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Financial reforms and income inequality: evidence from developing countries

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Abstract: The current context of the COVID-19 pandemic has exposed the most vulnerable socio-economic groups to greater financial risk and thus could lead to exacerbating income inequality. The crisis creates an opportunity to demand further structural and systemic reforms for redistributive justice. Our paper explores the distributional consequences of financial liberalization reforms implemented over the past four decades in 64 emerging and low-income countries. Our identification strategy is based on a `doubly robust' estimation approach, and impulse responses are generated by the local projection method. Our results indicate significant distributional consequences for both domestic and external finance reforms. These results are robust to alternative specifications. The results favour countries with better institutional quality. Taking the business cycle into account shows that it would be more beneficial for developing countries to implement financial reforms when the economy is growing relatively slowly. Moreover, financial reforms are effective in reducing income inequality in periods when there is no financial crisis.

Key words: financial reforms, inequality, local projection method, business cycle

JEL classification: E61, D31, J48, C54

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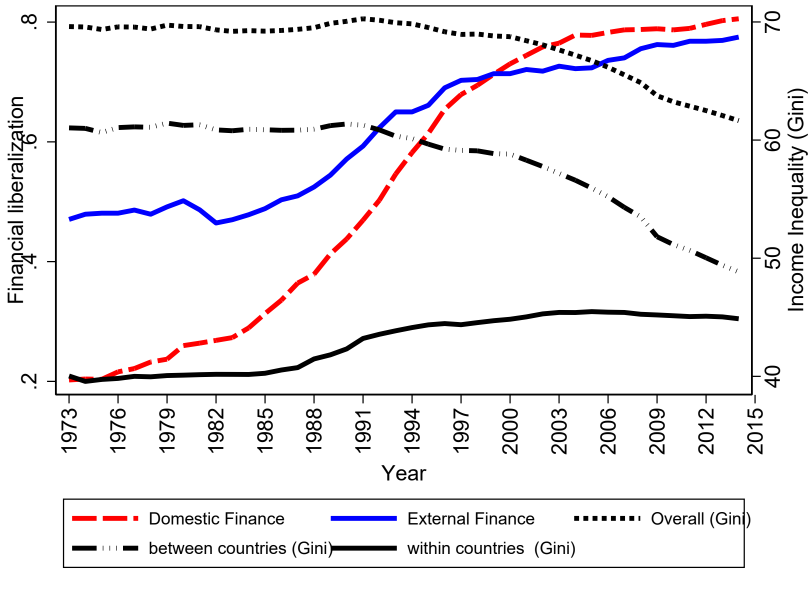
1 Introduction

Since the early 1990s, three out of four households in developing countries live in societies that have become more unequal (UNDP 2013).[1] In recent decades, academic and policy debates have increasingly focused on income inequality as a result of the global economic downturn following the global financial crisis. The persistence and emergence of inequality within countries (see Figure 1, solid black line) has prompted a number of leading economists to warn that the global market could collapse if the distribution of income within the world's countries becomes too unequal (Freistein and Mahlert 2016). This is why the UN's global development agenda through the Sustainable Development Goals (SDGs) has made tackling inequality one of its primary goals. Indeed, income inequality can reduce growth because it limits opportunities, skill development, and social and occupational mobility. Thus, reducing inequality has become an important concern not only for international institutions, but also for governments.

In addition to the persistence and emergence of inequality within countries, the past four decades have witnessed a notable surge in financial liberalization, encompassing both domestic and external markets. This has entailed a reduction of restrictions on domestic and cross-border financial transactions. Across all nations, there has been a significant upswing in market liberalization during the early 1990s, as depicted in Figure 1 (red and blue lines). The objective behind these reforms is to grant market forces a prominent role in constraining the actions of public policy-makers. However, numerous economic studies argue that these reforms can exacerbate inequality. In fact, individuals with more favourable living conditions tend to be the primary beneficiaries of competitive markets (Ostry et al. 2018).

Figure 1: Gini index and liberalization index over time, 1973–2014



Note: figure provides an illustration of the evolution of the mean Gini index (net) and an evolution of the mean index of financial reforms (domestic and external) over the period 1973–2014 for a set of 64 developing countries.

Source: author's elaboration based on the WIID Companion (UNU-WIDER 2021) and Alesina et al.'s (2020) datasets.

The link between reforms and income inequality has been the subject of scientific studies for many years. However, the theoretical and empirical results of these studies are mixed, leading to heated scientific and political debates between proponents and detractors on the distributional effects of reforms.

[1] According to KNBS and SID (2013), inequality is defined as the extent to which the distribution of economic welfare generated in an economy differs from that of equal shares among its inhabitants. As inequality is a multidimensional concept (inequality can mean a difference in access to basic services, opportunities, income, education, etc.), this study focuses on income inequality.

On the one hand, supporters of market-oriented reforms (aiming for smaller government size and scope) argue that such reforms contribute to a reduction in income inequality. In fact, market liberalization could potentially benefit high-income individuals and those with advanced skills. It enables them to invest in new industries, foster innovation, and create new businesses, thereby taking advantage of economic freedom and globalization. Moreover, business expansion and investments in new industries tend to generate more employment opportunities for low-skilled workers, resulting in increased income. Therefore, proponents of liberalization believe that when the incomes of low-skilled workers rise more significantly than those of high-skilled individuals, it can lead to a decrease in income inequality.

In contrast, critics of market-oriented reforms (those who call for a large size and scope of the state) argue that they generate income inequality. Indeed, the privatization of public enterprises, trade liberalization, deregulation of product, labour, and financial markets are a major source of income inequality. For these critics, economic freedom favours entrepreneurs at the expense of workers and employees. The increasing competition between firms and labour brought about by economic liberalization puts more pressure on low-skilled workers and employees but increases the profit of entrepreneurs. This competitive framework results in higher incomes for highly skilled citizens but lower or stagnant incomes for low-skilled citizens.

This study contributes to the empirical literature by examining the distributional consequences of financial reforms (domestic finance and external finance or capital account liberalization) for a set of 64 emerging and low-income countries. It should be noted that the study of the distributional consequences of reforms is a real Sisyphean task. Our study thus makes three main contributions. First, we provide strong empirical evidence by distinguishing the impact of domestic and external finance reforms on income inequality by focusing on developing countries and over a long period of time. Previous studies have focused on country case studies or in most cases on developed countries(Causa et al. 2016; Spatz and Steiner 2002; Larrain 2015). Moreover, previous studies have only focused on external financial reforms such as capital account liberalization. Second, reforms are not exogenous and are not randomized across countries. In order to address the problem of selection bias of reforms, the study combines the local projections method and the inverse probability weighted regression adjustment (LP-IPWRA) method à la Jordà and Taylor (2016). This so-called 'double Robust' approach allows to quantify the causal impact of reforms in contrast to previous studies. Third, the economic impacts of structural reforms could be influenced by the initial economic conditions present during their implementation. This study suggests conducting a set of analyses that consider factors such as the business cycle, institutional quality, access to financial markets, and the occurrence of financial crises.

We combine a reform dataset directly from Alesina et al. (2020) and the World Income Inequality Database (WIID) Companion (UNU-WIDER 2021).[2] As financial reforms are not random events, we focus on financial liberalization episodes. These reform liberalization episodes are identified by significant changes in the reform indices. we control for these reform episodes with a set of control variables using the covariate balancing propensity score algorithm (CBPS).

Our main results show that financial reforms contribute to a significant reduction in income inequality. While the impact of domestic financial reforms is immediate and persistent over time, the impact of external financial reforms occurs in the medium term (i.e. three years after the liberalization episode). Robustness analyses were carried out through the addition of control variables, alternative measures of income inequality, and the definition of liberalization episodes. These robustness analyses confirm the robustness of our results. By estimating income shares, our estimates reveal that financial reforms increase the income shares of citizens at the bottom of the income distribution and reduce those of citizens at the top of the income distribution. Indeed, financial liberalization reforms can provide the

[2] WIID Companion datasets, version 31 March 2021: https://www.wider.unu.edu/database/previous-versions-wiid.

poor with access to finance and thus access to schooling and health care and thus could be beneficial in the fight against poverty.

Taking into account the business cycle, our estimates show that it is beneficial for the countries studied to implement financial reforms during periods of economic recession. The basic results are explained by institutional quality. Indeed, while financial reforms reduce income inequality in countries with better institutional quality, they contribute to the increase in income inequality in countries characterized by low institutional quality. These results argue that developing countries could therefore reap distributional benefits from financial reforms by improving their governance systems. Furthermore, we control for the probability of financial crisis occurrence in our estimates. The results indicate that financial reforms help reduce income inequality in the absence of a financial crisis. However, during a financial crisis, domestic financial reforms lead to a medium-term increase in income inequality.

The rest of the study is organized as follows: Section 2 presents a brief review of the literature on the relationship between reforms and income inequality, Section 3 presents the data. Section 4 introduces the identification strategy, Section 5 shows the results, and Section 6 concludes.

2 Literature on reforms and inequality

We briefly discuss in this section some relevant theoretical and empirical studies that have analysed the distributional effects of financial reforms.

From a theoretical point of view, imperfections in financial markets constitute an obstacle to risk sharing. Thus, measures to reduce imperfections or relax credit constraints could facilitate access to financial markets for the poor and thus improve the egalitarian distribution of income (Becker and Tomes 1979; Galor and Zeira 1993; Banerjee and Newman 1993). Furceri and Loungani (2015) document the main channels through which financial reforms impact national income distribution by identifying three mechanisms. The first mechanism through which the effect of financial reforms on income inequality passes is their impact on risk sharing and consumption smoothing. Financial reforms can contribute to reducing income inequality in a market economy by ensuring that all citizens benefit from risk sharing and consumption smoothing. However, in economies where access to credit markets is limited to a few individuals, income inequality may worsen. In other words, the distributional effect of financial reforms depends on the quality of financial institutions. In economies with strong financial institutions, implementing financial reforms can help decrease income inequality by reducing volatility and smoothing consumption. On the other hand, in economies with exclusive financial institutions, the implementation of financial reforms may only provide credit access to financially healthy individuals, thereby contributing to inequality.

The second mechanism relates to the likelihood of a financial crisis following the implementation of financial reforms. During a financial crisis, high-income individuals are likely to experience a loss in income shares due to bankruptcies or a decline in their wealth. Additionally, as stated by de Haan and Sturm (2017), financial crises lead to economic recessions that directly impact low-income individuals through income declines.

The third mechanism involves the effect of financial reforms, such as opening up the capital account, on the income distribution by influencing the shares of labour and capital in the economy. In situations where labour institutions, such as labour bargaining power, are weak, external financial reforms tend to increase the share of capital at the expense of labour's share of income (Checchi and García-Peñalosa 2010). In other words, greater financial openness is likely to facilitate capital transfers and production relocation abroad.

There is a growing interest in the empirical literature on the impact of financial liberalization (internal and external) on income inequality. The empirical studies do not find a unanimous conclusion. Although the causal mechanism of financial reforms on income inequality is not well understood, some studies find that financial reforms increase income inequality (Bradley et al. 2003; Mahler 2004; Ang 2010; Alemán 2011; Asteriou et al. 2014; Furceri and Loungani 2015; Jaumotte and Buitron 2015; de Haan and Sturm 2017), others reach opposite results highlighting the inequality-reducing effect of financial reforms with a particular focus on the heterogeneous effects they can have (Beck et al. 2007; Abiad et al. 2008; Agnello et al. 2012; Li and Yu 2014; Delis et al. 2014). On the other hand, another part of the empirical studies focusing on the nonlinearity link find that the effect of reforms on inequality is conditioned by the economic and institutional environment (Lim and McNelis 2016; Cabral et al. 2016).

Improving the efficiency of the domestic financial system through capital account liberalization reforms can reduce credit market imperfections, thereby promoting equal access to credit services and potentially leading to lower income inequality (Beck et al. 2007; Abiad et al. 2008). In a study by Agnello et al. (2012), the authors examine the impact of financial reforms on income inequality in 62 countries from 1973 to 2005. Their findings suggest that eliminating directed credit policies and implementing less stringent reserve requirements, while also improving the real estate market, can contribute to reducing income inequality among the lower-income segments. However, an important limitation of their study is the omission of considering the level of development across different countries, which could provide insights into the specificities of banking regulations and their evolution as potential sources of income inequality.

To address these shortcomings, Delis et al. (2014) analyse how banking regulations affect income distribution in 81 countries over the period 1972–2005. Using an identification strategy based on double ordinary least squares and the GMM method, the authors find that financial sector liberalization contributes to the reduction of inequality (measured by the Theil index and the Gini coefficient). However, the authors find that there is heterogeneity in the effect obtained across different levels of economic and institutional development. Indeed, the authors find that the removal of barriers to entry as well as improved privatization laws yield distributional effects mainly in developed countries with strong institutions. In particular, the policies that contribute most to reducing income inequality include policies to abolish credit controls, control interest rates, and tighten banking supervision. In a theoretical model, Bumann and Lensink (2016) stipulate that financial reforms reduce the cost of capital, stimulate demand for loans while raising interest rates to attract savings deposits. Empirically, the authors find that financial reforms through capital account liberalization reduce income inequality if the level of financial depth is high.

It should be noted, however, that studies that state that financial reforms lead to a reduction in credit market imperfections in terms of improved access to credit do not take into account the fact that poor households are at greater risk. According to the work of Galor and Zeira (1993), an improvement in the quality and range of financial services through financial liberalization reforms actually improves the quality of financial services for the rich. Indeed, financial liberalization reforms do not tend to expand access to financial services for poor households, thus preventing them from financing their activities and creating wealth. Thus, for Claessens and Perotti (2007), improvements in the quality and range of financial services induce income inequality as these improvements benefit the elite rather than the poor. The authors conclude that financial liberalization reforms have distributional effects only when they are conditioned by supervisory institutions. Jaumotte and Buitron (2015) argue that institutional strength plays a crucial role in facilitating the distributional effects of financial reforms. In strong institutional settings, financial liberalization reforms can improve consumption smoothing and thus reduce volatility for the poor. Empirical studies by de Haan and Sturm (2017) show that the positive impact of financial liberalization on income inequality is not only a function of financial development but also of policy institutions. Thus, financial liberalization can only reduce income inequality in the presence of satisfactory redistributive institutions (Mandel 2010).

3 Data and stylized facts

Measuring reform efforts in a country and for a given year is a challenging task. Thus, financial reform indicators used in this study are derived from those constructed by Alesina et al. (2020). Financial reforms encompass both domestic and external financial liberalization. Domestic finance reforms cover six dimensions of financial regulation: credit control, interest rate controls, barriers to entry into banks, banking supervision, privatization, and security market development. The aggregate index on domestic finance reforms is a composite of these six sub-indicators. As for external finance, the reforms cover restrictions on capital outflows and restrictions on capital inflows. Thus, external finance reform is a composite of an index on capital outflow restrictions and an index on capital inflow restrictions.

To measure income inequality, we use data on the Gini (net) coefficient from the classic database for cross-national inequality analysis—the World Income Inequality Database (WIID) Companion datasets (UNU-WIDER 2021)—built by the United Nations University World Institute for Development Economics Research (UNU-WIDER). This database provides comparable estimates of net income inequality for 200 countries for as many years as possible from 1950 to 2019. Theoretically, the Gini coefficient is between 0 and 100, where 0 corresponds to a situation where each reference unit receives an equal share of income and 100 when one unit receives all income.

Data on control variables are obtained from various data sources, including World Bank World Development Indicators (WDI) and International Monetary Fund data (see full list in Appendix Table A12).

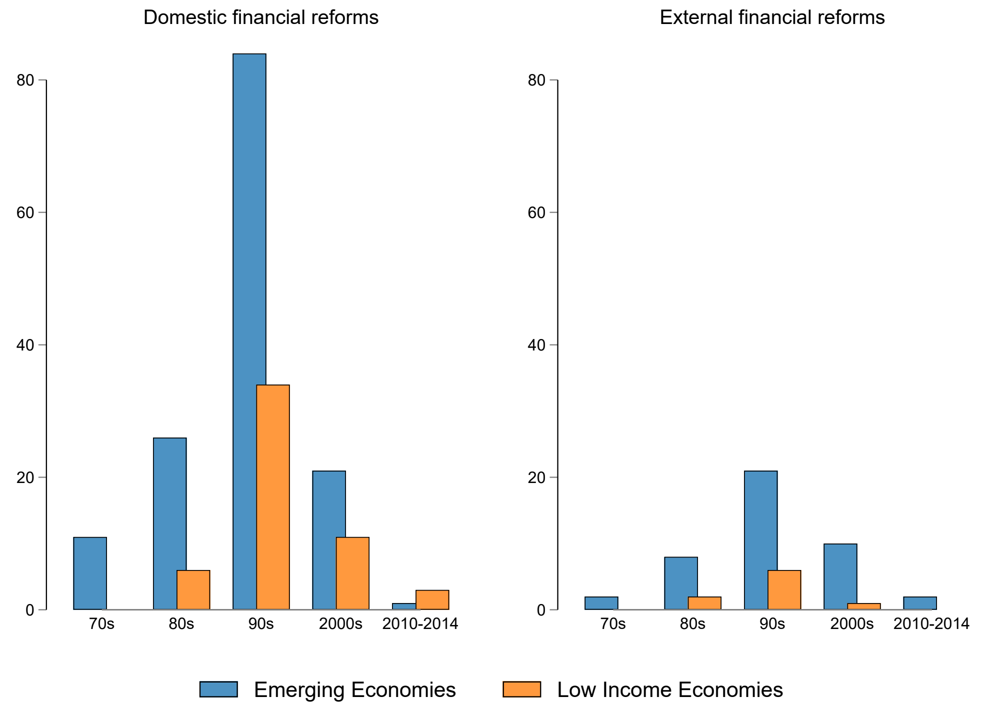
For reasons of endogeneity related to financial reforms, our analysis is based on financial reform shocks or episodes. The occurrence of reforms is not the result of a random process. Reform variables suffer from 'selection on observables' problems. Indeed, economic and political conditions may be at the origin of the reforms' initiation. We focus on reform episodes identified by significant changes in the reform indices.

To identify liberalization or reform episodes, it would be sufficient to have the year in which a liberalization decision was taken or information on the dates of policy decrees or legislative changes, but such information is difficult to obtain. We use the approach widely used in the literature on structural reforms to identify reform episodes (Bernal-Verdugo et al. 2013; Furceri and Loungani 2015; Dabla-Norris et al. 2016; de Haan and Wiese 2022). This literature identifies the episodes of liberalization when a given country experiences a variation of two standard deviations and an average variation of the reform index. Alesina et al. (2020) use the same definition to identify liberalization reform episodes in their database. Thus, we identify episodes of financial liberalization if, for a given country in a given year, the annual change in financial reforms exceeds two standard deviations. Thus, the financial reform indicator (domestic and external) is a dummy variable taking the value 1 if change in reform index is more than two standard deviations over the full sample and 0 otherwise.[3] This definition of reform episodes captures the relatively large variations in the domestic or external financial reform indicator.

[3] We perform a robustness analysis using an alternative definition used by Alesina et al. (2020) to identify reform episodes, where an episode is considered when the absolute annual changes in the reform indexes exceed the 75th percentile over the full sample.

Through this definition of reform episodes, we have identified 197 episodes of domestic finance reform and 53 episodes of external finance reform for the 64 developing countries.[4] Figure 2 presents the episodes by time period and by income level. It shows that for emerging and low-income countries, most financial reform episodes occurred in the 1990s.[5] In particular, the graphical analysis reveals that the occurrence of domestic financial liberalization reforms is greater than that of external financial liberalization reforms, and that emerging countries have more episodes of financial liberalization reforms than their counterparts in low-income developing counties.

Figure 2: Number of financial reform episodes



Source: author's illustration based on study data.

4 Estimation approach

In this section, we present the methodology used to assess the dynamic distributional effects of financial reforms in developing countries. In a first part, we describe the local projections approach and highlight its advantages in answering the research question. Then, we discuss the endogeneity problems of reforms and the solutions proposed to address them.

4.1 Local projections

We follow the methodology of Jordà (2005) and its recent applications in macroeconomic studies by estimating impulse response functions (Jordà and Taylor 2016; Furceri et al. 2021). Indeed, local pro-

[4] See Table A1 in the Appendix for details by time period and income level. We also show, in Appendix Figure A1, the years in which each country experienced reform episodes.

[5] This is consistent with the description of reform episodes when Alesina et al. (2020) use a definition of an episode whose financial reform index exceeds by two standard deviations the average annual change over all observations or others using the Kaopen indicator.

jections have increasingly become a widespread alternative econometric approach to structural vector autoregression (SVAR) methods.

Our approach is to estimate the following equation:

$$y_{i,t+h} - y_{i,t} = \alpha_i^h + \gamma_t^h + \beta^h SR_{r,i,t} + \theta^h X_{i,t} + {}_{i,t+h} \tag{1}$$

where h=*1. . . ,5* is the forecast horizon, $y_{i,t+h} - y_{i,t}$ denotes the cumulative Gini index over the forecast horizon. $SR_{r,i,t}$ is a dummy reform variable, the symbol r refers to the type of reform (domestic and external financial reforms), while i refers to the specific country, and t refers to the year. $X_{i,t}$ is a vector of control variables including the logarithm of lagged initial *GDP* per capita, lagged of dependent variable, and lagged output gap. The output gap is calculated using the Hamilton (2018) method based on the logarithm of GDP per capita.[6]

According to Jordà (2005), local projection methods have several advantages:[7] they are robust to model specification errors, they adapt more easily to non-linearities, they do not suffer from dimensionality problems (several control variables can be included), and they can be estimated by simple regression techniques. Regressing the dependent variable at different horizons on the same set of control variables is likely to lead to autocorrelation of the residuals, which could bias the estimated standard errors. In order to have robust standard deviations taking into account the possibility of serial correlation within and between the different equations, the statistical inference is based on the approach developed by Ramey and Zubairy (2018), Tenreyro and Thwaites (2016), and El Herradi and Leroy (2019). Indeed, these authors propose to estimate apparently unrelated equations by following the approach of Driscoll and Kraay (1998). In doing so, the standard deviations take into account the autocorrelation between the time and the time horizons defined.[8]

Equation 1 does not take into account the fact that reform may be implemented in a given country or at a given time period because of the expected benefits compared to the status quo. To solve the selection bias problem and to have an unbiased estimate, we combine the local projection method with the inverse probability weighted regression (IPWRA)[9] fitted estimator as described in Jordà and Taylor (2016) in order to orthogonalize the treatment with respect to the potential outcomes and estimate the dynamic impulse responses. In our analysis, we initially employ a binary model to estimate the likelihood of a country implementing reform during a specific period. This framework, based on latent variables, enables us to consider the fact that reforms are typically introduced during periods when the expected benefits of such reforms are substantial. In a second step, we use the local projections method while weighting the observations inversely to the estimated propensity score.

[6] Empirical evidence often employs the Hodrick and Prescott (1997) filter, which necessitates selecting a parameter value, a decision that warrants discussion. In our approach, we adopt the alternative method proposed by Hamilton (2018), which eliminates the need for parameter selection and avoids conditioning the results on the chosen parameter value (Gomado 2022).

[7] Local projection methods also have some drawbacks in terms of efficiency.

[8] For the choice of the number of lags, we define a maximum autocorrelation lag of $h+1$ according to the economic literature on the use of local projection methods.

[9] There is also the augmented inverse propensity weighted (AIPW) estimator. Indeed, the AIPW estimator adds a bias correction term to the IPW estimator. Thus, if the treatment model is correctly specified, the bias correction term is 0 and the model is reduced to the IPW estimator. If the treatment model is mis-specified but the outcome model is correctly specified, the bias correction term corrects the estimator. In fact, the bias correction term gives the AIPW estimator the same dual robustness property as the IPWRA estimator.

4.2 Inverse probability weighting estimator

A method commonly used in empirical studies, particularly in microeconomics, to correct the problem of 'selection on observables' is the adjusted estimator by inverse probability weighted regression. While this method is very widespread in microeconomic studies and particularly in the field of health, it should be noted that its application in macroeconomic studies is recent.

The method relies on two primary steps. Firstly, we estimate a binary model, specifically a propensity score model, to determine the probability of a country implementing reforms. This model utilizes a set of variables derived from the existing literature on the determinants of structural reforms.

The selection equation is as follows:

$$D^*_{i,t+1} = \mu + \gamma Z_{i,t} + \theta_{i,t} \tag{2}$$

where $D^*_{i,t+1}$ is the underlying continuous latent variable for the observed treatment variable, $D_{i,t+1}$ is the probability of occurrence of reform episodes. $Z_{i,t}$ is the vector of explanatory variables for reform episodes. The observed variable, $D_{i,t+1}$ is reform achievement when the latent variable is positive:

$$D_{i,t+1} = \begin{cases} 1 & if D^*_{i,t+1} > 0 \\ 0 & \text{otherwise.} \end{cases} \tag{3}$$

Secondly, we proceed with a re-randomization of our sample using an inverse weighting method based on the propensity score. This method reduces the importance of countries that were likely to experience a significant change in the reform indicator, while giving more weight to those that were less likely to experience such a change, based on their observable characteristics. As a result, it allows for comparability between countries that have undergone a significant change in the reform indicator and those that have not, as they would have had an equal probability of being subjected to the treatment if it had not actually been implemented.

More explicitly, let $P(D_{i,t} = d_j | Z_{i,t}) = p^j(Z_{i,t})$ for $j = 1,0$ denote the propensity score for country i in period t with treatment j. Therefore, we have $p^1(Z_{i,t}) = 1 - p^0(Z_{i,t})$.

$$\hat{\theta}^h = \frac{1}{T} \sum_{t=h+1}^{T} \frac{(y_{i,t+h} - y_{i,t})\mathbb{1}(D_{i,t} = 1)}{\hat{p}^1(Z_{i,t})} - \frac{1}{T} \sum_{t=h+1}^{T} \frac{(y_{i,t+h} - y_{i,t})\mathbb{1}(D_{i,t} = 0)}{\hat{p}^0(Z_{i,t})} \tag{4}$$

$\mathbb{1}(.)$ is an indicator function for the treatment variable, $D_{i,t}$, $\hat{p}^1(Z_{i,t})$, and $\hat{p}^0(Z_{i,t})$ are the estimated probabilities of treatment, $D_{i,t}$, from the binary model as described in Equation 2.

According to Equation 4, the first term on the right-hand side denotes the average effect on the outcome variable $(y_{i,t+h} - y_{i,t})$, h periods after the treatment intervention that takes place in period t weighted by the inverse of the estimated probability of treatment conditional on the country i receiving treatment in period t. The second term on the right gives the average effect on the outcome variable $(y_{i,t+h} - y_{i,t})$, h periods after the intervention of the treatment that takes place in period t weighted by the inverse of the estimated treatment probability conditional on country i receiving no treatment in period t.

The estimator proposed by Jordà and Taylor (2016) belongs to the class of 'doubly robust' estimators. While the regression adjustment method models the outcome variable to take into account the non-random assignment of the treatment, the IPW estimate models the treatment variable. Thus, the treatment effect estimate is consistent if either the treatment model or the outcome model (not both) are mis-specified (Imbens 2004; Lunceford and Davidian 2004; Wooldridge 2007, 2010).

5 Regression results and analysis

5.1 Some parsimonious regression models

We start with a parsimonious regression analysis as a benchmark for comparison.[10] We utilize the model specified in Equation 1 and estimate the effect of discrete reform episodes on income inequality using a simple ordinary least squares (OLS) estimator. Table 1 presents the impact of financial liberalization episodes on income inequality (Gini net). The effect is estimated over five time horizons through columns [*Year 1*] to [*Year 5*].

Both financial reforms have an effect on reducing income inequality in the medium term. Indeed, for both financial reforms, the effect on the Gini coefficient is negative but not significant in the short run. After four years (in the medium term) of liberalization episodes in domestic and external finance, income inequality is significantly reduced by 0.42 percentage points and 0.69 percentage points, respectively.

Table 1: Effect of financial reform episodes on Gini, ols-estimates

	Dependent: $y_{t+h} - y_t$ (Gini net)				
	Year 1	Year 2	Year 3	Year 4	Year 5
Domestic finance	0.05	-0.01	-0.08	-0.42**	-0.25**
	(0.10)	(0.12)	(0.09)	(0.18)	(0.11)
Observations	1352	1293	1233	1173	1113
External finance	-0.44	-0.28	-0.37	-0.69**	-0.67**
	(0.34)	(0.33)	(0.27)	(0.26)	(0.26)
Observations	1353	1294	1234	1174	1114

Note: Driscoll-Kraay standard errors in parentheses. *** $p \leq 0.01$, ** $p \leq 0.05$, * $p \leq 0.1$. Additional controls: 1 lag of dependent variable; 1 lag of logarithm of GDP per capita (constant 2005 US$); country and year fixed effects are also included.

Source: author's calculation based on study data.

The results of this section, although interesting, should be taken with great caution. Indeed, the specifications do not take into account the fact that reforms could be implemented in countries (years) where the expected benefits are high. It is therefore of crucial importance to take into account the initial conditions of the countries in order to limit the selection bias problem and have unbiased estimates. In the following section, we therefore use a quasi-experimental approach to study the causal effect of financial reforms on income inequality.

5.2 Estimating average treatment effects

The first step in our identification strategy is to estimate propensity scores for each reform episodes. To have a perfect balance of covariates across countries, we use the covariate balancing propensity score (CBPS) model to estimate the propensity score.[11] The estimated model follows Jordà and Taylor's (2016) and de Haan and Wiese's (2022) approach in estimating a saturated propensity score model.

The choice of explanatory variables for the adoption of financial reforms is based on the economic literature. First, we include the reforming character of neighbouring countries as a control variable. Indeed, when a country is close to reforming neighbours or to neighbouring countries with which it

[10] This strategy is possibly not properly identified but could serve as a stepping stone to a more robust identification strategy.

[11] It is a machine learning programme that has many advantages over traditional binary models. For more details, see Imai and Ratkovic (2014).

has trade relations, it could be led to adopt reforms either by imitation or by peer pressure (reform neighbourhood effect). In other words, geographic proximity could facilitate the transmission of reforms between countries. We also include the unemployment rate, the business cycle, and the inflation rate. These variables may signify periods of crisis or difficult economic conditions. Reforms are more likely to be implemented after a new government takes office (Haggard and Webb 2018). To account for this, we included in the score model a variable capturing executive or legislative elections. This variable could be related to the reforms studied. For example, Fiori et al. (2012) find that labour market reforms and product reforms are related. Thus, we include in the score model, financial, trade, labour reforms as a predictor of product market reforms, and vice versa. We also take into account the structural conditionality of IMF-supported programmes because previous studies have argued that these conditions could stimulate reforms (Ghosh et al. 2005).

As shown in Table A7 and Table A8 in the Appendix, there are significant differences in macroeconomic and institutional conditions between countries with and without a reform shock in the unweighted sample (Panel A). To ensure a balance between the covariates, we weighted them using the propensity score. As shown in the same tables in Panel B, weighting the covariates by the propensity score perfectly eliminated the differences in covariates between the treatment and control groups.[12] The models estimated for each category of reforms are of significant predictive quality. Indeed, the area under the receiver operating characteristic (ROC) curve (AUC) is greater than 0.65 in all the CBPS models reported (see Figure A3 in the Appendix). In other words, the area under the ROC curve is statistically significant and different from 0.5 confirming a good classification of countries into treated and untreated groups based on their macroeconomic and political characteristics. To verify the overlap of the distributions, we present through Figure A4, smooth kernel densities of the propensity score distribution for the treated and control countries. The different figures clearly show that we have considerable overlap between the distributions for the treated country groups and the control country groups.

After studying the properties of the treatment model, the weighting strategy used allows us to mimic a situation where financial reform episodes are random and allows us to correctly identify average treatment effects (ATEs) using a quasi-experimental estimation strategy.

Main results: IPWRA

Table 2 presents the IPWRA estimation results for the two financial reform indicators. A time horizon of five years is considered for the local projection (LP) impulse response (column [*Year* 1] to [*Year* 5]). Domestic finance reforms have a negative and statistically significant effect on the Gini index. The estimated effect is immediate and persistent over time. Indeed, after the first and fifth year of the domestic finance liberalization episode, income inequality is reduced by 0.11 and 0.72 percentage points, respectively. As for the external finance reforms (capital account liberalization), the income inequality-reducing effect appears after the third year of liberalization episodes. Indeed, in the short run, episodes of capital account liberalization have a non-significant positive effect on income inequality. However, in the medium term, external finance reforms reduce income inequality. Income inequality is reduced by 0.18 and 0.78 percentage points in the third and fifth years, respectively.

These results are similar to those found in Table 1 and confirm the importance of the identification strategy adopted to take into account the non-random nature of financial reforms. Our identification strategy adopting a quasi-experimental and dynamic approach argues that episodes of financial liberalization contribute to the reduction of income inequality.

[12] It should be noted that it is very rare to have perfect elimination of differences in covariates through traditional models such as probit and logit.

Table 2: Benchmark estimates: average treatment effect of reforms, IPWRA estimates

Dependent: $y_{t+h} - y_t$ (Gini net)					
	Year 1	Year 2	Year 3	Year 4	Year 5
ATE_{IPWRA}: Domestic finance	-0.11*	-0.56***	-0.68***	-0.75***	-0.72***
	(0.06)	(0.11)	(0.18)	(0.14)	(0.15)
Observations	600	600	600	570	543
ATE_{IPWRA}: External finance	0.01	0.26	-0.18**	-0.61***	-0.78***
	(0.11)	(0.15)	(0.08)	(0.15)	(0.24)
Observations	600	600	600	570	543

Note: Driscoll-Kraay standard errors in parentheses. *** p≤0.01, ** p≤0.05, * p≤0.1. **Additional controls**: 1 lag of dependent variable; 1 lag of logarithm of GDP per capita (constant 2005 US$); 1 lag of crisis dummy; GDP per capita growth (annual %); country and year fixed effects are also included. **Propensity score** is based on the covariate balancing propensity score (CBPS) model as described in the text and includes: GDP growth (annual %); Unemployment (% of total labour force); IMF programme; inflation; logarithm of GDP per capita (constant 2005 US$); legislative/executive election dummy; age dependency ratio; Output gap using the filter of Hamilton (2018); Reform gap (defined as the difference between the level of liberalization in particular country and the reform level achieved in a country near the reform 'frontier', as United States) and reformist neighbours (proxied here by the weighted average of all other countries' liberalization indices. The weigths are proportional to the inverse of their distance to the country under consideration). The two last variables' definitions come from Ostry et al. (2009). All variables are in 1 lag. IPWRA estimates do not impose restrictions on the weights of the propensity score.

Source: author's calculation based on study data.

5.3 Robustness checks

In the previous section, our estimates reveal that financial reforms, notably domestic financial reforms as well as external financial reforms, reduce income inequality. In this section we conduct a series of analytical exercises to test the robustness of our results.

Omitted variables, alternatives measures of inequality, and reforms episodes

First, income inequality may be affected by other variables and the failure to account for these variables could cause our results to suffer from an omitted variable bias. To limit the omitted variable bias, we build on the literature by including additional control variables: the human capital index to capture the role of education, foreign direct investment, unemployment rate, institutional quality. The results are presented in Appendix Table A3. We find that our results are robust to the addition of other control variables. Indeed, the distributional effects of financial reform estimated through the basic equation are not sensitive to the inclusion of other macroeconomic and institutional variables, confirming the robustness of our basic results.

In addition, it is possible that key policy variables may still be omitted. Indeed, other structural reform variables besides financial reforms may affect income inequality. Possible candidate reform variables are labour market reforms, tax reforms, product market reforms, and trade reforms. Since we do not have data on tax reform variables, we control for the effect of financial reforms on income inequality by introducing variables such as trade, product market, and labour market reforms into the base model.[13] The results are presented in the Appendix Table A4 and further prove that our baseline results are robust to the inclusion of other structural reform variables.

Second, to test the robustness of the results to the dependent variable, we use alternative measures of income inequality. We replace the net Gini index by S80/S20 ratio (i.e ratio of the income share between

[13] These reform variables are added as continuous variables and not as dummy variables. However, by including them as a dummy variable according to the definition of the reform episode, the results do not change and remain consistent with the baseline results.

the top 20% and bottom 20% income groups) and the Theil index.[14] Table A5 presents the results for the alternative measures of income inequality. The results are consistent with the baseline results. Indeed, financial reforms reduce income inequality between the richest and poorest 20 per cent. While the reduction in the gap is immediate for external finance reform shocks, it occurs from the fifth year onwards for domestic finance reforms. Using the Theil index as an alternative measure of income inequality, our estimates are consistent with the baseline estimates. Domestic finance reforms reduce income inequality with an immediate and persistent effect over time. As for external finance reform episodes, we note that they reduce income inequality after the third year of the episodes' occurrence.

Third, we analyse the effect of the financial reforms on different segments of the income distribution. To examine how financial reforms affect the income share, we extend our basic model by estimating the following equation:

$$\Phi_{s,i,t+h} - \Phi_{s,i,t} = \alpha_i^h + \gamma_t^h + \beta^h SR_{r,i,t} + \theta^h X_{i,t} + {}_{i,t+h} \tag{5}$$

where $\Phi_{s,i,t+h}$ denotes the income share of the s^{th} income percentile in country i at year t. In other words, we replace the dependent variable with income share of the bottom 5%, bottom 20%, bottom 40%, top 5%, top 10%, and top 20%.

Table 3 presents the results for the different income segments. In Panel [A], we present the effect of domestic financial reforms on income shares while in Panel [B], we present the effect of external finance reforms on income shares. The results show that domestic and external finance reforms increase the income share of citizens at the bottom of the income distribution (bottom 5% to bottom 40%) and reduce the income share of citizens at the top of the income distribution (top 5% to top 20%). To sum up, these results imply that in developing countries, financial reforms are associated with a reduction in the income gap between rich and poor.

Finally, we proceed to an alternative definition of financial reform episodes. We extend the threshold for identifying reform episodes by following the work of Larrain and Stumpner (2017) and David et al. (2020). The countries in our analysis sample are not major reformers compared to developed countries, and the use of two standard deviations is relatively more stringent and will miss small reforms in developing countries. Thus, We identify episodes of financial liberalization if, for a given country at a given time, the annual variation of financial reforms exceeds one standard deviation. Thus, this new definition allows us to capture small changes in the reform indicator and to see the effects on income inequality. The results of this alternative definition of reform episodes are presented in Appendix Table A6 and the estimated effects are consistent with the baseline estimates confirming the robustness of the baseline results.

[14] Like the Gini index, the lower the Theil index, the lower the income inequality.

Table 3: Income share: bottom and top percentile

	Panel [A]: Domestic finance					**Panel [B]**: External finance				
	Dependent variable: Income share					Dependent variable: Income share				
	Year 1	Year 2	Year 3	Year 4	Year 5	Year 1	Year 2	Year 3	Year 4	Year 5
Bottom 5	0.00	0.01**	0.03***	0.03***	0.02*	0.01**	-0.00	-0.01	0.01	0.04***
	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
Bottom 20	-0.00	0.06**	0.17***	0.19***	0.16***	0.06***	-0.02	-0.03	0.05	0.14***
	(0.02)	(0.02)	(0.03)	(0.04)	(0.05)	(0.01)	(0.02)	(0.03)	(0.04)	(0.05)
Bottom 40	0.02	0.16***	0.35***	0.39***	0.34***	0.12***	-0.07	-0.05	0.14*	0.32***
	(0.03)	(0.04)	(0.06)	(0.06)	(0.08)	(0.02)	(0.04)	(0.06)	(0.08)	(0.10)
Top 5	-0.16***	-0.62***	-0.42*	-0.42***	-0.41***	0.92***	0.99***	0.14	-0.22**	0.09
	(0.05)	(0.14)	(0.21)	(0.13)	(0.11)	(0.27)	(0.28)	(0.12)	(0.10)	(0.25)
Top 10	-0.18***	-0.71***	-0.53**	-0.54***	-0.54***	0.46**	0.60**	-0.10	-0.46***	-0.40*
	(0.06)	(0.16)	(0.21)	(0.14)	(0.11)	(0.18)	(0.21)	(0.08)	(0.09)	(0.20)
Top 20	-0.03	-0.17***	-0.30***	-0.36***	-0.36***	-0.76***	-0.59***	-0.40***	-0.47***	-0.84***
	(0.02)	(0.04)	(0.04)	(0.04)	(0.04)	(0.15)	(0.13)	(0.09)	(0.06)	(0.09)
Observations	579	579	579	551	526	579	579	579	551	526

Note: Driscoll-Kraay standard errors in parentheses. *** p≤0.01, ** p≤0.05, * p≤0.1. **Additional controls**: 1 lag of dependent variable; 1 lag of logarithm of GDP per capita (constant 2005 US$); 1 lag of crisis dummy; GDP per capita growth (annual %); country and year fixed effects are also included. **Propensity score** is based on the covariate balancing propensity score (CBPS) model as described in the text and includes: GDP growth (annual %); Unemployment (% of total labour force); IMF programme; inflation; logarithm of GDP per capita (constant 2005 US$); legislative/executive election dummy; age dependency ratio; Output gap using the filter of Hamilton (2018); Reform gap (defined as the difference between the level of liberalization in particular country and the reform level achieved in a country near the reform 'frontier'; as United States) and reformist neighbours (proxied here by the weighted average of all other countries' liberalization indices. The weigths are proportional to the inverse of their distance to the country under consideration). The two last variables' definitions come from Ostry et al. (2009). All variables are in 1 lag. IPWRA estimates do not impose restrictions on the weights of the propensity score.

Source: author's calculation based on study data.

5.4 The role of initial conditions

The distributional effect of financial reforms may depend on initial conditions. Indeed, the impact of reforms may be different depending on the state of the business cycle or on macroeconomic and institutional conditions. We study the interactions between financial reform episodes and conditions such as the business cycle, institutional quality, financial crises, and the quality of financial institutions. To do so, we take advantage of the local projection method which allows flexibility in dealing with non-linearities and state dependence (Jordà and Taylor 2016). We extend the basic Equation 1 to obtain the typical state-dependent specification according to the following equation:

$$y_{i,t+h} - y_{i,t} = \alpha_i^h + \gamma_t^h + \beta_m^h S_{i,t} * SR_{r,i,t} + \beta_n^h (1 - S_{i,t}) SR_{r,i,t} + \theta^h X_{i,t} + {}_{i,t+h} \tag{6}$$

where $S_{i,t}$ is a dummy variable capturing the state of the business cycle, institutional quality, financial crises, and financial market access. Thus, the results in the following subsections are based on Equation 6.

Do business cycle conditions matter?

The results of the previous section may be influenced by the economic conditions prevailing at the time of the reforms' implementation. Indeed, our sample encompasses very different economic regimes and these regimes could affect the results differently. We examine whether our baseline results change depending on the economic conditions prevailing at the time the reforms are implemented. We identify business cycle episodes based on the Hamilton (2018) filter (see Section 4.1). These episodes take the value of 1 in case of an economic expansion and 0 in case of an economic recession.

The results are presented in Table 4 . Both domestic and external financial reforms have a significant negative effect on the Gini index during economic recessions. The impact is immediate and remains significant over the entire time horizon. Indeed, in periods of economic slowdown, developing countries are increasingly faced with problems of domestic financing of growth. A reduction in the number of restrictions on cross-border financial transactions by these countries could allow for an inflow of capital in terms of foreign direct investment (FDI). There is empirical evidence that FDI contributes to reducing income inequality in developing countries (Nguyen 2021). Moreover, in times of low growth, foreign bank lending is likely to be associated with greater financial access for the poor and thus reduce income inequality (IMF 2008). Overall, financial reforms implemented during weak economic cycles can strengthen economic stability by reducing systemic risks and preventing financial crises, improve access to financing by developing a more inclusive financial environment, promote job creation by stimulating investment and economic activity, and strengthen social safety nets by guaranteeing adequate funding for social programs such as unemployment benefits and healthcare. Therefore, these factors contribute to reducing income inequality by providing opportunities for income growth and improving the well-being of individuals and communities that are more vulnerable during economic slowdowns.

Table 4: Impact of business cycles on the nexus between financial liberalization and income inequality

	Dependent variable: Gini net				
	Year 1	Year 2	Year 3	Year 4	Year 5
	Domestic finance				
ATE_{IPWRA}: boom	-0.21	-0.43	0.48	0.44	0.32
	(0.27)	(0.45)	(0.48)	(0.33)	(0.44)
ATE_{IPWRA}: slump	-0.58***	-1.38***	-1.41***	-2.60***	-2.12***
	(0.11)	(0.16)	(0.31)	(0.19)	(0.19)
Observations	579	579	579	551	526
	External finance				
ATE_{IPWRA}: boom	-0.43	0.20	0.41	0.76	0.90
	(0.32)	(0.57)	(0.89)	(1.05)	(1.34)
ATE_{IPWRA}: slump	-0.70***	-1.08*	-0.94**	-1.02***	-1.38***
	(0.18)	(0.54)	(0.37)	(0.33)	(0.16)
Observations	579	579	579	551	526

Note: Driscoll-Kraay standard errors in parentheses. *** p≤0.01, ** p≤0.05, * p≤0.1. **Additional controls**: 1 lag of dependent variable; 1 lag of logarithm of GDP per capita (constant 2005 US$); 1 lag of crisis dummy; GDP per capita growth (annual %); country and year fixed effects are also included. **Propensity score** is based on the covariate balancing propensity score (CBPS) model as described in the text and includes: GDP growth (annual %); Unemployment (% of total labour force); IMF programme; inflation; logarithm of GDP per capita (constant 2005 US$); legislative/executive election dummy; age dependency ratio; Output gap using the filter of Hamilton (2018); Reform gap (defined as the difference between the level of liberalization in particular country and the reform level achieved in a country near the reform 'frontier', as United States) and reformist neighbours (proxied here by the weighted average of all other countries' liberalization indices. The weigths are proportional to the inverse of their distance to the country under consideration). The two last variables' definitions come from Ostry et al. (2009). All variables are in 1 lag. IPWRA estimates do not impose restrictions on the weights of the propensity score.

Source: author's calculation based on study data.

The role of institutional quality

The basic results show that both financial reforms reduce income inequality. The countries in the analysis sample have different institutional levels. Can differences in institutional quality level explain the results? Indeed, countries with strong institutional quality tend to have higher penetration of financial services and better governance environments for the adoption of more inclusive policies. To control for institutional quality, we approximate institutional quality by International Country Risk Guide (ICRG) Indicator of Quality of Government.[15] We classify the sample of countries into two groups: higher quality of government and lower quality of government. A country is classified in the higher quality of government (lower quality of government) group if it has an average ICRG Indicator of Quality of

[15] See https://www.gu.se/en/quality-government/qog-data/data-downloads/basic-dataset.

Government higher (lower) than the median ICRG Indicator of Quality of Government of the whole sample.

Results are presented in Appendix Table A9. The effects of financial reform episodes are conditioned by institutional quality. Indeed, for countries with better institutional quality, the implementation of financial reforms contributes to the reduction of income inequality. The estimated effect is significant for both domestic financial reforms and capital account liberalization reforms. For example, in countries with better institutional quality, income inequality is reduced by 0.29 (1.35) percentage points in the first year after the domestic (external) financial liberalization episode. It is worth noting that in countries with low institutional quality, domestic finance reforms indeed increase income inequality in the short term (the first two years), but reduce it in the medium term (starting from the third year). However, for countries with low institutional quality, episodes of capital account liberalization have a statistically significant positive effect from the first year to the fifth year. Five years after the capital account liberalization, countries with low institutional quality experience an increase in income inequality by 1.88 percentage points. This result can be explained by the fact that in countries with weak institutional quality, external financial liberalization would rather reinforce privileged access to financial resources for the upper classes rather than the lower classes, leading to an increase in income inequality. This result does not imply that countries with weak institutions have no interest in implementing external financial reforms, but rather serves as a signal for these countries to develop their political institutions in order to benefit from the income inequality-reducing effects of financial reforms.

Financial crisis

As mentioned previously, this section aims to examine whether the presence of financial crises influences the connection between financial reforms and income inequality. Furthermore, one of the mechanisms discussed earlier suggests that the implementation of financial reforms, which can lead to rising income inequality, may also raise the likelihood of financial crises occurring. (LaGarda et al. 2016; Furceri and Loungani 2018).

To empirically test our hypotheses while taking into account the occurrence of financial crises, we utilize the crisis data provided by Laeven and Valencia (2020). This dataset includes information on various types of crises such as banking, inflationary, currency, and debt crises.

The impact of financial reforms on income distribution, taking into consideration the occurrence of financial crises, is presented in Appendix Table A10. Regarding domestic financial reforms, our findings indicate that income inequality decreases starting from the second year after reform episodes in periods without financial crises. However, in periods marked by financial crises, the implementation of domestic financial reforms contributes to an increase in income inequality in the medium term (fourth year after the occurrence of the liberalization episode).

Regarding external finance reforms, the reduction in inequality resulting from capital account liberalization is also observed in periods without financial crises. However, in periods characterized by financial crises, capital account liberalization leads to an increase in income inequality in the short run (one year after the reform episode).

These results can be attributed to the fact that financial crises generate economic recessions, which directly impact low-income individuals by reducing their income and consequently leading to an increase in income inequality.

5.5 Country case studies: synthetic counterfactuals

We present additional analyses to test the robustness of the previous results by analysing the impact of financial liberalization episodes in specific countries. To do so, we use the synthetic control method, which is a data-driven approach to construct a relevant counterfactual (Abadie and Gardeazabal 2003; Abadie et al. 2010, 2015). Following the work of Campos and Kinoshita (2010), our goal in using this method is to see the level of income inequality (or Gini index) of a specific country if it had implemented financial reforms in the same way as a selected group of other countries.

In practice, the synthetic control method allows us to estimate the causal effect of a policy/intervention (in this case, the occurrence of financial reform episodes) by comparing the evolution of the Gini index for a country affected by the reform episode with the evolution of the Gini index for a 'synthetic control group'. The synthetic control method has the advantage of providing a systematic way of identifying the 'control group'. The 'control group' is selected as a linear combination of all potential comparison countries that have similar characteristics to the 'treated' country before treatment. Thus, for each 'treated' country, the linear combination generates the 'control group' through an iterative optimization procedure that matches both the outcome variable and its determinants for the pre-intervention periods. Thus, a divergence in outcomes after the start of the intervention is analysed as a causal effect of the intervention as the 'treated' country and the 'control group' are made comparable in terms of outcome and observational characteristics.

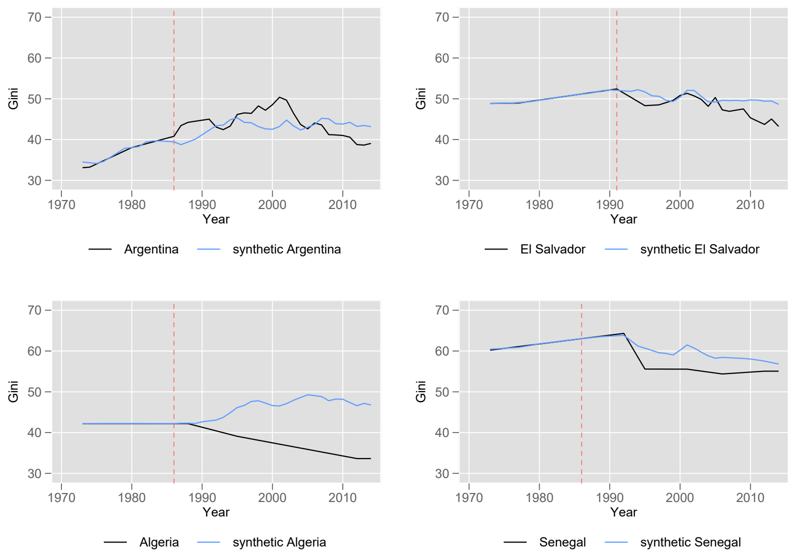
The choice of 'treated' country is based on any country that has experienced at least one episode of financial reform. Thus, there are obviously several interesting candidate countries, but we decide to report the results for Argentina, Algeria, El Salvador, and Senegal. The control group is selected from a global pool of countries. We define the first year of occurrence of reform episodes as the year of the intervention.[16] We use the Gini index as the outcome variable and the same control variables used in the previous sections as determinants of the outcome variable.

We answer the following question: what would have been the level of income inequality in Argentina (or Algeria, El Salvador, Senegal) if it had experienced episodes of financial reform in the same way as the global pool of countries?

We present the results from the synthetic method in Figure 3. The results show that for the four countries whose results are presented, there is a small change in their Gini index compared to the change in the Gini index of the synthetic control group. In other words, if Argentina (or Algeria, El Salvador, Senegal) had experienced episodes of financial reform in the same way as the synthetic control group, then the level of income inequality it would have had would have been significantly higher. For example, if Venezuela had implemented financial reforms like the 'Venezuela synthetic', the level of inequality would have exceeded 40 in terms of the Gini index.

[16] We use the *synth* command of stata. This command is equipped with options allowing to have more robust results from a statistical point of view. More precisely, we use the options '*nested*' and '*allopt*', which are the most time-consuming methods. Indeed, '*nested*' uses a fully nested optimization procedure while '*allopt*' provides a robustness check by running the nested optimization using three different starting points (Campos and Kinoshita 2010; Abadie et al. 2015).

Figure 3: Synthetic counterfactuals: Argentina, El Salvador, Algeria, and Senegal



Source: author's illustration based on study data.

6 Concluding remarks

The question of whether economic reforms have distributional consequences is a central issue in the political economy literature. Thus, the study of the relationship between reforms and income inequality has received increasing attention in recent years. Empirical studies that have examined the link between reforms and income inequality are inconclusive and limited.

This study analysed the impact of financial reforms (domestic finance and external finance liberalization) implemented over the last four decades in 64 emerging and low-income countries. Our main results reveal that domestic finance and capital account liberalization reforms contribute to a significant reduction in income inequality. While the effects are immediate and long-lasting for domestic finance liberalization reforms, the effects of capital account (external finance) liberalization reforms occur in the medium term (three years after the liberalization episodes). Robustness analyses were conducted and show that the results are robust to the inclusion of omitted variables, to other income inequality indicators other than the Gini index, and to alternative criteria for identifying financial reform episodes. Taking into account income shares, the results illustrate that financial reforms increase the income share of citizens at the bottom of the income distribution while reducing that of citizens at the top of the income distribution.

Considering the business cycle, our findings highlight the significant advantages of implementing financial reforms during economic recessions in the countries under study. Additionally, the results demonstrate that in countries with higher institutional quality, financial reforms play a crucial role in reducing income inequality, unlike countries with lower institutional quality. Moreover, we identify another pathway through which income inequality reduction occurs as a result of financial reforms, namely during periods without financial crises.

Although previous research on the distributional impacts of financial reforms has yielded inconsistent findings, the results of this study reignite the discussion regarding the significance of financial reforms in promoting inclusive growth and enhancing the well-being of the most vulnerable populations. To address the redistributive concerns associated with financial reforms, governments should prioritize initiatives aimed at improving institutional quality, expanding access to financial markets, and facilitating low-income individuals' ability to secure bank credit for income-generating endeavors. By undertaking these measures, policymakers can ensure that the benefits of financial reforms are accessible to all segments of society, particularly those with limited resources.

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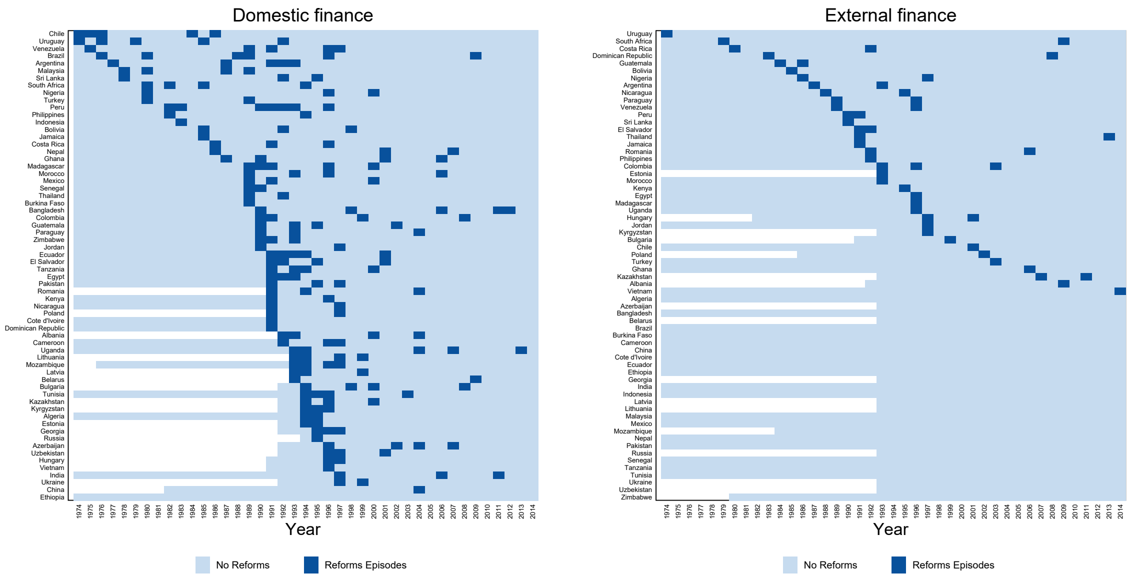
Appendix

Table A1: Number of financial reforms episodes

	70s	80s	90s	2000s	2010–14	1973–2014
Domestic finance						
Full sample	11	32	118	32	4	197
Emerging economies	11	26	84	21	1	143
Low income countries	0	6	34	11	3	54
External finance						
Full sample	2	10	27	11	2	53
Emerging economies	2	8	21	10	2	43
Low income countries	0	2	6	1	0	10

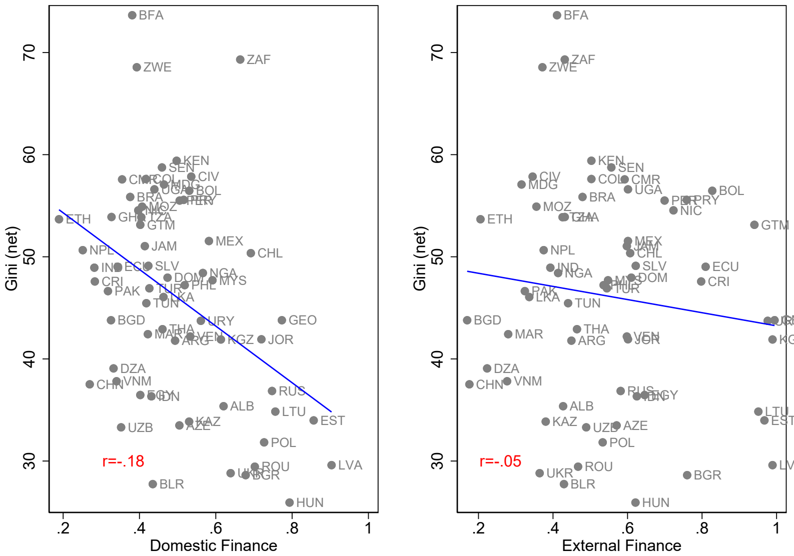
Source: author's calculation based on study data.

Figure A1: Financial reforms episodes by country-year



Source: author's illustration based on study data.

Figure A2: Scatter plot of financial reform and Gini (net) index



Source: author’s illustration based on study data.

Table A2: Descriptive statistics: Gini and reforms index

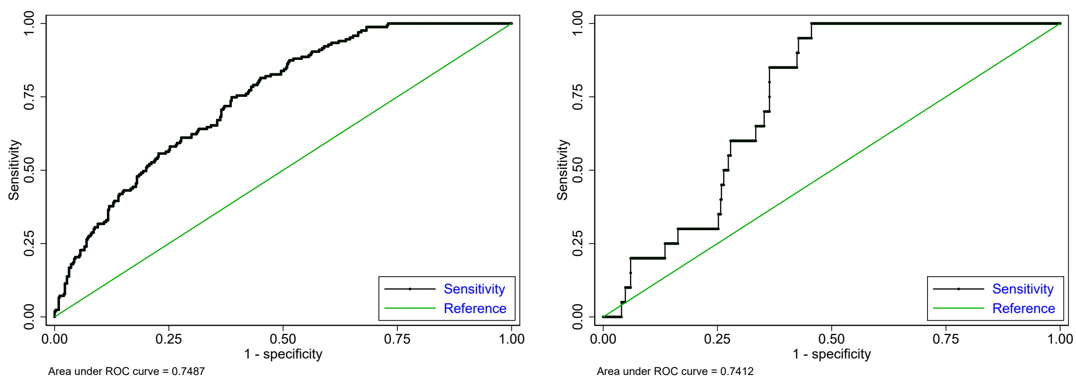
Summary [Panel A]						
Variable	Obs	Mean	Std. dev.	Median	Min	Max
Gini (net)	2688	46.15	11.15	46.83	16.75	77.09
Domestic finance	2371	0.48	0.29	0.5	0	1
External finance	2408	0.53	0.28	0.5	0	1
Matrix of correlations [Panel B]						
Variables	1	2	3			
(1) Gini (net)	1.00					
(2) Domestic finance	-0.17	0.73	1.00			
(3) External finance	-0.11	0.70	0.47	1.00		

Source: author’s calculation based on study data.

Figure A3: ROC curves for predicting reforms episodes

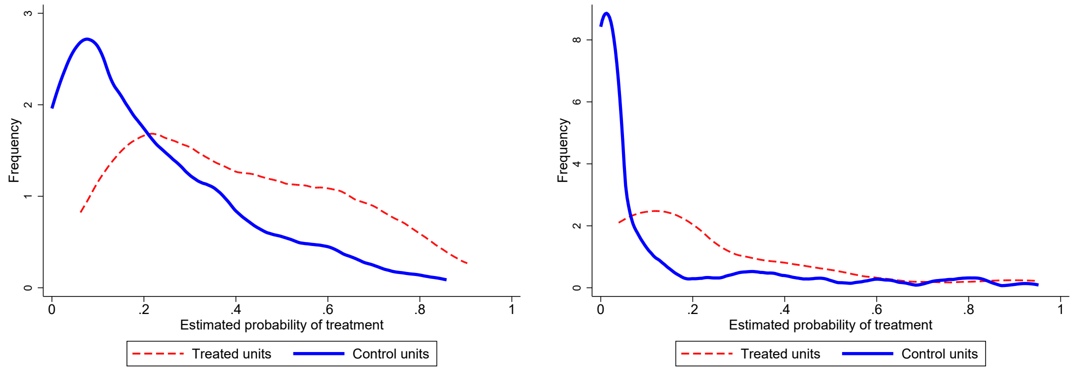
(a) Domestic finance

(b) External finance



Source: author’s illustration based on study data.

Figure A4: Kernel density of the distribution of the propensity scores for the treated and control groups
(a) Domestic finance (b) External finance



Source: author's illustration based on study data.

Table A3: Average treatment effect of reforms, IPWRA: additional control variables

Dependent: $y_{t+h} - y_t$ (Gini net)					
	Year 1	Year 2	Year 3	Year 4	Year 5
ATE_{IPWRA}: Domestic finance	-0.13*	-0.59***	-0.73***	-0.80***	-0.75***
	(0.06)	(0.12)	(0.18)	(0.12)	(0.14)
Observations	579	579	579	551	526
	0.01	0.26	-0.18**	-0.61***	-0.78***
ATE_{IPWRA}: External finance	(0.11)	(0.15)	(0.08)	(0.15)	(0.24)
Observations	600	600	600	570	543

Note: Driscoll-Kraay standard errors in parentheses. *** p≤0.01, ** p≤0.05, * p≤0.1. **Additional controls**: 1 lag of dependent variable; 1 lag of logarithm of GDP per capita (constant 2005 US$); 1 lag of crisis dummy; GDP per capita growth (annual %); country and year fixed effects are also included. **Propensity score** is based on the covariate balancing propensity score (CBPS) model as described in the text and includes: GDP growth (annual %); Unemployment(% of total labour force); IMF programme; inflation; logarithm of GDP per capita (constant 2005 US$); legislative/executive election dummy; age dependency ratio; Output gap using the filter of Hamilton (2018); Reform gap (defined as the difference between the level of liberalization in particular country and the reform level achieved in a country near the reform 'frontier', as United States) and reformist neighbours (proxied here by the weighted average of all other countries' liberalization indices. The weigths are proportional to the inverse of their distance to the country under consideration). The two last variables' definitions come from Ostry et al. (2009). All variables are in 1 lag. IPWRA estimates do not impose restrictions on the weights of the propensity score.

Source: author's calculation based on study data.

Table A4: Average treatment effect of reforms, IPWRA: control for other reforms variables

Dependent: $y_{t+h} - y_t$ (Gini net)					
	Year 1	Year 2	Year 3	Year 4	Year 5
ATE_{IPWRA}: Domestic finance	-0.1	-0.52***	-0.73***	-0.72***	-0.66***
	(0.06)	(0.11)	(0.18)	(0.12)	(0.14)
Observations	572	572	572	544	519
ATE_{IPWRA}: External finance	0.01	0.26	-0.18**	-0.61***	-0.78***
	(0.11)	(0.15)	(0.08)	(0.15)	(0.24)
Observations	600	600	600	570	543

Note: Driscoll-Kraay standard errors in parentheses. *** p≤0.01, ** p≤0.05, * p≤0.1. **Additional controls**: 1 lag of dependent variable; 1 lag of logarithm of GDP per capita (constant 2005 US$); 1 lag of crisis dummy; GDP per capita growth (annual %); country and year fixed effects are also included. **Propensity score** is based on the covariate balancing propensity score (CBPS) model as described in the text and includes: GDP growth (annual %); Unemployment(% of total labour force); IMF programme; inflation; logarithm of GDP per capita (constant 2005 US$); legislative/executive election dummy; age dependency ratio; Output gap using the filter of Hamilton (2018); Reform gap (defined as the difference between the level of liberalization in particular country and the reform level achieved in a country near the reform 'frontier', as United States) and reformist neighbours (proxied here by the weighted average of all other countries' liberalization indices. The weigths are proportional to the inverse of their distance to the country under consideration). The two last variables' definitions come from Ostry et al. (2009). All variables are in 1 lag. IPWRA estimates do not impose restrictions on the weights of the propensity score.

Source: author's calculation based on study data.

Table A5: Alternative measure of inequality: S80/S20 ratio and Theil Index

	Year 1	Year 2	Year 3	Year 4	Year 5
Dependent variable: S80/S20 ratio(Top 80% / Bottom 20%)					
ATE_{IPWRA}: Domestic finance	0.27	0.30	-0.12	-0.74**	-0.71**
	(0.26)	(0.52)	(0.60)	(0.32)	(0.32)
Observations	579	579	579	551	526
ATE_{IPWRA}: External finance	-1.18***	-0.27	-0.98***	-1.47***	-1.62***
	(0.20)	(0.20)	(0.19)	(0.26)	(0.27)
Observations	579	579	579	551	526
Dependent variable: Theil Index					
ATE_{IPWRA}: Domestic finance	-0.33	-1.21*	-1.88**	-2.37***	-2.24***
	(0.39)	(0.64)	(0.89)	(0.71)	(0.77)
Observations	579	579	579	551	526
ATE_{IPWRA}: External finance	-0.40	0.60	-0.75***	-1.70***	-1.60**
	(0.25)	(0.44)	(0.20)	(0.32)	(0.64)
Observations	579	579	579	551	526

Note: Driscoll-Kraay standard errors in parentheses. *** p≤0.01, ** p≤0.05, * p≤0.1. **Additional controls**: 1 lag of dependent variable; 1 lag of logarithm of GDP per capita (constant 2005 US$); 1 lag of crisis dummy; GDP per capita growth (annual %); country and year fixed effects are also included. **Propensity score** is based on the covariate balancing propensity score (CBPS) model as described in the text and includes: GDP growth (annual %); Unemployment(% of total labour force); IMF programme; inflation; logarithm of GDP per capita (constant 2005 US$); legislative/executive election dummy; age dependency ratio; Output gap using the filter of Hamilton (2018); Reform gap (defined as the difference between the level of liberalization in particular country and the reform level achieved in a country near the reform 'frontier', as United States) and reformist neighbours (proxied here by the weighted average of all other countries' liberalization indices. The weigths are proportional to the inverse of their distance to the country under consideration). The two last variables' definitions come from Ostry et al. (2009). All variables are in 1 lag. IPWRA estimates do not impose restrictions on the weights of the propensity score.

Source: author's calculation based on study data.

Table A6: Average treatment effect of reforms, IPWRA: alternative definition of reforms episodes

Dependent: $y_{t+h} - y_t$ (Gini net)					
	Year 1	Year 2	Year 3	Year 4	Year 5
ATE_{IPWRA}: Domestic finance	-0.36***	-0.26***	-0.21*	-0.05	0.17**
	(0.05)	(0.06)	(0.10)	(0.06)	(0.07)
Observations	599	599	599	569	542
ATE_{IPWRA}: External finance	-0.26*	-0.73***	-0.81***	-0.58***	-0.48***
	(0.13)	(0.19)	(0.23)	(0.16)	(0.14)
Observations	600	600	600	570	543

Note: Driscoll-Kraay standard errors in parentheses. *** p≤0.01, ** p≤0.05, * p≤0.1. **Additional controls**: 1 lag of dependent variable; 1 lag of logarithm of GDP per capita (constant 2005 US$); 1 lag of crisis dummy; GDP per capita growth (annual %); country and year fixed effects are also included. **Propensity score** is based on the covariate balancing propensity score (CBPS) model as described in the text and includes: GDP growth (annual %); Unemployment(% of total labour force); IMF programme; inflation; logarithm of GDP per capita (constant 2005 US$); legislative/executive election dummy; age dependency ratio; Output gap using the filter of Hamilton (2018); Reform gap (defined as the difference between the level of liberalization in particular country and the reform level achieved in a country near the reform 'frontier', as United States) and reformist neighbours (proxied here by the weighted average of all other countries' liberalization indices. The weigths are proportional to the inverse of their distance to the country under consideration). The two last variables' definitions come from Ostry et al. (2009). All variables are in 1 lag. IPWRA estimates do not impose restrictions on the weights of the propensity score.

Source: author's calculation based on study data.

Table A7: Balance diagnostics between the treated and control groups, domestic financial reforms episodes

	[A] Non weight					[B] Weight				
Variables	mean_T	mean_C	std_diff	sd_pool	var_ratio	mean_T	mean_C	std_diff	sd_pool	var_ratio
GDP growth (annual %)	4.19	3.87	0.08	3.92	1.39	4.19	4.19	0.00	3.95	1.37
Age dependency ratio, young (% of working-age population)	51.74	51.40	0.02	14.70	1.18	51.74	51.74	0.00	14.93	1.14
Reform in neighboring countries	0.54	0.58	-0.32	0.12	1.48	0.54	0.54	0.00	0.15	0.93
Reforms gap vis-à-vis of USA	0.31	0.28	0.16	0.14	1.02	0.31	0.31	0.00	0.15	0.90
Legislative or executive election	0.32	0.29	0.05	0.46	1.06	0.32	0.32	0.00	0.47	1.02
Human capital index	2.29	2.29	0.00	0.42	1.07	2.29	2.29	0.00	0.42	1.06
Crisis (Inflation > 40)	0.25	0.21	0.09	0.41	1.13	0.25	0.25	0.00	0.43	1.02
IMF programme active for 5 or more months	0.47	0.39	0.18	0.49	1.07	0.47	0.47	0.00	0.50	1.02
Gini coefficient	-0.06	-0.12	0.05	1.17	1.21	-0.06	-0.06	0.00	1.14	1.29
Cyclical component of GDP (Output gap)	0.01	0.02	-0.10	0.09	0.68	0.01	0.01	0.00	0.08	0.85
Unemployment rate, WDI	8.61	9.39	-0.15	5.26	0.89	8.61	8.61	0.00	4.89	1.04

Note: a significant difference between the two groups (treatment and control) exists if the absolute value of the standardized difference is at least 0.25 (Rubin 2001). In addition, Rubin proposes as an alternative measure the use of the ratio of the variances of the two groups. Indeed, if the ratio of variances is greater than 2, the variables are unbalanced between the two groups. Our covariate balancing tests show that the CBPS model eliminates all differences in characteristics between the two groups.

Source: author's calculation based on study data.

Table A8: Balance diagnostics between the treated and control groups, external financial reforms episodes

	[A] Non weight					[B] Weight				
Variables	mean_T	mean_C	std_diff	sd_pool	var_ratio	mean_T	mean_C	std_diff	sd_pool	var_ratio
GDP growth (annual %)	2.80	3.97	-0.30	3.92	1.52	2.80	2.80	0.00	4.56	1.09
Age dependency ratio, young (% of working-age population)	53.29	51.32	0.13	14.70	1.02	53.29	53.29	0.00	15.89	0.87
Reform in neighboring countries	0.53	0.58	-0.41	0.12	1.39	0.53	0.53	0.00	0.14	1.01
Reforms gap vis-à-vis of USA	0.33	0.28	0.30	0.14	1.14	0.33	0.33	0.00	0.15	1.06
Legislative or executive election	0.20	0.30	-0.22	0.46	0.78	0.20	0.20	0.00	0.40	1.03
Human capital index	2.30	2.29	0.00	0.42	1.00	2.30	2.30	0.00	0.44	0.90
Crisis (Inflation > 40)	0.34	0.21	0.34	0.41	1.42	0.34	0.34	0.00	0.48	1.03
IMF programme active for 5 or more months	0.60	0.38	0.44	0.49	1.04	0.60	0.60	0.00	0.49	1.03
Gini coefficient	-0.33	-0.10	-0.19	1.17	1.50	-0.33	-0.33	0.00	1.43	0.99
Cyclical component of GDP (Output gap)	0.00	0.02	-0.29	0.09	0.45	0.00	0.00	0.00	0.06	0.97
Unemployment rate, WDI	10.40	9.25	0.22	5.26	0.85	10.40	10.40	0.00	5.63	0.74

Note: a significant difference between the two groups (treatment and control) exists if the absolute value of the standardized difference is at least 0.25 (Rubin 2001). In addition, Rubin proposes as an alternative measure the use of the ratio of the variances of the two groups. Indeed, if the ratio of variances is greater than 2, the variables are unbalanced between the two groups. Our covariate balancing tests show that the CBPS model eliminates all differences in characteristics between the two groups.

Source: author's calculation based on study data.

Table A9: Average treatment effect of reforms, IPWRA: quality of government

Dependent variable: Gini net					
	Year 1	Year 2	Year 3	Year 4	Year 5
	Domestic Finance				
ATE_IPWRA: High quality of government	-0.29*	-0.86***	-1.35***	-1.15***	-1.12***
	(0.14)	(0.23)	(0.39)	(0.32)	(0.33)
ATE_IPWRA: Low quality of government	0.33***	0.17	-2.01***	-0.24	-1.43***
	(0.08)	(0.18)	(0.42)	(0.31)	(0.36)
Observations	579	579	579	551	526
	External finance				
ATE_IPWRA: High quality of government	-1.35***	-1.30***	-2.34***	-2.74***	-2.44**
	(0.11)	(0.40)	(0.46)	(0.72)	(0.91)
ATE_IPWRA: Low quality of government	1.28***	3.83***	4.54***	5.03***	4.68***
	(0.29)	(0.95)	(1.08)	(0.63)	(0.89)
Observations	579	579	579	551	526

Note: Driscoll-Kraay standard errors in parentheses. *** p≤0.01, ** p≤0.05, * p≤0.1. **Additional controls**: 1 lag of dependent variable; 1 lag of logarithm of GDP per capita (constant 2005 US$); 1 lag of crisis dummy; GDP per capita growth (annual %); country and year fixed effects are also included. **Propensity score** is based on the covariate balancing propensity score (CBPS) model as described in the text and includes: GDP growth (annual %); Unemployment(% of total labour force); IMF programme; inflation; logarithm of GDP per capita (constant 2005 US$); legislative/executive election dummy; age dependency ratio; Output gap using the filter of Hamilton (2018); Reform gap (defined as the difference between the level of liberalization in particular country and the reform level achieved in a country near the reform 'frontier', as United States) and reformist neighbours (proxied here by the weighted average of all other countries' liberalization indices. The weigths are proportional to the inverse of their distance to the country under consideration). The two last variables' definitions come from Ostry et al. (2009). All variables are in 1 lag. IPWRA estimates do not impose restrictions on the weights of the propensity score.

Source: author's calculation based on study data.

Table A10: Impact of financial crisis on the nexus between financial liberalization and income inequality

Dependent variable: Gini net					
	Year 1	Year 2	Year 3	Year 4	Year 5
	Domestic Finance				
ATE_{IPWRA}: Financial crisis	-0.40**	-0.18	0.86	2.46**	2.03***
	(0.15)	(0.35)	(0.70)	(0.97)	(0.69)
ATE_{IPWRA}: No financial crisis	0.84**	-1.14***	-2.99***	-3.88***	-2.86***
	(0.34)	(0.25)	(0.16)	(0.11)	(0.54)
Observations	579	579	579	551	526
	External finance				
ATE_{IPWRA}: Financial crisis	1.06**	0.61	-0.85	-1.12	-2.08
	(0.41)	(2.04)	(2.75)	(2.58)	(2.95)
ATE_{IPWRA}: No financial crisis	-1.04***	-2.05***	-2.30***	-2.20***	-2.50***
	(0.16)	(0.31)	(0.24)	(0.13)	(0.11)
Observations	579	579	579	551	526

Note: Driscoll-Kraay standard errors in parentheses. *** p≤0.01, ** p≤0.05, * p≤0.1. **Additional controls**: 1 lag of dependent variable; 1 lag of logarithm of GDP per capita (constant 2005 US$); 1 lag of crisis dummy; GDP per capita growth (annual %); country and year fixed effects are also included. **Propensity score** is based on the covariate balancing propensity score (CBPS) model as described in the text and includes: GDP growth (annual %); Unemployment(% of total labour force); IMF programme; inflation; logarithm of GDP per capita (constant 2005 US$); legislative/executive election dummy; age dependency ratio; Output gap using the filter of Hamilton (2018); Reform gap (defined as the difference between the level of liberalization in particular country and the reform level achieved in a country near the reform 'frontier', as United States) and reformist neighbours (proxied here by the weighted average of all other countries' liberalization indices. The weigths are proportional to the inverse of their distance to the country under consideration). The two last variables' definitions come from Ostry et al. (2009). All variables are in 1 lag. IPWRA estimates do not impose restrictions on the weights of the propensity score.

Source: author's calculation based on study data.

Table A11: Countries list (64)

Albania(ALB)	Dominican Republic(DOM)	Kyrgyzstan(KGZ)	Romania(ROU)
Algeria(DZA)	Ecuador(ECU)	Latvia(LVA)	Russia(RUS)
Argentina(ARG)	Egypt(EGY)	Lithuania(LTU)	Senegal(SEN)
Azerbaijan(AZE)	El Salvador(SLV)	Madagascar(MDG)	South Africa(ZAF)
Bangladesh(BGD)	Estonia(EST)	Malaysia(MYS)	Sri Lanka(LKA)
Belarus(BLR)	Ethiopia(ETH)	Mexico(MEX)	Tanzania(TZA)
Bolivia(BOL)	Georgia(GEO)	Morocco(MAR)	Thailand(THA)
Brazil(BRA)	Ghana(GHA)	Mozambique(MOZ)	Tunisia(TUN)
Bulgaria(BGR)	Guatemala(GTM)	Nepal(NPL)	Turkey(TUR)
Burkina Faso(BFA)	Hungary(HUN)	Nicaragua(NIC)	Uganda(UGA)
Cameroon(CMR)	India(IND)	Nigeria(NGA)	Ukraine(UKR)
Chile(CHL)	Indonesia(IDN)	Pakistan(PAK)	Uruguay(URY)
China(CHN)	Jamaica(JAM)	Paraguay(PRY)	Uzbekistan(UZB)
Colombia(COL)	Jordan(JOR)	Peru(PER)	Venezuela(VEN)
Costa Rica(CRI)	Kazakhstan(KAZ)	Philippines(PHL)	Vietnam(VNM)
Cote d'Ivoire(CIV)	Kenya(KEN)	Poland(POL)	Zimbabwe(ZWE)

Source: author's construction based on study data.

Table A12: Data sources

Variables	Sources
Structural reforms Index	(Alesina et al. 2020)
Trade (% of GDP)	WDI
Foreign direct investment, net inflows (% of GDP)	WDI
Unemployment, total (% of total labour force)	WDI
Inflation, GDP deflator (annual %)	WDI
IMF programme active for 5 or more months	(Balima and Sy 2019)
GDP growth (annual %)	WDI
Age dependency ratio, young (% of working-age population)	WDI
Reforms gap vis-à-vis of USA	Author's calculation
Reform in neighboring countries	Author's calculation
Legislative or executive election	Database of Political Institutions 2020 (DPI2020)
Crisis (Inflation $>$ 40)	(Laeven and Valencia 2020)
Gini Coefficient	UNU-WIDER (WIID)
Cyclical component of GDP (Output gap)	Author's calculation
Human capital index	PTW
Rule of law index	V-Dem

Source: author's construction.