HOW can fiscal policies MiTIGATE inequalitY?

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# Abstract

*How effective are the different fiscal instruments in mitigating inequality? Higher levels of market inequality have renewed the debate about whether fiscal policies could smooth the distribution of income. To address this question, we use a Local Projection approach to assess the dynamic effects of growth, fiscal and taxing policies on net income inequality for 23 advanced economies during the period 1990–2018. We find that government expenditure is more helpful in mitigating inequality than personal income taxation. In particular, transfers in cash seem to be more efficient in reducing inequalities than in-kind social spending. This translates into the particular countries that were hit by recession. Country-group estimates document no further significant differences, except that the personal income taxation effects on reducing inequality seem to be more significant in economies under recession.*

**JEL codes:** O40, O52, D30, E62, C33.

**Keywords:** fiscal policy, income inequality,, Local Projection, economic growth.

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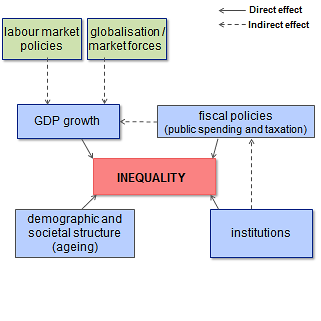
# Introduction

Income inequality has become an issue of considerable renewed attention in developed countries since the recent financial and economic downturn – the Great Recession (e.g., European Commission, 2017; IMF, 2016; OECD, 2014). This has also been reinforced by worries about the distributional consequences of the latest fiscal consolidation episodes (2011-2013), if the burden of the adjustment is not shared evenly.[[1]](#footnote-1) There is supporting evidence that rising income inequality may be associated with lower long-term economic growth.[[2]](#footnote-2) Increasing inequality could be a contributing factor to future global crisis.[[3]](#footnote-3)

Yet, the last two decades have witnessed an irregular evolution of income distribution in developed countries. Emerging common trends cannot be directly detected and there is a growing consensus that inequality is country-specific (Raitano, 2016). Market income inequality (i.e., labour and capital incomes plus private transfers, before taxes and government transfers) has remained at historical high levels in 2016 compared to 1990 in most advanced countries . It is remarkable to note that the generalised substantial deterioration of income market inequality already started during the pre-crisis period 1990-2007. Wealth inequality is another angle to look into the issue with a clearer pattern as wealth is more unequally distributed than income for developed countries.

There is no consensus on the specific channels through which income inequality are affected by economic growth and other factors, including the effect of fiscal policies (European Commission, 2017). But it is still an open empirical question whether government interventions can effectively reduce income inequality due to countervailing interactive effects in the real economy. For example, higher income taxes could eventually decrease growth rates and discourage employment in the short-run. Only few authors have attempted to model the relationship between economic growth and income inequality through the channel of fiscal policies. Roine et al. (2009) establish a negative relationship between government expenditure as well as top marginal tax rates and inequality. Muinelo-Gallo and Roca-Sagalés (2013) show that the best choice in a fiscal consolidation process is to cut non-redistributive expenditure in order to simultaneously increase growth and reduce income inequality. These arguments can be complemented by political-economy explanations. The main idea is that a more unequal economy demands a redistribution financed by distortionary taxes, and a rise in these taxes decreases private investment and consequently reduces economic growth.[[4]](#footnote-4) Empirical evidence, however, does not seem very supportive to this traditional explanation, as they show that redistributive policies are often correlated with income inequality among industrial democracies: more unequal economies tend to redistribute less, not more (Perotti (1996), Alesina et al. (2002)). Diagram 1 proposes the key drivers of income inequality from the literature review[[5]](#footnote-5).

**Diagram 1. Key drivers of income inequality**



This paper investigates the effectiveness of several fiscal policies instruments in reducing income inequality by examining 23 developed countries over 1990-2018.[[6]](#footnote-6) To our knowledge, there is limited updated empirical work testing this relationship after the Great Recession (e.g., Bargain et al. 2018; Fuceri et al. 2018; European Commission, 2017; IMF, 2015). Moreover, the bulk of previous empirical research on the determinants affecting inequality makes use of case studies, descriptive statistics and econometric techniques – mainly regressions on the Gini coefficient. In that way, our model provides evidence that fiscal policies are capable of reducing income inequality despite the possible negative behavioural responses from the beneficiaries. The estimates also consider the effect of the income tax progressivity, which adds additional relevant information about redistribution issues for developed countries. In this vein, the paper reassesses the impact of key fiscal variables on income inequality by using a large dataset including European countries, United Kingdom and the United States. This allows us to tackle any reverse causality and dynamics in the estimation and correct for possible cross-section dependence.

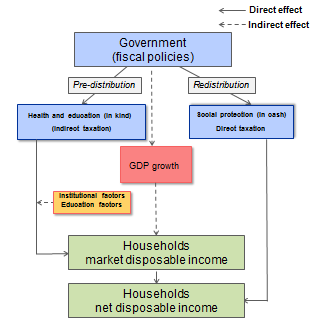
The rest of the paper is structured as follows. Section 2 introduces the role of fiscal policies in the inequality debate. Section 3 presents the data and justifies the econometric methodology used for the empirical analysis. Section 4 presents the main results. Section 5 concludes.

# The role of fiscal policies

Government spending can be classified according to their functions based on COFOG data (see ECB, 2019). Based on their distributive nature,[[7]](#footnote-7) social spending can be classified into health, education and social protection. In practice, social protection represents the largest government expenditure item in developed countries, followed by health and education. Social protection is mostly provided as social transfers in cash directly paid to individual households. The main underlying instruments are pensions (old age and survivors), unemployment, sickness and social assistance, and family and children benefit. Health and education are mostly provided as social transfers in kind, but also benefiting the individual households using these services and goods.

The basis of our paper lies on the relationships depicted in Diagram 2. The government action affects directly and indirectly inequality through the effect on the household income distributions. Government expenditure provided in kind (in particular health and education), economic growth, and institutional factors can affect the inequality of market disposable incomes (incomes before taxes and transfers) via the impact on future earnings of households. Transfers in cash and income taxes (redistributive policies) directly reduce the inequality of disposable income from the market outcomes. More progressive tax systems make the post-tax income distribution more equal. Analysing the net income inequality (this is the Gini coefficient of the household disposable income after direct tax and government transfers in cash) is an important indicator of the effectiveness of the government action in the economy.

**Diagram 2. Transmission channels of fiscal policies to income distribution**



A simple measurement of redistribution is the difference between market income and net income inequality, expressed as a percentage of market income inequality. Figure 1 shows how direct taxes and transfers in cash cushions market income inequality. The percentage reduction of market income inequality due to the direct government action has been growing since the crisis (by about 36% in 2016 on the sample’s average compared to 35% in 2007) and for most countries in the sample. The main exception is Ireland, followed to a lesser extent by Denmark, Italy, Slovakia, Lithuania and Luxembourg. This positive impact from fiscal policies would be actually even larger considering in-kind transfers (education and healthcare), which can be considered as pre-distribution of income. These findings are in line with micro-simulation studies (European Commission, 2017).

|  |
| --- |
| **Figure 1. Percentage reduction of market income inequality due to taxes and in-cash transfers (2007 compared with 2016 or later)**  Source: Authors’ based on the Standardized World Income Inequality Database (SWIID). |

It is also relevant to note the importance of targeting, as benefits in cash are not always tightly targeted to the poorest. We assume that changes in expenditures are implemented efficiently,[[8]](#footnote-8) but the shares of efficiency vary across countries, as indicated in Figure 2. On the one hand, more than 25% of cash benefits reach the poorest 20% working age households in Finland, Netherlands, Germany, Belgium, and Ireland. On the other hand, less than 15% of cash benefits seem go directly to the poorest 20% in Greece, Italy, Portugal, Spain and Luxembourg. These are countries with a strong social insurance dimension where most benefits are related to past earnings.

**Figure 2. Share of cash transfers received by working-age individuals in low and high-income groups, in 2016 (percent)**

Sources: OECD calculations based on OECD Income Distribution Database.

Notes: Data for Germany and Ireland refer to 2015.

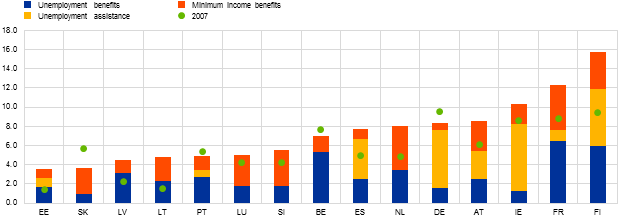
Most European countries also provide minimum pensions that are often income-tested or means-tested.This implies that the benefit is provided only if the income or wealth is below a certain threshold. Minimum pensions are meant to alleviate the poverty risk at the old age and are part of social assistance. Figure 3 shows that in Spain 12% of the total old age pension expenditure is linked to means-testing, followed by Ireland and the Netherlands (above 8%). On the other hand, there is no means-testing in other countries (for instance, Finland) or very little amount paid out after means-testing (for instance, Germany).

**Figure 3. Percentage of expenditure on means-tested old age pensions, in 2016**

Source: Eurostat.

Another angle to analyse the role of fiscal policies is by the provision of income safety net of last resort in case of long-term unemployment. This can be done by combining the unemployment benefits (insurance based on previous contributions) with secondary benefits for those who are no longer entitled to insurance unemployment benefits. The latter are support to the low-income families in the form of unemployment assistance or guaranteed minimum income benefits. Figure 4 shows the shares of working-age individuals receiving out-of-work benefits were highest in Finland, France, and Ireland, with rates above 10% in 2016. On the other hand, less than 5% received at least one of these payments in Estonia, Slovak Republic and Latvia. In general, out of work benefits are higher now than in 2007. The exceptions are Slovak Republic, Portugal, Belgium and Germany.

**Figure 4. Working-age cash transfers paid as a percentage of the working-age population, decomposed by benefit type in 2016 and total level in 2007 (percent)**



|  |
| --- |
| Source: OECD Benefit Recipients Database (SOCR). |

# The empirical strategy

This paper investigates the effect of fiscal policies instruments on income inequality trends by using a dynamic local projection model, starting with Jordà (2005), local projections (LPs) have become an increasingly widespread alternative econometric approach. We alternate different identification strategies and data sources to overcome potential problems of endogeneity.

We look at the effect of three explanatory policy variables of interest – government expenditure, social payments, and progressive taxation – on the Gini coefficient. As for the measurement of income inequality, we rely on the net Gini coefficient. To achieve a sufficient high coverage of countries and points of time, the reliance on the Gini coefficient is inevitable.

* 1. **Data sources and variables**

This study utilizes income inequality indicators from the Standardized World Income Inequality Database (SWIID) because of its better coverage and quality. The SWIID, which is obtained from Solt (2016), maximizes the comparability of income inequality data while preserving the broadest possible coverage across countries and over time. The remaining variables were taken from Eurostat, Federal Reserve Bank of Saint Louis, and the OECD (see Table 1 in the Appendix). The progressivity data are taken from the World Tax Indicators (Sabirianova-Peter et al., 2010).[[9]](#footnote-9) This large and new panel data set covers personal income tax structures at the country level in 189 countries over more than 30 years and contains various important variables such as average and marginal tax rates, progressivity or complexity.

The set of control variables are based on prior specifications of economic growth, inequality and fiscal policy. In order to reduce the specification error bias, we select a commonly accepted specification in the cross-country growth literature that considers growth of GDP per capita, international trade and government current expenditure (e.g., Lundberg and Squire, 2003).

In the analysis, we look at different groups of explanatory variables aimed at capturing the effect of fiscal policies on the net Gini. First, we focus on government expenditure and its in-cash and in-kind transfers, as contributors to redistribution and pre-distribution policies, respectively. Second, we analyse the main in-cash instruments. Third, we look at education and health expenditure, the main in-kind transfers. Fourth, we introduce a new variable for the degree of tax progressivity, as it is also important to analyse the degree of redistribution via the (progressive) direct income tax system, and the effects of direct and indirect taxation.

All variables have been transformed into logarithms. Table 1 below presents the descriptive statistics of the complete set of the model variables. Following De Gregorio (1992), we use log variables to avoid potential endogeneity in some of the regressors, the coefficients allow for an intuitive elasticity interpretation (see Table 1 in the Appendix for definitions and sources).

**Table 1. Descriptive statistics and model variables**

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
| **Variable** | **Observations** | **Mean** | **Std. Dev.** | **Min** | **Max** |
| Gini net income | 636 | 1.463 | 0.062 | 1.246 | 1.589 |
| GDP | 641 | 5.245 | 0.802 | 3.556 | 7.164 |
| GDP nominal growth per capita | 638 | 1.289 | 0.341 | -0.346 | 1.994 |
| Trade openness | 627 | 1.955 | 0.255 | 1.295 | 2.611 |
| Govern. expenditure | 558 | 1.648 | 0.069 | 1.405 | 1.815 |
| **Government expenditure** |  |  |  |  |  |
| Health expenditure | 549 | 0.772 | 0.108 | 0.408 | 0.948 |
| Education expenditure | 549 | 0.721 | 0.082 | 0.48 | 0.903 |
| Social transfers in kind | 563 | 1.132 | 0.111 | 0.845 | 1.364 |
| Social transfers in cash | 556 | 0.459 | 0.349 | -0.699 | 1.362 |
| Pensions transfers | 559 | 0.823 | 0.186 | 0.421 | 1.262 |
| **Government taxation** |  |  |  |  |  |
| Corporate taxes | 572 | 0.366 | 0.216 | -0.583 | 0.891 |
| Indirect taxes | 564 | 0.842 | 0.083 | 0.588 | 1.002 |
| Personal income tax rate | 615 | -1.268 | 0.258 | -2.854 | -0.809 |
| **Other variables** |  |  |  |  |  |
| Secondary educ. (in years) | 536 | 1.596 | 0.189 | 0.973 | 1.876 |
| Employment rate | 582 | 1.824 | 0.057 | 1.685 | 1.983 |
| R&D (% GDP) | 519 | 0.13 | 0.286 | -0.702 | 0.592 |
| Savings (% GDP) | 634 | 1.363 | 0.137 | 0.921 | 1.756 |

Sources: Eurostat, Federal Reserve Bank of Saint Louis, World Tax Indicators, and the OECD.

## **The empirical specification of Local Projection**

As noted in Section 1, the relationship between income inequality and growth has proven difficult to analyse for a number of reasons. First, the two variables may be intertwined, such that income inequality has an impact on growth; at the same time growth has an impact on income inequality. The failure to account for this source of endogeneity may bias the results. Endogeneity may also arise because of omitted variables that are correlated either with income inequality or growth. On the other hand, we followed a promising direction, based on the Rubin Causal Model. Considering semiparametric features are flexible with respect to its functional form, providing better control for observables, and offering a more reliable alternative when the putative instrumental variables for policy action are themselves possibly endogenous.

Second, dynamics are likely to be present, in the sense that the feedback between income inequality and growth occurs gradually over time, and with different intensities especially in the short-run. The previous point raises concerns that the narrative IMF variable could be an invalid instrument using three different checks. The empirical strategy that we propose is based on taking triple insurance against this potential endogeneity. First, we take the episodes of consolidation from the IMF narrative variable as the subset of all consolidation episodes that are a candidate for random allocation. Think of it as a pseudo-IV step. Second, we include the extended set of covariates from Tables 6 and 7 and add them as right-hand side variables in the LP of expression 1. Third, we use inverse propensity score weighting on this LP to re-randomize allocation of the IMF fiscal consolidation events. Third, there is no reason for which these relationships would be the same across countries. In fact, they may differ because of the economic structure, institutions, and inclusiveness policies, among others. Finally, common shocks might induce cross-section dependence and omitted variable bias.

We use an unbalanced panel of data from 23 developed countries over the period 1990-2018 and estimated a dynamic local projection model (Jorda, 2005), local projections (LPs) have become an increasingly widespread alternative econometric approach. The reason is that, LPs are a convenient pedestal on which all extensions of existing estimation methods can rest. The unified framework provides a way to compare the results across a set of nested estimation strategies. LPs provide a flexible semi-parametric regression control strategy to estimate dynamic multipliers and include, as a special case, impulse responses calculated with an SVAR. Traditional econometric methods such as ordinary least squares (OLS) are based on mean estimates of the parameters ignoring the distribution characteristics, and do not seem to estimate the genuine relationship between growth and inequality properly because they cannot capture the interactive dynamics. From this perspective, the structural panel VAR model captures such interactive feedback dynamics, LPs are efficient and are more robust to model misspecification. In this way, our paper sheds some light in identifying the relationship between economic fiscal policies, growth and inequality.

We use a Local Projection model, which controls for country fixed-effects. This method results in a distribution of *impulseresponse* functions permitting a much more robust inference than those relying on average estimates which assume commonly slopes homogeneity. The model allows to be fully heterogeneous amongst panel members. In that context, each member *i = [1…M]’*, being yi,t a vector of *n* endogenous variables with country specific time dimensions *t = [1,…Ti]’.[[10]](#footnote-11)* To control for individual fixed-effects, we demean the data, resulting in , where represents the average of the variable for each *i.* Thus, we estimate the following model, The LP impulse response function of yt with respect to *FVh, xt* are given by (βh)h≥0, and in Eq. (1).

To create a benchmark estimating equation that mimics the standard setup in the literature, the typical LP equation that we estimate has the form:

(1)

for h = 1, ..., 5, and where yi,t+h − yi,t denotes the cumulative change from time t to t + h in 100 times the log of real GDP, the are country-fixed effects, and Di,t denotes the differences fiscal policy variable (measured from time t to time t + 1), (economic growth and government expenditure), finally is the projection residual Notice also that we do not require xt to be a predetermined “shock” variable.

. Instead, we impose a temporal ordering on the set of variables to estimate the function that draws the current distribution into parameters characterizing the next period distribution, conditional on vector Xt. For example, the Gini index, which itself in turn does not react within the period to changes in fiscal policies and money growth. The basic idea for our empirical exercise is:

*Left-hand variables = Ginii,t, net income*

*Right-hand variables = Ginii,t, Growthi,t, Fiscal policies (FVi,t)* two exogenous variables Xi,t (trade openness and total government expenditure), and (D) dummy variable for 2008, which captures the effect of economic recession.

(2)

Intuitively, this projection uncovers the linear combination of the data that best explains long-run movements in the Gini index. By assumption, as possible explanatory variables we consider the GDP growth and Fiscal policies. Thus, to estimate impulse responses with respect to the *X* variable, we can run the local projection (Eq. 1) with and with *y­t* given by the response variable of interest (either ∆gdpt or FVt).

The Local Projection model provide a useful empirical methodology to investigate the issue at stake for the following reasons. First, we can infer dynamic properties since it captures the effects of the changes of income and inequality over time as influenced by growth and changes in the fiscal side. Thus, we can characterize the relationships between fiscal policies, growth and inequality via dynamic responses. Following, as long as the identification permits, any structure of the previous interactions on inequality can be allowed in the model. Previous studies of single equation empirical models did not consider such interactions among fiscal variables.

Here we test the extended specifications that assess the role of additional fiscal instruments that are commonly found in the literature (Cingano, 2014; Doerrenberg and Peichl, 2014; Muinelo-Gallo and Roca-Segalés, 2013), i.e. government transfers, and government taxation (in this case, we test the progressivity of income tax, as long as the effect of direct and indirect taxation).. We assume that the Gini is likely to respond contemporaneously to the group of fiscal policy variables (*FV*). Similarly, if parameter *β11* is between zero and one, it implies that the inequality tends to decline in regions/countries with higher initial inequality, analogous to the inequality convergence analysed by Benabou (1996) and Ravallion (2003).

Moreover, given the reasonable large time length of our data set, we exploit the panel structure of the data that is not reliable in a traditional VAR estimate. We calculate the impact of fiscal policy shocks based on LPs using a set of social transfers, government expenditure and taxing. We also restrict our attention to “large” shocks (changes in GDP) to introduce and observe the “booms and bust” results. However, when we condition on the state of the economy, we find that this result is driven entirely by what happens during a boom. The expansionary effects of fiscal consolidation evaporate when the economy is in a slump.

Fifth, in order to purge remaining allocation bias, we use inverse probability weighting (IPW) estimation based on a prediction model of the GDP shocks variable to estimate the LP responses, we consider these policy variables as a “fiscal treatment”—i.e., a binary indicator rather than a continuous variable—and we are interested in characterizing a dynamic average treatment effect (ATE). We follow an augmented regression-adjusted estimation instead, denoted AIPW, which combines inverse probability weighting with regression control adjusting the estimator to achieve semi-parametric efficiency (see, e.g., Lunceford and Davidian 2004). Our AIPW estimator falls into the broad class of “doubly robust” estimators of which Robins, Rotnitzky, and Zhao (1994). The “doubly robust” property means that consistency of the estimated ATE can be proved in the special cases where either the propensity score model and/or the conditional mean is correctly specified.

Note that this partition is meant to provide a more granular statistical summary of the main features of the data. We are not arguing whether or not a boom or a slump is more likely under a particular choice of fiscal policy or another.

**3.3. Fiscal policies and Inequality: IV Results**

To bring this approach into our framework, we present in Table 3 our IV estimates which make use of the fiscal policies as narrative variables (as a binary instrument). This approach is parallel is based on Stock and Watson (2012), and parallel to Jorda().

Table 3: Effect of Fiscal policies IV estimate

|  |  |  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- | --- | --- |
|  | (1) | (2) | (3) | (4) | (5) | | (6) |
|  | Year 1 | Year 2 | Year 3 | Year 4 | Year 5 | | Sum |
| Education expenditure | -0.114\* | -0.098 | -0.087 | -0.095 | -0.106 | | -- |
|  | (0.068) | (0.067) | (0.067) | (0.067) | (0.069) | |  |
| First stage F-statistic | 92.13 | 93.051 | 95.089 | 96.658 | 91.658 | |  |
| Health expenditure | 0.539 | 0.503 | 0.468 | 0.419 | 0.458 | | -- |
|  | (0.355) | (0.331) | (0.309) | (0.304) | (0.361) |  | |
| First stage F-statistic | 19.438 | 21.670 | 23.792 | 24.137 | 19.477 |  | |
| In-cash transfers | -0.967\* | -0.987\* | -1.028\*\* | -1.073\*\* | -1.153\* | | -4.342 |
|  | (0.389) | (0.383) | (0.379) | (0.387) | (0.511) | |  |
| First stage F-statistic | 4.714 | 4.501 | 4.238 | 3.946 | 3.676 | |  |
| In-kind transfers | -0.369\*\* | -0.373\*\* | -0.369\*\* | -0.373\*\* | -0.379\*\* | | -1.553 |
|  | (0.129) | (0.137) | (0.129) | (0.130) | (0.133) | |  |
| First stage F-statistic | 4.398 | 4.227 | 4.319 | 3.305 | 2.185 | |  |
| Property taxes | -0.033\* | -0.034\* | -0.034\* | -0.035\* | -0.035\* | | -0.144 |
|  | (0.017) | (0.018) | (0.018) | (0.018) | (0.018) | |  |
| First stage F-statistic | 13.62 | 12.90 | 12.48 | 13.47 | 13.65 | |  |
| Indirect taxes | 0.108 | 0.105 | 0.108 | 0.093 | 0.069 | | -- |
|  | (0.099) | (0.103) | (0.108) | (0.108) | (0.111) | |  |
| First stage F-statistic | 38.98 | 35.19 | 31.43 | 31.47 | 31.73 | |  |
| Income tax progresivity | 0.071\* | 0.0714\* | 0.071\* | 0.070\* | 0.072\* | | 0.298 |
|  | (0.038) | (0.038) | (0.038) | (0.038) | (0.039) | |  |
| First stage F-statistic | 16.65 | 16.70 | 18.20 | 17.12 | 11.33 | |  |

Notes: Standard errors (clustered by country) in parentheses. ∗∗∗/∗∗/∗ indicate p < 0.01/0.05/0.10. Additional controls: 1 lag of change in Gini, country fixed effects. d.CAPB instrumented by IMF fiscal action variable in binary 0-1 form (Treatment) in the top panel. First stage F-statistic reports the Kleibergen-Paap weak identification Wald test statistic. We omit the sum (col. 6) of some variables, due they are not significant in some of the periods.

The above results. Fiscal consolidation is unambiguously contractionary. Using the sum of coefficients reported in column (6) of Table 3, for every 1% in fiscal consolidation, the path of real GDP is pushed down by over 0.57 percent each year on average over the five subsequent years. This result is not sensitive to whether we use the binary or continuous instrument.

The results in the literature suggest that there is an effect of redistributive policies on levels of inequality, but, at the same time, inequality also influences government policies. This mechanism of reverse causality makes the econometric identification of the effect of redistributive measures on income inequality particularly difficult. As in most observational empirical applications that strive at finding the effect of policy measures, one has to be aware that policy measures are responsive to economic political conditions and, therefore, usually endogenous. To overcome any resulting empirical problems, careful empirical work requires identifying the channels of how the policies of interest are shaped.

3.4 Endogenous Fiscal Policy: Is the Binary Instrument Valid?

Before drawing any conclusions, we evaluate whether the binary variable might be a legitimate instrument. Have we identified the causal effect of fiscal policies on output? We cannot formally test the validity of the binary instrument since the LPs are just identified. However, if the binary variable can be predicted by excluded controls, and those controls are correlated with the Gini net income, at a minimum, the excluded controls should be added to the regression. At worst, predictability points to having failed to resolve the allocation bias in our estimates. This possible shortcoming of the “narrative identification” strategy has been noted before in the context of monetary policy (Leeper 1997) and we have the same concern here, for fiscal policies. To address this issue, we report three diagnostic tests in this section in Table 4.

We go beyond this simple check and we check if the outcome is predictable by a set of available controls not yet included in the analysis. Table 4 reports the results of such tests by re-examining whether our candidate model in expression (1) admits as additional explanation the following variables: real GDP growth, government expenditure, secondary education and employment rate. The tests are conducted with the 1-period ahead local projection (the equivalent of the corresponding equation in a VAR) using the full sample according to expression (1).

|  |  |  |
| --- | --- | --- |
| **Table 4: Omitted variables explain output fluctuations** | | |
| **Model** | **OLS** | **IV** |
| GDP growth | 0.022\* | 0.0487 \* |
| Government exp. | 0.00024\*\* | 0.3223 |
| Secondary education | 0.0022\*\* | 0.539 |
| Employment rate | 0.0806\* | 0.0651 \* |

Notes: See text. Entries are the p-value of a test of the null hypothesis that the given

variable is irrelevant in determining Gini changes given the fiscal treatment. The test

is applied to two models. “OLS” refers to the LP responses calculated in Table 2;

“IV” refers to the LP responses calculated using the binary instrument in Table 3.

The objective is to set a higher bar for the possibly omitted regressors to be significant. Table 4 reports the p-value associated with the joint null that the candidate variable is not significant. A rejection means that changes in Gini could be due to reasons other than the fiscal treatment variable. The message is clear: most of the excluded controls are highly significant in the OLS model, but half of them is for the IV model. For now, a cautious interpretation is to view these findings as a source of concern rather than conclusive evidence that the multipliers reported earlier are incorrect.

Considering the endogenous growth models, investment in human and physical capital does affect the steady-state growth rate. Consequently, there is scope for tax and government expenditure to play a role in the economic growth process.

Next, we check for another condition: Do excluded controls predict fiscal policy treatments? Table 5 asks whether variation in the fiscal policy binary treatment variable identified can be predicted. The results indicate that we have a reasonable basis for this concern.

Table 5 shows in column (1) that treatment is more likely, as expected, when public debt to GDP is high: the coefficient is positive, meaning that governments tend to pursue austerity when debt has run up. In column (2) we add y C (the output gap) and the growth rate of y to further condition on the state of the economy: when the economy is growing below potential, there is an increase in the likelihood of consolidation.

Finally, columns (3) and (4) add the lag of the dependent variable Treatment and this has a highly significant coefficient: as we know from the raw data series generated by the IMF study, the fiscal consolidation episodes are typically long, drawn-out affairs, so once such a program is started it tends to run for several years. Being in treatment today is thus a good predictor of being in treatment tomorrow. In these last two columns the lagged growth rate rather than the cyclical level of output emerges as the slightly better predictor of treatment.

|  |  |  |  |  |
| --- | --- | --- | --- | --- |
| **Table 5. Fiscal treatment reg. Pooled probit estimators (average marginal effects)** | | | | |
| **Model** | **Cash transfers** | **Education expenditure** | **Property tax** | **Income tax progressivity** |
| GDP growth | -1.88\* | -1.750 | -0.355 |  |
|  | (0.882) | (1.742) | (1.555) |  |
| Government exp. | 1.42\* | -2.498\* | 0.234 | -0.956 |
|  | (0.663) | (1.400) | (1.074) | (0.807) |
| Secondary education | 0.011 | -0.297 | -0.786\* | -0.511 |
|  | (0.348) | (0.364) | (0.360) | (0.342) |
| Employment rate | |  |  | 1.347\* |
|  |  |  |  | (0.733) |
|  |  |  |  |  |
| Observations | 492 | 471 | 479 | 494 |
| Classification test: AUC | 0.63 | 0.53 | 0.57 | 0.50 |

Notes: Standard errors in parentheses. ∗∗∗/∗∗/∗ indicate p < 0.01/0.05/0.10. AUC is the area under CCF

curve. AUC ∈ [0, 1].

Further confirmation of the predictive ability of these treatment regressions is provided by the AUC statistic. The AUC is commonly used in machine learning to evaluate classification ability (see, e.g., Jorda and Taylor 2011)[[11]](#footnote-13). Table 5 measures the classification ability of each specification. The AUC statistics show that the probits have very good predictive ability, with AUC around 0.65 when lagged treatment is omitted (Column 2), and over 0.8 when lagged treatment is included (Columns 3 and 4). The AUCs are all significantly different from 0.5.

The key lesson from Table 5 is simply that the IMF variable has a significant forecastable component.[[12]](#footnote-14) The question, then, is how to deal with the problem of potentially endogenous instruments. The remainder of this paper provides one answer.

1. **The statistic methodology**

The previous section raises concerns that the narrative IMF variable could be an invalid instrument using three different checks. The empirical strategy that we propose is based on taking triple insurance against this potential endogeneity. First, we take the episodes of consolidation from the IMF narrative variable as the subset of all consolidation episodes that are a candidate for random allocation. Think of it as a pseudo-IV step. Second, we include the extended set of covariates from Tables 4 and 5, and add them as right-hand side variables in the LP of equation 1. Third, we use inverse propensity score weighting on this LP to re-randomize allocation of the IMF fiscal consolidation events.

In order to facilitate the exposition, we momentarily drop the cross-sectional country index in the panel. Denote, as before, yt the outcome variable of interest, the log of real GDP. In other applications yt could be a ky-dimensional vector. Let Dt denote the fiscal policy variable. Dt will now be a discrete random variable Dt ∈ {0, 1} based on the IMF narrative indicator of exogenous fiscal consolidations, although earlier it was the continuous d.CAPB variable. The methods that we present next can be extended to settings in which the policy variable takes on a small number of discrete values. Next we allow for a kw-dimensional vector of variables, wt that are not included in the vector yt, but which could be relevant predictors of the policy variable Dt . Finally, denote Xt the rich conditioning set given by ∆yt−1, ∆yt−2, ...; Dt−1, .Dt−2, ...; and wt.

Assumption 1: The causal effect of a policy intervention is defined as the unobservable random variable given by the difference (yt,h (1) − yt) − (yt,h (0) − yt). Notice that yt is only used to benchmark the cumulative change and it is observed at time t. We assume that the parameters of the policy function do not change.

Consider the ideal randomized experiment to understand the role that the conditional independence assumption plays. The average causal effect of policy intervention on the outcome at time *t + h* given by

(3)

(4) for all h > 0

where n1 = , n0 = and are the number of observations in treatment and control groups, respectively. Alternatively, the average treatment effect, Λh, could be calculated from the auxiliary regression.

(5) for all h > 0

Even when data are randomly allocated across the treatment and control subpopulations, it would be natural to condition on the Xt to adjust for small-sample differences in subpopulation characteristics and therefore to gain in efficiency. The estimator is consistent for the average treatment effect (ATE hereafter) whether or not regressors are included. Notice that the model for the outcomes is unspecified. The estimate of the ATE does not depend on specific assumptions about this model if the conditional ignobility assumption is met.

Assumption 2: Assume that a linear regression control strategy suffices to do the appropriate conditioning for the Xt and hence obtain a consistent estimate of E[yt+h − yt |Dt , Xt ]. This is a big assumption that we relax later on in the paper. Note this is the assumption of studies based on VARs where identification does not rely on external information. Then the average causal effect of a policy intervention on the outcome variable at time t + h in the maintained example, can be calculated by expanding expression (4) with

(6) for all h > 0

If one imposes the , then the above expression is nothing more than a standard LP of expression (1) and is the policy response at horizon h. The standard linear LP is a direct estimate of the typical impulse response derived from a traditional VAR, as Jorda (2005) shows.

This naïve constrained specification, which characterizes responses derived from a VAR, imposes two implicit assumptions. First, the effect of the controls Xt on the outcomes is assumed to be stable across the treated and control subpopulations. Second, the expected value of Xt in each subpopulation is assumed to be the same. The first assumption is potentially defensible. The economic mechanism describing the transmission of interest rates on real GDP could be the same whether or not there is a fiscal consolidation, for example. The second assumption is more difficult to defend. It is unlikely that, say, government debt levels are the same in the treated and control groups. Fiscal consolidations are often driven by high levels of debt.

However, it is important to recall several features required to resolve the identification puzzle. These are: (1) the instrument is relevant, which appears to be the case as we discussed earlier; (2) the instrument is valid, which is untestable given just-identification and for which the analysis of the previous section raises concerns; and (3) predetermined and exogenous controls are not omitted from the specification. This latter requirement is not resolved by the use of the instrument, especially when there is substantial evidence that the controls are predictive for the instrument, as shown here.

More generally, if we do not impose the implicit assumptions of the naive LP specification, the analogous representation to the group means expression (4) is

(7) for all h > 0

where is a generic specification of the conditional mean of (yt+h – yt) in each subpopulation j = 1, 0 and ′ for the regression example in (6). Note that this more general form of regression adjustment allows the conditional means to be different for the treated and control subpopulations and allows their effect on the outcome to differ as well.

5.1. Re-randomization through the propensity score

Rosenbaum and Rubin (1983) show that

that is, the propensity score is all that is needed to capture the effect of the Xt in the selection-on-observables condition.9 This result provides further support for the IPW estimator. Recall the average treatment effect (ATE) is, by definition

(8)

using the law of iterated expectations. Looking inside the expectations in the final term above, the average policy response conditional on Xt, in terms of observable data, is

, (9) for all *h > 0*

where it is assumed that the policy environment characterized by remains constant. Estimation of these conditional expectations can be simplified considerably when a model for the policy variable Dt is available.

Angrist and Kuersteiner (2004, 2011) refer to the predicted value from such a policy model the policy propensity score. The policy propensity score is meant to ensure the estimation of the policy response (the average treatment effect in the microeconomics parlance) is consistent under the main assumption. In addition, it acts as a dimension reduction device. Ideally, any predictor of policy should be included, regardless of whether that predictor is a fundamental variable in a macroeconomic model. The probit results reported in Table 5 can be seen as candidate estimates of this policy propensity score. We will instead construct the policy propensity score using a richer specification that includes all the controls used in Table 4 as well.

Solving for and taking unconditional expectations, by integrating over Xt, the ATE in (9) can be calculated a

(10) for all *h > 0*

Under standard regularity conditions an estimate of expression (10) can be obtained using sample moments which generalize the sample moments presented earlier in expression (4) for the OLS case. Suppose that the first-stage treatment model takes the form of a probability of treatment at time t given by the estimated model

where is the estimated parameter vector, and . The inverse propensity score weighted (IPW) “ratio estimator” of the average treatment effect is. Some improvements can be made to this expression. Imbens (2004) and Lunceford and Davidian (2004) suggest renormalizing the weights so that they sum up to one in small samples.

(11)

Where

Note that ; similarly

and hence it follows that in large samples expression (11) apply the same weighting, since . These expressions are natural analogs of the Group Mean estimator in (4), with inverse propensity-score weighting to correct for allocation bias and to achieve a quasi-random distribution of treatment and control observations via reweighting.

4.2. Regression adjustment (IPWRA) and Augmented IPW (AIPW)

As a way to enhance robustness, researchers have derived estimators with a regression adjustment component added to the standard IPW estimator presented above. To further enhance efficiency, the augmented IPW or AIPW estimator combines the IPW and IPWRA estimators in a manner to be discussed shortly.

The inverse propensity-score weighted estimator with regression adjustment (IPWRA).

(12)

where again for j = 1, 0 is the conditional mean from the first-step regression of (yt+h - yt) on Xt as in expression (8) in Section 4. The nj\* for *j = 1,0* are the same as in expression (11). It is clear that equation (12) nests all the previous estimators, the Group Mean (4), the RA (7), and the IPW (11) as special cases.

The estimator in expression (12) falls into the class of doubly robust estimators (see, e.g., Imbens 2004; Wooldridge 2007; Lunceford and Davidian 2004; and Kreif et al. 2011). The intuition behind the estimator is to use the regression model as a way to “predict” the unobserved potential outcomes. However, although (17) is one of a large class of unbiased IPWRA estimators of ATE, it is not the most efficient in this class. More recently, Lunceford and Davidian (2004), the estimator within the doublyrobust class having the smallest asymptotic variance, is the (locally) semi-parametric efficient estimator.

(13)

Thus, the estimator in (18) can be seen as the basic IPW estimator plus an adjustment consisting of the weighted average of the two regression estimators. The adjustment term has expectation zero when the estimated propensity scores and regression models are replaced by their population counterparts. Moreover, the adjustment term stabilizes the estimator when the propensity scores get close to zero or one (Glynn and Quinn 2010), and this alleviates with the need to truncate the propensity score weights as suggested in Imbens (2004). Another way to interpret the AIPW estimator is to realize that .

4.3 Application and intuition

Although these techniques are relatively new to macroeconomics, matching estimators using inverse propensity score weighting have been frequently implemented in applied. microeconomics with cross-sectional data. Matching methods more generally constitute a benchmark within the medical research literature when trials are suspected of being contaminated by allocation bias. The provenance of the particular inverse propensity score weighting method we employ is thus well established.

The next section reports the results of applying the AIPW estimator (13) to measure the average treatment effect of fiscal consolidations as a counterpoint to the conventional OLS and IV results reported earlier. Next we allow for the parameters to vary across subpopulations, which is typically applied in the policy evaluation literature. These results deliver the same qualitative implication of contractionary austerity, but show that the effects of consolidations are quantitatively even more painful.

This section presents the empirical findings of the Local Projection approach discussed in the previous section. First, we present the overall analysis for the income inequality baseline model based on the whole sample (Table 2). Second, we carry out two additional exercises with to more precise country sub-samples (see Table 2 in the Appendix for criteria). We present the further results obtained by splitting the sample into countries with high public debt and economies where the crisis hit hardest.

We do this for two purposes. First, to capture the idea that higher economic growth may decrease income inequality via higher government expenditure. Second, adding a specific measure of economic policy allows us to investigate whether the effects of inequality shocks are indeed a result of a third factor, in our case, shocks to fiscal policies and taxation.

## **4.1. The effects on income inequality**

The baseline specifications embed a direct impact of the control variables (economic growth, general government expenditure, and trade openness). Once we corrected for the internal instruments in the model, we find evidence of negative effect of growth on income inequality. This is mostly in line with previous studies and economic intuition. Whereas GDP per capita growth has a significant negative effect on inequality, and it is more pronounced for its first lag.

Regarding the estimated effect of various government expenditure categories have the expected negative effect on Gini (see Table 2 below). Results show consistency again, although are not significant in some specifications. The data suggest that redistributive measures of government expenditure are indeed able to achieve less inequality. Roughly speaking, a 1% increase in social cash payments decreases inequality (as measured by the net Gini coefficient) from 1.0001%, although their impact differs through lags, which reflects a short-run impact. Regarding in-kind transfers, only health expenditure has the expected negative effect and is highly significant across our sample, while the impact of education expenditure is null.

On the other hand, the effect of personal income taxes, the progressivity is significant and consistent through the whole sample. The personal income tax (PIT, thereafter) has the expected negative effect on income inequality. And this effect increases with more progressive tax structure; for example, with zero progressivity of PIT, one percentage point increase in the share of PIT in a 1.001%-point decreases income inequality. In addition, one percentage point increase in PIT with the progressivity index reduces income inequality by almost 1.0045% points.

Finally, we control for an institutional labour market variable –employment protection- which measure the procedures and costs involved in dismissing workers and the procedures involved in hiring them on fixed-term or temporary contracts. For our sample, we found the expected sign, i.e., higher protection helps decreasing income inequality by 1.001%,.

**Table 2. PVAR model results for Gini net income**

These results suggest that increasing the progressivity of income taxes have a positive effect in reducing inequality. Zooming-in social transfers in cash, we find a higher positive redistributive effect of pensions on income inequality for the whole sample.[[13]](#footnote-17) In the same way, expenditure in health and education are also efficient as those provided in cash; peculiarly, education expenditure is only significant for the second lag, Table 4 confirms the result. It is possible a better reallocation of public expenditure towards human capital (education and health) would reduce income inequality. Our results agree with those of Bénabou (2002) pointing out that health and education may be both pro-growth and pro-equality, health expenditure seems to perform better in our sample. The results discussed above show that social transfers in cash play a role in alleviating income inequality in the short-run.

## **4.2. Effects on income inequality in countries with high public debt and economic recession**

Continuing, we divide the sample into two alternative sub-samples: countries with high debt and countries under recession. A first noteworthy result is that estimations on the whole sample and the two alternative sub-samples are fairly similar (see Tables 3 and 4 below). Indeed, analysing the estimates of the fiscal policy variables present significant changes from the estimated effects in Section 4.1. However, most estimated coefficients are still significant with the expected sign of the variables confirm the previous results.

**Table 6. Fiscal multiplier and tax effects, IV estimates**



**Notes**: Standard errors (clustered by country) in parentheses. ∗∗/∗ indicate p < 0.05/0.10. Additional controls: cyclical component of y, 2 lags of change in y, country fixed effects d.CAPB instrumented by IMF fiscal action variable in binary 0-1 form (Treatment) in the top panel. First stage F-statistic reports the Kleibergen-Paap weak identification Wald test statistic.

We illustrate in Table 3 what happens when we take into account highly indebted countries (average government debt above 60% of GDP over 2008-2015); we do not obtain results in line with the overall sample estimation. Regarding the fiscal variables, our results suggest that distributive measures of government expenditure in-cash social transfers are not significant for its three specifications. On the other hand, roughly speaking, a 1% increase in health expenditure decreases inequality by 1.0005% particularly for economies highly indebted. On the taxation side, the results are equal to the general sample, the interaction variable of progressivity and PIT is significant – evidence shows that higher taxations under high debt scenario decreases inequality around 1.004%.

Alternatively, we illustrate in Table 4 the effects when we consider economies in recession (average real GDP growth during 2008-2013 is negative). Analysing the estimates, the results differ from the latter sub-sample; here fiscal policy has played a significant role in reducing income inequality, materialised via in-cash transfers. The redistributive impact of social payments accounts for about 1.0002% of the decrease in the net Gini. Within social in-cash payments, pensions are significant and confirm its positive impact on redistribution. On the taxation side, we found that income tax progressivity represents an important contribution in reducing inequality, and even it’s a little higher than the other sum-sample estimates.

Taking the results of both sub-samples, we find that fiscal policy and strong employment protection promote equality of opportunity and greater intergenerational mobility, especially in countries that are not high indebted and the initial condition of income inequality (0.28 of Gini net income) is low compared to indebted countries (0.31). We can confirm the importance of government expenditure, focused on increasing access to health could enhance social mobility and help breaking the inter-generational transmission of poverty and inequality. In that way, our results show that in-cash payments (pensions) for disadvantaged groups would also enhance the dynamics and evolution of social payments in particular for the countries with recession. Finally, the results indicate that progressive taxes are still effective in both sub-samples countries; and it is important to mention the determinant effect on inequality of employment protection and salaries.

**Table 4. PVAR model results for severely crisis affected countries**

Note. - \* and \*\* indicate statistical significance at 1% and 5% levels respectively.

**4.3. Heterogeneous dynamics**

Figures 5 and 6 below report the corresponding median response of the Gini net income levels to shocks on different variables by samples. To ease their visualization, the median response is presented in terms of percent deviation from its steady state. Particularly, we would not be able to know how many countries were actually behaving as the average dynamics, we only discuss the results that were statistically significant in the previous point.

We now present the IRF impulses of our main variables (fiscal and labour variables) for the full sample and the two sub-samples. Figure 5 reports the median responses of Gini net income among the 23 countries in the sample over a 5-year horizon. As noted by Góes (2016), this is an informative way of presenting the additional results, which would not be available if we had imposed homogeneity in the previous estimation.

**Figure 5. Composite response of Gini net income by sub-sample**

Note: The y axis scale is different for each variable, so we could see more clearly the impact of the variable and the time it takes to phase out.

We do not find large differences in the responses of Gini among both sub-samples since we deal with a sample of advanced countries. While the median impact of health expenditure on Gini net income is negative and lasts for about four years, this is the representation for all samples. Indeed, the results for the highly indebted countries show that the estimated impact is actually lower but last the same time. Conversely, the results for the transfers in-cash, the negative impact is about six years and takes longer to die out. For the median response, one percentage point (pp) shock in in-cash transfer is associated with a 0.028 pp fall in Gini net income during the second and the third year after the shock, once again, the variation across sub-samples is not considerable.

The results in Figure 6 are also informative regarding the impact of income tax progressivity on income inequality. The median response is negative and reverts to about zero after three years. However, there is a reverse effect in the 4th year: we assume that higher income taxes eventually hurt redistribution in the long-term.

**Figure 6. Composite response of Gini net income by sub-sample**

Note: The y axis scale is different for each variable, so we could see more clearly the impact of the variable and the time it takes to phase out.

Our results so far suggest that the sign and the strength of the relationship between the variables vary widely across its significance and impact. Comparing the median response to a shock in pensions with respect to other variables, we see that the median response remains negative and takes longer to die out in almost 6 years. In terms of magnitude, the negative response to a one pp shock in pensions is largest in crisis sub-sample (about -0.03 pp during the second year after the shock).

1. **Conclusions**

Inequality is a complex phenomenon with several facets. Our central concern is on understanding the effectiveness of the fiscal policies in mitigating rising market inequalities in the developed world. Understanding the determinants of inequality is complex, but certainly its trends are partly related to previous explicit public policy choices in a given country.

We found an underlying general message when analysing cross-country variation in developed countries: there is a preference for coupling redistribution with pre-distribution fiscal policies. Government action affects household income directly (redistribution of public resources through transfers in cash and direct taxation) and indirectly (by modifying incentives and pre-distribution through transfers in kind). Our estimates point towards significant effects of government expenditure and - to lesser degree - the progressivity of direct income taxation on mitigating inequality. Our results show that social payments in cash, in particular pensions, seem to be an efficient tool in promoting redistribution. Also, direct taxes on income are needed, unless the countries are constrained by high debt-to-GDP ratios.

Additionally, we find that employment protection is correlated with the strength of the relationship between inequality and growth. In particular, a stronger control of employment protection can reduce the negative effect of income inequality. Wealth inequality is also widening in advanced countries, as briefly explained in Box 1. This is also a complex phenomenon that seems to be partly related to house ownership levels and price developments in real state in the euro area countries. Similarly, the relationship between real and financial wealth needs further analysis.

Further research could investigate within country variation in income and wealth inequality. Analysing the different income deciles could help to identify the strengths and weaknesses of the different transfer and tax systems within a country. In a more critical way, it would necessary to assess if the current systems are outdated in view of the upcoming challenges such as ageing populations and digitalisation. It would be useful to understand how the changes in the household composition may impact inequality trends or how fiscal policies can change incentives in wealth accumulation. Current debates include discussions in the effectiveness of taxing wealth and inheritance property taxes, in incentivising voluntary contributions to pensions in the third pillar, or in providing a minimum income to households as last resort.

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# Appendix

**Table 1. Variables in the model**

|  |  |  |  |
| --- | --- | --- | --- |
| **Variable** |  | **Comments** | **Source** |
| **Government expenditure in education** | % GDP market prices |  | Eurostat (COFOG database, category 9) |
| **Government expenditure in health** | % GDP market prices |  | Eurostat (COFOG database, category 7) |
| **Social protection (including old-age and survivor pensions)** | % GDP market prices | | Eurostat (COFOG database, category 10) |
| **Social transfers in cash** | % GDP market prices | | Eurostat (national accounts) |
| **Social transfers in kind** | % GDP market prices | | Eurostat (national accounts) |
| **Income personal tax** | % GDP market prices |  | OECD |
| **Statutory tax income** | % GDP market prices |  | OECD |
| **Progressivity personal tax index** | The measure of progressivity is the average rate progression (ARP) variable which is calculated as follows (Sabirianova-Peter et al., 2010): average tax rates for each country and year in the data set are first computed for 100 evenly spread pre-tax incomes. ARP is then constructed by regressing tax rates on the log of gross income. | | Saravinopa-Peter (2016) |
| **Trade openness** | (Exports + Imports)/GDP | | Eurostat (national accounts) and FRED |
| **GDP per capita growth rate** | GDP per capita constant prices | | Eurostat (national accounts) and FRED |
| **Minimum wages** | In real terms (adjusting for PPP referred to 2017) | | Eurostat and OECD. |
| **Employment protection** | Measures the procedures and costs involved in dismissing workers and the procedures involved in hiring them on fixed-term or temporary contracts. Higher the index reflects higher protection. | | OECD |

**Table 2. List of countries in the different samples**

|  |  |
| --- | --- |
| **Sample 1. High indebted countries** | **Sample 2. Countries**  **under recession** |
| Austria | Cyprus |
| Belgium | Denmark |
| Cyprus | Estonia |
| Finland | Finland |
| France | Greece |
| Greece | Ireland |
| Italy | Italy |
| Portugal | Latvia |
| Slovenia | Netherlands |
| Spain | Portugal |
| UK | Slovenia |
| US | Spain |

Note: The criteria for selection of high indebted countries are when the average of stock of debt-to-GDP over 2008- 2015 is superior to 60%. The criteria for the sample of countries where recession hit hardest is the average real GDP growth over 2008-2013 is negative.

**Table 3. Descriptive statistic by group of countries**

**Cointegration, Convergence and Integration**

As a first step, we analyse the stationarity properties of the Gini net and the log of per capita real GDP by employing the tests by Levin et al. (2002) (LLC thereafter), Im et al. (2003) (IPS), and Breitung (2000) (BR). These tests differ in that LLC and BR treat the parameter of interest as common across countries and focuses on the within-country dimension, IPS treats the parameter of interest as varying across countries and focuses on the between-country dimension.

We perform the tests on the (log) levels and the (log) first differences, including the intercept only. As expected, the results of the different tests reported in Table 4 in the Appendix suggest that levels are non-stationary as the null hypothesis of unit root cannot be rejected, only the tax income and unemployment transfers reject the null at the 10%. After first differencing, the series result stationary.

The presence of unit roots in levels warrants the investigation of the existence of a long-run relationship between (the log of) Gini net, per capita real GDP, and the rest of the variables. We employ the tests by Kao (1999) and Pedroni (2004). The former treats all parameters except fixed effects as homogeneous and therefore is misspecified if heterogeneous dynamics exist. The results of the cointegration tests in Table 5 in the Appendix suggest the existence of a long-run relationship. In particular, in the majority of cases and irrespective of the inclusion of a trend, both homogeneous and heterogeneous tests reject the null hypothesis of no co-integration, providing evidence of co-movements between the series in the long run.

Therefore, the null hypothesis of non-convergence can be resumed in the unit root null of panel unit root tests. We employ the LLC, and the IPS panel unit root tests, leveraging on the fact that the autoregressive parameter in the income inequality differentials is allowed to vary across countries (see Table 4 Appendix), the Openness variable presents the unit root for both tests.

Table 6 in the Appendix presents the results of the tests of conditional convergence for (the log of) net Gini and Gini market (in this case, we consider the Gini market to compare results). Beyond the full sample, we also consider the different two sub-samples to investigate the existence of convergence clubs. The first column reports the LLC test along with its significance level. For the full sample, we cannot reject the null hypothesis of unit root presence, suggesting no convergence. When looking at the t-statistic for every country, the majority of them cannot reject the null hypothesis at the 5% level. However, the BR test rejects the null, implying convergence for the full sample, as the sub-sample of highly indebted countries.

The fact that countries are not generally converging to the average for the sample and sub-sample of countries hit most by the crisis does not exclude the possibility that countries are also converging to an average of developed countries. The results of the income inequality unit root tests suggest that convergence is not occurring in Europe and in the United States. Such findings are once again corroborated by the results of the IPS test suggesting that there is no convergence when redistribution effects are netted out.

**Table 4. Panel Unit Root Tests**

|  |  |  |  |  |  |
| --- | --- | --- | --- | --- | --- |
|  | Levels | |  | First differences | |
|  | Levin, Lin, & Chu | Im–Pesaran–Shin |  | Levin, Lin, & Chu | Im–Pesaran–Shin |
| Gini net income | 3.30 | -6.86 |  | -4.82\*\*\* | 0.36 |
| GDP per capita | 17.13 | -3.79 |  | -7.60\*\*\* | -3.30\*\*\* |
| Cash trans. | 1.64 | 5.64 |  | -9.81\*\*\* | -4.05\*\*\* |
| In-kind transfers | 4.83 | 10.75 |  | -22.76\*\*\* | -5.06\*\*\* |
| Income tax | 1.71\*\*\* | -2.47 |  | -10.63\*\*\* | -8.21\*\*\* |
| Employment | -0.03 | -2.37 |  | -10.52\*\*\* | -4.35\*\*\* |
| Unempl. Transf. | -1.39\* | -3.48 |  | -9.77\*\*\* | -4.91\*\*\* |
| Pensions | 0.29 | -7.24 |  | -11.5\*\*\* | -6.73\*\*\* |
| Openness | 9.98\* | 0.27\* |  | -11.28\*\*\* | -3.92\*\*\* |
| Gover. exp. | -0.24\* | -6.64 |  | -11.19\*\*\* | -8.63\*\*\* |
| Education exp. | -0.83\* | -4.51 |  | -10.52\*\*\* | -3.11\*\*\* |
| Health exp. | -5.46\* | -1.33 |  | -11.08\*\*\* | -4.66\*\*\* |

Notes: The null hypothesis of the panel unit root tests is that all panels contain a unit root. Fixed effects are always included. The Schwartz Information Criterion is used to select the optimal lag length. \*\*\* and \* next to a number indicate statistical significance at 1 and 10 percent, respectively.

**Table 5. Panel Cointegration Tests**

|  |  |
| --- | --- |
| **Kao** | -3.66\* |
| **Pedroni** |  |
| Panel v-Statistic | 5.86\* |
| Panel rho-Statistic | -9.15\* |
| Panel PP-Statistic | 5.64\* |

Notes: The null hypothesis of the panel cointegration tests is that there is no cointegration between ln Gini net and ln real per capita GDP. For Pedroni (2004), we present the versions of the statistics that are not weighted by the member specific long-run conditional variances. The Schwartz Information Criterion is used to select the optimal lag length. \* and \*\* next to a number indicate statistical significance at 1 and 5 percent, respectively.

**Table 6. Convergence in income inequality**

|  |  |  |  |
| --- | --- | --- | --- |
|  | Levin, Lin & Chu | Breitung | Im, Pesaran & Shin |
| Full sample | -4.55\*\*\* | 1.12 | -6.47\*\*\* |
| Sub-samples |  |  |  |
| Highly indebt countries | -1.70 | 0.74 | -2.98\*\* |
| Crisis countries | -3.91\*\*\* | 0.58 | -6.15\*\* |

Notes: The null hypothesis of the unit root tests is that all panels contain a unit root, i.e. there is no convergence. Fixed effects are always included. The Schwartz Information Criterion is used to select the optimal lag length. \*\*\* and \*\* next to a number indicate statistical significance at 1 and 5 percent, respectively.

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**BOX 1. WEALTH INEQUALITY IN THE EURO AREA**

There is a renewed interest in wealth accumulation and wealth inequality. Wealth accumulation refers to total (gross) assets held by households, consisting of real assets and financial assets.[[14]](#footnote-18) Wealth inequality can be measured by the concept of **net wealth** – this is the difference between all household wealth (total assets) and debt. Net wealth inequality is likely to play a bigger role in the overall structure of inequality in the twenty-first century, see Piketty and Zucman (2014).

This box aim at showing estimates on the wealth accumulation in euro area and to analyze wealth inequality, by using the survey data of the second wave of the ECB’s Household Finance and Consumption Survey (HFCS). We highlight that cross-country differences in wealth inequality are largely accounted for by the bottom half of the wealth distribution and that these differences seem to be channeled through homeownership.

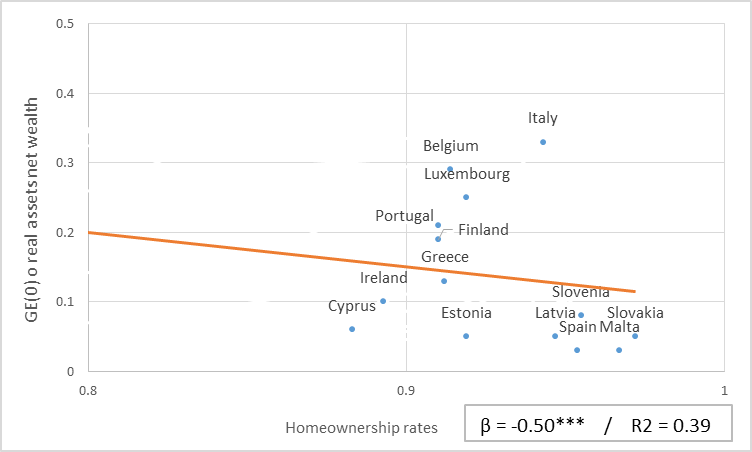
Zooming-in the components of wealth composition, Chart A shows that real estate wealth makes up the largest share of total gross assets owned by households (82.2% in the euro area on average). The remaining assets are financial (17.8% in the euro area). The household first residence is the largest component of real assets (half of total assets), followed by other real assets. Deposits represent the largest portion of financial assets, followed by voluntary pension/whole life insurance. Household debt is predominantly represented by mortgages, which account for 85.8% of the euro value of total household debt.

**Chart A. Wealth composition: real and financial assets, 2014 (% total)**

Source: Household Finance and Consumption Survey (HFCS).

There is a strong disparity in the wealth accumulation across several euro area countries. Chart B depicts the relationship between real state net wealth inequality (measured by the Generalized Entropy index with a parameter equal to zero) and homeownership rates for 2014. We can see a negative relationship: countries that have higher homeownership rates tend to have lower real state net wealth inequality. Germany, Austria and the Netherlands are not shown since they have lower homeownership rates (0.42, 0.49, and 0.55 respectively). On the other hand, Italy and Belgium have high ownership rates, but inequality is still relatively high. On the opposite spectrum, the lowest figures in real state net wealth inequality and high homeownership are found in Slovenia, Malta, Latvia, Slovakia, and Spain.

**Chart B: Homeownership rates and real state net wealth inequality across countries**



Source: Authors’ based on HFCS.

Note: Real assets net wealth includes the value of total real state.

Ownership wealth inequality is higher for the bottom half. The average of the GE(0) inequality coefficient across the sample countries for the below-median group is 0.32, whereas it is 0.05 for the 10% income distribution (almost inexistent). There is a large cross-country variation of homeownership rates and inequality in the bottom half. Homeownership rates for households below the median vary strongly across countries, with a coefficient of variation of 0.92. In contrast, the homeownership rates for the 10% richest households are very similar, with a coefficient of variation of 0.17. Finally, in countries with high ownership rates, a much larger fraction of the households in the bottom half own houses (65%). This might due to different savings incentives across these countries, which are channeled through homeownership.[[15]](#footnote-19)

Furthermore, Table 1 shows for selected countries the Gini coefficient of total net wealth and the Gini within for the two wealth main components (real estate and financial assets). We see that the magnitudes of Gini in net real assets are lower than the figures of financial assets; for countries with lower ownership rates, such as Austria and the Netherlands. The estimates suggest that real estate wealth is more evenly distributed than financial wealth. The evidence is almost the same for countries with high ownership rates. On average, own housing contributes around 82% of all net wealth, with the lowest contribution of 41%, being considerably below the euro area average (84%). In that way, it is evident that net real wealth has a low Gini coefficient among the euro area countries. Contrary, the difficult accumulation of net financial assets for the bottom half –only 6 % of households have net financial assets- reflecting the high inequality of this component in net wealth.

**Table 1. Gini for the main wealth components, 2014**

|  |  |  |  |
| --- | --- | --- | --- |
| **Country** | **Gini coefficient** | **Net real assets** | **Net financial assets** |
| Austria | 0.73 | 0.29 | 0.43 |
| Belgium | 0.59 | 0.27 | 0.59 |
| France | 0.68 | 0.37 | 0.56 |
| Germany | 0.76 | 0.48 | 0.53 |
| Greece | 0.60 | 0.41 | 0.46 |
| Ireland | 0.75 | 0.37 | 0.61 |
| Italy | 0.60 | 0.48 | 0.51 |
| Netherlands | 0.70 | 0.15 | 0.48 |
| Portugal | 0.68 | 0.41 | 0.52 |
| Spain | 0.60 | 0.40 | 0.59 |
| **EA average** | **0.66** | **0.39** | **0.51** |

Source: Authors’ calculations based on HFCS.

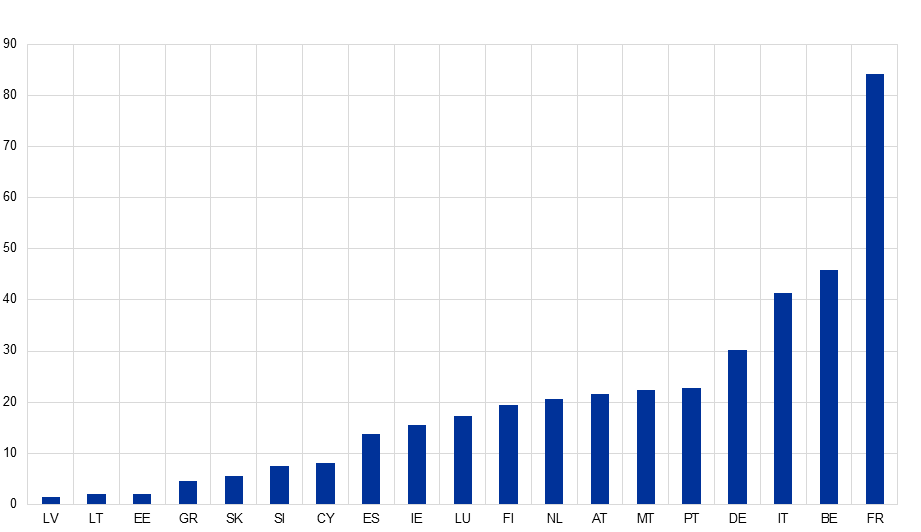
An important influence on real state wealth accumulation is the changes in the value of real estate. This may have an important impact on lower income groups, if housing prices fall. Chart C shows the evolution of housing prices across euro area countries over the period 2007-2014. We could expect that the top 10% income benefited more from lower housing prices. However, for some countries the important fall of real estate prices (i.e. Greece, Portugal, Slovenia, and Spain) contributed to increase real estate net wealth in the 20% bottom group.

**Chart C. Housing prices changes and real assets ownership by income groups (2007-2014)**

Sources: Housing prices (OECD House price indicators), and real state net wealth accumulation for different income groups (HFCS).

Finally, another angle for further research is to analyse the role of pensions and life insurance in the accumulation of financial wealth (see Chart D). Household savings in the third pillar can be an important complement of the old age pension income. This is the particular case for France, where these entitlements are much higher than for the rest of euro area countries (87% of GDP), as no funded schemes are organised by employers. The accumulated third pillar savings are also relatively high in Germany, Italy and Belgium (above 30% of GDP).

**Chart D. Stock of life insurance and annuity entitlements of households, % of GDP (2017)**



Source: ECB.

1. Fiscal consolidation usually leads to a short-run reduction in output and employment, which often goes along with an increase in income inequality and relative poverty rates (e.g., Rawdanowicz et al., 2013). Wo et al. (2013) show that on average, a consolidation of 1 percentage point of GDP goes along with an increase in the income Gini coefficient of around 0.4-0.7 pp over the first two years. Avram et al. (2012) and Callan et al. (2011) provide evidence in the EU countries that depending on the fiscal consolidation instrument the impact on inequality can vary substantially. An analysis of a set of fiscal consolidation packages between 1978 and 2009 in advanced economies (Agnello and Sousa, 2014) provides some insights: tax hikes have an equalising effect whereas spending cuts are detrimental to income inequality. The authors conclude that the size of fiscal consolidation and its composition matter for income distribution. [↑](#footnote-ref-1)
2. The view that inequality is negative for growth was advanced by Kuznets’ famous seminal work (1955), and later by Stiglitz (1969), by saying that under decreasing returns to capital and imperfect capital markets, redistribution poor can boost aggregate productivity and growth. In that sense, redistribution creates investment opportunities (Aghion et al. 1999). Zweimüller and Brunner (2005) deal with the channel of domestic market size: a more equal income distribution goes along with a broader variety of goods that consumers demand and enhances the development of local industries. Moreover, inequality may be harmful for growth because it deprives the poor of the ability to stay healthy and accumulate human capital (Galor and Moav, 2004), while impedes the social consensus required to adjust to shocks and sustain growth (Rodrik, 2000). Notwithstanding, the empirical literature on the relationship between inequality and growth is inconclusive as described by Muinelo-Gallo and Roca-Sagalés (2011). Several authors bring pro-growth arguments from rising inequality: differential saving propensities (Galor and Moav, 2004), wealth concentration needed to assume the big sunk costs required in new industries (Aghion and Howitt 1998), or entrepreneurship (García-Peñalosa and Wen, 2008). [↑](#footnote-ref-2)
3. Kumhof and Rancière (2010) explained that major economic crises were preceded by a sharp increase in both income and wealth inequality and a rise in the debt-to-income ratio among lower and middle-income households. Rajan (2010) suggests that rising inequality may be detrimental to financial stability by creating distortions on markets. Dabla-Norris et al. (2015) discusses that greater financial inclusion brings financial services to the poorer, often credit-constrained, portion of the population. If income gets more concentrated in a context of widespread access to financial services, credit to those that became poorer may be curtailed due to their lower ability to repay. This, in turn, may lead to a reduction in consumption, thereby exacerbating the impact of income inequality on economic development. [↑](#footnote-ref-3)
4. Schwabish et al. (2006), among others, confirm empirically that income distribution indeed impacts social expenditure suggesting that there is an effect of redistributive policies on levels of inequality, but, at the same time, inequality also influences governments’ policies. This rationale is based on the median voter assumption: as the median voter favours more redistribution, the further away he/she is from the mean income (Meltzer and Richard, 1981). High levels of inequality can provoke social and political instability and pressures for redistribution and expropriation that that reduces investment and growth (Alesina and Perotti, 1996). [↑](#footnote-ref-4)
5. Immervoll and Richardson (2011) used a variety of economic factors, such as increased globalization and demographic trends to explain the forces driving larger inequalities in market disposable incomes. The quality of institutions is another factor that seems to positively affect the income distribution (Chong and Gradstein, 2007). [↑](#footnote-ref-5)
6. The countries analysed are: Austria, Belgium, Cyprus, Denmark, Estonia, Finland, France, Germany, Greece, Ireland, Italy, Latvia, Lithuania, Luxembourg, Malta, Netherlands, Portugal, Slovakia, Slovenia, Spain, Sweden, the UK and the US. [↑](#footnote-ref-6)
7. Non-distributive government spending corresponds to the functions on a more collective basis of general public services, defence, public order and safety and economic affairs. Housing can be considered is also a distributive function, but its importance in the budget is marginal, and could be better analysed for studies treating poverty and income for low population strata. Hence, we do not include this instrument in our study. [↑](#footnote-ref-7)
8. There are three ways to link the benefit to the income: earnings-related (higher income deciles receive higher benefits); flat rate (recipients in lower income deciles receive larger amounts); and means-tested (recipients in lower income deciles receive larger amounts). [↑](#footnote-ref-8)
9. Sabirianova-Peter et al. (2010) uses a sophisticated measure of progressivity to examine whether inequality in the distribution of income is affected by their measure of structural progressivity of national income tax systems. Their main finding is that while progressivity reduces observed inequality in reported gross and net Gini coefficients, it has a significantly smaller impact on “true inequality”, which they argue is approximated by consumption-based measures of the Gini coefficient. The variable is calculated as follows: average tax rates for each country and year in the data set are first computed for 100 evenly spread pre-tax incomes. ARP is then constructed by regressing tax rates on the log of gross income. The tax system is progressive, proportional and regressive if the resulting slope coefficient of the income variable is positive, zero or negative, respectively. In all specifications employing ARP as the explanatory variable, we also control for the general level of taxation by including a variable for the top marginal income tax rate. This ensures that we identify the effect that directly stems from progressivity, rather than high tax rates. [↑](#footnote-ref-9)
10. By treating all the countries in the panel separately, it allows for heterogeneous slopes or responses, providing a much richer set of information about country effects and differences across them. Finally, this approach helps dealing with omitted variable bias and accounts for cross-section dependence. [↑](#footnote-ref-11)
11. The AUC under the null that the covariates have medium classification ability, AUC = 0.5. Perfect classification ability corresponds to AUC = 1. The AUC has an approximate Gaussian distribution in large samples. [↑](#footnote-ref-13)
12. Hernandez De ´ Cos and Moral-Benito (2013) have arrived at a similar conclusion. Their proposed solution to the lack of exogeneity problem is to use an instrumental variable approach. Instruments rely on data for pre-determined controls and on past consolidations. Since data on pre-determined controls already appear in the specification of previous studies (AA, GLP, etc.), the key question is whether past consolidation data predict current consolidation episodes. Fixed-effect panel estimation already takes into account heterogeneity in the unconditional probability of consolidation across countries. [↑](#footnote-ref-14)
13. There is a growing empirical literature investigating the effects of various social expenditure items on measures of inequality (i.e., labour earnings or disposable income inequality). The findings of this literature are mixed. Some empirical studies have found support for the inequality reducing effect of unemployment benefits (Causa et al., 2015 for unemployment benefits to the long- term unemployed), while others have not. Our estimates on unemployment benefits resulted with the contrary sign, it means a positive effect, and increasing inequality; this result and other PVAR outcomes on growth are available under petition to the corresponding author. [↑](#footnote-ref-17)
14. This is calculated by the market value of non-financial assets such as the household’s main residence, other property, self-employment businesses, and durables, together with the value of financial assets such as bank deposits, bonds, and shares, net of liabilities such as home mortgages and other loans. [↑](#footnote-ref-18)
15. See Köhler-Ulbrich et al. (2009) for a descriptive overview for a comparative study regarding mortgage interest deductions. One possible explanation is that the social safety net (in particular redistributive policies and public pensions) differs across countries, leading to different savings levels. These savings are then invested in housing, perhaps due to the lack of other suitable savings vehicles. In particular, many countries provide explicit or implicit subsidies to owning the house that is used as a main residence. These subsidies then not only affect ownership rates but at the same time might lead to implicit redistribution of wealth. [↑](#footnote-ref-19)