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Chapter 3

The Composition Effects of Tax-Based Consolidations on Income Inequality

3.1 Introduction

Following the build-up of large public debt stocks in the wake of the global financial crisis of 2007-2008 and the ensuing recession, consolidating public finances has become a priority in several advanced economies. As an example, in the Euro Area, the structural balance grew from -4.8% of potential GDP in 2009 to -0.9% in 2015. Fiscal policy in the United Kingdom and the United States followed a similar path, with the structural balance ratio being increased respectively from -8.5% to -4.1% and from -7.7% to -3.4% during the same period (IMF World Economic Outlook).

Many commentators have raised concerns that this 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 many 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 citizens are not in a condition to save or to invest in education, which reduces investments and the accumulation of human capital (Aghion, Caroli, and Garcia-Penalosa, 1999; OECD; OECD; Galor and Moav, 2004). Third, inequality may also create financial instability. Indeed, some studies maintain that inequality played a major role in the global financial crisis of 2007-2008 by contributing to the debt accumulation by lower- and middle-income class agents (Fitoussi and Saraceno, 2009; Kumhof, Romain, and Winan, 2015; Rajan, 2010). In this light, policy-makers have become more concerned about the consequences for inequality of their policy actions.

An obvious way in which fiscal consolidation may influence income inequality is through changes in the amount of government redistribution. For instance, lowering government transfers reduces the disposable income of low-income agents, thereby raising inequality. Conversely, increasing the top marginal income tax rate penalizes

richer agents and therefore should decrease disparities. Fiscal restraint may also induce behavioral responses by agents. As an example, a higher labor tax lowers the net wage and may induce agents to either substitute away labor for leisure (substitution effect) or supply more labor to maintain a similar level of consumption (income effect). Hence, depending on which effect dominates — and assuming agents' heterogeneity in either the type of utility function or labor supply elasticities — the inequality effects of higher taxes through the labor supply channel may differ.

Further, fiscal consolidations may affect inequality through its general equilibrium effects. Ball et al., 2013 argue that fiscal adjustments reduce output and increase unemployment. That decreases the wage share, which in turn tends to increase inequality due to the relatively higher share of wage income in lower-income groups. Moreover, Bastagli, Coady, and Gupta, 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. On the other hand, in countries with rigid labor markets, it might be more difficult for firms to shed off labor, thus limiting the scope of these channels. To sum up, a theoretical prediction on the impact of fiscal consolidations on inequality depends on both the specific policy measures used and the assumptions underlying the economic structure.

In this context, the aim of our analysis is twofold. First, we empirically assess the effects of tax-based consolidations (i.e., consolidations in which tax hikes are larger than spending cuts) on income inequality, output, and labor market conditions. Second, we establish some stylized facts about which particular tax instruments are typically used during tax-based consolidations and 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 on wealth inequality. Moreover, as income directly impacts living standards, rising income inequality has likely played a more prominent role in fueling the recent wave of social discontent that policy-makers are now trying to address (Rajan, 2010).

Our primary focus is on disposable income inequality since ultimately this is what matters for the relation between inequality and growth. However, to assess the direction and strength of the government's redistribution channel, we also evaluate the impact of fiscal adjustments on market income inequality. Further, we investigate the effects of tax-based consolidations not only on inequality but also on economic activity and labor market outcomes. We do so to disentangle the other channels through which consolidations affect inequality. We only analyze tax-based consolidations since, as we will see below, this is the area with the most disagreement in the literature.

The empirical literature on the effects of fiscal consolidations on income inequality has been limited in scope and has provided inconclusive evidence. Ball et al., 2013, Agnello and Sousa, 2014, and Woo et al., 2017 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, but employ different methodologies: Ball et al.,

2013 use local projections, Agnello and Sousa, 2014 adopt seemingly unrelated regressions (henceforth SUR), and Woo et al., 2017 use SUR and fixed effects. All these contributions distinguish between tax-based and spending-based consolidation episodes, where tax-based consolidations are 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, Ball et al., 2013, Woo et al., 2017, and Agnello and Sousa, 2014 find, respectively, significant positive, negative but insignificant and significant negative effects of tax-based consolidations on income inequality.

These different findings appear to be due to the way in which tax-based consolidations are accounted for and to the horizon considered. Agnello and Sousa, 2014 focus on the impact effects and employ both a dummy variable to denote all years of tax-based consolidations and the original variable constructed by Devries et al., 2011, which accounts for the differences in the size of the fiscal packages.[1] When employing the dummy variable, Agnello and Sousa, 2014 find negative but statistically insignificant effects on inequality. On the other hand, they find statistically significant negative effects (lower inequality) when accounting for the size of the consolidation. Woo et al., 2017 use the original Devries et al., 2011 variable to analyze the 1-year response of inequality to tax-based consolidations and find negative but not significant effects. Finally, Ball et al., 2013 employ the local projection method to derive impulse responses over an 8-year horizon and find positive effects. This result needs two qualifications. Firstly, the authors use a dummy variable taking value 1 in the first year of the tax-based consolidation cycle and 0 otherwise. As shown in Agnello and Sousa, 2014, this identification strategy might lead to estimates that are biased upward. Secondly, they report one-standard-error confidence bands for their impulse responses. Hence, their results are only significant at the 69% confidence level.

Our analysis contributes to the existing literature in several aspects. First, the studies reviewed above estimate the effects of fiscal consolidations in a single-equation setup.[2] To the extent that fiscal consolidations are not random assignments and that they also impact economic activity (see Guajardo, Leigh, and Pescatori, 2014; and Alesina, Favero, and Giavazzi, 2015), we argue that this approach might forgive potential feedback effects from economic activity to inequality. We opt instead for a multi-equation setup that takes into account both the direct effects of changes in fiscal policy on inequality, output and labor market variables and the indirect (feedback) effects among them. Second, we note that economic inequality is typically slow-moving. Hence, improving on Agnello and Sousa, 2014, and Woo et al., 2017, we study the dynamic (long-term) effects of tax-based consolidations on inequality.

[1] The difference between these two alternative approaches (accounting or not accounting for the size of the consolidation) seems particularly relevant since the tax-based consolidations identified in Devries et al., 2011 range from being as small as 0.3% of GDP (Denmark 1995) to being as large as 3.7% of GDP (Canada 1984-1990).

[2] An exception is Mourelo and Escudero, 2016 who consider the impact of fiscal consolidations on both employment and growth (but not inequality) for 32 countries during the Great Recession.

Differently from Ball et al., 2013, we account for the size of the consolidation. Third, we borrow from a recent and growing literature which documents how real-time fiscal measures tend to overstate the actual *ex-post* impact of fiscal consolidations (see Castro, Pérez, and Rodríguez-Vives, 2013, Frankel and Schreger, 2013 and Beetsma, Furtuna, and Giuliodori, 2017 among others). Hence, we use *ex-post* realized data as opposed to *ex-ante* real-time data (as instead done in the existing literature) to measure the true extent of a consolidation effort and avoid potential measurement errors.[3]

We also contribute to the literature in other aspects. The studies reviewed above limit the analysis to changes in overall tax revenues and neglects potential composition effects. An attempt to empirically study the impact of tax policy changes on inequality in a multivariate framework is carried out by Martínez-Vázquez, Vulovic, and Dodson, 2012 and Muinelo-Gallo and Roca-Sagalés, 2013. However, these contributions examine the effects of budget-neutral tax changes rather than of consolidation episodes. Moreover, they do not address the potential endogeneity between fiscal variables and economic activity. Our paper fills this gap in the literature by analyzing the effects of specific tax policy measures on income inequality in the context of fiscal adjustments that are not driven by output stabilization considerations.

Our sample includes 16 OECD countries during the period 1978-2012. To identify episodes of fiscal consolidations, we use the action-based datasets compiled by Devries et al., 2011 and Alesina, Favero, and Giavazzi, 2015. These two databases exclusively consider consolidation episodes aiming solely at reducing the government deficit, and not at stabilizing economic activity. This allows us to limit the potential endogeneity between tax-based consolidations and GDP. Next, we make use of the OECD Revenue Statistics Database (**OECD:Revenu**) to measure the actual extent of the tax consolidation effort. The OECD Revenue Statistics Database also allows us to quantify changes in specific tax instruments, and therefore pin down which particular instrument was most used during each consolidation year and analyze its effects. As measures of income inequality, we use both the market and the disposable income Gini indexes, as well as data on top income shares and income ratios. For the estimation, we rely on a panel vector autoregressive (PVAR) methodology. Although our analysis is at the macro level, we explore potential heterogeneities in the labor market outcomes of fiscal consolidations for groups of agents of different sex and age.

We begin by establishing some stylized facts about the design of tax-based consolidations. We show that governments normally rely on either direct or indirect taxes (rather than a combination of them) to consolidate the budget, which further highlights the relevance of our research question. We also show that personal taxes are by far the most preferred instrument, with general consumption taxes (such as

[3] Importantly, we still make use of narrative datasets to identify episodes of fiscal consolidations that do not depend on contemporaneous changes in output growth. Our analysis is different from previous ones insofar as we rescale the consolidation episodes by the *ex-post* realized changes in tax revenues rather than *ex-ante* real time estimates (see Section 3.2.1 for more details).

value added and sale) being a distant second. Turning to the empirical analysis, our results point to statistically significant and economically meaningful positive effects of tax-based consolidations in reducing income inequality. However, this comes at the cost of a contraction in economic activity. Next, we find that using different tax instruments matters; that is, indirect taxes reduce income inequality by more than direct taxes. Looking at the specific tax instruments, personal taxes seem to be the most suited to reduce inequality while at the same time minimizing the equity-efficiency trade-off. General consumption taxes also have substantial positive short-run effects on the labor force participation rate, which dampens their recessionary effects. Finally, we do not find corporate taxes to have any impact on income inequality.

Our findings point to the existence of a positive labor supply channel of indirect taxes. Higher indirect taxes decrease the number of goods that households can buy given a certain income and create incentives for agents voluntarily out of the labor force to start searching for a job. That is, the income effect dominates the substitution effect. This, in turn, promotes labor force participation, especially of middle-aged women, and reduces income inequality. Instead, we do not find evidence backing the hypothesis of a negative labor demand channel through which fiscal consolidations jeopardize income equality.[4] Lastly, we find little evidence pointing to a positive government redistribution channel of tax-based consolidations.

The remainder of the paper is structured as follows. Section 3.2 presents the dataset and explains the empirical methodology. Section 3.3 contains our baseline results on the overall effects of tax-based consolidations. Section 3.4 focuses on the composition of tax-based consolidations and disentangles the specific effects of each single tax instrument. Section 3.5 concludes. We report extensive robustness checks in Appendix B.

3.2 Dataset and Methodology

3.2.1 Dataset and Stylized Facts

Our empirical analysis covers 16 OECD countries between 1978 and 2012 at the annual frequency. To identify exogenous fiscal consolidation shocks, we follow a narrative approach and start from the action-based dataset compiled by Devries et al., 2011. Devries et al., 2011 make use of official policy records to gather real-time data on estimated changes in tax revenues and public spending resulting from consolidation measures decided in 17 OECD countries during the period 1978-2009. The peculiarity of this dataset is that it only selects those consolidation episodes that have the sole objective of reducing the budget deficit and are not driven by output stabilization, labor market conditions or income inequality developments. Examples of such episodes may include consolidations that are caused by the operation of fiscal

[4] Although some tax instruments, particularly specific consumption taxes, do cause the unemployment rate to increases, this is not accompanied by an increase in income inequality.

rules, by the presence of a ceiling on public debt or by commitments to reduce the public debt taken by governments in a supranational context. Next, we use another action-based dataset compiled by Alesina et al., 2015, which identifies additional fiscal consolidation episodes in 11 countries between 2010 and 2013, employing the same method of Devries et al., 2011.[5]

After merging the two action-based datasets of Devries et al., 2011 and Alesina et al., 2015, 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 period 1978-2013, and Australia, Canada, Finland, Japan, the Netherlands and Sweden over the period 1978-2009. In carrying out the analysis, we restrict the sample to the period 1978-2012 due to the availability of data on the Gini index. Following Alesina, Favero, and Giavazzi, 2015, we exclude the Netherlands from the sample, as for this country the consolidation episodes identified by Devries et al., 2011 are endogenous to output growth and cannot be used as fiscal policy instruments.

The consolidation episodes in our dataset differ significantly in their size and nature. We classify them into three categories: (i) any 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). We identify 188 consolidation episodes, of which 112 are spending-based, 73 tax-based, and three featured tax hikes exactly equal to spending cuts (hence, we classify them as neither spending- nor tax-based consolidations). 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

[5] An alternative approach to identify episodes of fiscal adjustments consists of simply considering periods of large changes in tax revenues and government spending. We refer to it as the statistical approach. The narrative approach and the statistical approach may yield very different results when analyzing the effects of fiscal policy. For instance, Guajardo, Leigh, and Pescatori, 2014 analyze the growth impact of fiscal consolidations using first the dataset compiled by Devries et al., 2011, based on the narrative approach, and then a dataset compiled by Alesina and Ardagna, 2013, based instead on the statistical approach. Estimates based on the former suggest that fiscal consolidation has Keynesian contractionary effects. On the contrary, estimates based on the latter find neoclassical expansionary effects (for similar findings see also Afonso and Jalles, 2014). Potential reasons behind these opposite results may be that the statistical approach (i) tends to classify periods in which the budget balance improved simply due to favorable economic conditions as periods of fiscal consolidation, and (ii) gives less weight to unsuccessful episodes of fiscal consolidation (i.e., when the government does not succeed in consolidating the budget). We acknowledge that the narrative approach may also have some drawbacks. First, it largely relies on subjective judgments to identify exogenous fiscal consolidations and quantify their magnitude. Second, it may not eliminate the endogeneity between fiscal policy and growth completely, if the debt level and changes in output are correlated. As it will be clear later, we try to address the first point by combining data from the action-based datasets with *ex-post* fiscal data. Overall, we believe that the narrative approach, although not immune from criticisms, is the most suited approach to address the endogeneity problem.

changes. Therefore, we mainly focus on tax-based consolidations. We use data on spending-based consolidations for a robustness check.

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 rely on the (OECD Revenue Statistics Database). This provides information on revenues generated by different tax instruments, measured as a share of GDP. Hence, our identification strategy, summarized in Appendix B.1, consists in analyzing *ex-post* changes in tax revenues in years of tax-based consolidations as identified through the narrative approach.

Our focus on *ex-post* changes of tax revenues also addresses a concern that the real-time information about the size of fiscal consolidations contained in narrative datasets are imprecise estimates of the actual fiscal policy changes implemented by governments. Indeed, a large literature has shown that fiscal plans and real-time fiscal measures tend to overstate the actual (*ex-post*) impact of fiscal consolidations (Beetsma, Furtuna, and Giuliodori, 2017; Beetsma, Giuliodori, and Wierts, 2009; Castro, Pérez, and Rodríguez-Vives, 2013; Frankel and Schreger, 2013; Gupta et al., 2017).[6] Of particular relevance for our analysis, Beetsma, Furtuna, and Giuliodori, 2017 compare the *ex-post* realizations of the consolidation episodes identified by Devries et al., 2011 and find a systematic shortfall of *ex-post* realized revenues relative to the narrative measure (of 0.15 percentage points of GDP for tax revenues). By rescaling the narratively identified tax-based consolidation episodes with the *ex-post* realized changes in tax revenues we mitigate the concern that our tax variable is an oversized representation of the real consolidation effort. Admittedly, this rescaling exercise might introduce a source of endogeneity in our shock variable, as tax revenues arguably respond to GDP. Following the methodology used in Guajardo, Leigh, and Pescatori, 2014, we show that this potential source of endogeneity is not important in practice (see Appendix B.1 for more details).

Turning to the role of specific tax instruments, we focus on two broad ones: direct and indirect taxes. Property and wealth taxes show only small changes relative to direct and indirect taxes during tax-based consolidations. Hence, we do not consider them in the analysis. Direct taxes include (i) personal taxes, (ii) corporate taxes, (iii) social security contributions (henceforth SSC) and (iv) payroll taxes. Indirect taxes include: (i) general consumption taxes (henceforth GT), such as value added and sale taxes, (ii) taxes on specific goods and services (henceforth SGS), such as for instance excises and fiscal monopolies, and (iii) taxes on the use of goods and services (henceforth UGS), such as taxes on vehicles. For illustrative purposes, a flow-chart of the composition of direct and indirect taxes is depicted in Appendix

[6] For instance, Beetsma, Giuliodori, and Wierts, 2009 and Castro, Pérez, and Rodríguez-Vives, 2013 explore how fiscal data revisions in the EU gradually develop as the time distance from the original fiscal plan increases and suggest that governments try to systematically exploit the margins of acceptable reporting, but are subsequently corrected by Eurostat.

B.2. Due to the marginal change of payroll and UGS tax revenues during tax-based consolidations, we exclude them from the analysis.

Table 3.1 shows the mean values of tax hikes and spending cuts identified through the narrative approach and the corresponding *ex-post* changes in tax revenues for the three different categories of consolidation episodes discussed above. The average *ex-post* change in tax revenues during tax-based consolidations (0.57% of GDP) was lower than the government's real-time estimate (0.72% of GDP). This figure is perfectly in line with the finding of Beetsma, Furtuna, and Giuliodori, 2017 and reinforces the case for rescaling the narratively identified consolidation episodes by the actual *ex-post* realized changes in tax revenues to correctly quantify the real consolidation effort and thus limit the potential for measurement errors. We will come back to this issue when performing robustness checks on our baseline specification.

Table 3.1: Mean values of consolidation episodes (% of GDP) – 1978-2012

	Narrative approach		**Realization**	**Obs.**
	Tax	**Spending**	**Tax**	
Any consolidation	0.45	0.64	0.45	188
Spending-based	0.28	0.91	0.37	112
Tax-based	0.72	0.24	0.57	73

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., 2015 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. All numbers are expressed as averages. The column obs. denotes the number of observations in the sample.
Source: Devries et al., 2011, Alesina et al., 2015, OECD Revenue Statistics Database and own calculations.

Table 3.2 presents descriptive statistics of the change in the different tax instruments we focus on during (i) the full and (ii) the tax-based consolidation samples. 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.

Next, we notice that the correlation between the changes in direct and indirect tax revenues during tax-based consolidations years is close to 0. That suggests that in most cases governments resorted either to direct or indirect taxes, rather than to a combination of them, to consolidate the budget. Table 3.3 further shows the correlation coefficients among the different direct and indirect tax instruments during tax-based consolidations. Personal and corporate taxes and SSC 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.

Along with the series of tax shocks, we also include other variables in our dataset. As a measure of inequality, we rely on the Gini index, calculated both from market and

Table 3.2: Mean changes of different tax instruments (% of GDP) – 1978-2012

	Full sample	Tax-based
Total	0.13	0.57
Direct	0.09	0.41
Indirect	0.02	0.15
Personal	0.01	0.22
Corporate	0.02	0.07
SSC	0.05	0.09
GT	0.05	0.12
SGS	-0.04	0.02

Notes: The tax-based sample comprises consolidation episodes in which tax hikes, as identified through the narrative approach, were larger than spending cuts. SSC, GT and SGS stand for, respectively, social security contributions, general taxes and specific goods and services. For a precise definition of the different tax categories refer to Appendix B.2.
Source: OECD Revenue Statistics database and own calculations.

disposable incomes.[7] This measure has two main advantages: it is Lorenz-consistent, and its estimates are widely available, both over time and across countries.[8] We collect the Gini indexes from the Standardized World Income Inequality Database (SWIID, Version 4.1), compiled by Solt, 2016. The advantage of using the SWIID is that it provides the most comparable series across countries.

For data on the labor force participation and the unemployment rates (both overall and by sex and age), as well as for real per capita GDP, we rely on information contained in the OECD Economic Outlook. The labor 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

[7] The Gini index measures the extent to which the distribution of income among individuals deviates from a perfectly equal distribution. It ranges between 0 (perfect equality) and 100 (perfect inequality). It is usually estimated from survey data, and it can be based on both market and disposable income. The Gini index of market income (or gross Gini index) is calculated on income before taxes and transfers. The Gini index of disposable income (or net Gini index) is calculated on income after taxes and transfers.

[8] A measure of inequality is said to be Lorenz-consistent if it satisfies the following four criteria: (i) the anonymity principle (i.e. it does not matter who is earning the income), (ii) the population principle (i.e., the population size does not matter), (iii) the relative income (i.e. only relative income matters), and (iv) the Dalton transfer principle (i.e. if an income distribution can be achieved from another by constructing a sequence of regressive transfers, then the newly created distribution must be deemed more unequal than the original one).

Table 3.3: Correlation of changes in tax revenues during tax-based consolidation years

	Personal	Corporate	SSC	GT	SGS
Personal	1.00	0.04	0.04	-0.07	-0.14
Corporate		1.00	-0.05	-0.10	0.12
SSC			1.00	0.11	0.06
GT				1.00	-0.34
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 own calculations.

jobless people in the labor force between 15 and 64 years who are available to work and are actively seeking employment.[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 as well as the ratios of income of the individuals at the 90^{th}, 50^{th} and 10^{th} percentiles of the income distribution.[10] Both top income shares and the ratios of income account for market income inequality and are measured in percentage points. We obtain top income share data from the World Wealth and Income Database (WID, 2015). Income ratios are taken from the OECD Economic Outlook. Due to some missing values, we linearly interpolate these alternative inequality series.

To construct alternative tax shock variables, we retrieve data on the standard rate of the general consumption tax for all the countries in our sample except the United States, where consumption taxes are set by the states rather than the federal government. For European countries, we use information contained in EC, 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 information contained in Laeven and Valencia, 2012. Moreover, from the OECD Economic Outlook we collect: (i) government consumption as a percentage of GDP, (ii) the consumer price inflation rate, (iii) the employment rate, defined as employed people as share of total population aged between 15 and 64 years, (iv) average hours worked per employed

[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 labor force participation and unemployment rates among all age groups.

[10] Top income shares are estimated from tax filing data and are based on market incomes. They are used as proxies for the concentration of incomes in the right tail of the income distribution. The ratios of income are estimated from survey data.

individual, (v) GDP per hour worked. Finally, we collect data on (i) imports and exports as a percentage of GDP, (ii) the trade balance as a percentage of GDP, and (iii) gross savings as a percentage of GDP from WB, 2015.

3.2.2 Methodology

For the econometric analysis, we make use of PVAR models. By adopting a multi-equation methodology, we account for potential interactions among the endogenous variables that might be otherwise overlooked within a single-equation framework.[11]

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 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 country-fixed effects and country-specific linear time trends.[12] Additionally, we include time-fixed effects to control for unobserved common factors. In Section B.3 of Appendix B, we show that our main results are robust to the inclusion of alternative deterministic components.

Our PVAR takes the following standard form:

$$y_{i,t} = A_0 + A_1 y_{i,t-1} + A_2 y_{i,t-2} + \alpha_i + \delta_t + \tau_{it} + \epsilon_{i,t} \tag{3.1}$$

where the sub-indexes (i, t) refer respectively to country and time, $y_{i,t}$ is the vector of endogenous variables, the As 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 labor force participation rate (both in percentage points).

Following Sims, 1980, the endogenous variables enter the PVAR in levels. This allows us to model possible cointegrating relationships among them. In line with 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. In Section B.3 of Appendix B we show that our results are robust to different lag specifications and to using first differences rather than levels. After adjustments, and due to some missing data, we have a total of 479 observations.

[11] 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, Leigh, and Pescatori, 2014, and Alesina, Favero, and Giavazzi, 2015.

[12] We include linear trends since the real GDP, the Gini index and the labor force participation all display a trending behavior.

To construct our tax-based consolidation shock variable, we create a dummy d_t^1 taking value 1 in years where governments implement a tax-based consolidation and 0 otherwise. We then interact this dummy variable with the first difference of total tax revenues as a percentage of GDP. Next, we define direct (indirect) tax-based consolidations as those episodes in which (i) governments implement a tax-based consolidation and (ii) the change in direct (indirect) tax revenues is larger than that of indirect (direct), property and wealth tax revenues. We then create a dummy variable d_t^2 (d_t^3) for direct (indirect) tax-based consolidations and we interact it with the change in direct (indirect) tax revenues. We further distinguish between personal, corporate, SSC, GT and SGS tax-based consolidations. To create the respective shock variables we proceed in a fashion similar to what described above.

To sum up, our shock variables are constructed according to the following formula:

$$X_{i,t}^j = d_{i,t}^j \Delta t_{i,t}^j \tag{3.2}$$

where $j = 1$ stands for overall tax-based consolidation, and $j = 2, .., 8$ for direct, indirect, personal, corporate, SSC, GT and SGS tax-based consolidation; $t_{i,t}^j$ denote revenues stemming from tax instrument j and Δ is the first difference operator.[13] We report the number of observations for each tax-based consolidation sample and its mean value in Table 3.4.

Table 3.4: Mean value and frequency of tax-based consolidation shocks

	Overall	**Direct**	**Indirect**	**PIT**	**CIT**	**SSC**	**GT**	**SGS**
Mean	0.57	0.66	0.51	0.52	0.52	0.41	0.61	0.27
Obs.	73	43	23	28	9	6	13	10

Notes: Mean refers to the mean value of tax-based consolidation shock variables (in percentage of GDP). Obs. refers to the number of each tax-based consolidation shock. PIT, CIT, SSC, GT and SGS stand for, respectively, personal income taxes, corporate income taxes, social security contributions, general taxes and specific goods and services. Direct and indirect tax-based consolidations do not sum up to the number of overall tax-based consolidations since in 7 instances the change in property and wealth taxes was higher than that of direct and indirect taxes.
Source: OECD Revenue Statistics Database and own calculations.

Our approach to construct the tax shocks using tax revenues has the advantage of capturing the effects of policy interventions on both the tax rate and the tax base. Indeed, changes to the tax base are fairly common. Tax credits, exemptions, or deductions are often introduced or removed. Even not indexing the nominal threshold defining the different brackets of the personal income tax to the price level amounts to a change in the base. Ideally, we would like to use a proxy for changes

[13] We also estimate the model using alternative shock variables, which we construct by interacting the change of revenues stemming from each tax instrument with the dummy variable $d_{i,t}^1$ taking value 1 in years where governments implement a tax-based consolidation and 0 otherwise. The results are qualitatively in line with those obtained using our standard shock variables.

in the tax rate and one for the tax base. However, due to lack of a quantifiable measure for the tax base, using tax revenues is the most suited approach to address our research question.

Given the characteristics of the action-based datasets, which identify consolidation episodes that were motivated by the sole objective of reducing the budget deficit, the most natural way to identify the PVAR in Equation (3.1) 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 first, we impose this to be contemporaneously unaffected by GDP, the Gini index, the unemployment rate or the labor 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 (see Christiano, Eichenbaum, and Evans, 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 both the unemployment and the labor force participation rates are in percentage points.

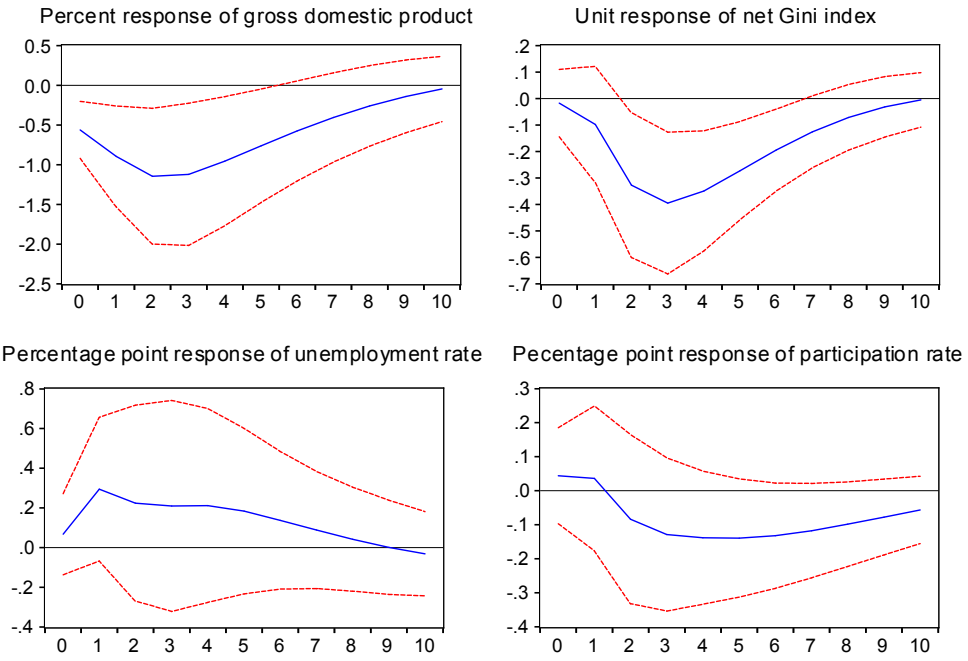
3.3 Overall Effects of Tax-based Consolidations

In this section, we present our baseline results. Figure 3.1 shows the response of real GDP, the disposable income Gini index, the unemployment rate, and the labor force participation rate to a 1% of GDP increase in total tax revenues during tax-based consolidation episodes. The response of the Gini index is not statistically different from 0 on impact, but it turns negative and significant two years after the shock (in the order of -0.3 percentage points), and it remains significant up to 6 years after the shock. In absolute value, the 0.4 drop in the Gini index after three years (the peak response) is equal to about a tenth of its sample standard deviation. Hence, in line with Agnello and Sousa, 2014, our estimates suggest that tax-based consolidations have statistically significant and economically meaningful effects in decreasing disposable income inequality. These effects disappear in the longer-run.

Consistently with the previous empirical literature (see, among others, Guajardo, Leigh, and Pescatori, 2014, Alesina, Favero, and Giavazzi, 2015, and Jordà and Taylor, 2016), a 1% of GDP increase in tax revenues has significant negative short to medium-run effects on real GDP. Output decreases by 0.6% on impact, by 0.9% after

[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 level of statistical significance.

Figure 3.1: The effects of 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.

1 year, 1.1% after 3 years, and 0.8% after 5 years. The effect becomes insignificant in the long run. The responses of the unemployment rate and the labor force participation rate are respectively positive and negative in the short to medium term, which can be interpreted as a consequence of the economic contraction. However, neither of them is statistically different from 0. In general, the dynamics of the macroeconomic effects that we estimate, which vanish within a 5-year horizon after the shock are consistent with the model-based evidence provided by Coenen et al., 2012.

We now investigate to what extent changes in government redistribution may be driving our result that tax-based consolidations decrease income inequality. To do so, we estimate a 6-variable PVAR featuring both the market and the disposable income Gini indexes. The difference between them can be interpreted as a measure of the reduction of inequality that is achieved through taxes and transfers, with higher values indicating more redistribution. If the two variables exhibit similar responses, then we could hypothesize that changes in disposable income inequality are mostly driven by changes in the market income distribution. Conversely, if the responses are different, changes in the amount of government redistribution could (also) be driving the response of disposable income inequality. Table 3.5 reports relevant results. The estimates for the market and the disposable Gini indexes are very similar. That

suggests that the reduction in inequality observed in our baseline specification is achieved mainly through a reduction in market income disparities.

Table 3.5: Augmented specification with both gross and net income Gini indexes

	Impact	1y	3y	5y	10y
GDP	**-0.64**	**-1.00**	**-1.15**	-0.65	0.10
Disposable Gini	-0.04	-0.13	**-0.41**	**-0.26**	0.02
Market Gini	-0.12	-0.18	**-0.57**	**-0.30**	0.03
Unemployment	0.06	0.29	0.19	0.12	-0.09
Participation	0.06	0.04	-0.14	-0.16	-0.04

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

To verify the validity of our results, we carry out a number of robustness checks on our baseline model. We report relevant results in Section B.3 of Appendix B. We first estimate responses to a 1% increase in total taxes during both any consolidation year and a spending-based consolidation year. The estimated coefficients suggest that the contemporaneous presence of spending cuts and tax hikes might confound the results on the effects of tax hikes. That is why we exclusively focus on tax-based consolidations. Next, we show that our baseline results are not biased by (i) anticipation effects, (ii) reverse causality issues, or (iii) episodes of consolidations during which revenues actually decrease. Although in Appendix B.1 we already checked that our tax shock variable is orthogonal to contemporaneous changes in output growth, we also show that our results are robust to using an alternative shock variable employing cyclically adjusted taxes. Finally, we estimate the model employing the original real-time tax estimates compiled by Devries et al., 2011 and Alesina et al., 2015 and show that using such measure the results are qualitatively in line but quantitatively weaker than under our baseline, thus highlighting the importance of using *ex-post* as opposed to *ex-ante* measures of the fiscal consolidation effort.

As additional robustness checks, we repeat the estimation (i) including different deterministic components, (ii) using different lags specifications, and (iii) relying on the local projection method. Moreover, we show that our results are not driven by (i) particular countries, (ii) time periods, and (iii) shock outliers. We also estimate the model including a set of control variables commonly used in the literature. Finally, we use alternative measures of inequality, such as income ratios and the top income shares. Overall, these robustness checks confirm the validity of our baseline results.

3.4 Composition Effects

3.4.1 Direct and Indirect Tax-based Consolidations

In the previous section, we have analyzed the effects of changes in overall taxes during fiscal consolidation episodes. In what follows, we disentangle the effects of specific tax instruments. As a first step, we estimate IRFs to a 1% of GDP in direct and indirect tax-based consolidation shocks. Results are presented in Table 3.6.

Table 3.6: Composition effects of tax-based consolidations

	Impact	1y	3y	5y	10y
a) Tax-based consolidation (baseline)					
GDP	**-0.56**	**-0.90**	**-1.12**	**-0.76**	-0.05
Disposable Gini	-0.02	-0.10	**-0.40**	**-0.27**	-0.01
Unemployment	0.07	0.29	0.21	0.18	-0.03
Participation	0.04	0.04	-0.13	-0.14	-0.06
b) Direct tax-based					
GDP	**-0.59**	-0.72	-1.13	-0.80	-0.06
Disposable Gini	0.03	0.13	-0.14	-0.17	0.00
Unemployment	0.06	0.19	-0.06	-0.04	-0.10
Participation	-0.06	0.06	-0.08	-0.08	0.00
c) Indirect tax-based					
GDP	**-1.79**	**-4.10**	**-4.90**	**-3.86**	-0.74
Disposable Gini	-0.32	**-0.79**	**-1.25**	**-0.88**	-0.17
Unemployment	**0.75**	**1.90**	**2.43**	**1.82**	0.10
Participation	**0.59**	0.22	-0.26	**-0.56**	-0.32

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

A typical direct tax-based consolidation does not have significant positive effects in reducing disposable income inequality, while an indirect-based one does. The Gini index decreases on average by 0.8 percentage points the year after an indirect tax-based consolidation shock. After three years the decline is equal to 1.3, while after five years it is 0.9. Regardless of the instrument used, a tax shock always has a negative and significant effect on real GDP on impact. However, this effect is stronger and more persistent in the case of indirect tax-based consolidations. Consistently with the large and persistent decline in output, the unemployment rate significantly increases both on impact and in the short to medium term in the case of indirect

taxes. Instead, the labor force participation rate increases by 0.6 percentage points on impact after an indirect tax shock. The reaction of the labor force participation rate decreases and even turns negative and significant after five years, which we interpret as a lagged response to the contraction in economic activity. In contrast, neither the unemployment rate nor the participation rate displays significant reactions to a direct tax-based consolidation.

Concerning the estimated response of real GDP, our results are supported by recent theoretical work by Gehrke, 2018. In analyzing fiscal policy rules in a new Keynesian model with labor market frictions, Gehrke, 2018 finds that multipliers are larger for consumption than labor taxes. This result hinges on the presence of wage bargaining between firms and employers and on the fact that the path of labor taxes is time-varying. Under Nash bargaining, the wage is a weighted average of the intertemporal firms' profits and the workers' reservation wage (the unemployment benefit) scaled by the labor tax. The latter drives a wedge between the value of working and that of not working. If labor taxes were constant, the wage would be as in typical Nash bargaining model. However, expected changes in labor taxes determine how the workers value the share of future firm profits. A decrease (increase) in the labor tax depresses (increases) wage demands by workers, as it increases (decreases) the value of working. Hence, the after-tax wage barely changes and so does labor supply. Changes in consumption taxes, instead, directly affect aggregate demand, with larger effects on output. This result also holds in a model without labor market frictions. For instance, Forni, Monteforte, and Sessa, 2009 estimate a DSGE model for the Euro area and find relatively strong multipliers for consumption taxes. This effect can be explained by monetary policy being tightened following an increase in consumption taxes (due to the increase in inflation). Higher taxes and tighter monetary policy induce agents to decrease consumption and firms to scale back production, with negative effects on employment.

Instead, our estimates contrast those of Muinelo-Gallo and Roca-Sagalés, 2013, who find that an increase in direct taxes has a negative impact on both net inequality and growth whereas increasing indirect taxes does not have significant effects. However, these authors analyze the effects of budget-neutral changes in fiscal policy rather than of fiscal consolidations. Moreover, their model neglects potential dynamic effects, and it assumes market income inequality to be exogenous, whereas it may well be endogenous to both growth and fiscal policy.

We now narrow the focus on the effects of indirect taxation on inequality. Most of the literature assumes indirect taxes to be regressive, since low-income agents normally spend a larger fraction of their income on consumption goods relative to high-income households. However, following this argument, to find a direct positive effect of indirect taxes on inequality, a measure of consumption-based inequality should be used. Unfortunately, limited cross-country data prevents us from investigating further the validity of this line of thought.

On the other hand, using data on income inequality, we find that indirect taxes improve equity. This outcome may be partly explained by the operation of a positive

labor supply channel. Our results suggest that indirect taxes create incentives for agents to participate more actively in the labor market. That may be due to a negative income effect, since indirect taxes raise the price of the consumption basket. Although we cannot estimate it due to lack of data on labor market outcomes by income groups, the extent of the labor supply channel is likely to be stronger for lower-income agents. The reason for this is that they tend to spend a more substantial fraction of their income on consumption goods. Hence, they should be relatively more affected by an increase in indirect taxes.

The mechanisms described above are supported by the theoretical analysis developed by Coenen, Mohr, and Straub, 2008. These authors develop a DSGE model with two types of agents: unconstrained (Ricardian) agents and liquidity constrained (hand-to-mouth) agents, who can be considered as a proxy for, respectively, high- and low-income agents. Coenen, Mohr, and Straub, 2008 show that an increase in the consumption tax dampens the consumption of both types of agents. However, while unconstrained/high-income agents reduce their labor supply, constrained/low-income agents tend to work even more, since they rely mainly on adjusting the supply of labor services to smooth consumption. Conversely, an increase in the labor income tax reduces both consumption and hours worked. Hence, similar to our analysis, in Coenen, Mohr, and Straub, 2008 the positive labor-supply channel is at work when indirect taxes increase, but not when the personal income tax increases. Other papers stressing the role of labor-supply channels in the analysis of the distributional effects of taxation are Buscher et al., 2001, Böhringer, Boeters, and Feil, 2005, and Pestel and Sommer, 2017. Crucially, in line with our results, these papers show that the labor-supply channel is more relevant for low-income agents.

The hypothesis that the marginal propensity to work may vary with income or wealth has also been theoretically investigated by Athreya, Owens, and Schwartzman, 2016. In their analysis, a wealth-based redistribution program from high-income to low-income households can decrease the labor supply of low-income agents by more than it raises that of high-income agents, if at least some of the former are borrowing-constrained. This is because borrowing-constrained agents will not save any portion of the transfer, thus increasing their consumption and leisure time.

Additionally, Blundell, Pistaferri, and Saporta-Eksten, 2016 have further highlighted another form of labor supply heterogeneity, namely within household heterogeneity. In their model, female labor supply serves as an important household insurance mechanism and their findings evidence how spouses react differently to wage changes. In this light, we next investigate the role of female labor supply in the context of our analysis.

We extend our baseline specification on the effects of an indirect tax-based consolidation by adding more variables. Results are reported in Table 3.7. First, we add the inflation rate (Panel (b)). This goes up by 1.9 and 2.3 percentage points respectively on impact and after one year, thus confirming that the price

Table 3.7: Additional results on indirect tax-based consolidations

	Impact	1y	3y	5y	10y
a) Indirect-tax based					
GDP	**-1.79**	**-4.10**	**-4.90**	**-3.86**	-0.74
Disposable Gini	-0.32	**-0.79**	**-1.25**	**-0.88**	-0.17
Unemployment	**0.75**	**1.90**	**2.43**	**1.82**	0.10
Participation	**0.59**	0.22	-0.26	**-0.56**	-0.32
b) Inflation rate					
GDP	**-1.64**	**-3.81**	**-4.56**	**-3.70**	-0.90
Disposable Gini	-0.29	**-0.72**	**-1.17**	**-0.85**	-0.17
Unemployment	**0.67**	**1.72**	**2.23**	**1.70**	0.17
Participation	**0.53**	0.13	-0.21	**-0.51**	**-0.34**
Inflation rate	**1.88**	**2.34**	-0.41	**-0.48**	-0.13
c) Men and women participation					
GDP	**-1.76**	**-3.93**	**-4.48**	**-3.41**	-0.64
Disposable Gini	-0.26	**-0.67**	**-1.06**	**-0.75**	-0.16
Unemployment	**0.70**	**1.77**	**2.16**	**1.55**	0.05
Men participation	0.06	0.19	-0.24	-0.41	-0.20
Women participation	**1.08**	0.21	-0.22	**-0.54**	-0.30
d) Men and women participation and employment					
GDP	**-1.85**	**-4.10**	**-4.75**	**-3.68**	-0.77
Disposable Gini	-0.27	**-0.66**	**-1.04**	**-0.76**	-0.18
Men employment	-0.64	**-1.70**	**-2.16**	**-1.72**	-0.10
Women employment	**0.64**	-0.56	**-1.48**	**-1.55**	-0.49
Men participation	0.08	0.21	-0.20	-0.37	-0.22
Women participation	**1.13**	0.24	-0.17	-0.48	-0.35
e) Men and women participation and employment, 45 to 54 year age group					
GDP	**-1.89**	**-5.27**	**-6.86**	**-4.82**	-1.11
Disposable Gini	-0.39	**-0.86**	**-1.30**	**-0.97**	-0.36
Men employment, 45 to 54	-0.55	**-1.90**	**-2.75**	**-2.21**	-0.41
Women employment, 45 to 54	**1.35**	-0.01	**-1.34**	**-1.53**	-0.85
Men participation, 45 to 54	-0.16	-0.51	-0.54	**-0.45**	-0.12
Women participation, 45 to 54	**1.59**	0.66	0.01	-0.34	-0.56

Notes: Panel (a)-Panel (e) report the response to a 1% of GDP indirect tax-based consolidation under alternative PVAR specifications. Bold numbers indicate significance at the 10% confidence level.

of the consumption basket does increase following an indirect tax-based consolidation. Second, we distinguish between women and men labor force participation rates (Panel (c)). In accordance with the several contributions emphasizing higher participation elasticities for women (see for instance Blundell, Pistaferri, and Saporta-Eksten, 2016 and Bargain, Orsini, and Peichl, 2011), we find a significant response of 1.1 percentage points in female labor force participation, while the response of male participation is not significant. Third, to check that higher participation is reflected in higher employment, we estimate the model including employment rates as percentages of the population instead of the unemployment rates (Panel (d)). Women employment increases by 0.6 percentage points on impact, whereas the change in men employment is negative (-0.6), but not significant. In the medium term, as the depth of the recession gets larger, both male and female employment rates significantly decrease. However, the magnitude of the declines is smaller for women than for men.

Finally, we estimate the model including both female and male participation and employment rates for different age groups (15 to 24, 25 to 34, 35 to 44, 45 to 54 and 55 to 64). In line with the findings of Blundell, Pistaferri, and Saporta-Eksten, 2016, we hypothesize that second earner spouses should display larger labor supply elasticities. In turn, these are more likely to be prime age women. Hence, we expect stronger responses for women in the 35 to 44 and the 45 to 54 age groups. In Table 3.7 we only report results for the 45 to 54 age group (Panel (e)). This, together with the 35-44 age group is the only one to have significant coefficients. For both groups, the impact responses of female participation and employment are positive and significant, at 1.6 and 1.4 percentage points respectively, whereas those of men are not.

These findings confirm our earlier hypothesis about the existence of a positive labor supply channel of indirect taxes and further point to important gender and age heterogeneities in the agents' labor supply responses. Our results further suggest that, by boosting women participation and employment, higher indirect taxes might also reduce gender inequality.[15] Unfortunately, scarce data availability prevents us to also explore income as another source of heterogeneity in the agents' response to changes in indirect taxes, which could determine the negative effects of indirect tax-based consolidations on equality.[16]

Before narrowing the analysis further down, we perform a number of robustness checks. First, we check whether our results remain valid once including both direct and indirect tax-based consolidation shocks simultaneously rather than one at a time. Second, we check whether our results are driven by a particular country. To

[15] We also estimated the model including both female and male participation by age group for direct tax-based consolidations and found again positive, although not significant, responses for middle-aged women, and negative responses for men.

[16] An alternative, but not mutually excluding, channel may be working through the effects of indirect taxes on inequality via capital income and labor 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.

this purpose, we estimate the model excluding one country at a time. Finally, we estimate the model using our alternative measures of income inequality: the top income shares and the income ratios. We report and discuss in greater detail all robustness checks in Section B.4 of Appendix B. Overall, our main results are robust to these variations.

3.4.2 Composition Effects of Direct Tax-based Consolidations

In this section, we examine the effects of specific direct tax instruments. In particular, we focus on personal, corporate and SSC tax-based consolidations. Our analysis so far has suggested that direct tax-based consolidations only have a significant negative impact effect on GDP. However, it may be that different instruments have different effects. For instance, personal taxes are generally deemed to be more progressive — and therefore more redistributive — than SSC. This is all the truer 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. Corporate taxes, instead, may have ambiguous effects on income inequality, as capital income owners may shift the tax burden on wage earners (see Bastagli, Coady, and Gupta, 2012 for a discussion).

Table 3.8 shows the estimates to a 1% of GDP personal, corporate and SSC tax-based consolidation shock.[17] To ease the comparison, in Panel (a) we also display the baseline results for direct tax-based consolidations. The estimates point out that personal tax-based consolidations have a significant effect in reducing income inequality. Following a 1% of GDP shock, the Gini index decreases by 0.7 percentage points after three years and by 0.6 percentage points after five years. Real activity also drops in the short to medium term, with GDP declining by 0.9%, 1.3%, 2.1%, and 1.7% respectively on impact and after one, three, and five years. Instead, neither the unemployment rate nor the labor force participation rate is significantly affected, although the estimates have the expected positive and negative signs.

A corporate tax-based consolidation does have negative, although not statistically significant, effects on GDP. The effects on the other variables are negligible. Since capital profit earners usually have higher incomes than wage earners, in principle, we 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, Coady, and Gupta, 2012). This would explain the muted response in the Gini index. Our estimates of an SSC tax-based consolidation show a significant positive short-term response of the labor force participation. However, this result is driven by one particular country and does not survive when this country is excluded

[17] These results are robust to different PVAR specifications in which several shocks enter at the same time. Estimates are available upon request. This comes as no surprise given the small correlation coefficients between SSC, personal and corporate taxes (see Table 3.3).

Table 3.8: Composition effects of direct tax-based consolidations

	Impact	1y	3y	5y	10y
a) Direct tax-based					
GDP	**-0.59**	-0.72	-1.13	-0.80	-0.06
Disposable Gini	0.03	0.13	-0.14	-0.17	0.00
Unemployment	0.06	0.19	-0.06	-0.04	-0.10
Participation	-0.06	0.06	-0.08	-0.08	0.00
b) Personal tax-based					
GDP	**-0.87**	**-1.34**	**-2.10**	**-1.72**	-0.38
Disposable Gini	-0.08	-0.09	**-0.66**	**-0.58**	-0.05
Unemployment	0.31	0.50	0.52	0.47	0.04
Participation	-0.04	0.06	-0.21	-0.30	-0.16
c) Corporate tax-based					
GDP	-1.23	-1.59	-2.14	-1.45	-0.22
Disposable Gini	0.25	0.42	0.10	-0.04	-0.03
Unemployment	0.15	0.23	0.17	0.08	-0.20
Participation	-0.10	-0.14	-0.14	-0.10	0.03
d) SSC tax-based					
GDP	1.87	1.91	-3.40	-3.67	-1.88
Disposable Gini	-0.29	0.00	0.77	0.28	-0.23
Unemployment	-0.89	-1.27	-2.92	-0.62	0.05
Participation	**1.24**	**2.63**	1.27	0.82	0.26

Notes: The table reports the response to a 1% of GDP direct, personal, corporate and SSC tax-based consolidation shock. SSC stands for social security contributions. Bold numbers indicate significance at the 10% level.

from the sample (see the country stability robustness check in Section B.3 of Appendix B).

Our results are partly in line with those of Martínez-Vázquez, Vulovic, and Dodson, 2012, who analyze how changes in tax revenues affect inequality in a panel of 150 countries over the 1970-2009 period using the Generalized Method of Moments estimation. They find that personal income taxes have a significant adverse 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.

3.4.3 Composition Effects of Indirect Tax-based Consolidations

Indirect taxes comprise several instruments. We focus on general taxes (GT) and specific goods and services (SGS) taxes, as revenues stemming from other instruments only show marginal changes during tax-based consolidations. 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 labor market variables. On the one hand, higher GT and SGS taxes decrease the marginal return of labor. Hence, agents might respond by substituting away labor for more leisure time (i.e., substitution effect). On the other hand, since agents' real income decreases, they could respond by supplying more labor (i.e., income effect). Our previous findings suggest that overall the income effect may dominate the substitution effect. We now investigate whether these results hold for both the sub-components of indirect taxes.

Estimates for GT and SGS tax-based consolidations are presented in Table 3.9 (Panels (b) and (c)). In the medium term, a GT shock significantly lowers inequality, with the Gini index decreasing 0.6 percentage points after five years. At the same time, GT produce a statistically significant decline in real economic activity, with GDP declining by 3.6 and 3.3 percentage points respectively after three and five years. The response of labor participation is positive and significant, at 0.5 percentage points, on impact and remains positive up to three years after the shock, although it loses significance. Therefore, the income effect seems to dominate the substitution effect.

To provide further empirical evidence backing our results on GT, we estimate responses to a one percentage point increase in either the standard VAT rate or the goods and service tax (GST) rate during both tax-based and GT-based consolidation years.[18] We report IRFs in Panel (a) and (b) of Table 3.10 below. The estimates confirm what already emerged above. A one percentage point increase in the standard GT rate raises the labor force participation rate by 0.2 percentage points both on impact and one year after the shock. The response of the disposable Gini index is negative throughout all the horizon, although not statistically significant.

[18] Several countries introduced a GT tax only after the beginning of our sample. This is the case for Australia (in 2000), Canada (in 1991), Finland (in 1994), Japan (in 1989), Portugal (in 1986) and Spain (in 1986). Moreover, in the United States, GST rates are fixed by local (State) governments and not by the federal government. Hence, we exclude the United States 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 United States. Of these 15 episodes, we exclude one episode, namely Ireland in 1984, since in that occasion the government drastically overhauled the VAT system. More precisely, 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 3.9: Composition effects of indirect tax-based consolidations

	Impact	1y	3y	5y	10y
a) Indirect tax-based					
GDP	**-1.79**	**-4.10**	**-4.90**	**-3.86**	-0.74
Disposable Gini	-0.32	**-0.79**	**-1.25**	**-0.88**	-0.17
Unemployment	**0.75**	**1.90**	**2.43**	**1.82**	0.10
Participation	**0.59**	0.22	-0.26	**-0.56**	-0.32
b) GT tax-based (sales and value added tax)					
GDP	-1.02	-1.89	**-3.61**	**-3.27**	-1.10
Disposable Gini	-0.14	-0.43	-0.72	**-0.61**	-0.22
Unemployment	0.59	0.91	1.62	**1.45**	0.26
Participation	**0.53**	0.35	0.28	-0.19	-0.23
c) SGS tax-based (excises, monopoly, customs and others)					
GDP	**-3.63**	**-10.75**	**-11.36**	**-8.10**	-0.50
Disposable Gini	**-0.97**	**-2.06**	**-3.06**	**-2.06**	-0.16
Unemployment	1.37	**4.95**	**5.86**	**4.10**	-0.16
Participation	1.08	0.05	**-1.66**	**-1.84**	-0.77

Notes: The table reports the response to a 1% of GDP indirect, GT and SGS tax-based consolidation shock. GT and SGS stand for, respectively, general taxes and specific goods and services. Bold numbers indicate significance at the 10% confidence level.

As an attempt to disentangle which income group is more likely to benefit during a GT-based consolidation, we also report estimates using the income ratios of the agents in the 90^{th}, 50^{th} and 10^{th} percentile of the income distribution (Panel (c), (d) and (e) of Table 3.10). These results suggest that both low and middle-income households gain relative to rich households, but low-income ones gain more.

To summarize, the underlying mechanism we have in mind works as follows: a hike in GT pushes up inflation and hence decreases households' real income. The income loss creates incentives for agents voluntarily out of the labor force to search for a job and for those working part-time to increase working hours. In turn, as agents join the labor force, their probability of becoming employed increases. We believe this labor supply channel to be particularly strong for female second-earners in low- and middle-income households.

Turning to SGS taxes (Panel (c) of Table 3.9), the estimates we obtain are more difficult to rationalize. An SGS tax-based consolidation displays extremely large negative multipliers. However, this result is driven by a single country in the sample, namely Portugal (see the country stability robustness check in Section B.3 of

Table 3.10: Additional results on GT tax-based consolidations

	Impact	1y	3y	5y	10y
a) Any tax-based consolidation year					
GDP	**-0.45**	**-0.68**	**-0.91**	**-0.63**	-0.10
Disposable Gini	-0.03	-0.01	-0.12	-0.11	-0.06
Unemployment	0.06	0.11	0.16	0.19	0.04
Participation	**0.18**	0.18	0.00	-0.04	-0.04
b) GT-based consolidation year					
GDP	-0.30	-0.48	-0.79	-0.57	-0.10
Disposable Gini	-0.01	-0.03	-0.16	-0.13	-0.05
Unemployment	0.03	0.08	0.05	0.13	0.04
Participation	**0.18**	0.16	0.00	-0.04	-0.04
c) P90/P10 Income ratio					
GDP	-1.02	**-2.67**	**-4.57**	**-3.32**	**-1.45**
P90/P10	**-16.11**	**-11.43**	-4.03	-1.35	0.46
Unemployment	**1.06**	**1.82**	**2.35**	1.04	0.18
Participation	0.29	0.26	0.09	-0.17	-0.05
d) P90/P50 Income ratio					
GDP	-1.02	**-2.65**	**-4.52**	**-3.31**	-1.45
P90/P50	**-6.56**	-3.33	-0.56	-0.69	0.13
Unemployment	**1.05**	**1.81**	**2.32**	1.04	0.18
Participation	0.28	0.26	0.09	-0.16	-0.05
e) P50/P10 Income ratio					
GDP	-1.10	**-2.73**	**-4.58**	**-3.26**	-1.40
P50/P10	-2.67	-2.79	-2.31	-0.17	0.28
Unemployment	**1.09**	**1.87**	**2.36**	1.01	0.17
Participation	0.29	0.25	0.08	-0.18	-0.05

Notes: Panels (a) and (b) report the response to a 1 percentage point increase in the standard general tax rate. Panels (c), (d) and (e) report the response to a 1% of GDP shock in GT tax-based consolidations, using income ratios as measure of inequality. Bold numbers indicate significance at the 10% confidence level.

Appendix B). When Portugal is excluded, the response of GDP is still negative, but much smaller in absolute value. SGS tax-based consolidations have larger and more

immediate effects in reducing the Gini index relative to GT-based consolidations, and this result is robust to the exclusion of Portugal. The Gini index decreases by one percentage points on impact, and it keeps declining over a five-year horizon. In parallel with the economic contraction, the unemployment rate increases in the short run, while the labor force participation rate decreases after three and five 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. That could be explained by deep recessions hitting high-income agents more strongly than low-income agents, thereby causing a decrease in disposable income inequality.

3.5 Conclusions and Further Extensions

In this paper, we use PVAR models to estimate the composition effects of tax-based consolidations on income inequality, real output, and labor market variables in 16 OECD countries during the 1978-2012 period. The results suggest that tax-based consolidations reduce both market and disposable income inequality, but at the cost of a decrease in output in the short to medium run.

Looking at the effects of specific instruments, our results point to a positive labor supply channel through which general consumption taxes decrease income inequality. 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. We observe this positive labor supply channel to be particularly relevant for middle-aged women in low- and middle-income households. Higher participation rates increase the probability of being employed and ultimately reduce income inequality. Among direct taxes, only personal income taxes increase equity, without having significant negative effects on labor force participation.

In general, our results suggest that incentives to labor market participation represent an important channel through which different tax instruments may affect income inequality. Instead, we do not find much evidence supporting the hypothesis of a government redistribution channel through which tax-based consolidations decrease inequality.

A possible criticism of our analysis is that we only look at agents' aggregate behaviors. We acknowledge that different population groups may react heterogeneously to taxation. We partially address this issue when we analyze the response of labor 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. Another interesting avenue for future research would also be to set-up a theoretical macroeconomic model

that could more formally explain the different channels emphasized in our empirical analysis, as well their propagation in the short, medium and long run.