0.17
c) local projections - with control variables					
GDP	**-0.54**	**-0.79**	-0.92	-1.19	-0.18
Net GINI	-0.01	-0.07	**-0.51**	-0.18	-0.03
Unemployment	0.12	0.18	0.18	0.57	0.03
Participation	0.08	-0.13	-0.31	-0.25	-0.25
d) local projections - dummy for first year of consolidation cycle					
GDP	-0.40	-0.69	**-2.18**	**-2.60**	-0.81
Net GINI	**-0.18**	-0.17	-0.25	-0.11	-0.05
Unemployment	0.18	0.34	**1.02**	**1.47**	0.50
Participation	0.16	0.11	-0.11	**-0.37**	-0.19

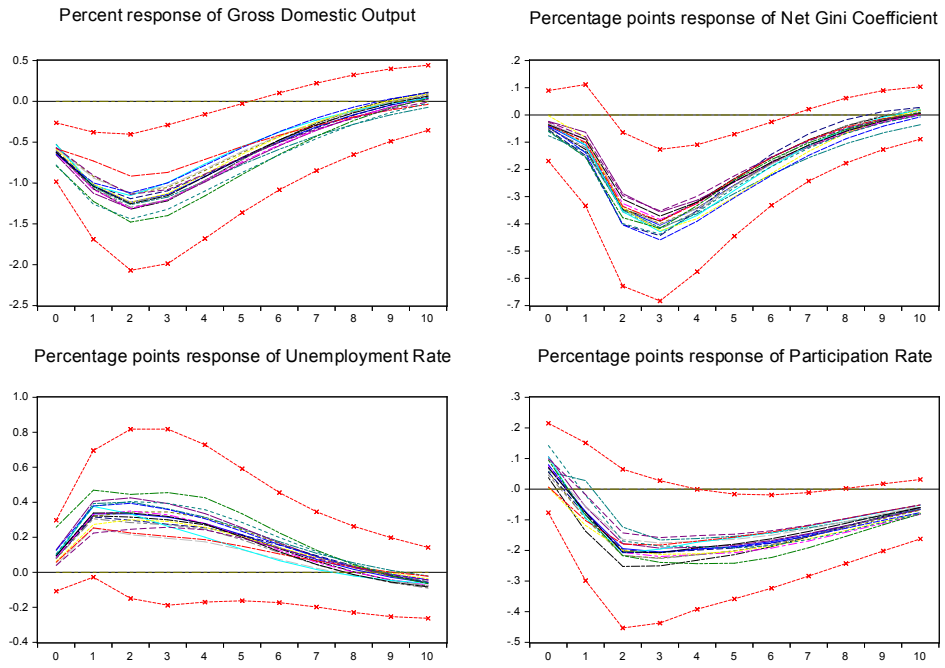
Notes: Panels (a), (b) and (c) report the response to a 1% of GDP tax shock in total taxes. Estimates from panel (d) are obtained replacing the total tax shock variable with a dummy taking value 1 in the first year of a tax-based consolidation episodes and 0 otherwise. Coefficients from Panel (a) are estimated using the PVAR methodology, according to Equation 1. Coefficients from Panels (b), (c) and (d) are estimated using local projections method, according to Equation 5 (with $X^j_{i,t-l}$ being an empty vector for estimates of Panels (b) and (d)). Bold numbers indicate significance at the 10% confidence level.

Table C6: Sample selection

	Impact	1y	3y	5y	10y
a) All sample (baseline)					
GDP	**-0.62**	**-1.03**	**-1.14**	**-0.70**	0.04
Net Gini	-0.04	-0.11	**-0.40**	**-0.26**	0.01
Unemployment	0.09	0.33	0.31	0.21	-0.06
Participation	0.07	-0.07	-0.20	**-0.19**	-0.07
b) Excluding great financial crisis period (sample 1978-2007)					
GDP	**-0.64**	**-1.01**	**-0.88**	-0.43	0.10
Net Gini	-0.05	-0.14	**-0.43**	**-0.21**	0.05
Unemployment	0.04	0.24	0.15	0.01	-0.08
Participation	0.01	-0.10	-0.17	-0.15	-0.03
c) Only EU countries					
GDP	**-0.49**	-0.68	-0.86	-0.48	0.15
Net Gini	-0.02	-0.12	**-0.36**	-0.22	0.01
Unemployment	0.04	0.19	0.28	0.22	-0.05
Participation	0.06	-0.12	-0.22	**-0.17**	-0.04
d) Excluding consolidations in years of banking crises					
GDP	**-0.66**	**-1.01**	**-1.09**	-0.64	0.06
Net Gini	-0.02	-0.08	**-0.38**	**-0.25**	0.01
Unemployment	0.08	0.30	0.26	0.17	-0.07
Participation	0.06	-0.09	-0.21	**-0.18**	-0.06
e) Excluding shock outliers					
GDP	**-0.61**	**-1.23**	**-1.28**	**-0.81**	-0.01
Net Gini	-0.03	-0.08	**-0.40**	**-0.26**	0.01
Unemployment	0.18	**0.51**	0.41	0.27	-0.06
Participation	0.12	-0.09	**-0.25**	**-0.21**	-0.06

Notes: The table reports the response to a 1% of GDP tax shock in total taxes. Bold numbers indicate significance at the 10% confidence level.

Figure C1: Sample stability - IRFs to a 1% of GDP tax-based consolidation shocks



Notes: The figure shows 16 different IRFs to a 1% of GDP tax-based consolidation shock. Each line represents an IRF estimated over a sample of 15 different countries, rather than all the 16 countries of the baseline specification. The red lines with crosses represent the confidence bands of the baseline specification (Figure 1).

Table C7: Alternative specifications with productivity, hours worked and employment

	Impact	**1y**	**3y**	**5y**	**10y**
GDP per hour worked	**-0.73**	**-0.83**	**-0.76**	-0.36	0.08
Net GINI	-0.07	-0.14	**-0.39**	**-0.24**	-0.01
Hours worked	0.04	-0.07	0.08	0.11	0.02
Employment	0.07	-0.11	-0.31	-0.27	-0.09

Notes: The table reports the response to a 1% of GDP tax shock in total taxes. Bold numbers indicate significance at the 10% confidence level. Hours worked refer to employed individuals. Employment is measured as employed individuals as share of the active population.

Table C8: Omitted variables (1)

	Impact	1y	3y	5y	10y
a) 5-variable PVAR (baseline)					
GDP	**-0.62**	**-1.03**	**-1.14**	**-0.70**	0.04
Net Gini	-0.04	-0.11	**-0.40**	**-0.26**	0.01
Unemployment	0.09	0.33	0.31	0.21	-0.06
Participation	0.07	-0.07	-0.20	**-0.19**	-0.07
b) Government consumption					
GDP	**-0.62**	**-1.03**	**-1.14**	-0.67	0.11
Net Gini	-0.04	-0.11	**-0.40**	**-0.26**	-0.03
Unemployment	0.10	0.34	0.33	0.21	-0.08
Participation	0.07	-0.08	-0.22	**-0.18**	-0.04
Government consumption	**0.12**	0.07	0.00	-0.08	-0.07
c) Inflation rate					
GDP	**-0.69**	**-1.10**	**-1.10**	-0.68	-0.04
Net Gini	-0.04	-0.11	**-0.39**	**-0.24**	0.00
Unemployment	0.07	0.31	0.27	0.17	-0.03
Participation	0.10	-0.03	-0.14	-0.15	-0.07
Inflation	**0.37**	0.20	**-0.37**	-0.04	-0.03
d) Saving rate					
GDP	**-0.62**	**-1.01**	**-1.14**	**-0.73**	0.14
Net Gini	-0.04	-0.12	**-0.41**	**-0.26**	-0.01
Unemployment	0.10	0.33	0.31	0.23	-0.10
Participation	0.08	-0.06	-0.20	**-0.20**	-0.03
Saving rate	**-0.31**	-0.36	-0.32	-0.05	**0.16**

Notes: The table reports the response to a 1% of GDP tax shock in total taxes. Bold numbers indicate significance at the 10% confidence level.

Table C9: Omitted variables (2)

	Impact	1y	3y	5y	10y
e) Trade balance					
GDP	**-0.54**	**-0.84**	-0.77	-0.32	0.26
Net GINI	-0.04	-0.12	**-0.42**	**-0.27**	0.01
Unemployment	0.08	0.27	0.15	0.03	-0.13
Participation	0.08	-0.06	-0.16	-0.13	-0.02
Trade balance	-0.12	0.34	0.20	0.22	0.02
f) Trade openness					
GDP	**-0.60**	**-0.99**	**-1.10**	**-0.69**	0.08
Net GINI	-0.05	-0.11	**-0.38**	**-0.23**	0.02
Unemployment	0.08	0.31	0.28	0.19	-0.10
Participation	0.06	-0.07	-0.18	-0.16	-0.04
Import + Export	0.04	0.75	-0.03	0.44	0.41
g) Employment					
GDP	**-0.63**	**-1.09**	**-1.26**	**-0.84**	-0.07
Net GINI	-0.03	-0.10	**-0.39**	**-0.25**	0.01
Unemployment	0.09	0.34	0.36	0.27	-0.03
Participation	0.09	-0.04	-0.20	**-0.19**	-0.07
Employment rate	0.02	-0.29	-0.44	-0.38	-0.05
h) Direct-to-indirect tax ratio					
GDP	**-0.58**	**-0.92**	**-0.93**	-0.54	0.09
Net GINI	-0.06	-0.15	**-0.47**	**-0.30**	0.00
Unemployment	0.12	**0.37**	0.33	0.22	-0.02
Participation	0.07	-0.07	-0.19	**-0.18**	-0.08
Direct/indirect tax ratio	**0.03**	0.01	0.00	0.00	0.00

Notes: The table reports the response to a 1% of GDP tax shock in total taxes. Bold numbers indicate significance at the 10% confidence level.

Table C10: Robustness check - alternative inequality measures

	Impact	1y	3y	5y	10y
a) Net Gini index (baseline)					
GDP	**-0.62**	**-1.03**	**-1.14**	**-0.70**	0.04
Net Gini	-0.04	-0.11	**-0.40**	**-0.26**	0.01
Unemployment	0.09	0.33	0.31	0.21	-0.06
Participation	0.07	-0.07	-0.20	**-0.19**	-0.07
b) Market Gini index					
GDP	**-0.67**	**-1.12**	**-1.25**	-0.62	0.27
Market Gini	-0.12	-0.13	**-0.53**	**-0.27**	-0.03
Unemployment	0.09	0.33	0.34	0.18	-0.17
Participation	0.09	-0.07	**-0.24**	**-0.22**	-0.03
c) Top 0.01% share					
GDP	**-0.83**	**-1.74**	-1.32	-0.58	-0.09
Top 0.01% share	-0.03	**-0.11**	0.00	0.01	0.00
Unemployment	0.27	**0.78**	0.40	0.03	-0.06
Participation	0.11	-0.05	-0.32	-0.19	0.01
d) 0.01-1% share					
GDP	**-0.85**	**-1.77**	-1.36	-0.60	-0.07
0.01-1% share	-0.08	**-0.29**	0.00	0.05	0.01
Unemployment	0.29	**0.81**	0.48	0.11	-0.07
Participation	0.10	-0.06	-0.31	-0.20	0.00
e) 1-10% share					
GDP	**-0.79**	**-1.70**	**-1.41**	-0.70	-0.07
1-10% share	-0.08	-0.04	0.17	0.11	0.00
Unemployment	0.23	**0.73**	0.50	0.18	-0.09
Participation	0.11	-0.03	-0.27	-0.19	0.00

Notes: The table reports the response to a 1% of GDP tax shock in total taxes. Bold numbers indicate significance at the 10% confidence level.

Appendix D. Additional results on tax composition

Table D1: Additional results on indirect taxes

	Impact	**1y**	**3y**	**5y**	**10y**
a) Indirect taxes					
GDP	**-0.90**	**-1.80**	**-2.24**	**-1.90**	-0.39
Net Gini	-0.28	**-0.67**	**-0.99**	**-0.64**	-0.07
Unemployment	0.17	0.67	**1.14**	**0.94**	0.14
Participation	**0.36**	0.21	-0.09	-0.31	**-0.23**
b) Indirect taxes - inflation					
GDP	**-0.88**	**-1.67**	**-1.98**	**-1.75**	-0.46
Net Gini	-0.26	**-0.63**	**-0.92**	**-0.60**	-0.07
Unemployment	0.09	0.54	0.97	**0.81**	0.16
Participation	0.30	0.16	-0.04	-0.27	**-0.23**
Inflation	**1.27**	**1.31**	-0.18	-0.23	-0.10
c) Indirect taxes - hours worked					
GDP	-0.67	**-1.84**	**-2.52**	**-2.17**	-0.63
Net Gini	-0.28	**-0.61**	**-0.85**	**-0.60**	-0.16
Unemployment	0.18	0.78	**1.37**	**1.08**	0.18
Participation	**0.33**	0.32	-0.06	-0.26	-0.21
Hours worked	-0.27	**-1.00**	-0.52	-0.13	0.07
d) Indirect taxes - women and men labour force participation rates					
GDP	**-0.84**	**-1.70**	**-2.12**	**-1.89**	-0.54
Net Gini	**-0.28**	**-0.67**	**-0.98**	**-0.65**	-0.11
Unemployment	0.17	0.65	**1.09**	**0.94**	0.23
Men participation	0.25	0.34	0.02	-0.18	-0.17
Women participation	**0.47**	0.11	-0.17	-0.37	**-0.29**
e) Indirect taxes - women and men employment rates					
GDP	**-0.85**	**-1.70**	**-2.07**	**-1.77**	-0.43
Net Gini	-0.28	**-0.66**	**-0.97**	**-0.61**	-0.08
Unemployment	0.16	0.64	**1.08**	**0.89**	0.16
Men employment	0.13	-0.26	-0.81	**-0.89**	-0.26
Women employment	**0.49**	0.07	-0.53	**-0.78**	**-0.43**

Notes: Panels a)-e) report the response to a 1% of GDP tax shock in indirect taxes under alternative PVAR specifications. Panel f) reports the response to a 1% increase in the standard VAT rate. Bold numbers indicate significance at the 10% confidence level.

Appendix E. Robustness checks on composition effects

Table E1: Ordering of shocks

	Impact	**1y**	**3y**	**5y**	**10y**
a) Baseline - direct taxes					
GDP	**-0.58**	-0.79	-0.98	-0.49	0.16
Net Gini	0.04	0.09	-0.30	-0.22	0.03
Unemployment	0.09	0.26	0.10	0.03	-0.12
Participation	0.01	-0.14	-0.26	-0.18	-0.03
b) Baseline - indirect taxes					
GDP	**-0.90**	**-1.80**	**-2.24**	**-1.90**	-0.39
Net Gini	-0.28	**-0.67**	**-0.99**	**-0.64**	-0.07
Unemployment	0.17	0.67	**1.14**	**0.94**	0.14
Participation	**0.36**	0.21	-0.09	-0.31	**-0.23**
c) Direct taxes (ordered first) and indirect taxes (ordered second) - shock to direct taxes					
Direct	**1.00**	**0.11**	-0.01	0.01	0.00
Indirect	0.03	**-0.07**	**0.02**	0.00	0.00
GDP	**-0.62**	-0.84	-1.00	-0.48	0.19
Net Gini	0.02	0.06	-0.29	-0.19	0.04
Unemployment	0.10	0.29	0.09	0.01	-0.14
Participation	0.01	-0.14	-0.28	-0.19	-0.02
d) Direct taxes (ordered first) and indirect taxes (ordered second) - shock to indirect taxes					
Direct	0.00	**-0.16**	**0.06**	0.01	0.01
Indirect	**1.00**	**-0.09**	0.02	0.00	0.00
GDP	**-0.88**	**-1.83**	**-2.16**	**-1.84**	-0.39
Net Gini	**-0.30**	**-0.76**	**-0.98**	**-0.61**	-0.08
Unemployment	0.15	0.60	**1.15**	**0.96**	0.14
Participation	**0.33**	0.18	-0.07	-0.31	**-0.24**
e) Direct taxes (ordered second) and indirect taxes (ordered first) - shock to direct taxes					
Indirect	0.00	**-0.07**	**0.02**	0.00	0.00
Direct	**1.00**	**0.12**	-0.01	0.01	0.00
GDP	**-0.59**	-0.78	-0.93	-0.42	0.20
Net Gini	0.03	0.09	-0.26	-0.17	0.04
Unemployment	0.10	0.27	0.06	-0.02	-0.14
Participation	0.00	-0.15	-0.28	-0.18	-0.02
f) Direct taxes (ordered second) and indirect taxes (ordered first) - shock to indirect taxes					
Indirect	**1.00**	**-0.09**	0.03	0.00	0.00
Direct	0.10	**-0.15**	**0.05**	0.01	0.01
GDP	**-0.94**	**-1.90**	**-2.25**	**-1.89**	-0.37
Net Gini	**-0.29**	**-0.75**	**-1.01**	**-0.63**	-0.08
Unemployment	0.16	0.63	**1.15**	**0.95**	0.13
Participation	**0.33**	0.17	-0.10	-0.32	**-0.24**

Notes: The table reports the response to a 1% of GDP tax shock in direct and indirect taxes under alternative ordering of shocks. Bold numbers indicate significance at the 10% confidence level.

Table E2: Robustness check - alternative inequality measures - direct taxes

	Impact	**1y**	**3y**	**5y**	**10y**
a) Net Gini index					
GDP	**-0.58**	-0.79	-0.98	-0.49	0.16
Net Gini	0.04	0.09	-0.30	-0.22	0.03
Unemployment	0.09	0.26	0.10	0.03	-0.12
Participation	0.01	-0.14	-0.26	-0.18	-0.03
b) Market Gini index					
GDP	**-0.63**	**-0.91**	**-1.18**	-0.54	0.37
Market Gini	-0.07	-0.04	**-0.56**	**-0.36**	-0.01
Unemployment	0.08	0.27	0.16	0.06	-0.20
Participation	0.03	-0.13	-0.30	**-0.23**	-0.02
c) Top 0.01% share					
GDP	-0.62	-1.05	-0.90	-0.39	-0.03
Top 0.01% share	**-0.06**	**-0.12**	-0.02	0.01	0.00
Unemployment	0.26	0.57	0.16	-0.03	-0.05
Participation	0.12	0.01	-0.29	-0.16	0.01
d) 0.01-1% share					
GDP	-0.59	-1.01	-0.91	-0.42	-0.03
0.01-1% share	-0.09	-0.20	-0.04	0.02	0.01
Unemployment	0.22	0.53	0.20	0.07	-0.05
Participation	0.11	0.01	-0.25	-0.13	0.00
e) 1-10% share					
GDP	-0.59	-1.00	-0.88	-0.46	-0.04
1-10% share	-0.03	0.08	-0.01	0.06	0.00
Unemployment	0.23	0.53	0.20	0.11	-0.05
Participation	0.09	0.00	-0.23	-0.11	0.00

Notes: The table reports the response to a 1% of GDP tax shock in direct taxes using alternative inequality measures. Bold numbers indicate significance at the 10% confidence level.

Table E3: Robustness check - alternative inequality measures - indirect taxes

	Impact	1y	3y	5y	10y
a) Net Gini index					
GDP	**-0.90**	**-1.80**	**-2.24**	**-1.90**	-0.39
Net Gini	-0.28	**-0.67**	**-0.99**	**-0.64**	-0.07
Unemployment	0.17	0.67	**1.14**	**0.94**	0.14
Participation	**0.36**	0.21	-0.09	-0.31	-0.23
b) Market Gini index					
GDP	**-0.96**	**-1.90**	**-2.26**	**-1.48**	0.10
Market Gini	-0.25	-0.39	**-0.89**	-0.29	-0.02
Unemployment	0.14	0.64	**1.11**	0.73	-0.13
Participation	**0.37**	0.20	-0.14	-0.30	-0.08
c) Top 0.01% share					
GDP	**-1.14**	**-2.91**	-2.16	-1.00	-0.17
Top 0.01% share	0.05	-0.04	0.01	0.01	0.01
Unemployment	0.14	0.99	1.03	0.30	-0.08
Participation	0.23	0.02	-0.30	-0.24	0.01
d) 0.01-1% share					
GDP	**-1.30**	**-3.12**	-2.28	-0.93	-0.06
0.01-1% share	-0.09	**-0.41**	-0.05	0.09	0.02
Unemployment	0.28	**1.18**	1.17	0.28	-0.12
Participation	0.23	-0.03	-0.41	-0.33	0.01
e) 1-10% share					
GDP	**-1.20**	**-3.04**	-2.64	-1.53	-0.14
1-10% share	-0.27	**-0.46**	**0.38**	0.26	0.01
Unemployment	0.04	0.86	1.17	0.54	-0.19
Participation	0.35	0.15	-0.26	-0.32	0.00

Notes: The table reports the response to a 1% of GDP tax shock in indirect taxes using alternative inequality measures. Bold numbers indicate significance at the 10% confidence level.

Appendix F. Descriptive statistics for variables presented in Section 5

Table F1: Country averages of different proxies for government's redistribution

	Market Gini-Net Gini	Direct/Indirect tax revenues
Australia	15.29	**1.94**
Austria	17.68	0.93
Belgium	**19.33**	**1.49**
Canada	14.05	**1.74**
Denmark	**20.77**	**1.74**
Finland	**20.67**	1.20
France	**18.57**	0.72
Germany	**19.40**	1.14
Ireland	**17.86**	0.98
Italy	13.69	**1.29**
Japan	13.55	**2.45**
Portugal	**18.70**	0.60
Spain	13.22	1.06
Sweden	**22.53**	**1.56**
United Kingdom	17.72	1.22
United States	12.04	**2.58**
Total	17.79	1.25

Notes: Market-Net refers to the difference between the Gini index of market income and the Gini index of disposable income. For a precise definition of direct and indirect taxes refer to Figures B1-B2 in Appendix B. The median of all the country averages is shown for the overall sample. Bold numbers indicate countries with average values above median (below the median for Net Gini).
Source: OECD Revenue Statistics database, SWIID, and authors' own calculations.

Table F2: Mean changes in tax revenues during tax-based consolidations in 1978-2012 by groups of countries

	Low redistribution	High redistribution
Total	0.64	0.51
Direct	0.49	0.26
Indirect	0.11	0.23
Personal	0.26	0.11
Corporate	0.08	0.05
SSC	0.13	0.06
GT	0.09	0.17
SGS	0.03	0.03
Observations	42	27

Notes: High/low-redistribution countries are those having the difference between market and the net Gini respectively above and below median. Direct taxes are generally defined as to comprise (i) taxes on income, profits and capital gains, (ii) social security contributions and (iii) taxes on payroll and workforce. For the precise breakdown, we refer the reader to Figure B1 in Appendix B. Indirect taxes include all taxes on goods and services. For a precise breakdown, we refer the reader to Figure B2 in Appendix B.
Source: OECD Revenue Statistics database and authors' own calculations.