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Effects of fiscal consolidation on income inequality: narrative evidence from South America

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Effects of fiscal consolidation on income inequality: narrative evidence from South America

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Abstract:

Based on a narrative dataset constructed by David and Leigh (2018) for annual fiscal consolidation shocks, this paper estimates the dynamic effects of fiscal consolidations on income inequality from Jordá (2005)'s local projections method for nine South American economies in the 1991-2017 period. By decomposing fiscal shocks, the baseline results suggest that spending-based fiscal consolidations significantly increase the Gini index, while tax-based fiscal consolidations do not show statistically significant effects on income inequality. The Gini index for disposable income rises 2.48% in eight years after a spending-based fiscal adjustment of 1% of GDP. The magnitude of this effect is higher than in most of the previous studies carried out for OECD countries. Our main finding for the impact of spending-based fiscal consolidation on inequality in the medium run is robust when using alternative control variables, lag structures, country samples, and the Cyclically Adjusted Primary Balance (CAPB) strategy for identifying the fiscal shocks.

Keywords: income inequality; fiscal consolidation; fiscal austerity; South America; local projections

JEL Codes: D30; D63; E60; E62

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1 – Introduction

As the world emerges from the Covid-19 economic crisis with higher public debt levels, countries in the Global South seem more likely to engage in a new round of fiscal consolidation packages amid strong market pressure. Known as the most unequal region in the world (IMF 2014), Latin America has suffered a relatively strong impact of the pandemic on health, social and economic fronts (OECD 2020) after a decade of low growth and rising inequality. The potential implementation of new austerity measures aimed at achieving debt sustainability in these countries, in the short to medium run, are thus raising concerns over the impact of these policies on poverty and inequality levels in a context of high social vulnerability.

Fiscal austerity can potentially contribute to a rise in income inequality through different mechanisms. From indirect channels, wage disparities may increase as wages at the bottom respond more intensely to economic recessions caused by fiscal consolidation.

Another indirect effect involves a change in income composition: households at the top of the distribution earn a relevant share of their income from the capital, while poor households receive wages or informal job earnings. As economic recessions tend to weaken the bargaining power of workers and, accordingly, reduce the share of wages in the functional distribution of income, fiscal adjustments tend to disfavor families at the bottom of the distribution.

Regarding the direct impacts of fiscal policy on income inequality, results may depend on the type of adjustment measures. While cuts in social transfers, for instance, tend to disfavor individuals at the bottom of the distribution, increases in tax rates on capital income, wealth, or inheritance may reduce income inequality by decreasing the share of national income that goes to the top of the distribution.

Since the end of the commodity price boom of the 2000s and the political shift away from the so-called Pink Tide governments in South American countries (Loureiro 2018), the implementation of austerity measures (see Table 1) has been accompanied by a reversal in previously declining levels of income inequality in the region. In 2015 for

Brazil, the Gini index for income reached its lowest level in the 21st century, 0.519, and rose to 0.538 in 2018, according to World Bank estimates. Other South American economies have experienced a similar trajectory (see Figures 8, 9, and 10 in Appendix).

Especially after the Global Financial Crisis, a growing empirical literature has delved into estimating the effects of fiscal shocks on economic growth and public debt in developed and developing countries, with varying results depending on the adopted methodology, the composition of the fiscal adjustment, and the macroeconomic context. On the methodological front, the econometric literature has two main groups: studies using cyclically adjusted fiscal variables in VAR estimations (Blanchard and Perotti 2002; Alesina and Ardagna 2010); investigations based on the construction of narrative datasets containing specific historical episodes of fiscal shocks (Romer and Romer 2010; Alesina, Favero and Giavazzi 2019; Carrière-Swallow, David and Leigh 2021; Gechert, Horn and Paetz 2019; Gechert, Paetz and Villanueva 2021). Using either one of these empirical approaches, a smaller group of authors have researched the impact of fiscal shocks on income inequality.

The evidence, so far, suggests that spending-based and tax-based adjustments have contributed to a rise in income disparity (Ball et al. 2013; Agnello and Sousa 2014; Schategger and Weder 2014; Furceri, Jalles and Loungani 2016; Klein and Winkler 2019; Heimberger 2020).

However, these studies have only considered OECD countries. Our motivation to focus on Latin American economies has been twofold. First, the existing empirical findings on the effect of fiscal consolidation episodes on GDP in advanced economies already differ from the recent evidence for Latin America: in OECD countries (Alesina, Favero, and Giavazzi 2019), tax-based episodes are more contractionary, while spending-based seem to have a more negative effect on GDP in Latin America (Carrière-Swallow, David and Leigh 2021). Second, the distributive effects of fiscal policy in Latin America are smoother than in OECD countries (Goñi, López, and Servén, 2011). In contrast to the OECD progressive tax systems that rely heavily on personal income taxes and social contributions (see Tables 7 and 8 in Appendix), the tax structure in Latin America is neutral or even regressive due to the high burden of indirect taxes (ECLAC, 2021).

Based on the narrative dataset constructed by David and Leigh (2018) for estimating the macroeconomic effects of fiscal shocks in Latin America, this paper aims to measure the impacts of spending-based and tax-based fiscal consolidations on inequality using Jordá's (2005) local projections method. Due to the scarcity of annual data for Gini indexes in several Latin American economies, we limited the sample to nine countries in South America (Argentina, Bolivia, Brazil, Chile, Colombia, Ecuador, Paraguay, Peru, and Uruguay) in the period from 1991 to 2017.

Our findings provide strong evidence of the positive impact of spending-based fiscal consolidations on income inequality for these nine countries in the medium run. The baseline results show that a reduction in government expenditures of 1% of GDP generates a 2.48% increase in the Gini index for disposable income. This general finding is robust under alternative specifications that include utilizing different control variables, lag length structures, country samples, and the Cyclically Adjusted Primary Balance (CAPB) strategy for identifying the fiscal shocks. When it comes to tax-based fiscal consolidation episodes, our evidence is not robust to a change in the country sample. Although our baseline results do not show a statistically significant impact of tax-based fiscal consolidation episodes on inequality, this effect turns positive when we exclude Bolivia from the sample.

The rest of the paper is structured as follows. Section 2 describes our dataset and methodology in the context of the existing empirical literature on the effects of fiscal austerity. Section 3 presents and discusses our baseline results. Section 4 tests the robustness of our findings based on alternative specifications, lag structures, samples, and identification strategies. Section 5 concludes.

2. Data and Methodology

2.1 Identification of fiscal shocks: statistical vs. narrative approach

In the 1990s and early 2000s, the growing empirical literature on the macroeconomic effects of fiscal policy shocks employed the so-called statistical or Cyclically-Adjusted Primary Balance approach (CAPB) (McDermott and Wescott, 1996; Lambertini and

Tavares, 2005; Alesina and Ardagna, 2010). In short, the CAPB adjusts the budget balance to account for the effects of the business cycle on government revenues and expenditures. For instance, this method would avoid considering a cyclical fall in government revenues as part of a fiscal expansion. As a result, when observed GDP is lower (higher) than potential, the fiscal balance would be adjusted upward (downward).

However, this approach has been questioned since the beginning of the 2010s. Devries et al. (2011) suggested endogeneity problems in the cyclical adjustment method due to the intrinsic correlation between such measures and economic fluctuations. In addition, even if fluctuations in the CAPB could accurately reflect discretionary changes in fiscal policy, the intrinsic motivation for these movements may be related to a response to cyclical fluctuations.[1] In this case, causality would run from the economic cycle to fiscal policy. For instance, governments may cut spending when the economy overheats. In addition, unemployment insurance and other categories of social benefits will respond to the economic cycle, linking recessions to an increase in these types of spending.

Based on the case of Finland in the 2000s, which implemented a fiscal consolidation not captured by CAPB in a context of strong economic growth and a boom in asset prices, Ball et al. (2013) argue that this approach ignores the motivations behind fiscal actions.[2] Additionally, Agnello and Sousa (2014, 2016) criticize the arbitrary nature of the statistical smoothing technique used to extract the impact of the economic cycle on fiscal indicators and the unrealistic assumption of a constant elasticity of budgetary components relative to economic activity.

Other authors developed similar criticisms to the statistical approach and utilized alternative methods to identify fiscal shocks (Woo et al. 2013; Schaltegger and Wedder 2014; Furceri, Jalles, and Loungani 2016, 2018; Jalles 2017; Klein and Winkler 2019; Heimberger 2020). In particular, based on the work of Devries et al. (2011), inspired by Romer and Romer (2010), the narrative approach arguably reduces the recognized

[1] Cyclical adjustment methods fail to remove the impact of high fluctuations in economic activity and asset prices from fiscal data, generating changes in the CAPB not necessarily linked to fiscal policy. A boom in the stock market, for instance, raises the CAPB through tax revenues generated by capital gains. A commodity price boom can stimulate private investment and raises cyclically adjusted government revenues (David and Leigh 2018).

[2] If a fiscal adjustment is itself a response to pressures generated by strong domestic demand, then it does not make sense to estimate the effects of fiscal policy through this approach.

endogeneity problems in the CAPB method by focusing on specific historical episodes of fiscal consolidation. These episodes are identified from the actions and intentions of policymakers as described in official documents such as the IMF's Recent Economic Development and Staff reports, the OECD Economic Surveys, and other historical records. To avoid the endogeneity problem, only policy actions that explicitly intend to reduce the budget deficit and respond to past economic conditions - not prospective ones - are included in the database.[3]

This procedure intends to eliminate endogenous responses of fiscal policy to economic fluctuations, capture the decision components of policymakers primarily related to the reduction of the budget deficit, and exclude other political, economic, and institutional factors that may motivate fiscal consolidations. As the effect of the fiscal consolidation on the budget balance is recorded in the year that the adjustment occurs, the announced policy measures that end up not being implemented are not included in the database. Finally, this strategy also facilitates decomposing fiscal adjustments into spending-based or tax-based episodes, allowing for a more refined understanding of the different impacts of austerity.

It is worth mentioning that Devries et al.'s (2011) approach also has some disadvantages. Jordà and Taylor (2016) suggested that the strategy depends on the subjective judgment of those who build the database and may not eliminate the endogeneity problem. Escolano et al. (2014) express concern over utilizing many different sources with potentially incompatible methodologies to obtain estimates of the budgetary impact of fiscal policy actions.

While we have chosen to adopt a narrative approach in our baseline estimations, we have also tested the CAPB conventional approach as a robustness check. As it comes out, our main findings – namely that spending-based fiscal consolidations significantly raise income inequality in the medium run – are similar when using narrative and statistical approaches (see section 4.3).

2.2 Database

[3] Therefore, the fiscal shocks identified from this strategy should not result from other economic fluctuations.

The narrative dataset constructed by David and Leigh (2018) includes fiscal consolidation episodes in 14 Latin American economies between 1989 and 2016. The authors examine the intentions and actions of policymakers as described in contemporaneous policy documents and identify measures motivated primarily by deficit reduction and long-term fiscal health objectives. Such fiscal actions do not respond to developments that affect the economic activity in the short run and, therefore, we use them as exogenous shocks in the estimation of the impacts of fiscal adjustments.

Historical sources examined by the authors include reports by multilateral institutions, such as IMF Staff Reports and the OECD Economic Surveys, budget documents[4], as well as reports by Central Banks. In some cases, they supplemented these sources with information from Working Papers or other research documents.

David and Leigh (2018) have not considered some observed shocks in these countries between 1989 and 2016 because they are potentially endogenous to the business cycle. For instance, if consolidation is motivated primarily by restraining domestic demand or in response to an economic contraction, they included its occurrence in the paper but did not include it in the database.[5] To deal with potential measurement errors related to the

[4] Such as the *Informe de Finanzas Publicas* from Chile and Paraguay, *Marco Fiscal de Mediano Plazo* from Colombia, *Criterios Generales de Política Económica* from Mexico, and *Marco Macroeconomico Multianual* from Peru.

[5] "An increase in the VAT rate by 3 percentage points with an estimated revenue yield of 2 percent of GDP was implemented in Argentina in 1995 with the objective of reducing the fiscal deficit in the context of a loss of confidence in debt markets. In line with Gunter et al. (2017), we consider that this episode was primarily motivated by responding to a fall in confidence and a large capital outflows. Therefore, we do not record it as fiscal consolidation motivated primarily by deficit-reduction and medium-term fiscal sustainability considerations" (David and Leigh 2018: 8).
"An adjustment program aimed at reducing very high inflation with 1.5 percent GDP in tax measures and 2.5 percent of GDP in expenditure cuts was implemented in Brazil in 1990. This episode was determined to be related to cyclical conditions and was not included in the database. According to the 1991 Recent Economic Developments report (SM 91/201) page 26: "In 1990 tax revenue of the Central Administration (excluding earmarked social taxes) increased by 1.5 percentage points of GDP as a result of measures implemented in March". Same report page 30 states that: "Expenditure (excluding outlays shifted to the expanded Social Security Budget) declined by 10.5 percentage points of GDP in 1990 (see Table 1.5); most of the decline (8 percentage points) reflected the effect of negative real interest payments on domestic debt and the remainder resulted from cutbacks in current and capital transfers to public enterprises and a number of measures related to the administrative reform". The 1993 Recent Economic Developments report (SM 93/125) sheds light on the motivation of the adjustment program on page 4: "Upon assuming office in March 1990 the new Administration introduced an economic program (the Collor I Plan) that aimed at bringing about a sharp drop in inflation, which had reached 72 percent per month in February" (David and Leigh 2018: 12).

projected budgetary impact of the shocks, David and Leigh (2018) used other documents and retrospective descriptions of fiscal actions to adjust the dataset and consider announced measures not fully implemented.

Our database combines David and Leigh's (2018) narrative dataset with data on inequality. Because income inequality indicators rely on national sources, such as household sampling surveys or household budget surveys, there are difficulties in making international comparisons. In light of these difficulties, the literature that focuses on the distributional impacts of fiscal shocks converges on the use of the Standardized World Income Inequality Database (SWIID), which provides information on the Gini index for market income and disposable income for a sample of 196 countries from 1960 to 2018 (Solt, 2019). Based primarily on the Luxembourg Income Study (LIS) data, this dataset utilizes a Bayesian approach to standardize observations obtained from different sources.[6]

Although Furceri, Jalles, and Loungani (2016) and Furceri et al. (2018) highlight downsides in the use of modeling to then estimate missing information from the LIS in the construction of SWIID, it remains the best available database when considering both coverage and quality of the data (Woo et al. 2013).

Among the authors who estimated the impacts of fiscal consolidations on income inequality (Agnello and Sousa 2012, 2014, 2016; Ball et al. 2013; Woo et al. 2013; Schaltegger and Weder 2014; Furceri, Jalles, and Loungani 2016; Furceri et al. 2018; Klein and Winkler 2019; Heimberger 2020), only Agnello and Sousa (2016) used a different database due to their focus on European regional inequality.

Based on the (low) availability of data for the Gini index for disposable income and fiscal shocks, we were able to construct a panel with nine countries (Argentina, Bolivia, Brazil, Chile, Colombia, Ecuador, Paraguay, Peru, and Uruguay) for the period between 1991 and 2017. We present information and descriptive statistics related to annual fiscal consolidation shocks included in our panel in Tables 1 and 2.

[6] Such as the OECD Income Distribution Database, the Socio-Economic Database for Latin America and the Caribbean (CEDLAS), Eurostat, World Bank PovcalNet, and others.

Table 1 – Annual fiscal consolidation shocks between 1991 and 2016, year of implementation

Country	Spending-based fiscal consolidations	Spending-based fiscal expansions	Tax-based fiscal consolidations	Tax-based fiscal expansions
Argentina			1996, 1997	
Bolivia			1995, 2004, 2005	
Brazil	2015		2015	
Chile	2003	2008	1991, 2004, 2014, 2015, 2016	
Colombia	2000, 2015, 2016		2003, 2011, 2012	
Ecuador	1993		1993, 2000	
Paraguay	2001, 2016		2001, 2003, 2004, 2014	2005, 2006
Peru			1992, 2002, 2003, 2012	2011
Uruguay	1995, 2000, 2002, 2003, 2015		1995, 1996, 2002, 2003	2004, 2005

Source: Based on David and Leigh (2018).

Table 2 – Annual fiscal shocks between 1991 and 2016 with descriptive statistics

	Number of annual shocks	Average size (% do GDP)	Min size (% GDP)	Max size (% GDP)
Consolidation	35	0.95	0.1	4.1
Tax-based	29	0.8	0.1	4.1
Spending-based	13	0.75	0.2	1.7
Expansion	6	-0.6	-0.38	-0.9
Tax-based	5	-0.62	-0.38	-0.9
Spending-based	1	-0.5	-0.5	-0.5

Source: Based on David and Leigh (2018).

A detailed description of fiscal shocks is available in Table 9 in Appendix, which sets out the countries, years, implemented measures, and estimated budgetary impact for each observation. We show the relationship between fiscal shocks and the Gini index for the nine countries of the sample in Figures 8, 9, and 10 in the Appendix.

2.3 Econometric strategy

Regarding econometric methods to estimate the distributive impacts of fiscal adjustments, one can distinguish between (i) static models, such as Seemingly Unrelated Regressions (SUR) or panel data with fixed effects estimators; and (ii) dynamic models, such as Autoregressive Distributed Lag (ARDL), or Panel Vector Autoregressive (PVAR), or Local Projections (LP) method (Jordà 2005) to estimate Impulse Response

Functions (IRFs). This section shows how the literature has evolved over the past few years to widespread use of Jordà's (2005) method.

In order to estimate contemporaneous impacts of fiscal consolidations on income inequality, several authors use static models. While Agnello and Sousa (2012, 2014) employ Seemingly Unrelated Regressions (SUR) as a baseline model, Jalles (2017) and Woo et al. (2013) use SUR as a complement to their main strategies. This method consists of estimating two regressions – one for the Gini index for disposable income and another for the Gini index for market income (the errors of these equations are considered correlated). If the unobserved determinants of these two indexes are correlated, the SUR estimator is an efficient and plausible strategy.

Several authors applied static models to estimate the contemporaneous impacts of fiscal consolidations on income inequality. While Agnello and Sousa (2012, 2014) employed Seemingly Unrelated Regressions (SUR) as a baseline model, Jalles (2017) and Woo et al. (2013) utilized SUR as a complement to their main strategies. Other authors applied panel data models with fixed effects as their baseline strategy (Woo et al. 2013; Schaltegger and Wedder 2014). This method allows us to account for unobservable factors that do not vary over time for each sample unit or do not vary between countries for each temporal unit. However, as distributional impacts of fiscal consolidation tend to change over time, static approaches may not be sufficient.

Among available alternatives utilized to capture the dynamic effects of fiscal consolidations, one possibility is to apply IRFs from Panel Vector Autoregressive models (PVAR), but this method has several weaknesses. Jalles (2017) observes that the intrinsic characteristics of the PVAR models, such as the endogeneity of relevant regressors generating narrowness, would imply an accurate ordering of each regressor to estimate the system, although economic theory rarely provides this information.[7] In addition, while a VAR model represents a linear global approach to the actual data-generating process, it is optimally designed to project one period ahead. The shift of all measurement errors or misspecifications of the model over time hinders the

[7] Choleski decomposition is often used as a solution to this issue, although it has no value to provide structural information to a VAR.

interpretation of IRFs. Thus, the PVAR traditional approach may suffer from identification problems and length limitations (Heimberger 2020).

Utilizing Autoregressive Distributed Lag (ARDL) is suggested by several authors in order to account for dynamic effects (Ball et al. 2013; Furceri, Jalles, and Loungani 2016; Jalles 2017; Furceri et al. 2018; Heimberger 2020). However, as stated by these authors, the IRFs derived from this approach tend to be sensitive to the number of lags in the model, which generates potential instability in the face of slight changes. Furthermore, when the dependent variable is highly persistent, which is the case of the Gini index, then the significance of long-lasting effects can be simply driven by the use of one-type-of-shock models, i.e., the response of the dependent variable will be the same over time, regardless of the presence of shocks in the system.

The literature has recently converged towards utilizing the Local Projections approach to address these issues on the estimation of IRFs (Ball et al. 2013; Furceri, Jalles, and Loungani 2016; Jalles 2017; Furceri et al. 2018; Klein and Winkler 2019; Heimberger 2020). Jordà (2005) derived the local projections from sequential regressions of the endogenous variable shifted several steps ahead, similarly to direct forecasts in several stages. Therefore, these projections are "local" to each forecast horizon and more accurate than projections derived from PVARs (Klein and Winkler 2019). Different from ARDL models, the method of Jordà (2005) does not use lags of the dependent variable to derive the IRFs (Ball et al. 2013; Jalles 2017), allowing to estimate the confidence intervals of these impulse responses directly from the standard errors of the coefficients, without the need for Monte Carlo simulations (Furceri, Jalles, and Loungani 2016; Heimberger 2020).

In other words, the estimation of VARs is based on the sample and represents a global linear approximation that can be optimally designed for a period ahead even when misspecified. However, an impulse response is a function of predictions in increasingly distant horizons, which causes the aggravation of the specification errors over time. In contrast, the Local Projections method relies on sequential regressions of the dependent variable shifted to horizons ahead, thus generating consistent estimates of the impulse response coefficients. The Local Projections may be estimated from usual techniques,

such as Ordinary Least Squares (OLS), and are robust to specification errors.[8]

Hence, as in several other studies on the same topic (see Table 3), we have adopted Jordà's (2005) Local Projections method in our baseline estimations.

Table 3 – Summary of econometrics studies on the impact of fiscal policy on income inequality

Authors	Gini Database	Identification of Fiscal Shocks	Sample (Years)	Econometric Method
Agnello and Sousa (2012)	SWIID	CAPB	18 OECD economies (1970 - 2010)	SUR
Ball et al. (2013)	SWIID	Narrative approach	17 OECD economies (1978 - 2009)	IRFs from LPs
Woo et al. (2013)	SWIID	Narrative approach	17 OECD economies (1978 - 2009)	FEE, SUR
Agnello and Sousa (2014)	SWIID	Narrative approach	18 OECD economies (1978 - 2009)	SUR
Schaltegger and Weder (2014)	SWIID	Narrative approach	17 OECD economies (1978 - 2009)	FEE
Agnello et al. (2016)	ERD	Narrative approach	13 European countries (1980 - 2008)	FEE
Furceri, Jalles and Lougani (2016)	SWIID	Narrative approach	17 OECD economies (1978 - 2009)	IRFs from LPs
Jalles (2017)	Milanovic (2014)	CAPB	28 emerging economies (1980 – 2014)	SUR / IRFs from LPs
Furceri et al. (2018)	SWIID	Forecast errors in goverment spending[a]	103 emerging economies (1990 – 2015)	IRFs from LPs
Klein and Winkler (2019)	SWIID	Narrative approach	17 OECD economies (1980 – 2011)	IRFs from LPs
Heimberger (2020)	SWIID	Narrative approach[b]	17 OECD economies (1978 - 2013)	IRFs from LPs

Note:
CAPB: Cyclically-Adjusted Primary Balance approach.
WIID: World Income Inequality Database.
SWIID: Standardized World Income Inequality Database.
ERD: European Regional Database.
IRFs from LPs: Impulse Response Functions from Local Projections (Jordà 2005).
SUR: Seemingly Unrelated Regressions model.
FEE: Panel data with Fixed Effects Estimator.
a: Auerbach and Gorodnichenko (2013).
b: Based on Devries et al. (2011) and Alesina et al. (2015) databases.

[8] Accordingly, the impulse responses calculation for a time series vector based on Local Projections does not require an identical specification for the Data Generating Process (DGP). Thus, this method is appropriate when the DGP is unknown.

Our regressions are estimated from Ordinary Least Squares (OLS) on a panel with fixed effects for countries and time, along with Driscoll-Kraay standard errors to account for heteroscedasticity, serial and spatial autocorrelation. Thus, the OLS estimators are consistent and unbiased. For each period h, we estimated the following equation:

$$y_{i,t+h} - y_{i,t} = \sum_{k=0}^{2} \beta_k^h X_{i,t-k} + \sum_{j=0}^{1} \delta_j^k \Delta y_{i,t-j} + \varsigma^h Z_{i,t} + \alpha_i^h + \gamma_t^h + \varepsilon_{i,t+h}. \quad (1)$$

Where h = 1, …, 8, such as in studies for OECD countries; y denotes the Gini index for disposable income, in log; Δy is the change in the Gini index for disposable income, in log, including two lags of this measure in the baseline specification; X is the measure of fiscal consolidation, as a percentage of GDP; Z is a vector of additional control variables used in robustness checks that deal with potentially omitted variables, including one lag of the real GDP growth rate, one lag of the change in real GDP per capita, one lag in the change in the unemployment rate, and one lag in the change in trade openness as measured by the sum of exports and imports relative to GDP. The equation also includes time (γ^h_t) and country (α^h_i) fixed effects.

Note that β_0^h corresponds to the cumulative response of income inequality to the fiscal shock in a given horizon, i.e., the estimated multiplier. We addressed reverse causality by estimating the distributional effect in periods after a consolidation shock (Ball et al. 2013) and constructed IRFs by plotting the estimated β_0^h for h = 1, …, 8, with confidence intervals. Bands around the IRFs are associated with the standard deviations from the estimated coefficients β_0^h.

Table 4 shows the information on the explanatory variables of the model. The model specification is related to the previous studies that applied Jordá's (2005) method for the same purpose (Ball et al. 2013; Furceri, Jalles, and Loungani 2016; Furceri et al. 2018; Klein and Winckler 2019; Heimberger 2020).

Table 4 – Our explanatory variables

Variable	Description	Source
Change in the income inequality measure	First difference of the log of the Gini index for disposable income.	SWIID 8.2.
Fiscal	Fiscal shock measures (total, spending-based, or tax-	David and Leigh (2018).

consolidation measure	based, as a % of GDP) for 9 South American countries between 1989 and 2016.	
Real GDP growth rate[a]	First difference of the log of real GDP	WDI – World Bank.
Change in real GDP per capita[b]	First difference of the log of real GDP per capita	WDI – World Bank.
Change in unemployment rate	First difference of the unemployment rate	International Labour Organization – ILOStat database.
Change in trade openness	First difference of the following relation: Exports of goods and services (% of GDP) + Imports of goods and services (% of GDP).	WDI – World Bank.

Note:
a: Real GDP is denominated in US dollars in 2010 prices.
b: Real GDP per capita is denominated in US dollars in 2010 prices.

We implemented unit root tests to verify the stationarity of the model variables in baseline estimations and robustness checks. Note that fiscal variables assume a value equal to zero in the absence of consolidation shocks. Tables 10, 11, and 12 in Appendix present the results of the Levin-Lin-Chu[9] (LLC) tests.

3. Baseline results

By following the econometric strategy outlined in section 2.3, we estimated the distributional effects of an annual fiscal consolidation shock. We obtained IRFs based on local projections by plotting the consolidation coefficients β_0^h for each future time h, and utilized one standard error bands associated with them, allowing for comparability with previous studies (Ball et al. 2013; Furceri, Jalles and Loungani 2016; Heimberger 2020). Grey areas in the IRF plots indicate the confidence intervals.

Therefore, IRFs show the estimated response of income inequality as measured by the Gini index to an annual fiscal consolidation shock of 1% of GDP. The local projection is carried out from year zero to year eight, with the first impact of the shock appearing in the first year.

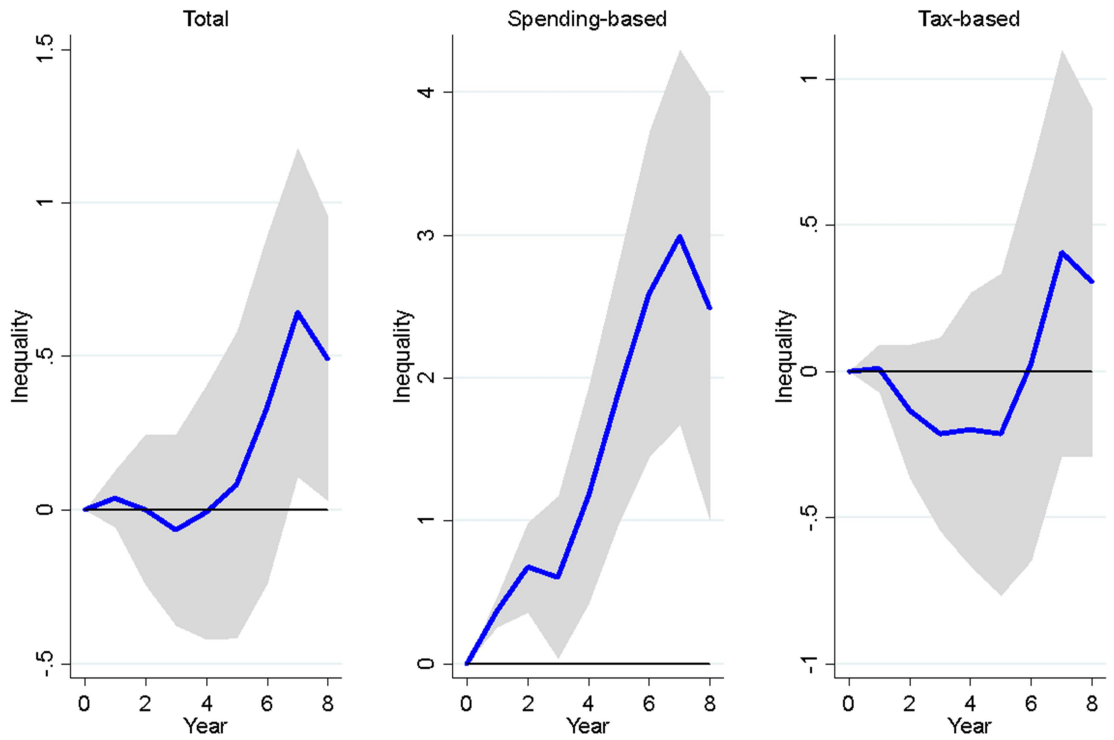
Figure 1 suggests that an annual fiscal consolidation shock has long-lasting effects on income inequality. Tables 13, 14, and 15 in Appendix present more details on the

[9] It has an alternative hypothesis of stationarity. This test is suggested when "(n/t) → 0" along with balanced panels, which is the case in our study.

magnitude of this impact, including the coefficients of the lagged dependent variable and their standard deviations. The Gini index for disposable income increases by about 0.037% in the short run, for year 1, after a fiscal consolidation shock of 1% of GDP. In the medium run, for year 8, the increase in inequality reaches almost 0.493%, being statistically significant.

Moreover, results for the baseline model indicate a strong and statistically significant impact of spending-based fiscal consolidations on income distribution. While a spending-based adjustment of 1% of GDP increases income inequality by 0.365% in the short run, it rises by 2.485% in the medium run, with statistical significance. In contrast, the impact of tax-based fiscal consolidations on inequality is smoother, increasing by 0.01% for year 1, and by 0.3% in the accumulated for year 8, and it was not statistically significant.

Fig. 1 – Cumulative Response of Inequality (change in %) to a fiscal consolidation of 1% of GDP – baseline results - IRFs



Note: Grey areas represent one standard error bands around the coefficients.

Table 5 shows that according to our estimates, spending-based fiscal consolidations in South America have a higher effect on inequality than in 8 out of 10 studies in our literature review.

Table 5 – Results observed in the empirical literature

Authors	Consolidation of 1% of GDP or dummy for consolidation episode	Spending-based adjustment (1% of GDP or dummy for consolidation episode)	Tax-based adjustment (1% of GDP or dummy for consolidation episode)
Agnello and Sousa (2012)	Reduction of 0.011 in the Gini index.	-	-
Ball et al. (2013)	Increase in the Gini index for disposable income: 0.2 ppt., after 2 years; ~0.9 ppt., after 8 years.	Increase in the Gini index for disposable income: ~0.9 ppt., after 8 years.	Increase in the Gini index for disposable income: ~0.9 ppt., after 8 years.
Woo et al. (2013)	Increase in the Gini index for disposable income: 0.13 ppt., after 2 years; 0.4 ppt., after 5 years.	Increase in the Gini index for disposable income: 1.5%. With statistical significance.	Negative relationship, but with no statistical significance.
Agnello and Sousa (2014)	Increase in the Gini index for disposable income: 0.026.	Increase in the Gini index for disposable income: 0.035.	Increase in the Gini index for disposable income: 0.004.
Schaltegger and Weder (2014)	Increase in the Gini index for disposable income: 0.4 ppt.	Increase in the Gini index for disposable income: 0.609 ppt.	Increase in the Gini index for disposable income: 0.28 ppt.
Agnello et al. (2016)	Increase in the Gini index: 0.1, after 1 year; 0.3, after 5 years.	Increase in the Gini index: 0.2, after 1 year; 0.5, after 5 years.	Fiscal consolidations seem to be neutral both in the short and medium terms, with no statistical significance.
Furceri, Jalles and Lougani (2016)	Increase in the Gini index: 0.2, after 1 year; 0.9, after 8 years.	Increase in the Gini index for disposable income: ~0.21 ppt., after 1 year; ~0.77 ppt., after 8 years. With statistical significance.	Increase in the Gini index for disposable income: ~0.21 ppt. (after 1 year); ~0.92 ppt. (after 8 years). With statistical significance.
Jalles (2017)	Increase in income inequality: 0.65 ppt., after 1 year; 0.8 ppt., after 3 years.	Increase in income inequality: 2.3 ppt., after 1 year; 3.2 ppt., after 4 years.	Decrease in income inequality: -0.8 ppt. (after 1 year); -2.6 ppt. (after 4 years).
Furceri et al. (2018)	-	Increase in income inequality: ~1 ppt., after 5 years.	-
Klein and Winkler (2019)	Increase in the Gini index for disposable income: 0.42, after 4 years.	Increase in the Gini index: with high debt after 4 years, 2.9; with low debt after 4 years, 0.	Increase in the Gini index: with high debt after 4 years, 1.5; with low debt after 4 years, 0.
Heimberger (2020)	Increase in the Gini index for disposable income: 0.35 ppt., after 3 years; 0.6 ppt., after 5 years.	Increase in the Gini index for disposable income: 0.5 ppt., after 3 years; ~0.4 ppt., after 8 years. With statistical significance.	Increase in the Gini index for disposable income: 0.2 ppt., after 3 years; ~0.3 ppt., after 8 years. With statistical significance.
This study (baseline)[10]	Increase in the Gini index for disposable income: 0.03% (0.012 ppt.), after 1 year; 0.493% (0.21 ppt.), after 8 years.	Increase in the Gini index for disposable income: 0.365% (0.155 ppt.), after 1 year; 2.48% (1.056 ppt.), after 8 years. With statistical significance.	Increase in the Gini index for disposable income: 0.01% (0.004 ppt.), after 1 year; 0.3% (0.12 ppt.), after 8 years. With no statistical significance.

[10] The calculations for the results in percentage points (ppt.) are based on the average of the Gini index for disposable income in our sample, which is 0.4262.

4. Robustness checks

To assess the sensitivity of our baseline results for changes in the estimation, we performed several robustness checks. Besides testing for different country samples, lag structures, and control variables, we employed CAPB to identify fiscal shocks and applied the Local Projections Instrumental Variable approach of Ramey and Zubairy (2018).

When it comes to the medium-run effect of spending-based fiscal adjustments, our main finding was robust for all specifications, samples, and methods, increasing income inequality with statistical significance. When it comes to the impact of tax-based fiscal consolidations, which appeared not to be statistically different from zero in the baseline, the results changed in the sample that excluded Bolivia or outliers for fiscal shocks. Short-run results for year 1, along with the effect of total fiscal consolidations, also varied depending on the method, country samples, and control variables used in the estimations.

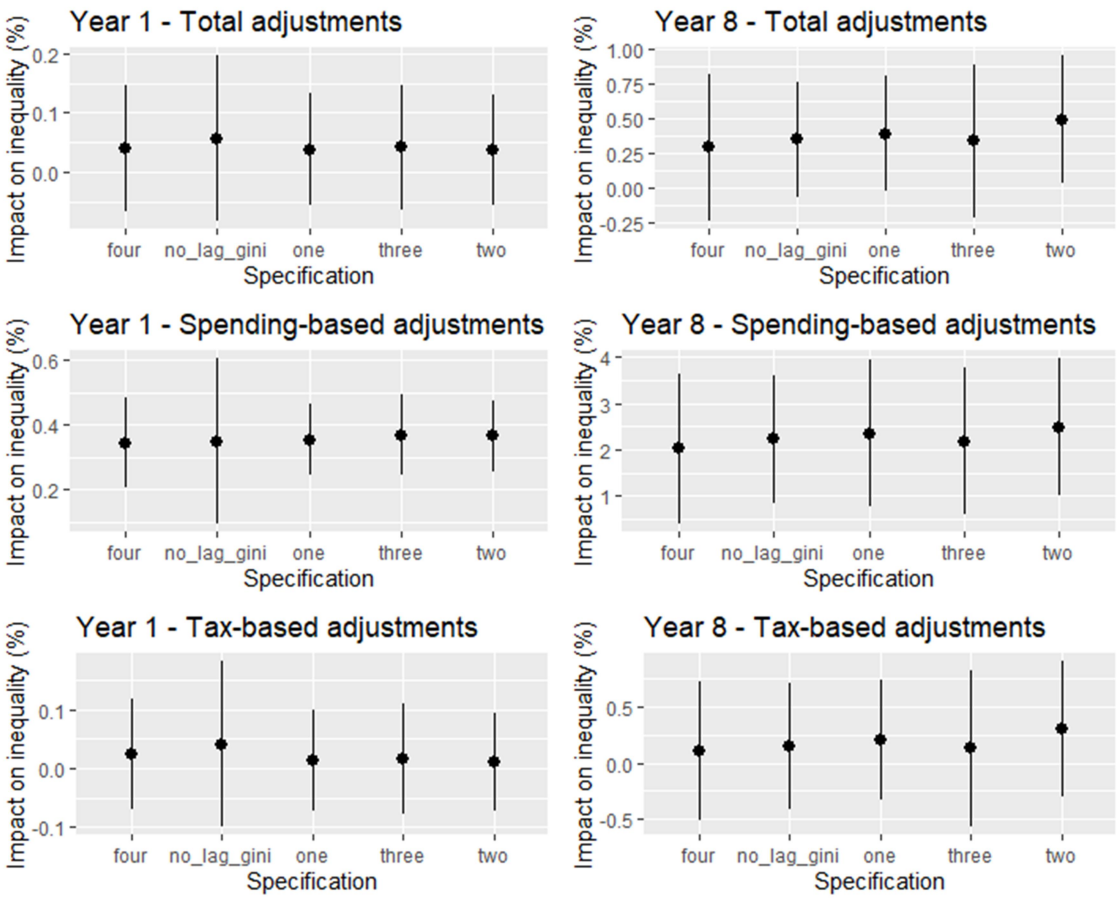
4.1 Alternative specifications

Our baseline model considered two lags of the change in income inequality for the right-hand side of the equation (1), allowing for comparability with previous studies (Ball et al. 2013; Woo et al. 2013; Furceri, Jalles, and Loungani 2016; Jalles 2017; Heimberger 2020). To verify the robustness of the results presented in section 3.2 for the choice of the lag structure, we tested for five different specifications, with one that does not include the change in income inequality as an explanatory variable and four specifications that included lags from the change in the Gini of disposable income. These specifications were named as “no_lag_gini”, “one”, “two” (baseline), “three”, and “four”, representing the number of included lags of this variable.

As shown in Figure 2, our main baseline results are robust in all these estimations, except for the impact of total fiscal consolidation in year 8. While spending-based fiscal consolidation shocks raised income inequality in the short and medium run, tax-based

fiscal adjustments did not present statistical significance when considering one standard-error band around the estimated coefficients.

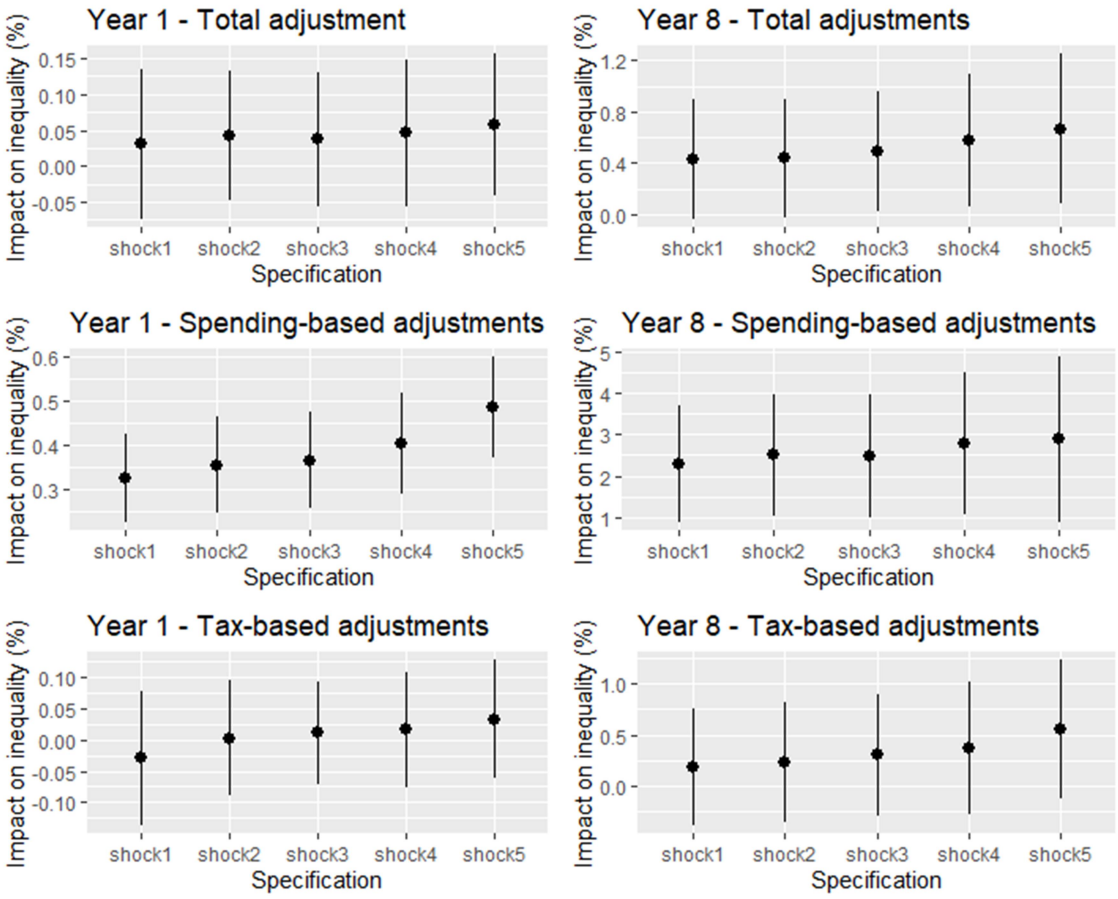
Fig. 2 – Impact of a 1% of GDP fiscal consolidation on income inequality, different lags of change in income inequality, year 1 and accumulated for year 8



Note: The figure presents the estimated coefficients in each specification and one standard error bands around them.
The X-axis indicates the number of lags of the change in Gini of disposable income used in each specification, with "two" as the baseline.

We also tested whether the results are robust for different lag lengths of fiscal shocks. Figure 3 presents the results of these checks and the respective nomenclature for the specifications "shock1", "shock2", "shock3" (baseline, with three lags), "shock4", and "shock5", with the suffix indicating the number of lags utilized in each of them. While spending-based fiscal consolidation shocks increased income inequality for year 1 and 8, tax-based adjustments did not yield a statistically significant effect. Again, baseline results for total fiscal consolidation effects were not robust in these tests for year 8.

Fig. 3 – Impact of a 1% of GDP fiscal consolidation on income inequality, different lags of fiscal shocks, year 1 and accumulated for year 8



Note: The figure presents the estimated coefficients in each specification and one standard error bands around them.
The suffix of the label on the X-axis indicates the number of lags of fiscal shocks utilized in each of the specifications, with “shock3” as the baseline.

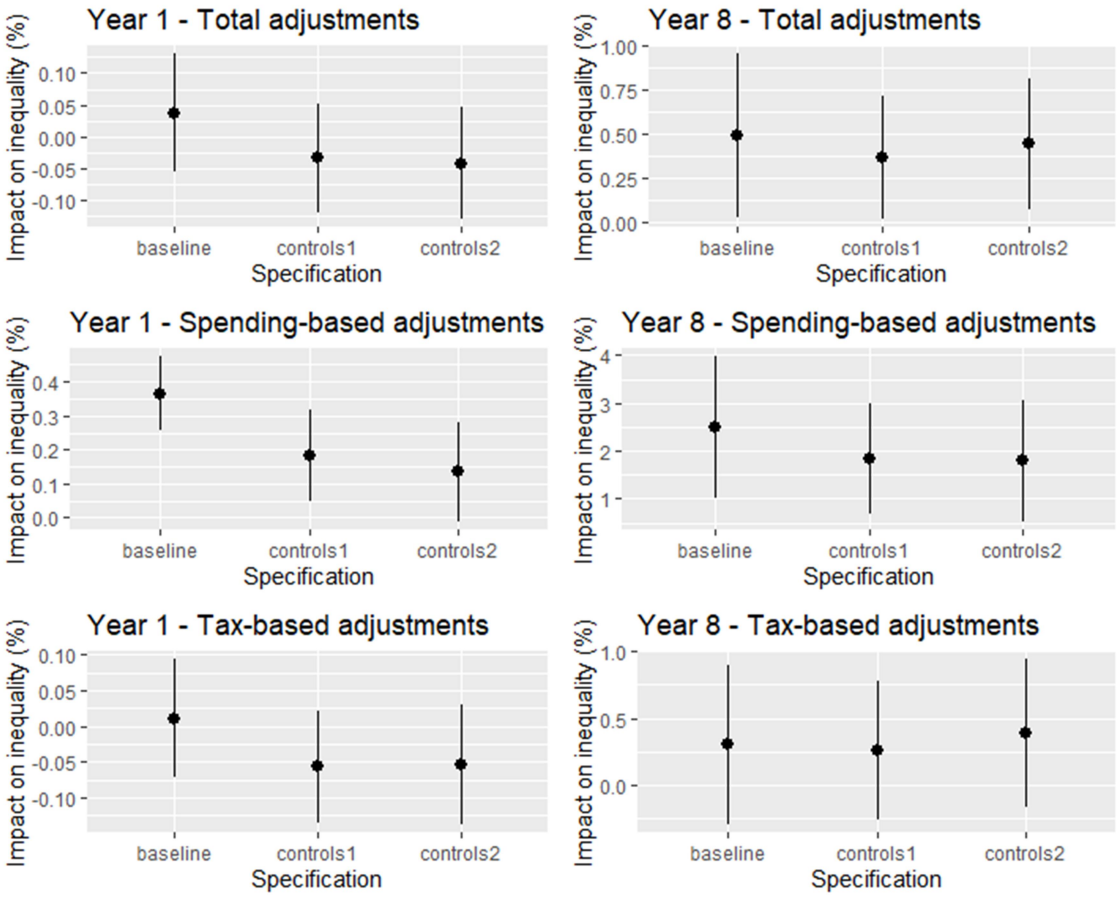
Finally, we ran different specifications with additional controls to address the possibility that omitted variables could bias our results. As shown in Table 4, we followed Heimberger (2020) and incorporated most of the control variables employed by the author, named "i - real GDP growth rate", "ii - change in the unemployment rate", and "iii - change in trade openness." Because of the unavailability of data for some of the series utilized by Heimberger (2020) for South American countries, we also added another variable suggested by Jalles (2017), which is "iv - change in real GDP per capita."

Figure 4 presents the results for all alternative specifications. While in the model “controls1”, we added one lag of variables “i”, “iii”, and “iv” to the baseline, in

“controls2”, we also included one lag of the variable “ii". As we could not obtain data since 1991 for “ii”, we utilized a sample from 1992 to 2017 in the model “control2”.

In the medium run, baseline results remained robust for spending-based, tax-based, and total fiscal adjustments. In the short run, the spending-based result did not show statistical significance when we considered the specification “controls2”, which, as mentioned, utilized a shorter time sample due to a lack of data.

Fig. 4 – Impact of a 1% of GDP fiscal consolidation on income inequality, specifications with additional controls, year 1 and accumulated for year 8



Note: The figure presents the estimated coefficients in each specification and one standard error bands around them.
Specification “controls1” included for additional controls one lag of each of these variables: real GDP growth rate, change in real GDP per capita, and change in trade openness.
Specification “controls2” utilized a sample from 1992 to 2017 and included for additional controls one lag of each of these variables: real GDP growth rate, change in real GDP per capita, change in trade openness, and change in the unemployment rate.

4.2 Alternative samples

In this section, we verified whether the baseline results are robust using different samples for the fiscal shocks and the countries. First, we performed an analysis by dropping outliers of the fiscal shocks (see averages in Table 2).[11] As shown in Figure 5, while we excluded observations at least two standard deviations distant from the mean in the sample "outliers1",[12] we did the same but considered one standard deviation in "outliers2".[13]

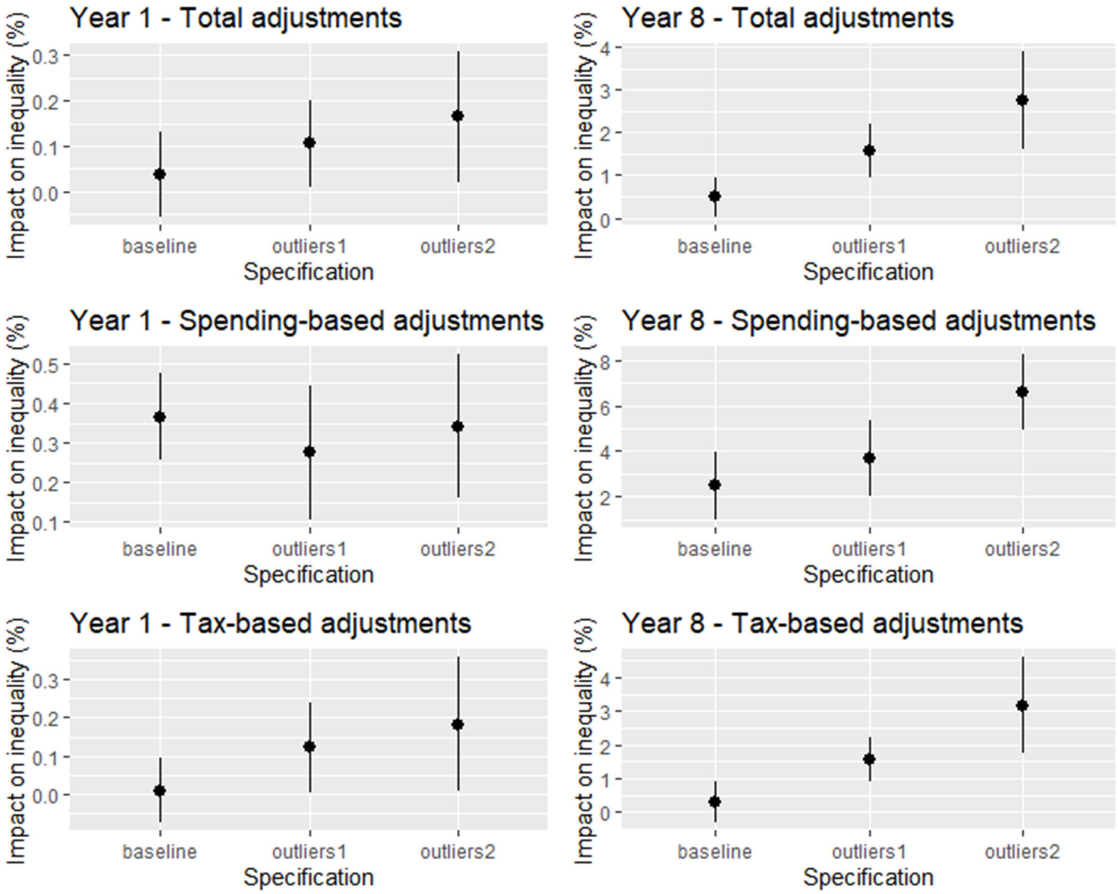
Spending-based fiscal consolidations increased income inequality for these different samples in the short and medium run. When it comes to tax-based fiscal consolidations, the effect on inequality turns positive and statistically significant when excluding outliers of the fiscal shock. Hence, some specific shocks in the sample seem to be responsible for driving our baseline results towards a non-statistically different from zero effect of tax-based episodes. Finally, total fiscal adjustments still generated an accumulated increase in income inequality after eight years.

Fig. 5 – Impact of a 1% of GDP fiscal consolidation on income inequality, excluding fiscal shocks outliers, year 1 and accumulated for year 8

[11] Note that the averages and the standard deviations are calculated from their different groups and subgroups: fiscal expansions (total, spending-based, tax-based); fiscal contractions (total, spending-based, tax-based).

[12] The observations that we dropped were the following: total adjustments higher than 2.67% of GDP (Uruguay in 2002, +3.275% of GDP; Bolivia in 2005, +4.1% of GDP); spending-based adjustments higher than 1.52% of GDP (Uruguay in 2002, +1.7% of GDP); tax-based adjustments higher than 2.04% of GDP (Bolivia in 2005, +4.1% of GDP).

[13] The observations that we dropped were the following: total adjustments higher than 1.8% of GDP (Uruguay in 2002, +3.275% of GDP; Bolivia in 2004 and 2005, +2% and +4.1% of GDP; Ecuador in 1993, +2.2% of GDP); spending-based adjustments higher than 1.14% of GDP (Uruguay in 2002, +1.7% of GDP; Paraguay in 2001, +1.7% of GDP); tax-based adjustments higher than 1.6% of GDP (Bolivia in 2004 and 2005, +2% and +4.1% of GDP; Ecuador in 1993, +1.7% of GDP); total fiscal expansions lower than -0.76% of GDP (Uruguay in 2005, -0.9% of GDP); tax-based fiscal expansions lower than -0.79% of GDP (Uruguay in 2005, -0.9% of GDP).



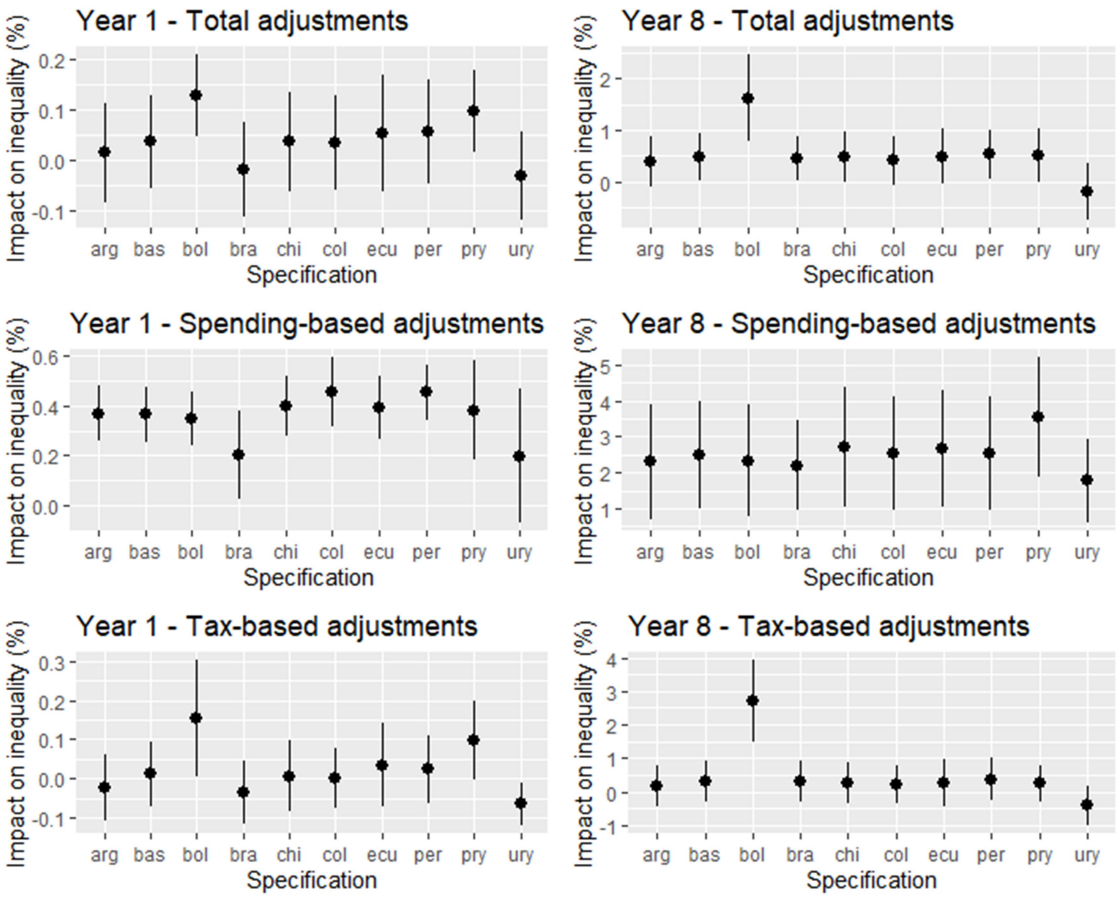
Note: The figure presents the estimated coefficients in each sample and one standard error bands around them.
Specification "outliers1" excluded observations for fiscal shocks at least two standard deviations distant from the averages calculated for their groups/subgroups.
Specification "outliers2" excluded observations for fiscal shocks at least one standard deviation distant from the averages calculated for their groups/subgroups.
Groups (subgroups): fiscal expansions (total, spending-based, tax-based); fiscal contractions (total, spending-based, tax-based).

We then considered nine different country samples by dropping one country at a time. The X-axis in Figure 6 indicates which country we excluded in each sample: "arg" for Argentina; "bas" for baseline, with no exclusion; "bol" for Bolivia; "bra" for Brazil; "chi" for Chile; "col" for Colombia; "ecu" for Ecuador; "pry" for Paraguay; "per" for Peru; "ury" for Uruguay.[14]

[14] The null hypothesis of unit root for "$y_{t+1} - y_t$" was not rejected for the samples in which we excluded Bolivia (p-value = 0.12), Chile (p-value = 0.11), Colombia (p-value = 0.11), Paraguay (p-value = 0.15), or Peru (p-value = 0.11). We followed Söderbom et al. (2015) and analyzed the graphical evolution of this variable, which suggests that the series are indeed stationary in the figures 11, 12, and 13 in Appendix. When we excluded 1991's year for these rare cases, we rejected the null hypothesis of a unit root and found that our results remained robust (see Table 10 and Figure 14 in Appendix).

The positive and statistically significant accumulated effect of spending-based consolidations on income inequality in year 8 holds in all country samples when considering one standard error bands around the estimated coefficients. Results for year 1 only lost statistical significance when excluding Uruguay from the sample. As for tax-based fiscal adjustments, a statistically significant positive effect on inequality in the medium run appeared when we excluded Bolivia. When combined with previous results on positive effects when excluding large fiscal shocks, these results suggest that specific fiscal episodes in Bolivia drove tax-based baseline results closer to zero. When it came to the short-run impact, tax-based consolidations had a negatively significant effect on inequality when excluding Uruguay. Finally, when we considered fiscal adjustment shocks as a whole, results varied more depending on the country's sample.

Fig. 6 – Impact of a 1% of GDP fiscal consolidation on income inequality, different samples dropping one country at a time, year 1 and accumulated for year 8



Note: The figure presents the estimated coefficients in each sample and one standard error bands around them.
The X-axis indicates which country we excluded in each sample: "arg" for Argentina; "bas" for baseline, with no exclusion; "bol" for Bolivia; "bra" for Brazil; "chi" for Chile; "col" for Colombia; "ecu" for Ecuador; "pry" for Paraguay; "per" for Peru; "ury" for Uruguay.

4.3 – Alternative methodological approaches

In this section, we tested whether the main results of the baseline model also held when utilizing a different strategy to identify fiscal shocks, the conventional approach, and another econometric method, the Instrumental Variable Local Projections.

To apply the conventional approach, we extracted data for the central government's primary balance and total primary revenues and expenditures as a percentage of GDP from CEPALSTAT for all countries and years in our sample.[15] We also applied Fedelino, Horton, and Ivanova's (2009) approach to cyclically adjust the data, assuming a zero elasticity of government expenditures relative to the output gap (Blanchard and Leigh 2013). The output gap series was obtained from Carrière-Swallow, David, and Leigh (2021).

Following Furceri, Jalles, and Loungani (2016) and Jalles (2017), we considered three alternative measures (Giavassi and Pagano 1996;[16] Alesina and Ardagna 1998;[17] Afonso 2010)[18] to obtain a variable that takes a value equal to one when there is an episode of fiscal consolidation, minus one when there is an episode of fiscal expansion, and zero otherwise. Based on Jalles (2017), we classified a consolidation episode as spending-based (tax-based) when more than 50% of the total adjustment occurred via expenditure cuts (tax increases).

Results presented in Figure 7 show that, once again, spending-based fiscal consolidation episodes increased inequality after 8 years in all the estimations, no matter the criteria applied for defining the fiscal shocks (narrative or conventional). The same did not hold for the positive effect of total fiscal consolidations on inequality, which lost significance after 8 years in two out of three estimations based on the CAPB approach. Such as

[15] For Bolivia, we could only obtain data for the general government.

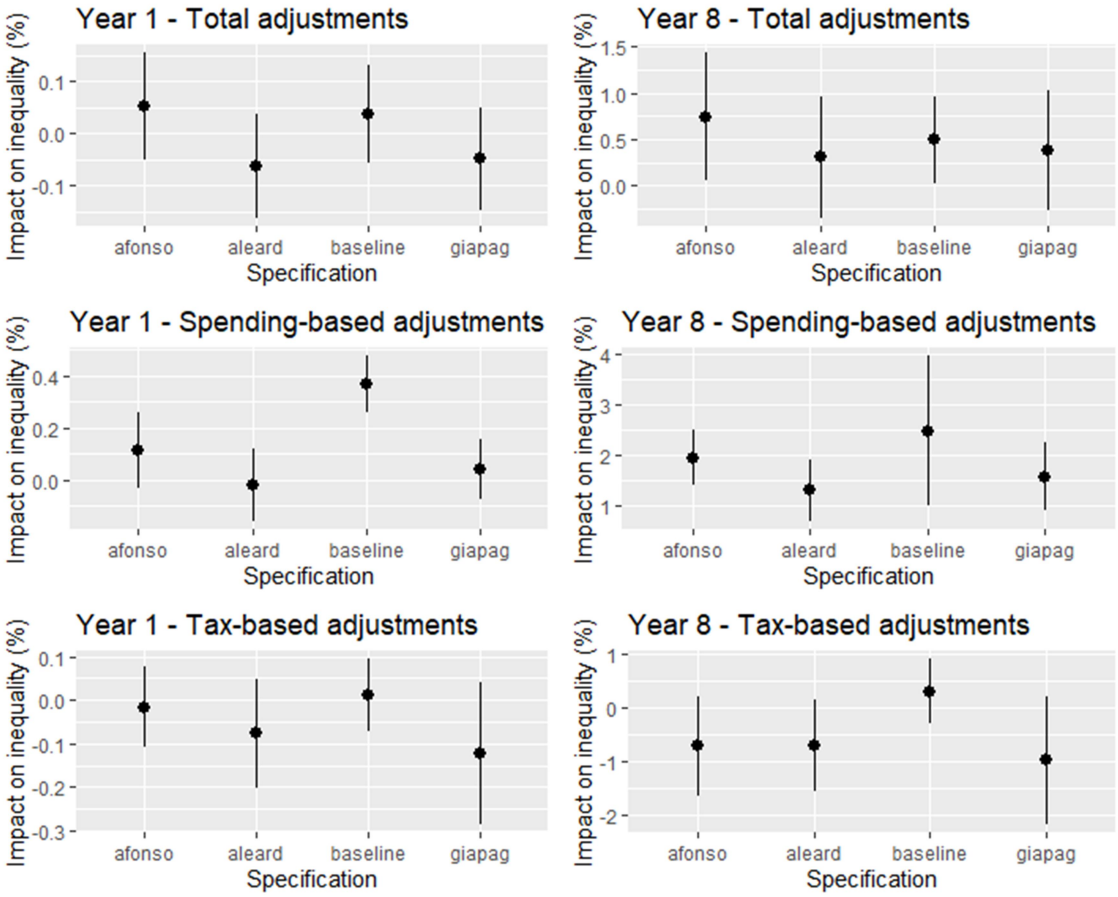
[16] Giavassi and Pagano (1996) identified fiscal adjustment episodes when the cumulative changes in the CAPB reached at least 5, 4, 3 percentage points of GDP in respectively 4, 3, or 2 years, or 3 percentage points in one year.

[17] Alesina and Ardagna (1998) assumed a fiscal consolidation episode when the change in the CAPB reaches at least 2 percentage points of GDP in one year or at least 1.5 percentage points for two consecutive years in the last two years.

[18] Afonso (2010) identified fiscal shocks when the change in the CAPB reaches at least one and a half times the standard deviation for the last year, or one standard deviation on average for two years.

baseline results based on a narrative dataset, the impact of tax-based fiscal adjustments did not show statistical significance when using the CAPB approach, but the medium-run effects now appeared negative in all three estimations.

Fig. 7 – Impact of a 1% of GDP fiscal consolidation on income inequality, narrative and conventional approaches, year 1 and accumulated for year 8



Note: The figure presents the estimated coefficients in each sample and one standard error bands around them.
Specification "baseline" refers to the narrative approach.
Specification "aleard" refers to Alesina and Ardagna's (1998) conventional approach.
Specification "giapag" refers to Giavassi and Pagano's (1996) conventional approach.
Specification "afonso" refers to Afonso's (2010) conventional approach.

Finally, to address the possibility of measurement errors in the narrative approach that, according to Escolano et al. (2014), could derive from the use of multiple sources to obtain estimates of the budgetary impact of fiscal policy actions, we implemented the Local Projections Instrumental Variable (LP – IV) approach from Ramey and Zubairy (2018).

This method has some advantages, such as the direct estimation of standard errors of the multiplier and the possibility that the instrument and instrumented variables have measurement errors, as long as they are uncorrelated. To implement it, we followed Carrière-Swallow, David, and Leigh (2021) and employed narrative shocks as instruments for changes in the CAPB[19] (d.CAPB, in % of GDP). Table 6 shows that the results of the IV estimation were very similar to those obtained in the baseline specification for h = 1, although the IV coefficient was lower for h = 8 and did not present statistical significance.

Table 6 – Effect of a 1% of GDP Fiscal Consolidation in year h

Specification	**h = 1**	**h = 8**
Baseline	0.0371	0.493
	(0.0929)	(0.463)
Observations	216	153
R^2	0.6461	0.6409
IV estimator	0.0507	0.359
	(0.071)	(0.748)
Observations	207	144
R^2	0.662	0.667
F-Stat	31.93	15.45

Note: The table reports estimates for the β^h coefficient in equation (1), where h refers to the horizon in years. Additional controls include two lags of the change in income inequality and fixed effects for years and countries. Driscoll–Kraay standard errors are in parentheses for the baseline specification. For IV regressions, we applied Newey-West correction for standard errors (Newey and West 1987) with automatic bandwidth selection at all horizons. Kleibergen–Paap Wald F-statistic was reported.
*Significant at 10%.
**Significant at 5%.
***Significant at 1%.

5. Conclusion

This paper contributed to the empirical literature on the effect of fiscal consolidations on income inequality by focusing on South American economies. Thus, we based on David and Leigh's (2018) narrative dataset for estimating impulse response functions using Jordà's (2005) Local Projections method for a panel covering nine South American countries between 1991 and 2017.

[19] F Kleibergen-Paap Wald statistics indicate that narrative shocks are strong instruments for the d.CAPB. The standard rule of thumb is that an F-statistic below 10 shows a potential problem with instrument relevance (Staiger and Stock 1997).

Our baseline results indicated that while spending-based fiscal austerity measures have significantly increased inequality as measured by the Gini index for disposable income, tax-based consolidation shocks effects were not statistically significant.

These results showed that the Gini index for disposable income rose 0.365% in the short run and 2.48% in the medium run after a spending-based fiscal adjustment of 1% of GDP. The magnitude of this effect is higher than in most studies in the recent literature on OECD countries. In contrast, the impact of tax-based fiscal consolidations on inequality is smoother, increasing by 0.01% for year 1, and by 0.3% in the accumulated for year 8, and it was not statistically significant.

We carried out a series of robustness checks using different control variables, lag structures, country samples, and econometric strategies. When it comes to tax-based fiscal adjustments, the effect after eight years was still not statistically significant in most of our estimations, except for the samples that excluded Bolivia or large shocks. Excluding Bolivia, it became positive. In the specifications that we employed the CAPB approach to identify fiscal consolidations, tax-based adjustments showed a negative non-significant effect on inequality in the medium run. As a consequence of the mixed findings for tax-based results, the medium-run impact of the total fiscal adjustments, including tax-based and spending-based, was still positive in most specifications, but it lost statistical significance in some of them.

Instead, our main finding that spending-based fiscal consolidations significantly increased inequality after eight years remained robust to all alternative specifications, including when we applied conventional CAPB approaches for identifying the consolidation shocks. Therefore, given the adverse socio-economic effects of the Covid-19 pandemic currently faced by South American countries, the potential implementation of a new round of spending-based fiscal consolidation plans raises concerns.

Declarations

Conflict of interest All authors declare that they have no conflict of interest.

Ethical statement This article does not contain any studies with human participants performed by any of the authors. This article also does not contain any studies with animals performed by any of the authors.

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Appendix

Table 7 – Tax structures for the OECD and LAC in 2019, % of total taxation

Category	Latin America and the Caribbean (LAC)	OECD countries
Personal Income Tax	9	23
Corporate Income Tax	16	10
Social security contributions	17	26
Value Added Taxes	28	20
Taxes on goods and services	22	12
Other Taxes	8	8

Source: Revenue Statistics in Latin America and the Caribbean (OECD 2021).

Table 8 – Tax structures for the OECD and LAC in 2019, % of GDP

Category	Latin America and the Caribbean	OECD countries
Taxes on income and profits	6.2	11.3
Taxes on property	0.8	1.8
Social security contributions	3.9	8.9
Taxes on goods and services	11.2	10.8
Other Taxes	0.4	0.2

Source: Revenue Statistics in Latin America and the Caribbean (OECD 2021).

Table 9 – Fiscal consolidation packages in the narrative dataset by David and Leigh (2018)

Country/Year	Composition of the Fiscal Shock	Summary of the Measures	Impact – in % of GDP
Argentina (1996)	↑Tax	- ↑Rates of corporate and personal income taxes. - ↑Tariffs on imports of capital goods and removal of subsidies for domestic producers of capital goods. - ↓Tax rebates for exporters. - ↑Fuel excises.	+ 0.25%.
Argentina (1997)	↑Tax	- Continuation of measures implemented in 1996.	+ 0.75%.
Bolivia (1995)	↑Tax	- ↑Rate of transaction tax. - ↑Excise tax on vehicles. - ↑Beer taxes.	+ 0.9%.
Bolivia (2004)	↑Tax	- Introduction of a financial transactions tax.	+ 2%.
Bolivia (2005)	↑Tax	- Introduction of a new direct tax on hydrocarbons. - Increase in the level of royalties from 18% to 50% of turnover.	+ 4.1%.
Brazil (2015)	↑Tax / ↓Expenditure	On the tax side: - ↑Taxes on fuels, household credit operations, car sales, imports, and cosmetics. - Elimination of electricity subsidies (supported by tariff adjustments).	+ 0.3% via Tax / + 0.5% via Expenditure.

		On the expenditure side: - ↓Benefits and tighter eligibility criteria for survivor pensions, unemployment, and sickness benefits, and salary bonuses for private employees.	
Chile (1990)	↑Tax	- ↑Value Added Tax (VAT) rate. - Change in the base for Corporate Income Tax (CIT) from distributed to earned profits and ↑CIT from 10% to 15%.	+ 0.5%.
Chile (1991)	↑Tax	- Continuation of measures implemented in 1990.	+ 0.17%.
Chile (2003)	↑Tax / ↓Expenditure	On the tax side: - ↑Effective VAR rate (VAT receipts/domestic demand). On the expenditure side: - ↓Spending limits of several ministries amounting to the equivalent of US$ 300 MM.	+ 0.2% via Tax / + 0.4% via Expenditure.
Chile (2004)	↑Tax	- Continuation of measures implemented in 2003	+ 0.4%.
Chile (2008)	↑Expenditure	- ↓Level of the structural balance target from 1% of GDP to 0.5% of GDP. - ↑Spending on education.	- 0.5%.
Chile (2014)	↑Tax	- ↑Corporate income tax rates, increases in excise duties, among other changes to the tax system. - Changes to the taxation of dividends.	+ 0.1%.
Chile (2015)	↑Tax	- Continuation of measures implemented in 2014.	+ 0.18%.
Chile (2016)	↑Tax	- Continuation of measures implemented in 2014.	+ 0.31%.
Colômbia (2000)	↓Expenditure	- ↓Capital expenditure. - Initiatives to strengthen efficiency and expenditure control at all levels of the public sector.	+ 0.9%.
Colombia (2003)	↑Tax	- One-time wealth tax. - Income tax surcharge and a broadening of the VAT base. - Impact of reforms was offset by ↑expenditure.	+ 1.1%.
Colombia (2011)	↑Tax	- Closing of loopholes in financial transactions tax. - Elimination of tax credits and ↑net wealth tax. - Impact of the measures was offset by ↓import tariffs.	+ 0.4%.
Colombia (2012)	↑Tax	- Elimination of the fixed asset tax credit, ↑progressivity in personal income tax, simplification of the VAT structure, and introduction of a new tax on corporate profits. - Measures offset by ↓payroll and corporate income taxes.	+ 0.8%.
Colombia (2015)	↓Expenditure	- ↓General government expenditures.	+ 0.5%.
Colombia (2016)	↓Expenditure	- Expenditure freeze that affects investment, wage bill, and transfers. - On the other side, protection of key social programs.	+ 0.7%.
Ecuador (1990)	↑Tax	- ↑Domestic prices of petroleum products. - Measures offset by ↓import tariff rates, in addition to changes in income and indirect taxes.	+ 0.33%.

Ecuador (1993)	↑Tax / ↓Expenditure	On the tax side: - Adjustment in fuel prices, in electricity tariffs. - One-time levy in the range of 0.2-0.7% on company assets. On the expenditure side: - ↓Current expenditures. - ↓Public investment.	+ 1.7% via Tax / + 0.5% via Expenditure.
Ecuador (2000)	↑Tax	- ↑Domestic prices for petroleum products (reduction of subsidies).	+ 0.8%.
Paraguay (1989)	↑Tax / ↓Expenditure	On the tax side: - ↑Public tariffs and an effort to improve the operational efficiency of public enterprises. - Efforts to improve tax administration and combat evasion. On the expenditure side: - ↓Public investment.	+ 2% via Tax / + 0.6% via Expenditure.
Paraguay (2001)	↑Tax / ↓Expenditure	On the tax side: - ↑Excise tax on diesel. - Inclusion of transport and personal services in the VAT tax base. - Elimination of VAT exemptions on goods in the re-export trade. On the expenditure side: - ↓Public investment and ↓government consumption. - Freeze of public sector wages in nominal terms and restriction of the public employment, while overtime pay being sharply curtailed.	+ 0.5% via Tax / + 1.3% via Expenditure.
Paraguay (2003)	↑Tax	- ↑Excise taxes. - ↑Excise duties.	+ 1.25%.
Paraguay (2004)	↑Tax	- ↑Excise tax on diesel. - Introduction of a soy exports tax. - Introduction of a new agricultural income tax and a new personal income tax. - Broadening of the VAT base, in addition to adjustments in some excise tax rates, and strengthening of the legal authority for tax administration.	+ 0.8%.
Paraguay (2005)	↓Tax	- Elimination of the export tax on soy. - ↓CIT rate.	- 0.6%.
Paraguay (2006)	↓Tax	- Further ↓CIT rate.	- 0.7%.
Paraguay (2014)	↑Tax	- Broadening of the VAT base (at a reduced tax) to include unprocessed agricultural products. - A revamped tax on agricultural income.	+ 0.24%.
Paraguay (2016)	↓Expenditure	- ↓Current expenditures (especially a decrease of the wage bill in real terms). - Measures partially offset by ↑public investment.	+ 0.8%.
Peru (1992)	↑Tax	- ↑VAT rate (from 16% to 18%), with a	+ 1%.

		broadening of its base. - ↑Rates of several excise taxes. - Elimination of certain deductions to the CIT and continued efforts to strengthen tax administration.	
Peru (2002)	↑Tax	- Broadening of the income tax base. - ↑Kerosene excise. - Elimination (or restriction) of some VAT exemptions. - On the tax administration, measures aimed at reducing tax evasion.	+ 0.2%.
Peru (2003)	↑Tax	- Continuation of the measures implemented in 2002.	+ 0.8%.
Peru (2011)	↓Tax	- ↓Trade tariffs. - ↓Financial transactions tax and ↓general sales tax. - Measures were partially offset by a new mining taxation framework that included a new special mining tax and a new royalties system based on operating profits.	- 0.39%.
Peru (2012)	↑Tax	- Continuation of new mining taxation regime.	+ 0.38%.
Uruguay (1990)	↑Tax	- ↑VAT rate, ↑public sector tariffs, ↑agricultural income taxes, ↑several excise taxes. - Creation of a tax on real estate transfers and a temporary surcharge on certain imports. - Efforts to reduce smuggling and tax evasion and to improve the efficiency of collections.	+ 1.7%.
Uruguay (1995)	↑Tax / ↓Expenditure	On the tax side: - ↑VAT rates while reducing VAT exemptions. - ↑Rates of the tax on wages and retirement pensions. - Various changes in the corporate income, agricultural, and sales taxes to strengthen collections. On the expenditure side: - ↓Public investment. - Curtailing hiring in the public sector and ↓current expenditures.	+ 0.75% via Tax / + 0.9% via Expenditure.
Uruguay (1996)	↑Tax	- Continuation of the tax measures implemented in 1996.	+ 0.25%.
Uruguay (2000)	↓Expenditure	- ↓Public investment.	+ 0.8%.
Uruguay (2002)	↑Tax / ↓Expenditure	On the tax side: - ↑Tax on wages and pensions. - New excise taxes and a broadening of the VAT base. On the expenditure side: - ↓Public investment. - ↓Government consumption.	+ 1.58% via Tax / + 1.7% via Expenditure.
Uruguay (2003)	↑Tax / ↓Expenditure	On the tax side: - Tariff adjustments. On the expenditure side: - Centralization of public sector procurement of medical supplies and food.	+ 1.4 via Tax / + 0.2 via Expenditure.

Uruguay (2004)	↓Tax	- Elimination of emergency surcharges (on wage tax and CIT) and tax (on commissions and public utilities) implemented since 2002.	- 0.5%.
Uruguay (2005)	↓Tax	- Measures related to the elimination of emergency surcharges implemented in 2004 continued in 2005.	- 0.9%.
Uruguay (2015)	↓Expenditure	- ↓Public investment, partially offset by ↑current expenditures.	+ 0.6%.

Source: David and Leigh (2018).
Note: A negative sign indicates a fiscal expansion.

Table 10 – Levin-Lin-Chu unit root tests – p-values (null hypothesis of unit root)

Sample	VAR1	VAR2	VAR3	VAR4	VAR5	VAR6	VAR7	VAR8
Baseline	0.09	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Argentina	0.09	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Bolivia	0.12	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Bolivia2	0.04	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Brazil	0.07	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Chile	0.11	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Chile2	0.07	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Colombia	0.12	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Colombia 2	0.06	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Ecuador	0.08	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Paraguay	0.15	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Paraguay2	0.08	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Peru	0.11	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Peru2	0.06	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Uruguay	0.08	0.00	0.00	0.00	0.00	0.00	0.00	0.00

Note:
VAR1: $y_{t+1} - y_t$.
VAR2: $y_{t+2} - y_t$.
VAR3: $y_{t+3} - y_t$.
VAR4: $y_{t+4} - y_t$.
VAR5: $y_{t+5} - y_t$.
VAR6: $y_{t+6} - y_t$.
VAR7: $y_{t+7} - y_t$.
VAR8: $y_{t+8} - y_t$.
"y" = Gini (in log) – disposable income.
The column "Sample" represents which country we excluded from the sample in the robustness tests. In "Baseline," we included all countries.

The suffix "2" indicates time samples between 1992 and 2017, excluding the 1991's year.

Table 11 – Levin-Lin-Chu unit root tests – p-values (null hypothesis of unit root)

Specification	VAR1	VAR2	VAR3	VAR4	VAR5	VAR6	VAR7	VAR8
LP - IV	0.00	0.00	0.00	0.00	0.02	0.03	0.01	0.05

Note:
VAR1: x_{t-0}.
VAR2: $x_{t+1} + x_{t-0}$.
VAR3: $x_{t+2} + x_{t+1} + x_{t-0}$.
VAR4: $x_{t+3} + x_{t+2} + x_{t+1} + x_{t-0}$.
VAR5: $x_{t+4} + x_{t+3} + x_{t+2} + x_{t+1} + x_{t-0}$.
VAR6: $x_{t+5} + x_{t+4} + x_{t+3} + x_{t+2} + x_{t+1} + x_{t-0}$.
VAR7: $x_{t+6} + x_{t+5} + x_{t+4} + x_{t+3} + x_{t+2} + x_{t+1} + x_{t-0}$.
VAR8: $x_{t+7} + x_{t+6} + x_{t+5} + x_{t+4} + x_{t+3} + x_{t+2} + x_{t+1} + x_{t-0}$.
"x" = change in the Cyclically Adjusted Primary Balance.
"LP - IV" refers to the fiscal consolidation variable employed in the Local Projections Instrumental Variable robustness test.

Table 12 – Levin-Lin-Chu unit root tests – p-values (null hypothesis of unit root)

Specification	VAR1	VAR2	VAR3	VAR4
Controls 1	0.00	0.00	0.00	
Controls 2	0.00	0.00	0.00	0.00

Note:
VAR1: change in real GDP per capita.
VAR2: real GDP growth rate.
VAR3: change in trade openness.
VAR4: change in the unemployment rate.
Controls 1 (sample from 1991 to 2017) and Controls 2 (sample from 1992 to 2017): both specifications included additional variables as controls to implement robustness tests.

Table 13 – Impact on inequality from fiscal consolidation shocks

	Year 1 (1)	Year 2 (2)	Year 3 (3)	Year 4 (4)	Year 5 (5)	Year 6 (6)	Year 7 (7)	Year 8 (8)
Shock (t)	0.037	0.0013	-0.0644	-0.007	0.082	0.329	0.644	0.493
	(0.092)	(0.246)	(0.312)	(0.414)	(0.500)	(0.569)	(0.536)	(0.463)
Shock (t-1)	-0.0686	-0.141	-0.136	-0.0962	0.0496	0.243	0.0004	-0.092
	(0.112)	(0.164)	(0.258)	(0.340)	(0.341)	(0.306)	(0.273)	(0.285)
Shock (t-2)	-0.0302	0.0309	0.117	0.327	0.516*	0.362	0.342	0.272
	(0.0855)	(0.122)	(0.137)	(0.168)	(0.238)	(0.342)	(0.452)	(0.537)
Change in Gini (t)	0.519**	0.931**	1.178**	1.305**	1.679***	1.981***	2.093***	2.093**

	(0.140)	(0.266)	(0.331)	(0.423)	(0.303)	(0.338)	(0.491)	(0.667)
Change in Gini (t-1)	0.0748	0.0763	-0.0437	-0.165	-0.645	-1.021**	-1.38***	-1.8***
	(0.0957)	(0.222)	(0.323)	(0.464)	(0.417)	(0.328)	(0.306)	(0.394)
Observations	216	207	198	189	180	171	162	153

Note: ****p<0.05;** p<0.01; ***p<0.001***
Standard errors in parentheses

Table 14 – Impact on inequality from spending-based fiscal consolidation shocks

	Year 1 (1)	**Year 2 (2)**	**Year 3 (3)**	**Year 4 (4)**	**Year 5 (5)**	**Year 6 (6)**	**Year 7 (7)**	**Year 8 (8)**
Shock (t)	0.365**	0.673*	0.605	1.178	1.902	2.587*	2.989*	2.484
	(0.109)	(0.315)	(0.570)	(0.762)	(0.920)	(1.141)	(1.314)	(1.487)
Shock (t-1)	0.211	0.0573	0.572	1.441	1.864	2.385	1.855	1.490
	(0.198)	(0.485)	(0.674)	(0.815)	(0.993)	(1.187)	(1.395)	(1.627)
Shock (t-2)	-0.318	0.0256	0.819	1.380	1.811	1.495	1.284	0.797
	(0.269)	(0.541)	(0.696)	(0.994)	(1.199)	(1.418)	(1.727)	(2.062)
Change in Gini (t)	0.517**	0.930**	1.147**	1.241**	1.544***	1.856***	1.989***	2.033**
	(0.142)	(0.267)	(0.326)	(0.402)	(0.314)	(0.320)	(0.452)	(0.607)
Change in Gini (t-1)	0.0815	0.0692	-0.0456	-0.162	-0.576	-0.939**	-1.3***	-1.8***
	(0.0935)	(0.212)	(0.302)	(0.419)	(0.399)	(0.313)	(0.294)	(0.344)
Observations	216	207	198	189	180	171	162	153

Note: ****p<0.05;** p<0.01; ***p<0.001***
Standard errors in parentheses

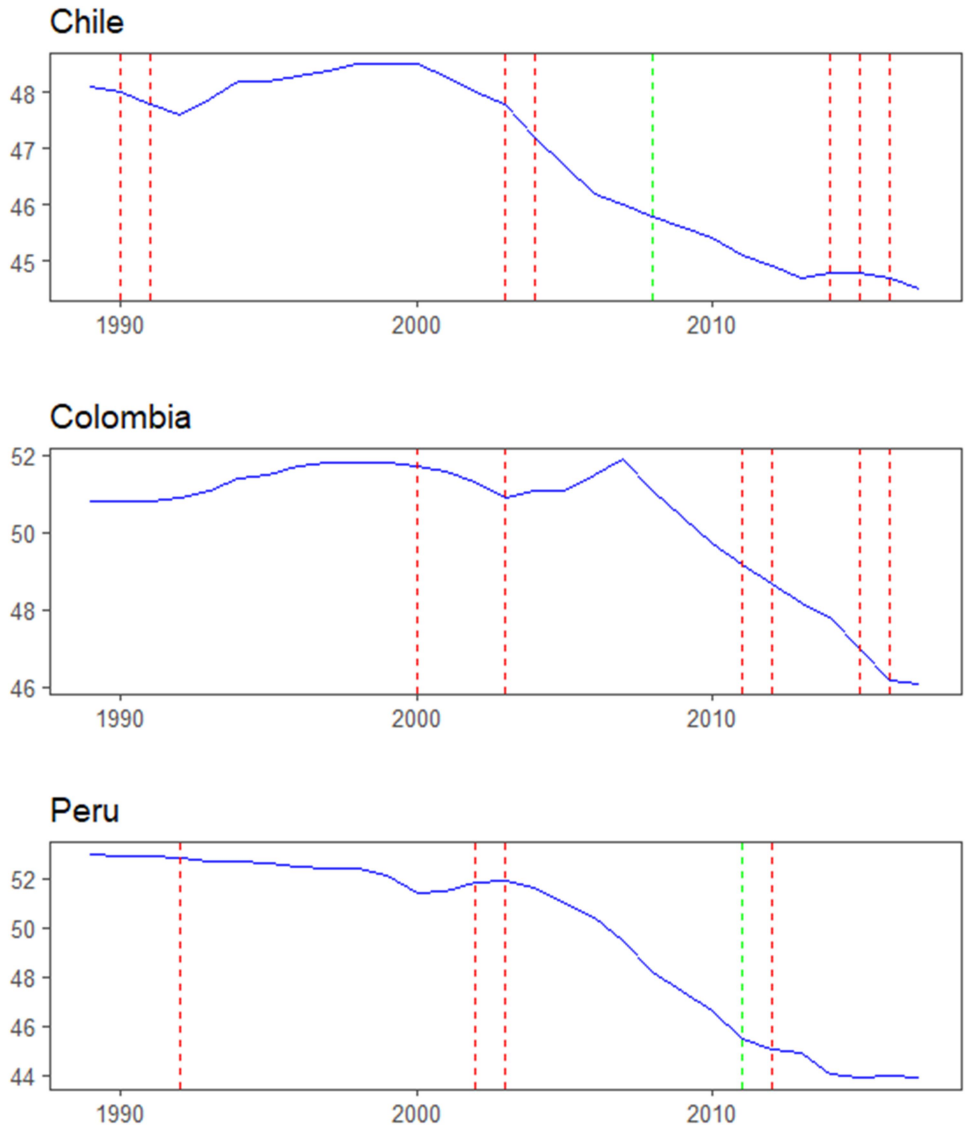
Table 15 – Impact on inequality from tax-based fiscal consolidation shocks

	Year 1 (1)	**Year 2 (2)**	**Year 3 (3)**	**Year 4 (4)**	**Year 5 (5)**	**Year 6 (6)**	**Year 7 (7)**	**Year 8 (8)**
Shock (t)	0.0109	-0.136	-0.215	-0.199	-0.216	0.0259	0.406	0.304
	(0.0826)	(0.229)	(0.331)	(0.469)	(0.552)	(0.673)	(0.696)	(0.597)
Shock (t-1)	-0.148	-0.160	-0.210	-0.276	-0.0793	0.114	-0.148	-0.270
	(0.0952)	(0.164)	(0.301)	(0.354)	(0.399)	(0.379)	(0.350)	(0.294)
Shock (t-2)	0.029	0.008	0.0067	0.235	0.402	0.244	0.252	0.257
	(0.0579)	(0.145)	(0.175)	(0.209)	(0.258)	(0.375)	(0.479)	(0.531)
Change in	0.518**	0.927**	1.168**	1.294**	1.681***	1.980***	2.075***	2.067**

Gini (t)								
	(0.141)	(0.265)	(0.332)	(0.426)	(0.297)	(0.331)	(0.484)	(0.655)
Change in Gini (t-1)	0.0765	0.0820	-0.0345	-0.149	-0.634	-1.015**	-1.37***	-1.8***
	(0.0965)	(0.219)	(0.322)	(0.467)	(0.416)	(0.329)	(0.310)	(0.398)
Observations	216	207	198	189	180	171	162	153

Note: ***p<0.05;** p<0.01; ***p<0.001***
Standard errors in parentheses

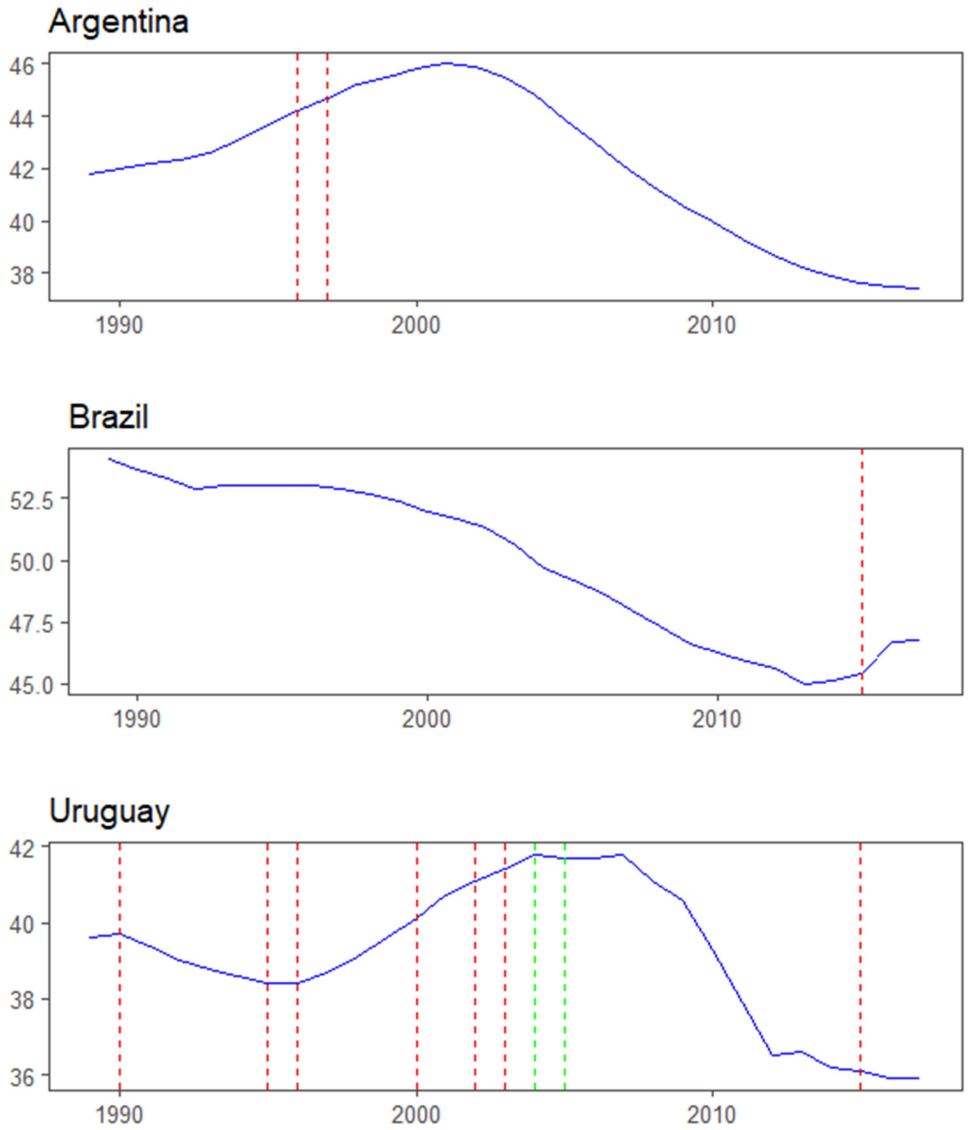
Fig. 8 – Gini index for disposable income and Fiscal shocks for Chile, Colombia and Peru (1989 – 2017)



*Dashed lines: fiscal shocks; red (adjustment) and green (expansion).

Note: The Gini index can range from 0 to 100.

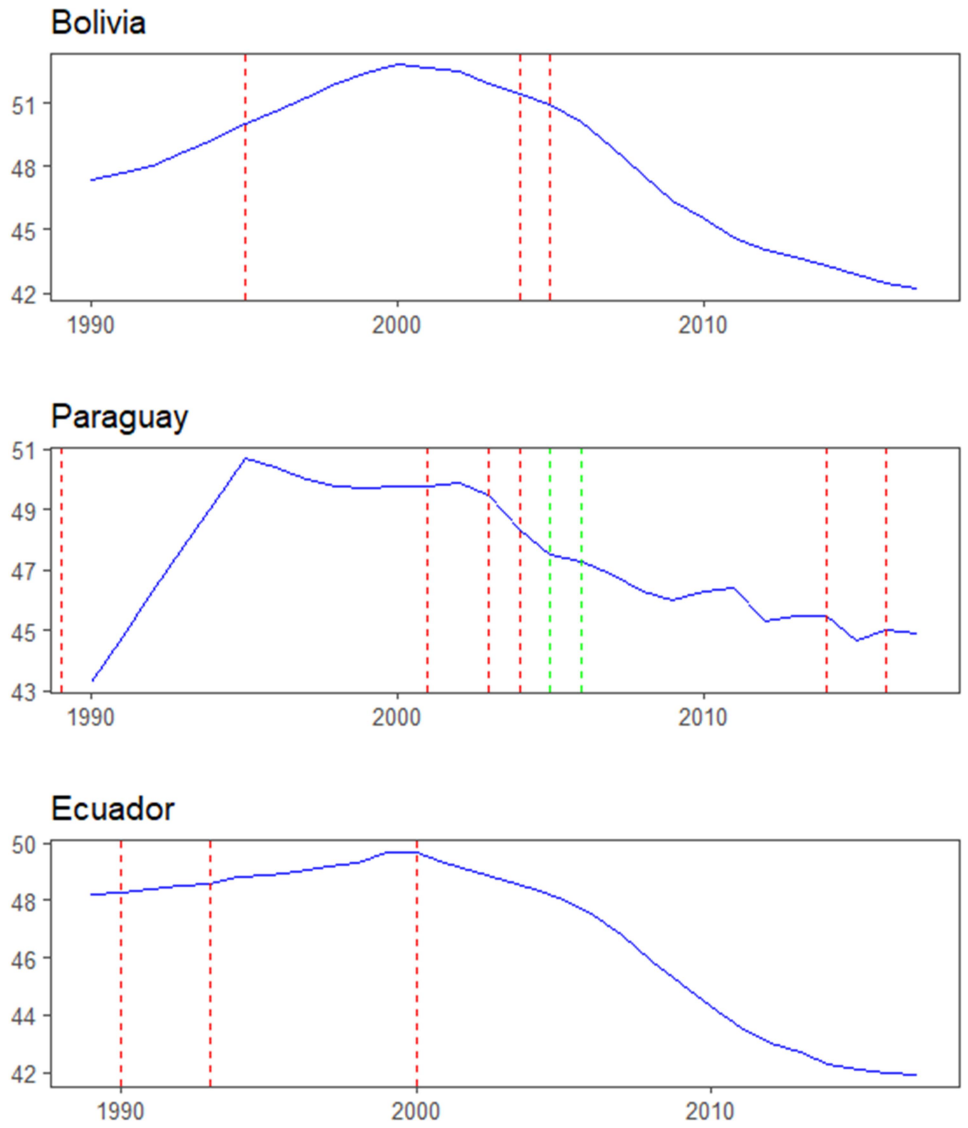
Fig. 9 – Gini index for disposable income and Fiscal shocks for Argentina, Brazil and Uruguay (1989 – 2017)



*Dashed lines: fiscal shocks; red (adjustment) and green (expansion).

Note: The Gini index can range from 0 to 100.

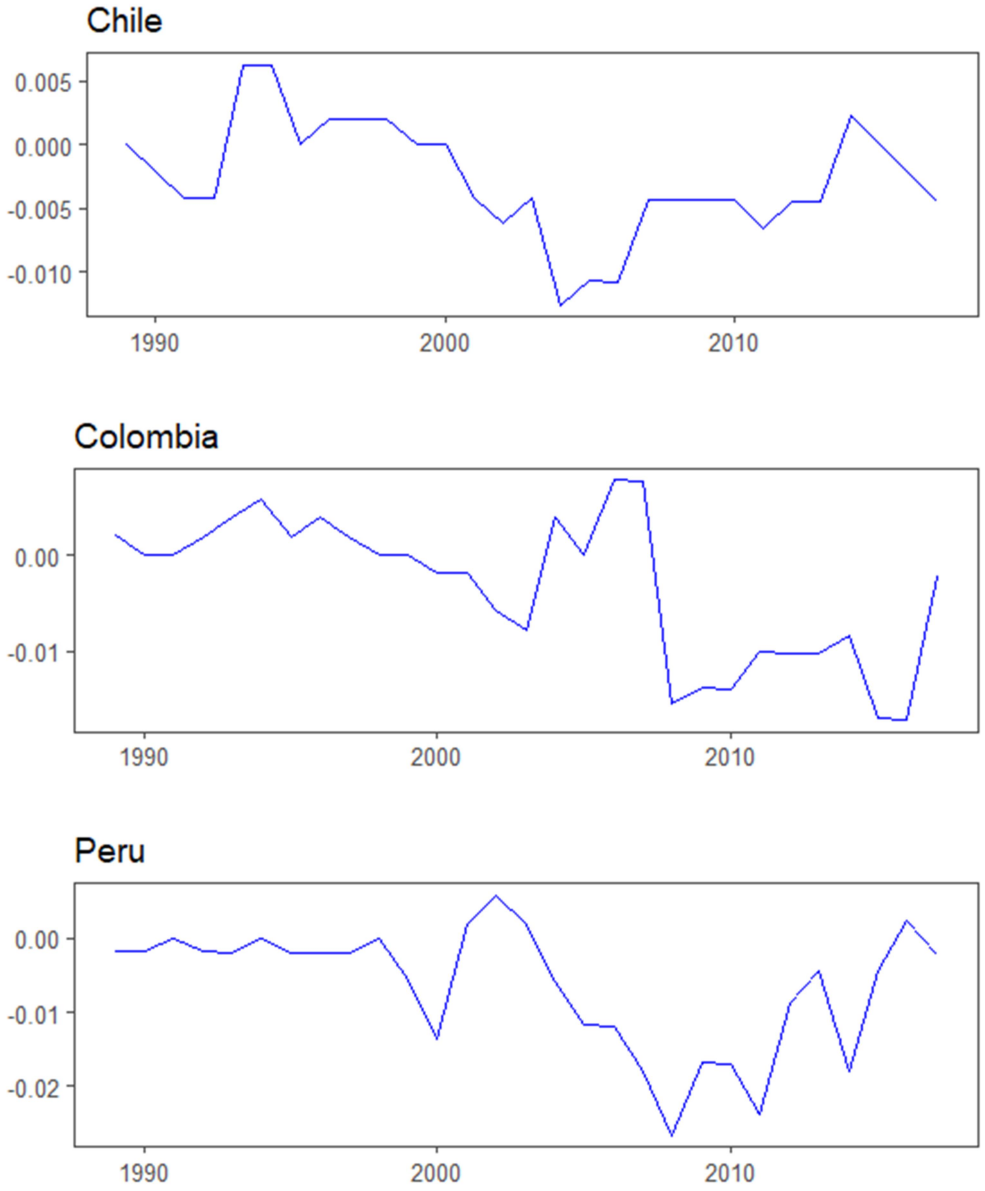
Fig. 10 – Gini index for disposable income and Fiscal shocks for Bolivia, Paraguay and Ecuador (1989 – 2017)



*Dashed lines: fiscal shocks; red (adjustment) and green (expansion).

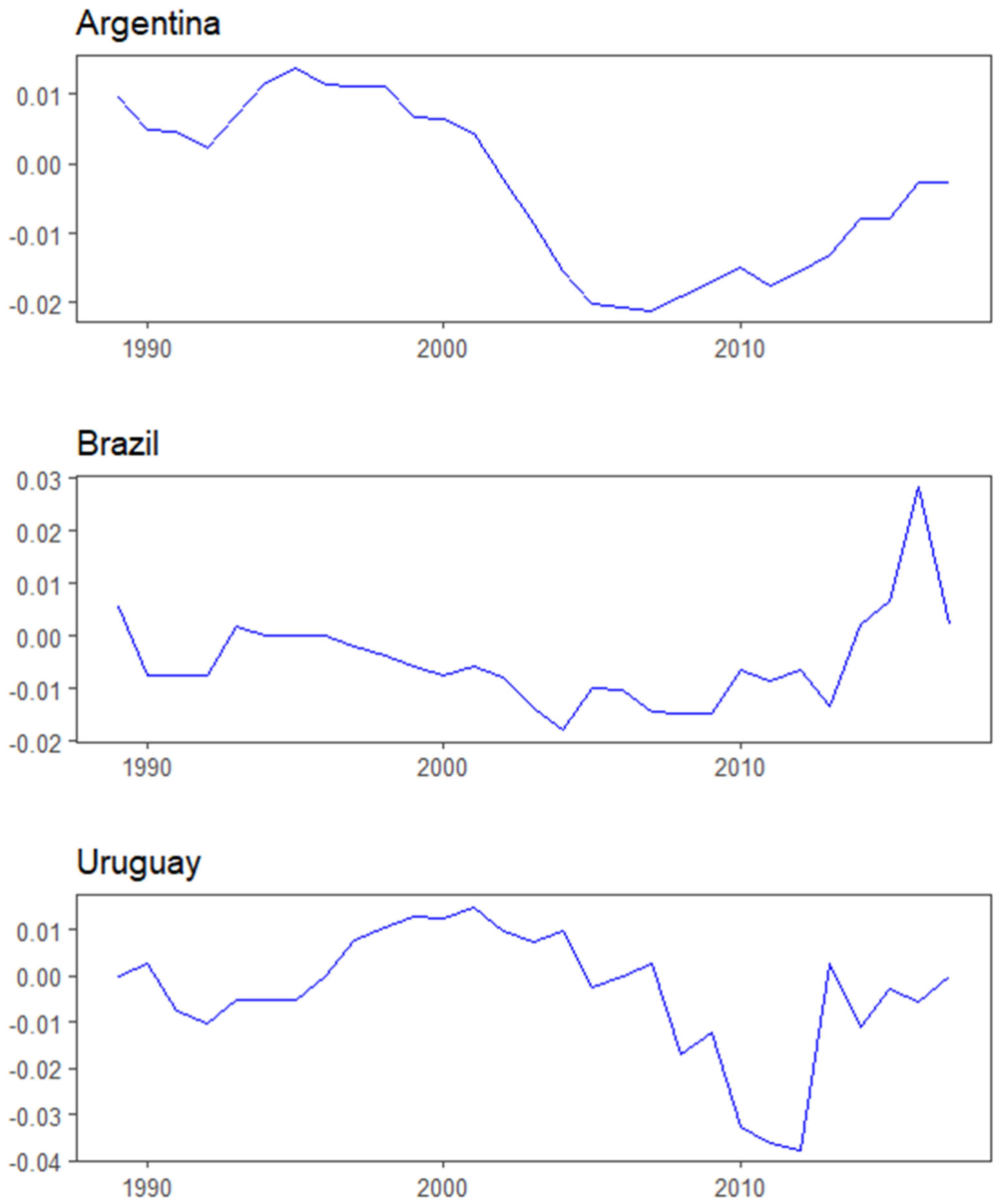
Note: The Gini index can range from 0 to 100.

Fig. 11 – Changes in income inequality for Chile, Colombia and Peru (1989 – 2017)



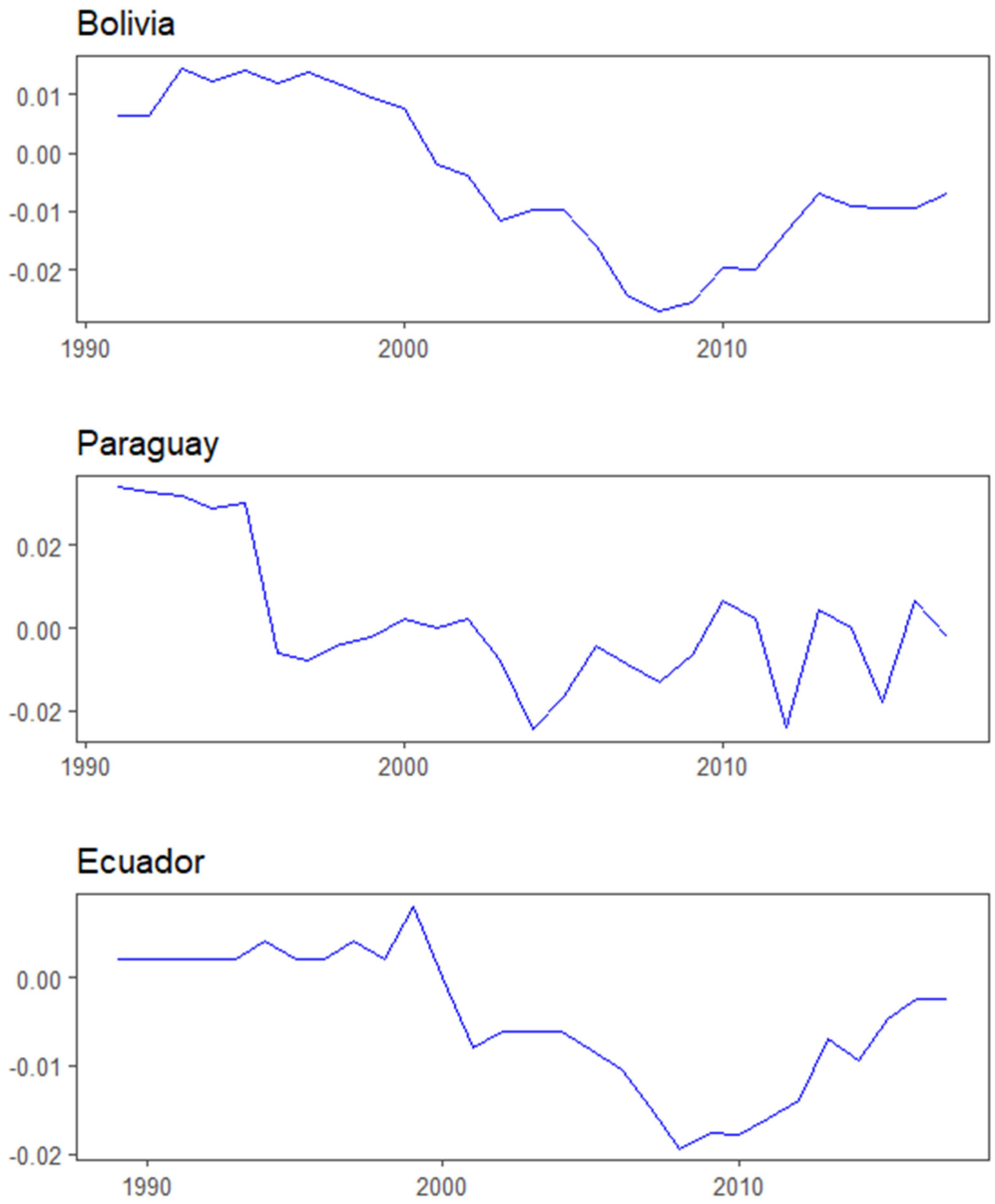
Note: In Y-axis, we consider 0.01 as 1%, for instance.

Fig. 12 – Changes in income inequality for Brazil, Argentina and Uruguay (1989 – 2017)



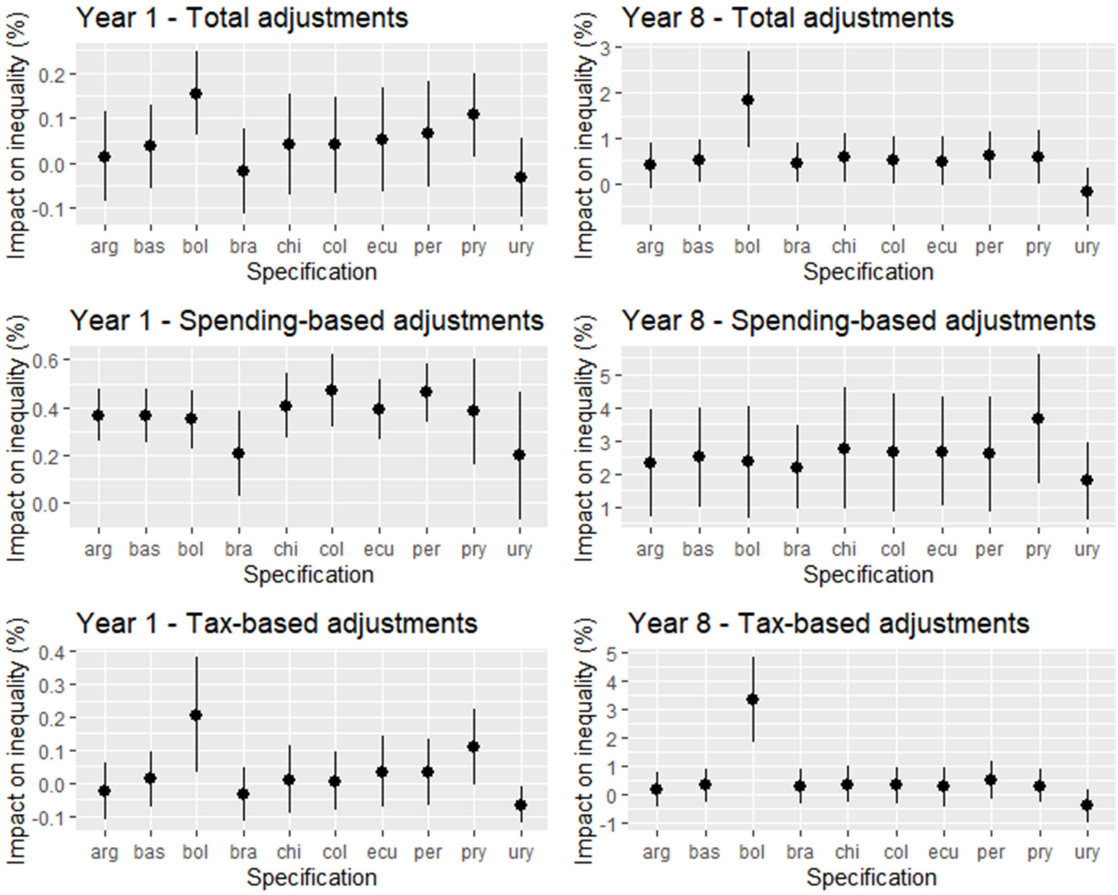
Note: In Y-axis, we consider 0.01 as 1%, for instance.

Fig. 13 – Changes in income inequality for Bolivia, Paraguay and Ecuador (1989 – 2017)



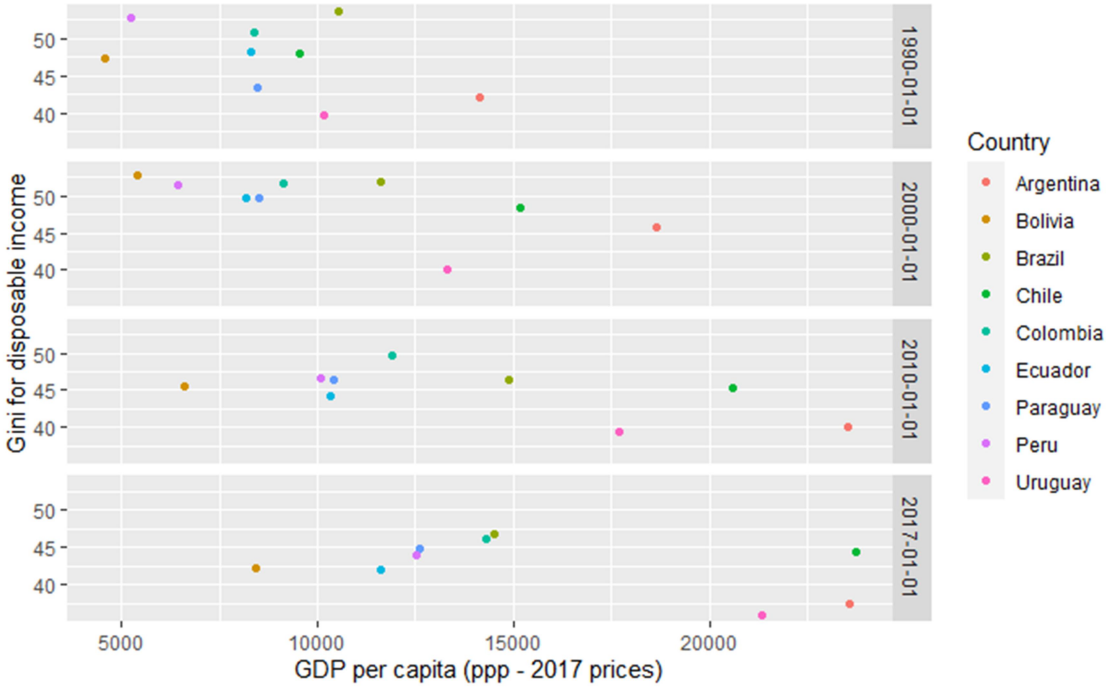
Note: In Y-axis, we consider 0.01 as 1%, for instance.

.**Fig. 14 – Impact of a 1% of GDP fiscal consolidation on income inequality, considering different samples dropping one country at a time (year 1, accumulated for year 8)**



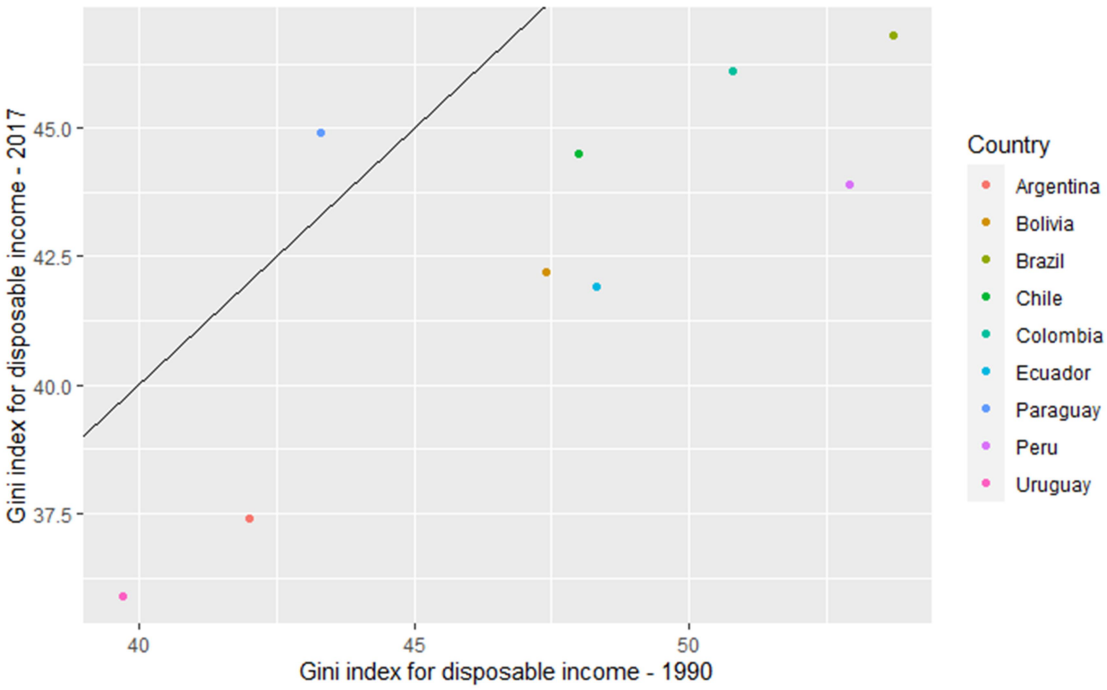
Note: The figure presents the estimated coefficients in each sample and one standard error bands around them.
The X-axis indicates which country we excluded in each sample: "arg" for Argentina; "bas" for baseline, with no exclusion; "bol" for Bolivia; "bra" for Brazil; "chi" for Chile; "col" for Colombia; "ecu" for Ecuador; "pry" for Paraguay; "per" for Peru; "ury" for Uruguay.
Because of unit root test issues, we utilized a sample from 1992 to 2017 for “bol”, “chi”, “col”, “pry”, and “per”. For the rest, we utilized a sample from 1991 to 2017.

Fig. 15 – Relationship between the Gini index for disposable income and GDP per capita (1990, 2000, 2010 and 2017)



Note: Gini index data extracted from SWIID 8.2. GDP per capita data extracted from WDI-World Bank.

Fig. 16 – Gini index for disposable income in 1990 and 2017



Note: Gini index data extracted from SWIID 8.2.