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From Debt Reset to Growth Onset?
Sovereign Debt Restructuring and Firm Performance in Developing Countries*

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Abstract

Leveraging a unique dataset that combines country-level information on debt restructuring with firm-level data from theWorld Bank Enterprise Surveys (WBES) spanning from 2004 to 2023, we analyze the effects of debt restructuring on firm sales growth. .../...

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.../... Using recent advancements in difference-in-differences estimation to account for the staggered implementation of restructurings, we find that sovereign debt restructuring increases firm performance by 5–9 percentage points, with stronger effects for private, domestically-owned firms and those reliant on public and financial services. The impact varies by debt type (domestic or external), creditor composition, and implementation speed. Swift external restructurings led by official creditors, such as the Paris Club, yield the most substantial positive effects, whereas other types of restructurings show no significant impact on private sector growth.

Introduction

Since 2020, the polycrisis—marked by COVID-19, geopolitical tensions, climate hazards, and economic fragmentation—has severely strained growth and fiscal capacity in developing countries. Combined with reduced external financing and rising interest rates, it has heightened concerns over escalating debt distress and reignited debates on sovereign debt relief. As of 2024, over 50% of LICs are in or at high risk of debt distress, making sovereign debt relief a key international priority

While extensive research has examined the macroeconomic and financial stability implications of sovereign debt restructuring (DR), its effects on private sector growth remain understudied. Understanding how DR influences firm performance is crucial not only for assessing its broader economic impact but also for identifying its underlying transmission mechanisms. Addressing this gap, this paper investigates whether sovereign debt restructuring stimulates or hampers firm growth in recipient countries.

To this end, we combine historical data on sovereign debt restructurings from various creditors with firm-level information from the World Bank Enterprise Survey (WBES). Our final sample consists of a repeated cross-section of approximately 90,000 formal firms observed between 2004 and 2023 across 88 countries, 51 of which underwent at least one sovereign debt restructuring during the study period. Leveraging advancements in the difference-in-differences literature, we compare the performance of firms in countries undergoing debt restructuring before and after the treatment, relative to firms in countries that did not experience restructuring. To further minimize omitted variable bias, our specification incorporates firm-level characteristics, time-varying country-level covariates, and country and industry-year fixed effects.

Using the sales growth rate of surveyed firms as the outcome variable, our results indicate that sovereign debt restructuring increases firm growth by approximately 5 to 9 percentage points. These findings satisfy the parallel trends assumption underlying the difference-in-differences estimator. Additional robustness checks confirm that the positive effect of debt restructuring is not driven by any single country in the sample and remains significant when restricting the time window around the restructuring event. The effect also holds when limiting the control group to countries classified as facing at least moderate debt distress risk. While the average impact is economically meaningful relative to the sample's mean sales growth rate, it masks important heterogeneity. The effect does not significantly vary by firms' export status, website ownership, or perceptions of corruption and financial constraints. However, firms with partial foreign ownership and state-owned enterprises—though underrepresented in the sample for the latter—do not appear to benefit from debt restructuring. Small firms exhibit slightly greater gains than larger firms (100+ employees), but this difference is only marginally significant. More pronounced heterogeneity emerges when considering structural industry characteristics. Following Rajan and Zingales (1998), we examine the heterogeneous impact of debt restructuring according to sectoral structural characteristics. We find that firms operating in sectors structurally more reliant on financial services and human capital experience significantly larger benefits. These results highlight key supply-side transmission mechanisms through which debt restructuring supports private sector growth.

Lastly, our analysis of restructuring heterogeneity shows that the positive average effect is primarily driven by debt treatments from official creditors, including Paris Club members and China. Given that our study period covers a broad range of restructurings under the HIPC initiative, the positive effects may also reflect the conditionality attached to these debt relief programs. However, further analysis of restructuring frictions suggests that only swiftly implemented official restructurings significantly benefited private sector growth. To examine the role of restructuring duration, we use default start and end dates from Asonuma and Trebesch (2016) and Erce et al. (2022) for private creditors. For official restructurings, we approximate the process length using Paris Club data on IMF program approvals and final debt relief agreements. These findings provide novel evidence that accelerating the restructuring process is crucial for maximizing its economic impact.

Our study contributes to the existing literature in four key ways. First, it leverages a more comprehensive dataset on sovereign debt restructurings than previous studies, covering nearly all episodes since the early 1950s (though not all are used in the analysis). This dataset includes agreements with Paris Club members, China, private external creditors, and domestic debt holders, enabling a more granular examination of firm growth based on rescheduling and creditor types.

Second, we apply recent advancements in difference-in-differences estimation to address potential biases in measuring the impact of DR on firm growth. The literature on DID warns of estimation biases in cases of staggered policy adoption—such as sovereign debt restructurings—particularly when treatment effects vary over time, potentially leading to "forbidden comparisons" (Goodman-Bacon, 2021a). Beyond standard Two-Way Fixed Effects (TWFE) models with country and industry-year fixed effects, as well as appropriate firm- and country-level controls, we employ the imputation method developed by Borusyak et al. (2024). This approach ensures comparability between treated and control units, including those never treated or not yet treated, under the parallel trends assumption.

Third, we explore potential transmission channels linking DR to private sector growth. We examine the heterogeneity of DR effects by firm characteristics and restructuring features. Unlike previous studies focusing mainly on manufacturing firms, we extend the analysis to the services sector, differentiating firms based on structural input intensities (e.g., utilities, transport and construction, finance, and human capital) following Rajan and Zingales (1998) and Levchenko (2007).

Lastly, we assess the impact of delays in debt restructuring using newly aggregated data to provide fresh insights into how restructuring frictions affect firm decisions and sales. As noted by Horn et al. (2022), the sovereign default literature has largely overlooked the role of time lags in official debt restructurings, which tend to be more complex than those involving a limited number of private creditors. Our findings on the heterogeneous effects of DR based on restructuring duration thus represent a significant step forward in understanding the economic consequences of these debt treatments.

The rest of the paper is structured as follows: Section 1 provides a review of economic literature; Section 2 describes the data and empirical methodology, while Section 3 presents the results, robustness checks, and analysis of heterogeneity. The last section concludes.

1 Literature review

The macroeconomic effects of sovereign debt crises and restructuring have been widely studied. Debt restructuring is typically seen as a last-resort measure to contain the costs of debt crises—whether as a response to default or a preventive action—when sustainability cannot be restored through credible policy adjustments or sufficiently concessional lending (IMF, 2024). In this context, Krugman (1988) and Sachs (1988) argue that DR is the preferred solution to mitigate "debt overhang", which occurs when public debt service becomes unsustainably high relative to government revenue (the "real debt burden") and private investment is crowded out due to expectations of future tax increases (the "virtual debt burden").

Beyond restoring debt sustainability, sovereign DR—regardless of the creditor—can be viewed as a form of grant when it significantly reduces the present value of debt. Official DR often qualifies as Official Development Assistance (ODA), while private creditor relief, though not classified as ODA, similarly alleviates financial burdens. As a result, its effects on firm performance likely follow transmission channels similar to those identified for ODA in Chauvet and Ehrhart (2018).

DR can influence firm growth through both demand-side and supply-side mechanisms. On the demand side, it boosts net disposable income by reducing the tax burden previously allocated to debt service, enhancing household and business spending capacity. This, in turn, stimulates firms' production, as predicted by a basic Keynesian framework. Additionally, the fiscal space created by debt cancellation (Cassimon et al., 2015) can finance public investment, including infrastructure and service improvements. Governments may contract local firms for project implementation, further stimulating business activity and firm growth.

On the supply side, DR strengthens firms' productive capacity by easing constraints on business activity, particularly in high public debt scenarios, *i.e.*, in "debt overhang" situations. Among these constraints, restricted access to finance is especially significant, as shown in both cross-country (Beck et al., 2005; Beck and Demirguc-Kunt, 2006; Harrison et al., 2014; Chauvet and Jacolin, 2017) and country-specific studies (Sleuwaegen and Goedhuys, 2002; Poncet et al., 2010; Lashitew, 2017). The adverse effects of debt crises could thus be transmitted through financial channels, with credit rationing—triggered by sovereign downgrades—primarily affecting the non-financial sector (Arteta and Hale, 2008). In emerging economies, where domestic banks hold substantial sovereign debt, public debt crises can further constrain domestic credit. In this context, DR could help reallocate financial resources toward private firms, facilitating business expansion and improving growth prospects. A second key structural constraint to firm activity is the availability of public infrastructure, such as roads and electricity, which has been widely documented (Rud, 2012; Jedwab and Moradi, 2016; Cole et al., 2018; Barzin et al., 2018). According to the "real burden effect", by freeing up fiscal resources for infrastructure projects, DR could alleviate this constraint, though its impact on firm growth may take longer to materialize. Lastly, corruption and weak institutions have been shown to hinder business activity (Athanasouli and Goujard, 2015; Chauvet and Ferry, 2021), though the "grease the wheels" hypothesis suggests that, in some contexts, corruption may help offset bureaucratic inefficiencies (Martins et al., 2020). In countries with fragile institutions, DR may fail to translate fiscal space gains into productive public spending that benefits firms (Djimeu, 2018). Moreover, institutional quality influ-

ences a country's ability to negotiate and benefit from debt restructuring. In EMDEs, DRs are often linked to IMF programs, and their effectiveness depends on authorities' commitment to macroeconomic stability, structural reforms, and program credibility—key drivers of private investment and international financing. Since the HIPC initiatives, donors have increasingly directed DR efforts toward countries with stronger institutions and policies, aiming to incentivize institutional reforms (Freytag and Pehnelt, 2009; Presbitero, 2009).

The relationship between private sector growth and DR remains ambiguous. While some studies find no significant impact on aggregate economic performance (Chauvin and Kraay, 2005; Presbitero, 2009; Johansson, 2010), DR has been shown to create fiscal space (Cassimon et al., 2015), potentially reducing uncertainty and improving sales expectations and investment. Using a broader sample of 52 developed and developing economies, Claessens et al. (2012a) finds that a 1% fiscal stimulus increases corporate profitability by 0.3 pp, with stronger effects on firms sensitive to the business cycle. However, DR is often accompanied by fiscal consolidation measures tied to concessional financing, particularly under IMF programs. One of the few firm-level studies finds that such macroeconomic adjustments hinder firm growth and profitability, with large firms and those reliant on infrastructure being more affected than exporters (Pahula et al., 2024). The impact of DR may also depend on the composition of fiscal adjustments (spending cuts vs. tax hikes) (Chauvet and Ferry, 2021), IMF interventions (Bomprezzi and Marchesi, 2021), and shifts in foreign aid (Chauvet and Ehrhart, 2018). Excessively restrictive fiscal policies and poor policy mixes can trigger self-defeating economic contractions (Fatás and Summers, 2018), exacerbating business and financial cycles, financial instability, and crises (Claessens et al., 2012b).

Lastly, few studies assess the magnitude of DR effects based on the type of restructuring implemented—debt relief vs. debt rescheduling, external (EDR) vs. domestic (DDR) debt restructuring, and public vs. private creditors. Yet, the impact of DR can vary significantly depending on the nature of the creditors involved. Recent research shows that different types of DR lead to distinct macroeconomic outcomes. EDR involving official creditors does not result in permanent output per capita losses during or after the crisis, whereas private EDRs may have more detrimental effects (Marchesi and Masi, 2021), possibly due to the absence of market stigma in official EDRs and the persistent credit rating downgrades linked to private ones (Marchesi et al., 2024). Reinhart and Rogoff (2009) find that output declines following DDRs are significantly worse than after EDRs, likely because DDRs often impose greater losses on domestic creditors than private EDRs (Erce et al., 2022). Moreover, combined EDR/DDR episodes tend to induce sharper output contractions than stand-alone DDRs or EDRs (IMF, 2021). The nature of debt agreements also influences economic outcomes. Using a database of 422 Paris Club restructurings across 86 debtor countries, Cheng et al. (2018) show that only restructurings involving nominal debt relief (haircuts) have a significant positive impact on GDP per capita growth, regardless of the net present value (NPV) gains. Similar patterns emerge in the case of Chinese loan restructurings, though with more moderate effects (Bon and Cheng, 2020).

Given these mixed findings, the impact of debt restructuring on firm performance remains an open question. The effect likely depends on firm characteristics and the specific features of the restructuring process, underscoring the need for a rigorous empirical investigation.

2 Empirical approach

2.1 Data

2.1.1 Sovereign Debt Restructuring

Interest in sovereign debt issues has surged over the past decade, particularly following the Eurozone debt crisis and Greece's partial default in 2012. This renewed focus has led to the gradual development of several databases documenting debt restructuring episodes and their characteristics. EDR databases cover Paris Club restructurings (Cheng et al., 2018; Horn et al., 2022), Chinese debt restructurings (Bon and Cheng, 2020; Horn et al., 2022), and private restructurings by external lenders (Asonuma and Trebesch, 2016). DDR episodes have also been compiled by Erce et al. (2022).[1]

Our database consolidates existing datasets into a comprehensive record of 899 debt restructuring episodes from 1956 to 2021. Each entry includes details on the debtor, creditor(s), agreement year, amounts involved, and whether nominal relief was provided. However, data on net present value reductions is not systematically available, particularly for restructurings by China and private creditors. Additionally, the duration of negotiations (*i.e.*, from initiation to completion) is often missing, except for private creditor restructurings (Asonuma and Trebesch, 2016). While extensive, the dataset is not entirely exhaustive. Debt restructurings granted by developing country governments (excluding China) to other developing countries are not systematically tracked. Information on debt relief under the Multilateral Debt Relief Initiative (MDRI) is also incomplete. However, this gap is partially addressed through debt stock cancellations granted at the completion point of the Heavily Indebted Poor Countries (HIPC) Initiative by Paris Club members. For countries that reached their completion point from 2005 onward, multilateral debt treatments under the MDRI are captured through Paris Club restructurings occurring at the same time. For HIPCs that exited the initiative before 2005, MDRI treatments have been incorporated for 2006.

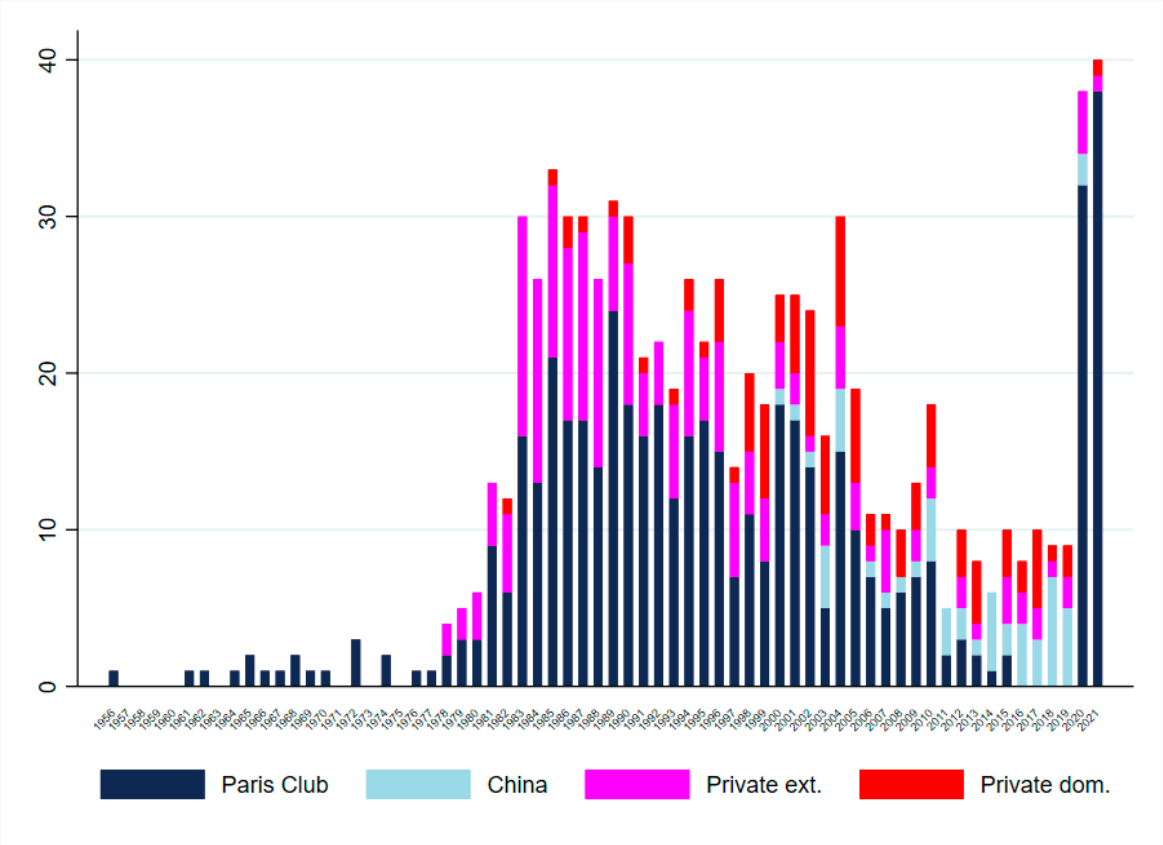
Additional Paris Club data To address these limitations, we supplement existing datasets with Paris Club data, which documents all restructuring operations—including extensions and amendments—from 1956 to 2023. These events encompass classic Paris Club treatments (involving both member countries and ad hoc creditors) as well as multilateral restructurings under the HIPC and DSSI initiatives. The dataset provides granular details on different stages of HIPC restructurings, categorized by restructuring terms. Furthermore, Since Paris Club restructurings are conditional on the adoption of IMF programs and the full participation of all creditors under comparable terms, this data allows us to approximate restructuring complexity and costs with time lags between agreements among Paris Club members and IMF program adoption and/or implementation. Using data from the Paris Club Secretariat, we leverage these time lags to explore the heterogeneous effects of debt restructuring on firm growth rates.

Figure 1 illustrates the evolution of debt restructuring (DR) episodes by creditor type. As discussed, while Paris Club treatments have historically dominated sovereign debt restructurings in developing countries, the first large-scale debt crisis of the early 1980s triggered a

[1] See the Appendix for a detailed discussion of these datasets.

wave of sovereign external debt restructurings granted by private creditors (EDRs).

Figure 1: Temporal evolution of debt restructuring - by creditors



Notes: Y axis reports the number of sovereign debt restructuring episodes (rescheduling and debt relief), by type of creditors. Data for Paris Club debt restructurings come from Cheng et al. (2018) and were completed with Horn et al. (2022). Data for Chinese debt restructurings come from Horn et al. (2022). Data for external private creditors restructurings stem from Asonuma and Trebesch (2016) while data for restructuring on domestic-law debt are from Erce et al. (2022).

In the following decades, Paris Club restructurings became predominant, driven by the HIPC initiative (1996) and its Enhanced version (1999), followed by the MDRI (2005). The 2000s saw a notable rise in domestic debt restructurings (DDRs), while the 2010s marked the emergence of Chinese debt treatments, particularly in the years preceding the COVID-19 crisis. In the 2020s, the pandemic led to the implementation of the Debt Service Suspension Initiative (DSSI), which benefited 30 countries in 2020–2021, as reflected in the sharp increase at the end of the period.

Figure A1 in the appendix replicates this analysis but focuses on restructurings involving nominal debt relief, which became significant in the 1990s. The first modern-era large-scale sovereign debt relief agreements were granted by private creditors under the Brady Plan in the early 1990s, with substantial guarantees from official creditors. Paris Club members later initiated more extensive nominal debt reductions through the HIPC initiatives and subsequent bilateral agreements at the completion point. Over time, Paris Club restructuring terms evolved from short-term debt rescheduling to long-term restructuring and, eventually, stock debt relief. Figure A2 ranks countries by the number of sovereign debt restructurings since 1956 across different creditor types. The Democratic Republic of Congo

and Argentina top the list, each with 22 restructuring episodes, followed by Senegal and Côte d'Ivoire with 21. Nicaragua, Madagascar, Ecuador, and Brazil rank next with 18, while the Republic of Congo follows with 17. Togo, Niger, and Jamaica each recorded 16 restructurings. The distribution of top restructuring cases is diverse, reflecting engagement with various creditor types. However, when considering only Paris Club restructurings—the most common form—Sub-Saharan African countries dominate. Senegal leads with 16, followed by the Democratic Republic of Congo, the Republic of Congo, and Togo (14 each), then Madagascar (13) and Niger (12).

2.1.2 World Bank Enterprise Surveys

Data coverage In order to investigate the impact of debt restructuring on private sector performance, we match the collected debt restructuring information with firm-level data provided by the *World Bank Enterprise Surveys* (WBES). Using the latest version of the WBES,[2] we compile data on over 200,000 firms across 159 economies between 2004 and 2023. Given our focus on sovereign debt restructuring, we prioritize WBES repeated cross-sectional data, which offers broader country coverage compared to the more limited firm panel dataset (covering roughly half as many economies). To extend temporal coverage and incorporate additional restructuring episodes, we also include older WBES waves, retaining only those with sampling weights to maintain comparability with data collected from 2006 onward.

WBES are standardized surveys conducted by the World Bank, representing the formal private sector across industries, enterprise sizes, and geographical levels.[3] In addition to detailed firm-level data on characteristics and activities, the surveys capture information on actual and perceived access to infrastructure and services, the extent of competition, and the relationship between public authorities and business entities. For this study, our primary measure of private sector performance is firms' sales growth rate for fiscal year t. Sales data are among the few WBES variables available as far back as $t-4$, allowing us to compute the average annual sales growth rate at the firm level between $t-1$ and $t-3$.

Final sample To assess the impact of DR on firm growth, we refine the selection of WBES waves based on a specific rule (detailed in the Appendix) that classifies surveys as either "ex-ante" or "ex-post" relative to sovereign restructuring events. Applying this criterion, we identify 51 countries that meet two conditions: (1) they experienced at least one restructuring during the study period, and (2) they were covered by at least two WBES waves. Figure 2 illustrates the WBES waves available for each country undergoing DR during the study period and highlights the selected waves that meet our selection rule.

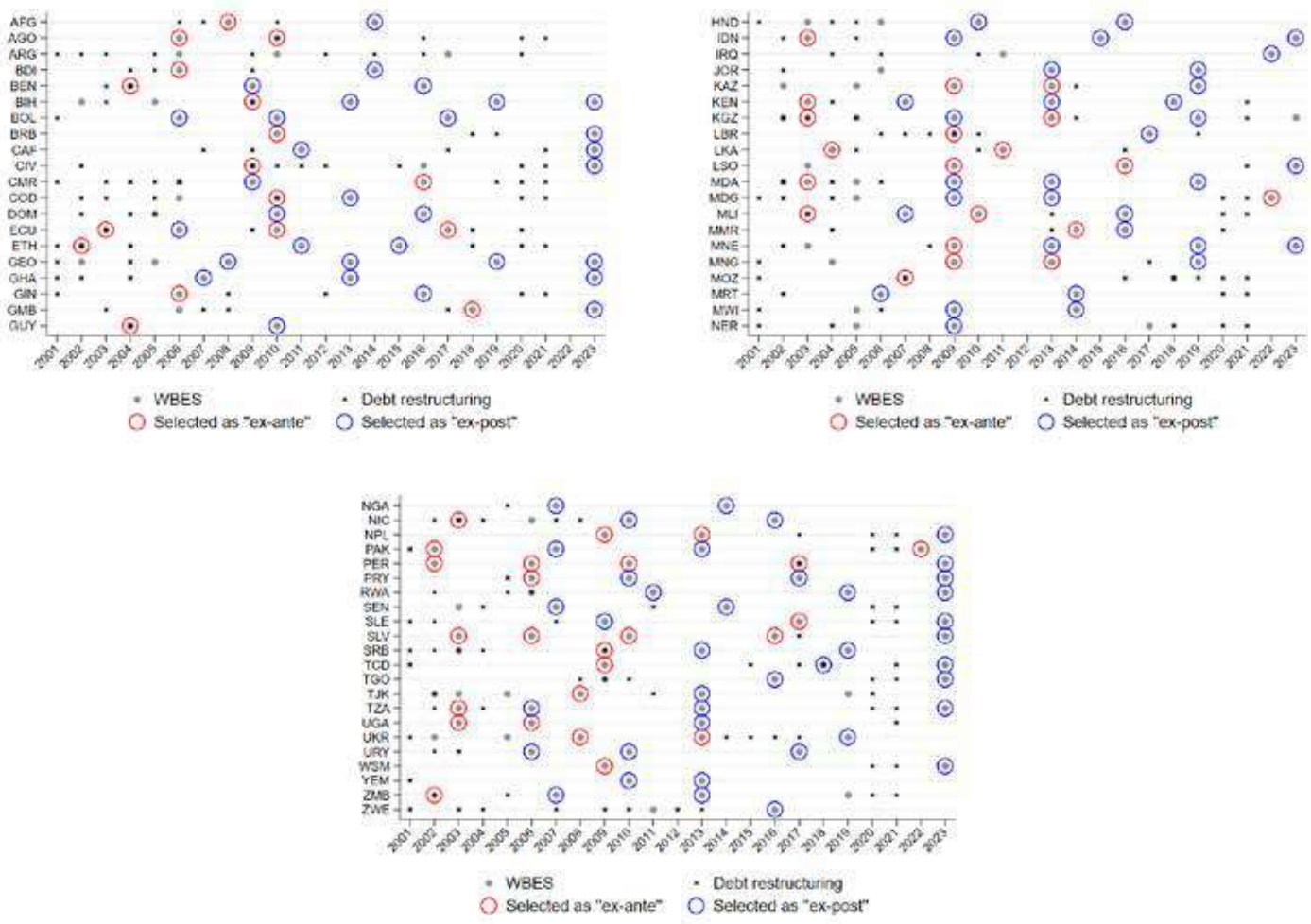
In addition, and to account for the evolution of the private sector in developing countries without DR, we incorporate WBES waves from 37 additional developing countries, each with at least two survey rounds over the study period. Our final sample comprises 88 developing countries, with an average of just over three WBES waves per country between 2004 and 2023, resulting in 294 country-year observations. This dataset includes 103,520

[2]The New Comprehensive Dataset as of March 12, 2024, available at `https://www.enterprisesurveys.org/en/data`.

[3]The geographical level used for sampling stratification is the first administrative region (ADM1).

formal private firms, of which 44,124 are in countries that experienced debt restructuring, while the remaining 59,396 are in non-restructuring countries. Table A1 in the appendix provides details on the sample composition, listing the number of firms per country and the corresponding WBES survey years. The final dataset used in the analysis, including all relevant covariates, contains 98,619 observations at the firm-level (of which 92,218 are used for regressions).

Figure 2: "Treated sample" - Debt restructuring episodes and WBES



Notes: Figures report the occurrence of debt restructurings and rounds of WBES for each country of the initial WBES sample over 2001-2023. WBES waves that are not circled are excluded from the analysis as they do not meet the criteria aforementioned to be considered either ex-ante or ex-post restructuring. Circled waves before 2004 meet the selection rule but are dropped from the sample in the absence of sampling weights.

2.2 Empirical strategy

Given the limited number of WBES rounds per country and the database tracking debt restructuring events at the country-year level, we analyze the impact of debt restructuring by comparing the average growth rate of private formal firms before restructuring to the average growth rate observed after restructuring within the same country. This initial

analysis is conducted without distinguishing between creditor types, restructuring terms, or negotiation duration, serving as a simple before-and-after comparison. However, to account for what would have occurred in the absence of restructuring, it is necessary to establish a counterfactual. While the true counterfactual is unobservable, it can be approximated using data from a non-treated group—in this case, firms in countries that did not experience sovereign debt restructuring. To implement this double-difference approach, we employ the standard two-way fixed effects estimator, using first OLS estimates of the specification presented below.

$$\begin{aligned} Sales\_Growth_{i,k,j,(t-1;t-3)} = \alpha + \tau POST\_DR_{j,(t-1;t-3)} + \Phi X'_{i,k,j,t} \\ + \Omega X'_{j,(t-3;t-5)} + \mu_j + \delta_t + \nu_k + \epsilon_{i,k,j,t} \end{aligned} \tag{1}$$

with $Sales\_Growth_{i,k,j,(t-1;t-3)}$ representing the sales growth of each enterprise i, operating in industry k within country j, calculated between $t-1$ and $t-3$. This measure is derived from the WBES questionnaire, where firm managers report sales figures for the most recent fiscal year and three fiscal years prior, in current local currency. To ensure cross-country and temporal comparability, we adjust reported sales figures using GDP deflator indices from the World Development Indicators database, deflating sales for $t-1$ and $t-3$. The average annual sales growth is then computed over this period, excluding the top 1% of values to mitigate the influence of outliers. Across the entire sample, the average annual sales growth rate is approximately 8.8% (see Table 1 below), which is slightly higher than estimates from prior studies (by around 1 percentage point). This difference likely reflects the broader country coverage enabled by incorporating the latest WBES waves provided by the World Bank.

The variable of interest, denoted as $POST\_DR_{j,(t-1;t-3)}$ is a binary indicator capturing whether country j experienced debt restructuring during the study period, conditional on the timing of the firm's growth rate calculation (cf. Figures A4 and A3, as well as the discussion on WBES waves selection). This variable equals one for firms in debt restructuring country j if the last observation used to compute their growth rate falls after the restructuring event and if no subsequent restructuring occurs within the following two years. Conversely, it takes a value of zero for firms in the same country j whose sales growth rate is measured "ex ante" (i.e., before debt restructuring) and for firms in non-restructuring countries. Post-restructuring firms constitute approximately 30% of the sample, while the remaining observations include firms either from restructuring countries but observed "ex ante" or from countries that did not undergo sovereign debt restructuring.

Turning then to control variables, $X'_{i,k,j,t}$ is a set of firm-level controls that captures characteristics such as: the size of the firms ($\text{Size}_{i,k,j,t}$), coded as small ($[5;20[= 1$), medium ($[20;100[= 2$), or large $100 \geq = 3$); the age of the firms ($\text{Age}_{i,k,j,t}$), coded as ($[0yr;5yr] = 1$, $]5yr;10yr] = 2$, $]10yr;25yr] = 3$, $]25yr;...[= 4$), exporting firms ($\text{Export}_{i,k,j,t}$); partial or complete state ownership ($\text{State}_{i,k,j,t}$) and the presence of foreign shareholders ($\text{Foreign}_{i,k,j,t}$). State companies are scarce in our sample and foreign shareholders are present in only 10% of sampled firms (see Table 1).

Table 1: Summary Statistics

Variables	unit	#	mean	std. dev.	min	max
Firm-level						
$\text{Sales\_Growth}_{i,k,j,(t-1;t-3)}$	%	98,619	8.64	48.54	-100	550.44
$\text{Size}_{i,k,j,t}$	[1;3]	98,619	1.76	0.77	1	3
$\text{State}_{i,k,j,t}$	0/1	98,619	0.01	0.11	0	1
$\text{Age}_{i,k,j,t}$	0/1	98,619	2.89	0.86	1	4
$\text{Foreign}_{i,k,j,t}$	0/1	98,619	0.10	0.29	0	1
$\text{Export}_{i,k,j,t}$	0/1	98,619	0.22	0.42	0	1
$\text{Website}_{i,k,j,t}$	0/1	98,619	0.50	0.50	0	1
$\text{Corruption}_{i,k,j,t}$	0/1	98,619	0.32	0.46	0	1
$\text{Finance}_{i,k,j,t}$	0/1	98,619	0.23	0.42	0	1
$\text{Sales}_{i,k,j,t-3}$	log.	98,619	8.68	2.61	0	24.09
Country-year level						
$\text{GDP\_pc}_{j,(t-3;t-5)}$	log.	98,619	8.86	0.74	6.62	10.27
$\text{POP}_{j,(t-3;t-5)}$	log.	98,619	17.50	1.93	12.16	21.06
$\text{POST\_DR}_{j,(t-1;t-3)}$	0/1	98,619	0.30	0.46	0	1
Length of restructurings						
Treated firms only						
$\text{Paris\_Club\_Cred.}_{j,(t-1;t-3)}$	months	24,909	13.71	16.56	0	58
$\text{Dom.\_Priv.\_Cred.}_{j,(t-3;t-5)}$	months	11,658	123.52	123.57	2	303
$\text{Ext.\_Priv.\_Cred.}_{j,(t-3;t-5)}$	months	7,197	65.19	86.31	1	271

Firm-level variables are derived from the WBES. The GDP deflator and exchange rate used for deflating and converting firms' sales into USD are obtained from *WDI* and *WEO* databases, respectively. $\text{POST\_DR}_{j,(t-1;t-3)}$ is the treatment variable based on the occurrence of debt restructurings, as reported in our comprehensive dataset on debt restructuring episodes, which aggregates multiple data sources (see discussion in the data section). $\text{GDP\_pc}_{j,(t-3;t-5)}$ and $\text{POP}_{j,(t-3;t-5)}$ are both obtained from the *WDI* database.

We also account for internet access by including a binary variable, $\text{Website}_{i,k,j,t}$, which indicates whether the firm has an official website, with approximately half of the firms in the sample reporting having one. In addition to internet infrastructure, factors such as access to finance and the prevalence of corruption can also impact firm performance. Therefore, we incorporate additional dummy variables based on subjective assessments provided by firm managers, indicating whether access to financial services (financial inclusion) or the extent of corruption is considered a major obstacle to business operations ($\text{Finance}_{i,k,j,t}$) and $\text{Corruption}_{i,k,j,t}$, respectively). These dummy variables are equal to 1 if managers report that access to finance or corruption is a major obstacle to their business. Around one-third of managers identify corruption as a significant problem, while slightly less than a quarter report that limited access to financial services hinders their business activities. Then, to control for the catching-up effect, we include the lag of sales from $t-3$, as smaller firms in terms of turnover are expected to grow faster than those that have reached their critical size. Similar to the sales growth rate, these sales are deflated using the GDP deflator from the *WDI* database. Additionally, they are converted into US dollars using the annual exchange rate

of the local currency from the latest *World Economic Outlook database* (*WEO*) database, ensuring comparability across countries and over time.

In addition to these firm-level controls, we incorporate variables to capture the impact of time-varying factors at the country level, while maintaining parsimony to avoid the inclusion of bad controls. We include the log of per capita GDP (in PPP, atlas method) obtained from the *WDI* database. This variable, expressed in log form, facilitates running difference-in-differences estimates across comparable levels of development. We also add the log of population size to control for market size, which is sourced from the *WDI* database. Figure 1 below presents descriptive statistics for the final sample, including figures for the duration of the restructuring process (in months) categorized by the types of creditors involved.

Lastly, specification (1) includes controls for country fixed effects (μ_j), survey-year fixed effects (Δ_t), and industry (sector) fixed effects (ν_k)[4]. The inclusion of these fixed effects helps control for the structural characteristics of countries and industries, as well as global time-varying factors that affect all firms in the sample at a given time. Alternatively, equation (1) is estimated by replacing survey-year and industry fixed effects with an interaction between the two sets to capture industry-specific business cycles. Standard errors are clustered at the more conservative country level to account for serial correlation of firm data within each country, irrespective of the interview year.

3 Results

3.1 Average impact of debt restructuring

Two-way fixed effects results Table 2 presents the results from Two-Way Fixed Effects (TWFE) OLS estimates of equation (1). Columns (1) to (4) include the country, survey-year, and industry fixed effects, while columns (5) to (8) incorporate country and survey-year × industry fixed effects.

Firm-level controls generally exhibit expected signs: larger, younger, foreign-owned, exporting, and well-connected firms tend to report higher sales growth rates. The lagged level of sales is negatively associated with the growth rate, reflecting the catching-up effect, where firms starting from a lower base grow faster. The state-owned variable does not show a differential impact on sales growth rates, likely due to the relatively low prevalence of state-owned firms in the sample. Financial inclusion constraints are negatively associated with firm sales growth, though the correlation is not statistically significant. Lastly, the perception of corruption is associated with a lower growth rate. Country-level controls do not appear to have a significant effect on the dependent variable, although the signs of the coefficients align with findings from existing literature. Country-year controls are included to account for each country's level of development and population size. These results highlight the importance of firm-specific factors in explaining sales growth, beyond macroeconomic conditions.

Turning then to our variable of interest, TWFE estimates show that the sales growth rate of firms observed after debt restructuring is, on average, higher by between 5.36 and 9.39 percentage points compared to when firms are observed before debt restructuring, relative to

[4]Sectors have been aggregated and aligned to match the ISIC Rev4 classification.

the growth rates exhibited by firms from non-restructuring countries. The magnitude of this effect varies depending on the set of controls and fixed effects imposed, reaching a maximum of 9.39 percentage points under the most conservative specification, which includes country and survey-year × industry fixed effects, as well as the full set of firm-level and country-level covariates.

Table 2: Two-Way Fixed Effects Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.:	$Sales\_Growth_{i,k,j,(t-1;t-3)}$					
$POST\_DR_{j,(t-1;t-3)}$	5.36*	7.09**	8.18**	5.91*	8.53**	9.39***
	(3.06)	(3.46)	(3.15)	(3.12)	(3.40)	(3.16)
Firm-level controls						
$Size_{i,k,j,t}$		8.82***	8.79***		9.26***	9.25***
		(2.06)	(2.07)		(2.11)	(2.12)
$State_{i,k,j,t}$		3.19	3.13		3.16	3.20
		(3.85)	(3.73)		(3.73)	(3.58)
$Age_{i,k,j,t}$		-2.47**	-2.47**		-2.46**	-2.46**
		(0.96)	(0.96)		(0.98)	(0.98)
$Foreign_{i,k,j,t}$		5.20***	5.23***		5.30***	5.33***
		(0.90)	(0.91)		(0.84)	(0.84)
$Export_{i,k,j,t}$		2.57*	2.50*		2.46*	2.37
		(1.42)	(1.44)		(1.45)	(1.47)
$Website_{i,k,j,t}$		5.39***	5.41***		5.45***	5.47***
		(0.78)	(0.78)		(0.75)	(0.74)
$Corruption_{i,k,j,t}$		-3.11*	-3.14*		-3.29**	-3.29**
		(1.64)	(1.64)		(1.59)	(1.59)
$Finance_{i,k,j,t}$		-0.22	-0.26		-0.20	-0.23
		(0.96)	(0.97)		(0.95)	(0.96)
$Sales_{i,k,j,t-3}$		-6.10***	-6.07***		-6.26***	-6.24***
		(1.07)	(1.07)		(1.12)	(1.12)
Country-year controls						
$GDP\_pc_{j,(t-3;t-5)}$			11.33			14.44
			(16.35)			(15.14)
$POP_{j,(t-3;t-5)}$			30.27			22.18
			(20.80)			(18.77)
Control		✓	✓		✓	✓
svy-year-FE	✓	✓	✓			
indus.-FE	✓	✓	✓			
svy_year x indus. FE				✓	✓	✓
Var. Dep. (mean)	4.931	4.931	4.931	4.931	4.931	4.931
#	92,218	92,218	92,218	92,218	92,218	92,218
R^2	0.061	0.125	0.126	0.072	0.138	0.138

Note: Robust standard errors in parentheses clustered at the country-level. Estimates are obtained using WBES sampling weights and all regressions include country fixed effects. *, **, *** denote significance at the 10, 5, and 1% level, respectively.

Correcting for forbidden comparisons However, in light of recent advancements in the difference-in-differences literature (see De Chaisemartin and d'Haultfoeuille (2023) and Roth et al. (2023) for a literature review), it is essential to address potential bias stemming from the 'forbidden comparison' issue, as outlined in the Beacon-Goodman decomposition (Goodman-Bacon, 2021b). In a classic TWFE specification, the staggered adoption of debt rescheduling leads to treating firms in restructuring countries that have not yet undergone debt restructuring as control units for firms observed ex-post in restructuring countries. However, these comparisons between 'treated units' and 'not yet treated units' become problematic when there are heterogeneous treatment effects across time and units (De Chaisemartin and d'Haultfoeuille, 2020; De Chaisemartin and d'Haultfoeuille, 2024). If the effect of debt restructuring were uniform across countries and time, comparing 'treated' and 'not yet treated' firms would not be an issue. However, in the case of DR, this assumption is unlikely to hold. Debt treatments implemented at the beginning of the study period differ from those provided towards the end, with the latter primarily involving initiatives such as the DSSI, which postpones debt service, while earlier periods focused on debt relief through HIPC and MDRI, particularly for Paris Club debt restructuring. As a result, the effects of debt restructuring are likely to vary, violating the parallel trends assumption that is essential for difference-in-differences estimates.

Various alternative estimators have been developed to address the 'forbidden comparison' issue. Key proposals include those by Callaway et al. (2024), Sun and Abraham (2021), De Chaisemartin and d'Haultfoeuille (2024), and Borusyak et al. (2024). The efficiency of these estimators depends on the specific design of the difference-in-differences setup, such as whether the treatment is absorbing (i.e., a unit can switch from not treated to treated and then exit the treatment). These estimators also differ in their assumptions regarding the parallel trend requirement. Some suggest using the last observation before treatment as the reference point, while others require a similar dynamic on average during the ex-ante period. Additionally, the computational intensity of these methods varies. For example, Callaway et al. (2024) compute an average of each double-difference across treated and non-treated unit groups. In contrast, Borusyak et al. (2024) propose a more efficient method based on imputation, where control units have their outcomes imputed from a first-stage regression that includes fixed-effects and controls determined by the researcher. This method is particularly suitable for repeated cross-section data and offers the advantage of being relatively less computationally intensive compared to other estimators. When using this approach, the sample size is slightly reduced compared to the TWFE estimates, which is attributed to the imputation technique that imputes the outcome variable (i.e., the dependent variable) conditioned on the set of control variables and the structure of fixed effects imposed. Although the full sample of approximately 90,000 observations is used, as in the TWFE OLS estimates, the table statistics only report the number of observations actually used in the imputation procedure.

Table 3 presents the results of the difference-in-differences estimates using the imputation method proposed by Borusyak et al. (2024). Most coefficients related to control variables remain statistically significant without any sign reversal. The impact of debt restructuring on firm sales growth continues to be significant, but its magnitude is lower, and the effect at the mean is reduced once we adjust for forbidden comparisons, under the most conservative specification.

Table 3: DID Imputation Estimates

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.:	$Sales\_Growth_{i,k,j,(t-1;t-3)}$					
$POST\_DR_{j,(t-1;t-3)}$	8.18***	7.19**	5.30**	9.29***	8.67***	7.94***
	(3.05)	(2.83)	(2.11)	(3.27)	(3.08)	(2.52)
Firm-level controls						
$Size_{i,k,j,t}$		8.69***	8.65***		9.14***	9.13***
		(2.84)	(2.83)		(2.93)	(2.93)
$State_{i,k,j,t}$		3.43	3.14		3.09	3.06
		(4.62)	(4.56)		(4.38)	(4.28)
$Age_{i,k,j,t}$		-2.75**	-2.73**		-2.73**	-2.72**
		(1.25)	(1.25)		(1.29)	(1.29)
$Foreign_{i,k,j,t}$		5.19***	5.24***		5.34***	5.37***
		(1.22)	(1.19)		(1.14)	(1.12)
$Export_{i,k,j,t}$		1.57	1.67		1.24	1.25
		(1.71)	(1.77)		(1.84)	(1.88)
$Website_{i,k,j,t}$		5.29***	5.28***		5.28***	5.29***
		(0.82)	(0.82)		(0.81)	(0.81)
$Corruption_{i,k,j,t}$		-3.64*	-3.68*		-3.82**	-3.83**
		(1.89)	(1.89)		(1.82)	(1.82)
$Finance_{i,k,j,t}$		-0.49	-0.56		-0.39	-0.44
		(1.25)	(1.26)		(1.24)	(1.25)
$Sales_{i,k,j,t-3}$		-6.09***	-6.08***		-6.24***	-6.23***
		(1.58)	(1.57)		(1.66)	(1.66)
Country-year controls						
$GDP\_pc_{j,(t-3;t-5)}$			-14.11			-0.94
			(32.04)			(29.54)
$POP_{j,(t-3;t-5)}$			51.50			29.61
			(34.94)			(32.52)
Control		✓	✓		✓	✓
svy-year-FE	✓	✓	✓			
indus.-FE	✓	✓	✓			
svy_year x indus. FE				✓	✓	✓
Dep. var. (mean)	8.424	8.424	8.424	8.395	8.395	8.395
#	65,683	65,683	65,683	65,593	65,593	65,593

Note: Robust standard errors in parentheses clustered at the country-level. Estimates are obtained using WBES sampling weights and all regressions include country fixed effects. *, **, *** denote significance at the 10, 5, and 1% level, respectively.

Parallel trend assumption The assumption underlying the difference-in-differences specification is that the outcome dynamics of control units, which belong to groups that are either never treated or not yet treated, represent the pattern that would have followed for the outcomes of treated units in the absence of the treatment. In our context, this implies that the evolution of sales growth rates for firms in countries that have not undergone debt restructuring yet or never will, reflects the counterfactual evolution of firms in countries that

benefited from sovereign debt restructuring. This assumption is strong and cannot be empirically tested, as it is impossible to observe the outcomes of treated units in both states. However, one could reasonably infer that if the dynamics of outcomes for both control and treated units are similar ex-ante, i.e., before debt restructuring, then control units provide a relevant counterfactual for assessing the impact of restructuring.

As crucial as it is, assessing such ex-ante parallel trends, i.e., pre-trends, is complicated in our setting. The difference-in-differences specification requires comparing firms before and after debt restructuring within the same country, which is achieved by imposing both time and country fixed effects. Therefore, to estimate pre-trend similarity, at least two, and ideally three, rounds of WBES before debt restructuring are needed, which is rarely the case in our sample. Only a few countries have more than one round of WBES ex-ante to the episode of debt restructuring considered, following the above-defined rule. This number can be extended if we assume that observing the last point used for growth rate calculation as the year of debt restructuring allows for considering firm observations as ex-ante. However, pre-trend is calculated for each year further away from the first year after the restructuring. Each year further away from this date must be available for each country, as estimates are obtained within-country, which is not the case in our context due to the patchiness of WBES, which are available, on average, every 4 to 5 years within each country. Consequently, we define two ex-ante periods that capture the most WBES rounds within each country and estimate parallel trends over these multiple-year periods. The first ex-ante period (period = 1) runs from the year of occurrence of debt restructuring (i.e., $t = 0$) back to 6 years before ($t = -6$). The second period (period = 2) runs from 7 years before debt restructuring (i.e., $t = -7$) back to 12 years before ($t = -12$). Additionally, the last period steps back to -18 before debt restructuring. Thanks to the definition of these multi-year periods, we can estimate pre-trends over two ex-ante periods using the `pre(2)` option, over a sub-sample of firms observed up to 18 years before debt restructuring and 14 years after.

Table 4 reports the results for the reduced sample of control firms and treated firms within the above-defined temporal window surrounding debt restructuring episodes. The variable denoting the effect of debt relief restructuring remains significant irrespective of the fixed effects structure. Regarding the estimates associated with the pre-trend over our ex-ante multiple-year periods, the reported coefficients are non-significant, except for the most conservative specification, where the farthest period is significant at the 10% level. However, most of the coefficients remain overall non-significant, leading us to accept the null hypothesis of no difference in firms' sales growth rate dynamics over the two ex-ante periods between firms from restructuring countries and those from non-restructuring countries.

Robustness tests We begin by assessing the robustness of the results through sensitivity tests, focusing on the composition of the sample. Using the largest sample from the two-way fixed effects estimates, we re-estimate the specification presented in column (6) of Table 2, systematically excluding one country at a time to examine the potential influence of outliers. While excluding specific countries does alter the magnitude of the coefficient, the average effect continues to remain positive and statistically significant at the 10% level (see Figures A5 in the Appendix).

We then refine the definition of the temporal window surrounding debt restructuring

Table 4: DID Imputation - Including Pre-Trends Esitmates

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.:	$Sales\_Growth_{i,k,j,(t-1;t-3)}$					
$POST\_DR_{j,(t-1;t-3)}$	10.44**	9.10*	7.46*	11.73**	10.84**	10.24***
	(4.95)	(4.71)	(4.06)	(4.81)	(4.23)	(3.60)
$PTREND^{[0/-6]}_{j,(t-1;t-3)}$	-10.58	-4.81	2.21	2.12	9.61	12.85
	(13.05)	(13.94)	(15.43)	(11.13)	(11.86)	(12.90)
$PTREND^{[-7/-12]}_{j,(t-1;t-3)}$	4.56	5.74	9.69	12.27	14.07	15.96*
	(10.16)	(10.23)	(10.34)	(9.00)	(8.94)	(9.18)
Firm-level Control		✓	✓		✓	✓
Country-level Control		✓	✓		✓	✓
svy-year-FE	✓	✓	✓			
indus.-FE	✓	✓	✓			
svy_year x indus. FE				✓	✓	✓
Dep. var. (mean)	8.424	8.424	8.424	8.395	8.395	8.395
#	65,683	65,683	65,683	65,593	65,593	65,593

Note: Robust standard errors in parentheses clustered at the country-level. Estimates are obtained using WBES sampling weights and all regressions include country fixed effects. *, **, *** denote significance at the 10, 5, and 1% level, respectively.

episodes to further test the robustness of our results. As described in the data section, surveys are classified as 'ex-ante' when the last observation used to calculate the sales growth rate occurred before, or at the latest, within the year of the restructuring. Since restructuring may take place early in the fiscal year, the final observation may still be recorded months after the event. To clarify the distinction between 'ex-ante' and 'ex-post', we re-estimate the main specification, incorporating varying fixed effects and control settings, as shown in Tables 2 and 3, but exclude surveys where the last observation coincides with the restructuring year. This ensures that only 'ex-ante' surveys with growth rates recorded before restructuring and 'ex-post' surveys with rates recorded afterward are considered. Table A2 in the appendix demonstrates that the results from two-way fixed effects estimators and the difference-in-differences imputation procedures (with and without parallel trends) on the reduced sample remain significant and similar in magnitude to those in the previous tables.

Secondly, we re-estimate our main specification by narrowing the 'ex-ante' and 'ex-post' periods during which firms may have been surveyed. As shown in Figure A6 in the Appendix, the distribution of firms from countries that underwent debt restructuring spans from 16 years before to 18 years after the event. While the difference-in-differences estimates restrict the 'ex-post' period to 6 years after restructuring, this is not necessarily the case for the 'ex-ante' period or the two-way fixed effects estimates. We therefore sequentially limit the 'ex-ante' and 'ex-post' periods to 7, 6, and 5 years before and after the restructuring. Finally, we also run estimates where both periods are symmetrically constrained around the restructuring event. Table A3 in the appendix presents the results. Although

the sample size decreases by approximately 10% under the narrowest time frame (restricting to 6 years before and after the restructuring), the findings remain consistent. Firms, on average, continue to exhibit higher growth rates following debt restructuring compared to those in countries without sovereign debt restructuring, regardless of the restrictions placed around the restructuring event.

Lastly, we gather information about the creditworthiness of each country without debt restructuring (i.e., control countries). For control countries that are also Low-Income Countries (LICs), we collect data from their Debt Sustainability Assessment (DSA) and rank the risk of debt distress as low, moderate, high, or in debt distress. To ensure a more comparable control group for countries that underwent debt restructuring, we restrict the control group to LICs with a DSA ranking of moderate risk of debt distress or higher (high risk or debt distress) during the period over which the sales growth rate is calculated. This restriction ensures greater comparability between treated and control firms. However, it also significantly reduces the size of the control group, as most LICs that experience high risk of debt distress, or are already in debt distress, typically resolve this issue through sovereign debt restructuring (especially when the debt is owed to Paris Club members). To address this challenge, we also gather Debt Sustainability Assessments for Lower-Middle-Income Countries (LMICs) and Upper-Middle-Income Countries (UMICs) by using data from rating agencies about their "investment grade". We then restrict control countries to those that fall into the "non-investment grade" category, as it suggests that they are likely facing economic distress and probably debt liquidity or solvency issues as countries with DR. Restricting the control group to countries that are similar to the 'treated' countries in terms of debt (un)sustainability significantly reduces the number of observations. However, as shown in the results in Table A4, the coefficient remains positive and is associated with an increase of approximately 4.9 percentage points.

3.2 Heterogeneity analysis

Above results and robustness checks indicate that debt restructuring, irrespective of its size or features, boosts firm growth in restructured countries by an average of 5 to 9 pp—a substantial increase compared to the average sales growth rate in the sample. However, since firms surveyed in the WBES vary significantly in terms of size, ownership, export orientation, and constraints, the benefits of sovereign debt restructuring could be different from one type of firm to another.

3.2.1 Firm characteristics

In what follows we therefore examines the impact of sovereign debt restructurings on firms with different key characteristics: size, ownership, exports status, website, perceived corruption and perceived financial access. As shown in Table 5, we find that the impact of debt restructuring varies slightly according to firm size. Small size firms appear to benefit a bit more from debt restructuring, with an estimated sales growth effect of 8.31 pp, followed by medium-sized firms (7.37 pp) and large firms (5.30 pp), albeit these coefficients are not statistically different from each other. Larger impacts of DRs on small firms may stem from their lack of diversification and higher dependence on the local business cycle, in particular

public spending and revenue spending, as they have access to fewer financial buffers. In contrast, larger firms benefit from their integration in international commodity trade and value chains, access to international diversification of both revenue streams and financing and therefore are more insulated from local macroeconomic and financial cycles.

Table 5: DID Imputation - Firm Heterogeneity Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. var.:	$Sales\_Growth_{i,k,j,(t-1;t-3)}$							
VAR.:		Size	State	Foreign	Export	Website	Corr.	Fina.
$POST\_DR_{j,(t-1;t-3)}$	7.94***							
	(2.52)							
$POST\_DR^{(VAR=0)}_{j,(t-1;t-3)}$			7.99***	8.38***	8.02***	9.78***	7.63***	8.47***
			(2.51)	(2.47)	(2.73)	(2.93)	(2.82)	(2.63)
$POST\_DR^{(VAR=1)}_{j,(t-1;t-3)}$			0.00	0.40	7.44**	6.55**	8.63***	6.42**
			(0.00)	(3.98)	(2.90)	(2.57)	(2.99)	(3.05)
$POST\_DR^{(Small\_firms)}_{j,(t-1;t-3)}$		8.31***						
		(2.76)						
$POST\_DR^{(Med\_firms)}_{j,(t-1;t-3)}$		7.37***						
		(2.15)						
$POST\_DR^{(Large\_firms)}_{j,(t-1;t-3)}$		5.30**						
		(2.70)						
p-val$_{(VAR(=0)=VAR(=1))}$			0.001	0.002	0.852	0.124	0.710	0.376
p-val$_{(Small=Med)}$		0.535						
p-val$_{(Small=Large)}$		0.208						
p-val$_{(Med=Large)}$		0.150						
#	65,593	65,593	65,593	65,593	65,593	65,593	65,593	65,593
Dep. var. (mean)	8.395	8.395	8.395	8.395	8.395	8.395	8.395	8.395

Note: Robust standard errors in parentheses clustered at the country-level. Estimates are obtained using WBES sampling weights and all regressions include country and survey year×industry fixed effects as well as country and firm-level controls. *, **, *** denote significance at the 10, 5, and 1% level, respectively.

Privately-owned firms experience a post-restructuring sales growth boost of 7.99 pp, in contrast with state-owned firms which do not benefit from any effect. This latter effect should be interpreted with caution given the small share of these firms in the sample. This may reflect the fact that privately owned firms are more exposed to fluctuations in the business and financial local cycle and thus benefit more from the macroeconomic environment improvement following a debt restructuring. Public-owned entities, often financially intertwined with the State and shielded from the business cycle, and are thus being subject to different set of public constraints (cross subsidization and lending, transfers, constraints on capital growth). Their debt may sometimes be included in the perimeter of public debt.

Results also suggest that domestically owned firms experience significant sales growth of 8.38 pp post-restructuring, whereas the effect of debt restructuring is lower (0.40 pp) but not significant for foreign-owned firms. Similarly, non-exporting firms benefit from debt restructuring, with a sales growth effect of 8.02 pp, but the difference with non-exporting

firms is modest (7.44) and we can't reject the null hypothesis of no difference between exporting and non-exporting firms. Companies without a website show a post–restructuring sales boost at 9.78 pp, contrasting with a strong but smaller sales boost for firms equipped with a website (6.55 pp), but again we can't reject the null hypothesis of no difference. This suggests that foreign owned firms may be less dependent on the local cycle, hence less affected by fiscal policy and public debt constraints than domestic firms.

Regarding perception variables included in the WBES survey, firms that don't consider corruption to be a major obstacle and those that do experience a relatively close post-restructuring boost in sales growth (+8.63 pp vs. 7.63 pp), and the p-value is not small enough to reject the null hypothesis of no difference. This inconclusive result aligns with the literature discussing if corruption "sand" or "grease" the wheels of development, which has not yet established a clear positive or negative net impact of corruption on development prospects. While corruption may distort economic incentives, it can also help circumvent excessive bureaucratic regulations and foster firm development. Finally, firms without significant perceived financial constraints benefit from a 8.47 pp increase in sales growth, indicating possible linkages between public debt restructuring and banks credit policy. Given the importance of public debt in banks asset portfolios in emerging markets (bank-sovereign nexus), public debt restructuring may reduce perceived risks by banks and free capital to finance the private sector (crowding-in effect). Firms that report difficulty in accessing finance still see a positive sales growth effect of 6.42 pp, but we can't reject the null hypothesis of no difference with firms that don't.

However, heterogeneity analysis based on these perceived variables may not fully capture the differentiated impact of sovereign debt restructuring on structural constraints faced by private firms in debtor countries. Since these perceptions are recorded at the time of the survey—either before or after the restructuring—they may be influenced by the debt treatment itself. In particular, improvements in perceived financial access or reductions in perceived corruption could directly result from the stabilization of public finances following sovereign debt restructuring and the first results of the IMF program-led reforms.

3.2.2 Transmission mechanisms

To assess whether debt restructuring alleviates structural constraints that intensify under high sovereign debt burdens, we move beyond perceptions of financial access and institutional inefficiencies, focusing instead on the fundamental characteristics of industries.

We follow Chauvet and Ehrhart (2018) and Marchesi et al. (2022) who examine the heterogeneous impact of ODA on firm performance based on industry-specific structural characteristics. Their approach involves estimating an interaction term between the variable of interest and a sectoral intensity measure at the industry level. Originally proposed by Rajan and Zingales (1998) and applied to our research question, this approach filters the effect of debt restructuring on firm performance through exogenous industry traits, such as structural reliance on public infrastructure, financial services, or administrative capacity. Industry-specific dependency levels are inferred from a frictionless market—one where such constraints are minimal due to the absence of market failures. Following Rajan and Zingales (1998), and Chauvet and Ehrhart (2018), we use the U.S. market as a benchmark for these

structural dependencies.

The assessment of the institutional mechanism *i.e* whether the effect of debt restructuring varies according to the institutional dependency of firms given the industry they operate into, builds on data from Levchenko (2007). This study measures product complexity using the Herfindahl index of intermediate input use in the U.S. for 1992. The key assumption is that industries with more fragmented input chains require a greater number of contracts, increasing their reliance on a strong regulatory environment and well-functioning institutions to safeguard input buyers and enforce agreements effectively. To measure input fragmentation and institutional dependence, the Herfindahl index—initially a measure of product concentration—is inverted so that higher values indicate greater reliance on institutional quality. Using the *sect_strata* variable from the WBES, we classify sub-sectors into 23 broad industries spanning manufacturing and services. We then integrate institutional intensity measures with this classification by re-aggregating the finer industry-level dependence indices from Levchenko (2007), averaging across sub-sectors within each of the 23 industries. We apply the same approach using data from Rajan and Zingales (1998) and Kroszner et al. (2007) to capture industries' reliance on external financing, defined as dependence on non-operational cash flow for capital expenditures. Since these data are also available in Levchenko (2007), we align the reported sub-sectors with our 23 industry classifications. However, a key limitation of these intensity measures is their restriction to manufacturing industries, excluding services from the analysis. This constraint is significant, as column (2) in Table 6 indicates that the positive impact of debt restructuring on firm growth extends across both manufacturing and services sectors.

We address this limitation with additional regressions (columns (5) to (8)) where we follow Chauvet and Ehrhart (2018) by computing structural intensities for both manufacturing and services industries using the 2000 U.S. input-output table from the World Input-Output Database (WIOD, (Timmer et al., 2015)). Specifically, for each U.S. industry k, we calculate the share of intermediate inputs sourced from a given industry j relative to total inputs from all industries. This measure captures the structural intensity of industry k in industry j (which may encompass multiple sub-sectors) for the U.S. in 2000, providing an exogenous benchmark for industry characteristics independent of conditions in our sample of developing countries.

We begin by examining structural dependence on public utilities by calculating the share of inputs sourced from the "*electricity, gas, steam, and air conditioning supply*" and "*water collection, treatment, and supply*" sub-sectors. This measure captures industry-level reliance on public utilities and the provision of public goods, reflecting a key transmission channel through which debt restructuring may operate—specifically by alleviating the "real debt burden" and enabling greater public investment in infrastructure.

Similarly, we compute industry dependence on transport and construction by measuring the share of intermediary inputs sourced from the transport sector (including land, water, and air transport) and construction sub-sectors. Following the approach of Rajan and Zingales (1998) and Kroszner et al. (2007), we also calculate industry intensity in financial services.[5]

[5]Considering intermediary inputs from the "*Financial service activities, except insurance and pension funding*", "*Insurance, reinsurance and pension funding, except compulsory social security*" and "*Activities auxiliary to financial services and insurance activities*" sub-sectors.

Additionally, given that debt restructuring—particularly under the HIPC initiatives—can expand fiscal space for social sector investments, we include an indicator for "human capital" intensity, defined as the share of intermediary inputs sourced from the "education" and "human health and social work activities" sub-sectors. Table A5 in the Appendix reports these intensity measures for each of our 23 industries, along with their median levels. Columns (3) to (8) of Table 6 then present the heterogeneous effects of debt restructuring, distinguishing between firms in industries with structural dependence above or below the median for each intensity measure.

Table 6: DID Imputation - Industry Heterogeneity Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. var.:	$Sales\_Growth_{i,k,j,(t-1;t-3)}$							
	Whole Sample		Manufacturing only		Whole Sample			
Source:			Levchenko	R&J	WIOD			
Intensity in:			Institutions	External. Finance	Public Utilities	Transport & Construct.	Finance	Human Capital
			[1] ≥ median intensity; [0] ≤ median intensity					
$POST\_DR_{j,(t-1;t-3)}$	7.94***							
	(2.52)							
$POST\_DR^{[Manuf.]}_{j,(t-1;t-3)}$		6.70**						
		(3.27)						
$POST\_DR^{[Servic.]}_{j,(t-1;t-3)}$		8.66***						
		(2.52)						
$POST\_DR^{[0]}_{j,(t-1;t-3)}$			-3.26	-0.08	7.09**	9.85***	4.26	5.47
			(3.75)	(2.52)	(3.43)	(2.59)	(3.32)	(3.38)
$POST\_DR^{[1]}_{j,(t-1;t-3)}$			-0.70	-1.16	9.16***	6.78**	9.85***	9.37***
			(3.17)	(3.40)	(2.55)	(3.43)	(2.61)	(2.61)
p-val$_{(Manuf.=Servic.)}$		0.410						
p-val$_{(DR^{[0]}=DR^{[1]})}$			0.409	0.409	0.283	0.283	0.017	0.111
#	65,593	65,480	35,536	35,536	64,340	64,340	64,340	64,340
Dep. var. (mean)	8.395	8.391	7.021	7.021	8.169	8.169	8.169	8.169

Note: Robust standard errors in parentheses clustered at the country-level. Estimates are obtained using WBES sampling weights and all regressions include country and survey year×industry fixed effects as well as country and firm-level controls. *, **, *** denote significance at the 10, 5, and 1% level, respectively.

While no differentiated effect of debt restructuring is found concerning institutional or external financing dependence when restricting the analysis to manufacturing firms, results based on industry intensities derived from the WIOD are more conclusive. Specifically, the impact of debt restructuring appears relatively homogeneous across firms with varying degrees of dependence on public utilities, as the larger coefficient for firms in highly reliant industries does not differ statistically from that of firms in less dependent sectors. A similar pattern emerges for industries dependent on transport and construction inputs. However, results in columns (7) and (8) of Table 6 indicate a that this positive effect concentrates on firms in industries highly reliant on the financial sector, suggesting that financial access constraints are particularly detrimental and likely exacerbated in high public debt environments. This result sheds light on a possible transmission mechanism of DR via the financial

sector, possibly via credit supply. Such transmission channel is likely to vary according to financial development, as well as financial inclusion and bank competition (Chauvet and Jacolin, 2017), notably on how DR affect the bank-sovereign nexus. Similar conclusions apply to firms operating in industries highly dependent on human capital, although the difference in coefficients is only marginally significant (at the 11% level). This result may reflect heterogeneity in the type of debt restructuring implemented, as official restructurings—particularly those under the HIPC initiatives—were specifically designed to improve social sectors such as education and health. These improvements could have ultimately benefited firms in labor- and skill-intensive industries, though with a lagged effect.

Results from the DID imputation procedure closely align with those obtained from the baseline two-way fixed effects estimates reported in Table A6 in the Appendix, despite differences in functional form. Specifically, the interaction terms capture the additional impact of debt restructuring relative to the average effect, for firms in industries structurally more dependent on certain sectors. The findings suggest a reinforcing effect for firms reliant on public utilities, financial services, human capital, and external financing, as defined in Rajan and Zingales (1998). The results remain robust when adopting a more conservative fixed-effects specification by incorporating country × survey-year fixed effects. While this approach absorbs the average effect of debt restructuring, it allows for the estimation of its heterogeneous impact based on industry intensities. However, these results should be interpreted with caution due to potential biases arising from heterogeneous treatment effects over time and the possible presence of "forbidden comparisons".

3.2.3 Debt restructuring characteristics

In addition to firm characteristics, the average effect of restructuring may also conceal significant disparities based on the types and features of sovereign debt restructuring.

Type of creditors As outlined in the data section, these restructurings could involve Paris Club creditors, the Chinese government (specifically the Chinese public sector, including semi-public companies in some cases), external private creditors (such as banks, bondholder associations, and other private entities), or domestic creditors holding local-currency denominated debt. The next section investigates the heterogeneous effects of restructuring based on the origin of the creditors involved, and distinguishing between restructurings that entail face value reduction (i.e., debt relief) and those that involve maturity extensions and debt repayment rescheduling.

Restricting the occurrence of restructuring to the sample under analysis, figure 3 shows that over half of the restructurings involve Paris Club members, while the remaining restructurings are distributed evenly among China, domestic creditors, and external private creditors.

However, these restructurings are not mutually exclusive, as restructuring processes typically arise when debtor governments encounter difficulties in repaying multiple creditors. It is common for countries undergoing sovereign debt treatment with the Paris Club or China to also negotiate with private creditors, as stipulated under the comparability of Treatment principle (CoT). The comparability of treatment principle – which guarantees each creditors

Figure 3: Debt restructurings - Breakdown by types of creditors

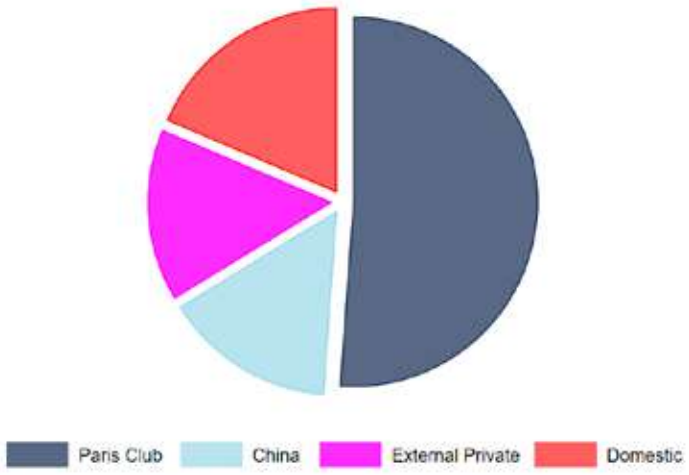
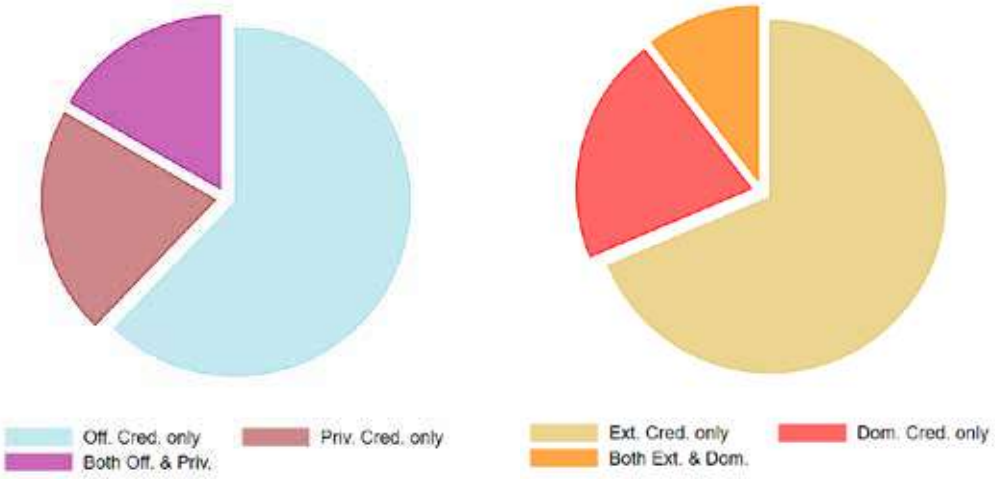


Figure 4: Debt restructurings - Breakdown by official and residency creditors' status



to obtain as favorable terms of treatment as other creditors – is supported by mechanisms such as claw-back clauses and the IMF's lending into arrears policy (LIA and LIOA). Figure 4 illustrates that slightly less than one-quarter of the restructurings in our sample involved both official and private creditors. It further highlights that coordination among lenders may occur not only between official and private creditors but, much less frequent cases, among private creditors themselves, including those holding external public debt and domestic-law-denominated debt. Lastly, the right-hand pie chart in Figure 4 indicates that, although less common, some countries underwent domestic debt restructuring exclusively, without any corresponding treatment of their external public debt.

For some restructurings, it is also important to consider the broader environment in which DR were implemented. A significant portion of restructuring occurred under the Heavily In-

debted Poor Countries Initiative (HIPC)[6], which aimed at reestablishing Debt sustainability and helped providing additional fiscal space to finance investments Millennium Development Goals by 2015. Firms may have benefited indirectly from public spending on education and health, and more directly in public utilities, which represent significant bottlenecks to growth in developing countries. DR implementation may also be instrumental in reducing the crowding-out effect on the domestic credit markets as it reduces the sovereign-banking nexus. In addition, as demonstrated by Ferry et al. (2021), these initiatives also enabled beneficiary governments to resume external borrowing in international private markets.

DR involving private creditors may have send much more mixed signal to international investors, as many of these restructurings included significant haircuts on outstanding claims and because of reputation effects, potentially impeding foreign capital inflows in countries with access to international markets, hence the growth of large and foreign firms. Domestic DR involving mostly private domestic creditors (for a large part domestic banks for debt issued on domestic financial markets) may have different effects: these DR mostly impact the private sector in favor of the public sector, which benefits from additional fiscal space, with no clear indication on whether the overall impact would be positive for firms. Moreover, domestic DR are not associated with any international inflows (or lower international debt service outflows), lowering their potential overall macroeconomic effect. However, as with official creditor debt restructurings, such effects may be offset by fiscal space generated by these DR, with indirect positive on firm growth.

Overall, the differentiated impact of debt restructuring on firm growth, based on the conditions attached to the debt treatment and the type of creditors involved, remains uncertain and an open question. Building on information from the various compiled datasets, we first aim to empirically evaluate if the average effect of DR differs according to whether it involves a reduction in the face value—i.e., debt relief—or is limited to debt rescheduling. We further classify the types of debt restructuring (DR) by identifying whether each episode involved only official creditors (including Paris Club members and China), only private creditors (both external and domestic), or a combination of official and private creditors. While a more granular decomposition would have been desirable, the numerous potential combinations among official (Paris Club members, China) and private (external, domestic) creditors would have resulted in categories with too few countries to be analytically meaningful.

Table 7 presents the results of this decomposition, first reporting in column (1) the average effects obtained using the more restrictive specification from Table 3. Focusing first on columns (2) and (3), we find that debt rescheduling yields a significant positive effect on firm sales growth (between 8.66 and 10.04 depending on specifications). Coefficients associated with Face Value Reduction (FVR) are negative but (between -3.77 and -2.99) but are statistically non significant. While FVR may initially free up resources allocated for debt repayments, other effects appear to offset this contribution to fiscal space, weighing on firms' activity. Such discrepancies in the effects of debt restructuring may reflect heterogeneity among creditors, as private external creditors typically agree to substantial haircuts when providing DR to sovereign debtors whereas public ones are in most cases adverse to forgiving debt and prefer debt rescheduling. In particular it may also reflect longer time lags in

[6]Including the Multilateral Debt Relief Initiative (MDRI) implemented from 2005 onward.

reaching FVR from private creditors than debt rescheduling from official ones (as shown in figure 6) .

Table 7: Debt Restructuring Impacts - By type of creditors

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. var.:	$Sales\_Growth_{i,k,j,(t-1;t-3)}$						
				Off. (1) vs. Priv. (2)		Ext. (1) vs. Dom. (2)	
By type of restruct.							
$POST\_DR_{j,(t-1;t-3)}$	7.94***						
	(2.52)						
$DR^{(Reschedul.)}_{j,(t-1;t-3)}$		10.04***	8.66***				
		(3.33)	(2.54)				
$DR^{(FVR)}_{j,(t-1;t-3)}$		-2.99	-3.77				
		(3.83)	(3.98)				
By type of creditors							
$DR^{(1)}_{j,(t-1;t-3)}$				11.41***	9.36***	11.05***	8.90***
				(3.53)	(2.58)	(3.66)	(2.69)
$DR^{(2)}_{j,(t-1;t-3)}$				2.99	6.94*	0.19	5.56
				(3.31)	(3.99)	(4.08)	(5.00)
$DR^{(Multiple)}_{j,(t-1;t-3)}$				-3.34	-12.75	4.57	-3.22
				(8.10)	(8.03)	(5.78)	(4.89)
p-val$_{(POST_DR=FVR)}$		0.000	0.001				
p-val$_{(1)=(2)}$				0.042	0.577	0.047	0.549
p-val$_{(1)=MULTIPLE}$				0.026	0.003	0.112	0.003
p-val$_{(2)=MULTIPLE}$				0.395	0.015	0.407	0.107
#	65,593	65,593	65,593	65,593	65,593	65,593	65,593
Controls	✓		✓		✓		✓
Dep. var. (mean)	4.589	4.589	4.589	4.589	4.589	4.589	4.589

Note: Robust standard errors in parentheses clustered at the country-level. Estimates are obtained using WBES sampling weights and all regressions include country and survey year×industry fixed effects. *, **, *** denote significance at the 10, 5, and 1% level, respectively.

Columns (4) and (5) of Table 7, show results when creditors are categorized as official creditors only ($DR^{(1)}j,(t-1;t-3)$), private creditors only ($DR^{(2)}j,(t-1;t-3)$), or multiple creditors ($DR^{(Multiple)}_{j,(t-1;t-3)}$), *i.e.*, restructurings that involved at least one official and one private creditor. Debt restructurings with official creditors are found to significantly boost firm sales growth by approximately 9.34-11.41 pp, whereas private-only DRs bring smaller gains (2.99-6.94), with weaker statistical significance (column 5). In contrast, restructurings involving solely private creditors are associated with a significant decrease in firm growth (by around -5 pp), with the negative effect being even larger (-22 pp) when both official and private creditors are involved. This substantial negative impact may reflect the severity of the debt crisis and the challenges of initiating DR and subsequent recovery, particularly in more complex and extensive restructuring cases. In contrast, restructurings involving coefficients of DR combining official and private creditors exhibit a negative sign, albeit non-significant.

This suggests that the challenges of initiating more complex and time consuming DRs may end up negating any positive impact for firms.

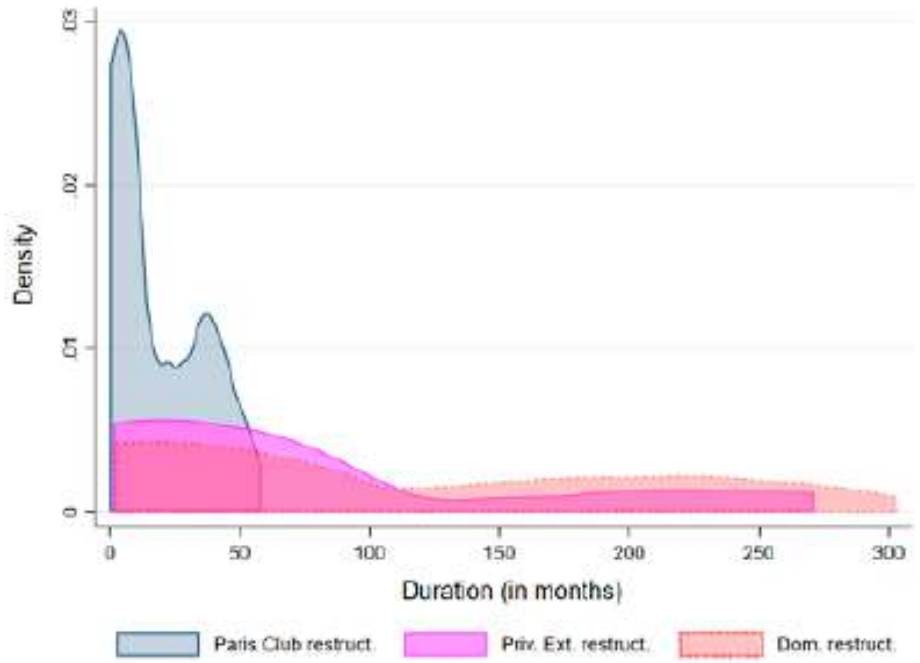
Lastly, columns (6) and (7) present differentiated results for domestic and external debt restructurings. DR involving external creditors (Paris Club or external private creditors) leads, on average, to a 10 percentage point increase in firm growth, with the magnitude of the effect closely aligning with results for restructurings involving solely official creditors. Domestic DR boost firms' sales by 0.2 to 5.5 pp, albeit the results are non-significant. This ambiguous outcome for DDR aligns well with earlier studies showing no clear impact of DDR on firms growth, as they represent a mere transfer of wealth between domestic agents with no net transfers from abroad. These results suggest that the negative effects observed for private creditor DR are primarily driven by domestic debt restructuring, as reclassifying episodes involving external private creditors from the private to the external category does not substantially alter the results. They also show that combined official and private restructurings and combined external and domestic ones exhibit negative coefficients (resp. -12.75 and -3.22), albeit not statistically significant, consistent with the conclusion that more complex and extensive DR episodes are not likely to spur private sector growth, particularly when associated with longer time lags.

Frictions in the debt restructuring process Building on the above results and leveraging the information on the start and end dates of debt restructuring processes provided in the datasets of Asonuma and Trebesch (2016) and Erce et al. (2022), we calculate the length of time of external private and domestic debt restructuring processes. The start dates correspond to the point at which the sovereign debtor begins to experience repayment difficulties, with arrears already accumulating. The end dates represent the conclusion of the DR process, marked by an agreement between private creditors and the sovereign debtor, involving either debt rescheduling or a haircut on the debt stock. We thus calculate the length of time of the restructuring process as the difference between the end date and the start date for each restructuring episode.

Establishing a comparable metric for official debt restructuring is more challenging. Pinpointing the start date is particularly difficult, as the accumulation of debt arrears may be tolerated or may reflect factors other than repayment difficulties. Using information specific to each official DR undertaken at the Paris Club, we attempt to identify a proxy for the length of time of the DR process. To access DR at the Paris Club, a debtor country must first agree to an IMF program. The program's approval requires assurances that Paris Club members will engage in debt restructuring negotiations. Once the members coordinate and reach an agreement with the sovereign debtor on the restructuring terms, all parties sign the DR agreement at the Paris Club. The time elapsed between the IMF program signing and the Paris Club agreement serves as a proxy for the length of time necessary to complete the DR process. Longer lengths of time may indicate higher coordination costs, either among Paris Club members or between Paris Club members and the debtor or lags reflecting instability or unforeseen events (exogenous shocks, political instability...). By collecting the dates of IMF program signings and Paris Club agreements, we calculate this proxy in the DR process provided by official creditors. This measure does not account for China, which has operated DR mostly on a bilateral basis during the period covered by our study.

Figure 5 below depicts the distribution of DR process lengths of time (lags or frictions) in months by creditor type, where such calculations were feasible—namely, external private, domestic, and official (Paris Club) creditors. Although the speed of these processes has accelerated since the mid-2010s, as shown in Figure A7 in the appendix, domestic and external private DR processes differ significantly from official DR in terms of duration. Debt restructurings involving private creditors tend to take longer, reflecting the challenges of reaching agreements that satisfy both parties.

Figure 5: Length of time in debt restructuring episodes - by Creditors



Notes: The figure displays the duration of debt restructuring processes for three types of creditors: Paris Club members, external private creditors, and domestic creditors. These creditor categories indicate that the respective type of creditor was involved in the restructuring. Consequently, if all three types of creditors participated in the same restructuring, the duration of that process is included in the distribution for each category. Source: Authors' calculation.

Examining the heterogeneous effects of DR based on process lags requires accounting for discrepancies between creditors. To address this, and to consider the various combinations of creditors involved in each restructuring episode, we categorize DR lags as either below or above the median for different creditor groups. The median lag is calculated over the distribution of process lags, excluding the top 1% of cases, as some countries experience restructuring processes spanning multiple years.

Table 8 first focuses on DR undertaken exclusively by official creditors, distinguishing between short (below median) and long (above median) processes. These are then compared to DR involving all other combinations of creditors—such as official creditors with private creditors, private creditors alone, or private creditors collectively (external and domestic)—again differentiating between short and long durations. Columns 2-7 focus on DR involving official creditors (alone or with other types of creditors) while columns 8-10 shed lights on the distinction between external vs domestic creditors.

First, the results confirm that the positive impact of DR on firms growth is mainly driven

Table 8: Debt Restructuring Impacts - By length of the DR process

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. var.:	$Sales\_Growth_{i,k,j,(t-1;t-3)}$									
		Off. involved [1] vs. Priv. only [2]			Off. only [1] vs. Others [2]			External only [1] vs. Others [2]		
$POST\_DR^{Short}_{j,(t-1;t-3)}$	11.40***									
	(2.69)									
$POST\_DR^{Long}_{j,(t-1;t-3)}$	1.66									
	(4.08)									
Group [1] *Length*										
$POST\_DR[1]^{Short}_{j,(t-1;t-3)}$		15.20***	16.79***	16.79***	15.63***	17.13***	17.12***	14.88***	16.06***	16.04***
		(4.18)	(4.91)	(4.91)	(4.24)	(4.96)	(4.95)	(3.90)	(4.40)	(4.49)
$POST\_DR[1]^{Long}_{j,(t-1;t-3)}$		-0.12	-4.70	-4.47	2.82	8.68*	8.74	0.18	2.04	2.27
		(5.20)	(5.17)	(10.09)	(3.46)	(4.99)	(16.68)	(3.38)	(3.33)	(4.37)
Group [2] *Length*										
$POST\_DR[2]^{Short}_{j,(t-1;t-3)}$		3.71	5.05*	5.22*	2.95	4.39	4.56	10.91***	13.81***	13.88
		(3.66)	(2.95)	(3.15)	(3.62)	(2.94)	(3.20)	(3.45)	(4.25)	(10.68)
$POST\_DR[2]^{Long}_{j,(t-1;t-3)}$		-2.01	3.93	2.29	-1.39	0.29	-1.62	-0.14	2.15	0.56
		(4.13)	(5.11)	(5.16)	(4.31)	(4.74)	(4.93)	(4.10)	(4.55)	(4.40)
Pre-trend check										
$PTREND^{[0/-6]}_{j,(t-1;t-3)}$				12.85			12.85			12.85
				(12.88)			(12.88)			(12.88)
$POST\_DR[2]^{Long}_{j,(t-1;t-3)}$				16.04*			16.04*			16.04*
				(9.19)			(9.19)			(9.19)
P-val (Short[1]≠Long[1])	0.114	0.069	0.042	0.042	0.037	0.204	0.613	0.010	0.023	0.041
P-val (Short[1]≠Short[2])		0.065	0.074	0.074	0.038	0.048	0.055	0.294	0.587	0.839
P-val (Short[1]≠Long[2])		0.000	0.003	0.003	0.000	0.002	0.000	0.000	0.005	0.000
#	63,785	63,785	63,785	63,785	63,785	63,785	63,785	63,785	63,785	63,785
Dep. var (mean)	8.335	8.335	8.335	8.335	8.335	8.335	8.335	8.335	8.335	8.335
Controls	✓		✓	✓		✓	✓		✓	✓

Note: Robust standard errors in parentheses clustered at the country-level. Estimates are obtained using WBES sampling weights and all regressions include same covariates as in the previous estimates as well as country and survey year×industry fixed effects. $POST\_DR^{Short}$ and $POST\_DR^{Long}$ denotes restructurings below and above median restructuring process length for each of the reported groups. *, **, *** denote significance at the 10, 5, and 1% level, respectively.

by DR with external official creditors either alone of involved with other creditors, with DR with private creditors showing smaller gains, with lower statistical significance. The smaller gains from DRs with private creditors can be attributed to DR from external private creditors more than domestic ones, given the lack of effects from DRs with domestic private creditors in column 9-10 (after controlling for parallel pre-trend). Second, only DR conducted within shorter time lags (below the median) exhibit strong positive coefficients. The coefficient for short DR is strongly positive (11.40) and significant, while the coefficient for long DR is much smaller (1.66) and not significant, suggesting that prolonged negotiations can erode the benefits of restructuring. Official creditors involvement in debt restructuring amplifies the benefit of short DR, reaching a peak when official creditors are the sole participants in the restructuring process (column 6 and 7, resp. 17.13 and 17.12). Short DR involving only private creditors (column 2 and 3) show a positive, albeit smaller estimated effect, with reduced significance. DR with only external creditors with short duration (column 8 to 10), exhibit large positive coefficients (between 14.88 and 16.06).

On the contrary, long DR bear mixed and non-significant results. This stark shift from

short DRs show that positive effects of DRs for firm growth can be negated by longer time lags. Longer time lags may reflect complexity in DRs processes with multiple stakeholders or delays related with macroeconomic shocks or political instability of the debtor country. It is also likely that the serial recourse to DR may reduce the positive impact of each debt treatment on the private sector. In any case, these results indicate that time efficiency is a key factor to spur firm growth and that coordination between private and public external creditors to shorten time lags may be key to that effect.

Overall, findings form Table 8 provide new evidence on the heterogeneous effects of debt restructurings and highlight the critical importance of delivering swift and targeted solutions to overindebted countries to minimize adverse effects on the broader economy.

Conclusion

Findings of this study suggest that firms experience a significant increase in sales growth rates following debt restructuring, with an average boost of 5 to 9 pp compared to their performance before restructuring and to non-DR countries. This positive impact remains robust even after accounting for various methodological adjustments and potential outliers. Additionally, the results are consistent even when narrowing the time frames for pre- and post-restructuring observations, reinforcing the conclusion that debt restructuring fosters improved firm performance.

Our findings offer insights into the impact of debt restructuring on private sector development and, by extension, economic growth. First, external debt restructuring, especially those led by the Paris Club, tends to stimulate private sector growth, while domestic debt restructuring may have a negative effect. This difference in impact could arise from the distribution of losses and gains among stakeholders. Official creditors debt restructuring provides net positive transfers to beneficiary countries, functioning as a form of international assistance, which as suggested in our study, effectively supports private sector growth. In contrast, domestic debt restructuring can have a negative impact, as financial losses are borne internally, including by the private sector, such as banks. Restructuring involving foreign private creditors can also create challenges in accessing global capital markets, as highlighted in the existing literature, which may significantly hinder the growth of firms, particularly those with international connections.

Second, most types of firms tend to benefit from debt restructuring, except state- and foreign-owned enterprises. However, our investigation of transmission channels examined through industry intensity highlights the crucial role played by human capital and finance. Firms structurally reliant on financial services as well as education and health sectors disproportionally benefit from debt restructuring. This findings shed light on the potential channels through which DR could affect private sector growth. Following debt restructuring, public policy measures are necessary to foster sustainable growth and development. In addition to policies aimed at improving the business climate, targeted measures for MSEs may be essential to ensure they can capitalize on debt restructuring opportunities, such as enhancing access to credit (financial inclusion) and information technologies. Further research is needed to establish a connection between the macroeconomic impacts of debt restructuring and financial stability issues.

Third, the effects of debt restructuring on firm growth also rely on speed. Speedy DR, particularly in the case of official creditors (Paris Club, China) have significant positive effects on firms, as they minimize uncertainty for private investment and anchor private sector expectations and help sustain demand. Time lags arising from coordination costs and complexity on the creditor side , as well as unexpected events in debtor countries (conflicts, political instability exogenous shocks), may delay DR implementation and hinder firm investment and growth, either minimizing potential DR gains or reversing them.

These results call for focusing on improving DR processes, as shown by progress in the G20-PC sponsored Common Framework implementation since 2022: early engagement of IMF with creditors and IMF policy, reaching consensus on Debt sustainability analysis and concepts of comparability of treatment e, selecting appropriate sequencing and timetables of debt treatment negotiations. The issue of parallel or sequential DR of official and private external creditors may be key to reduce time lags between agreements with public and private ones to ensure evenhandedness and reduce the need to include claw-back clauses in Paris Club Agreements. Building sustainable DR processes with optimal coordination of the growing and more diverse set of external creditors is still in the making.

Given the current threats on official development aid, debtor countries may need to further develop local currency financial markets to finance Sustainable Development Goals. Widening the investor base from domestic banking systems to international private investors will be essential to provide net positive cash flows to these countries. Coordination between domestic and external debt restructuring is likely to represent a growing policy challenge to optimize benefits of debt restructuring for private sector growth.

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Appendix

A1. Existing debt restructuring datasets

Paris Club restructuring The first comprehensive database on sovereign default towards official creditors was initiated by Das et al. (2012), who compiled accessible but non-manipulable data from the Paris Club website, encompassing most of the Paris Club agreements on official debt restructuring up to 2012. This effort was later expanded by Cheng et al. (2018), who gathered data on 422 official DR provided by Paris Club members from 1956 to 2015. Their dataset systematically details each debt treatment, including the beneficiary debtor, involved creditors, exact arrangement date, amount canceled if nominal relief was involved, and rescheduling terms. This dataset remains the most comprehensive publicly available information on Paris Club debt treatments? More recently, Horn et al. (2022) released an updated dataset which includes the year, amounts treated, and whether nominal relief was involved, covering 493 debt restructurings up to 2021. This dataset thus includes treatments granted under the *Debt-Service Suspension Initiative* (DSSI) and some debt rescheduling under the classic terms, which make up all of the treatment granted by the Paris Club since 2015.

Chinese Official restructuring Although Chinese development finance initiatives have been in place since the 1950s, particularly in African states (Brautigam, 2011), interest in China's role has surged with two major initiatives quantifying its lending policy and ODA outflows: the SAIS-CARI and the Chinese Official Finance Dataset (Dreher et al., 2021). Once China's position relative to traditional official lenders was established, attention gradually shifted towards assessing Chinese policy DR policies, particularly since the late 2010s and the Covid crisis amid the resurgence of over-indebtedness problems in developing countries. Similar to development finance flows, which were significantly underestimated before the publication of the aforementioned datasets, tracking debt restructurings —referred to by Chinese authorities as 'credit events'— is a challenging task, as they are conducted on a bilateral basis, with China traditionally operating outside the Paris Club.[7] Complexity may also result from the status of Chinese governmental banks and export credit agencies. They may operate with both public and private objectives (leading to different classifications by public authorities), and with some degree of opacity (Chen, 2024).[8] Despite these challenges and drawing upon specialized press, researchers such as Bon and Cheng (2020), Horn et al. (2021), and more recently Horn et al. (2022), have successfully compiled data on 149 'symbolic debt relief' instances and 55 debt restructuring operations conducted by Chinese authorities between 2000 and 2021. Even though details on these debt treatments are less comprehensive than other datasets, in particular on time lags, they provide enough essential information such as the date (year) and the type of restructuring (rescheduling versus nominal relief) for inclusion in the broader analysis of debt restructuring events.

[7] China holds observer status at the PC : it participates in the monthly "tour d'horizon" meeting, but not restructuring negotiations.

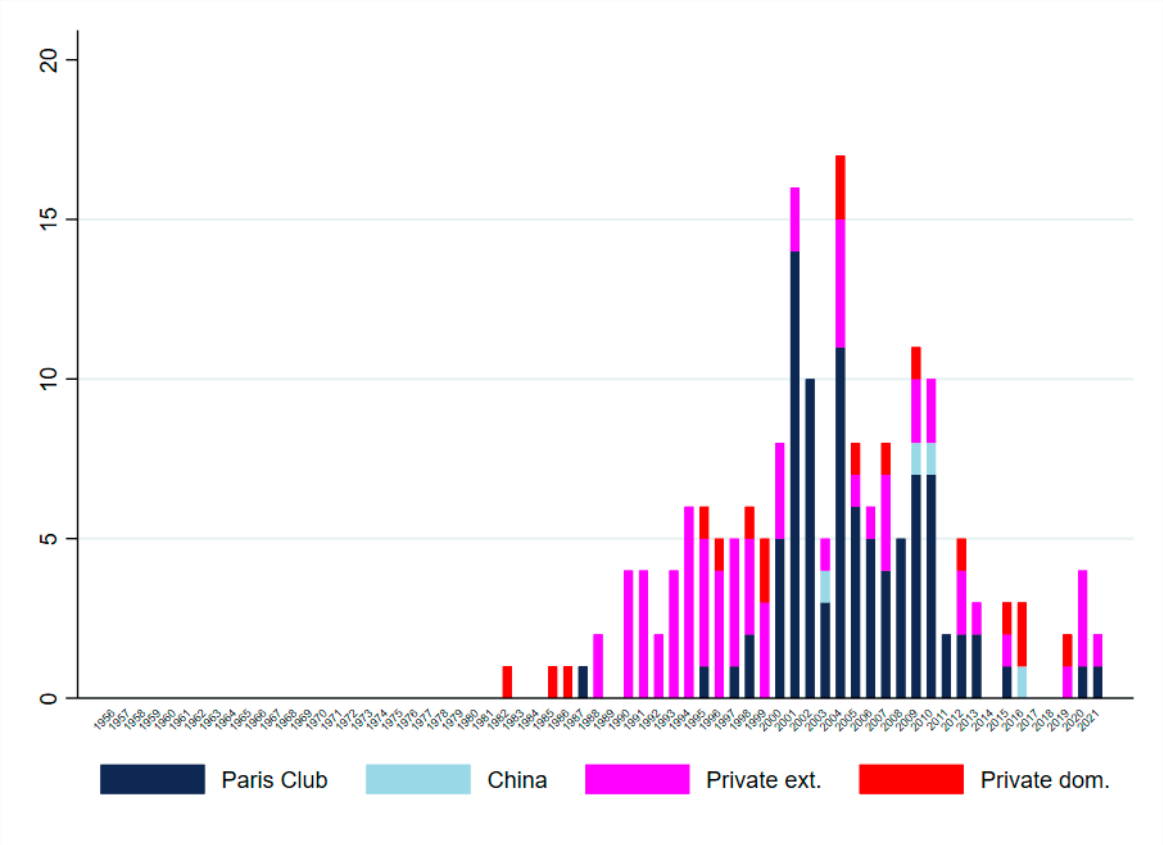
[8] In databases referred to in this study, these institutions are considered as public creditors.

Private External restructuring A growing number of developing countries have accessed international capital markets over the past 60 years, either as emerging countries (with continuing access) or as frontier countries (with only episodic access). Notable waves of market opening borrowing from private creditors by developing countries' government occurred, in the wake of a surge in financial liquidity due to rising oil prices and positive commodity price forecasts, followed by reversal of commodity prices after the second oil shock, combined with restrictive monetary policies, and massive debt crisis in developing countries (Debt overhang in the 80s). Similarly, the end of the super cycle of commodity prices from 2003 until 2014 resulted in large market swings and market lockouts, associated with rising rollover risks, especially since the Covid crises (lockout of African issuers in 2022-2023). To document such crises, Asonuma and Trebesch (2016) compiled the first comprehensive dataset on debt restructuring granted by external private creditors. This dataset complemented by Asonuma et al. (2023), records up to 202 DR from external private banks and bondholders' association between 1978 and 2021. It includes data on both entry and exit dates of the restructuring), as well as whether the agreement involves nominal relief.

Domestic Debt restructuring Data on domestic debt in developing countries, particularly the occurrence of domestic debt defaults, has been rather scarce and most studies focus on external or domestic debt separately. As noted by the Paris Club (2023), domestic DR follow distinct processes, its time line may not be coincidental with that of external DR, and there is not consensus on comparability of treatment. However, concurrent EDR and DDR are becoming more frequent, especially in the case of low and middle-income countries. Overall, Erce et al. (2022) finds that 30% of restructuring episodes in his sample involve both domestic and external debt. Together with its growing share of total public debt (46% in 2020, against 31% in 2000, see Grigorian and Grigorian (2023)), this makes it difficult to disregard when assessing potential impact of DDR on the domestic private sector. A first comprehensive dataset was consolidated by Erce et al. (2022): using various sources of information, they recorded up to 134 episodes of default on public debt governed by domestic law, referred to as domestic debt. This dataset encompasses episodes from emerging and developing economies between 1980 to 20211. Similarly to the Asonuma and Trebesch (2016) dataset, it provides information on the starting and ending dates of the restructuring, the debt instruments and amounts concerned, whether the restructuring included face value reduction, maturity changes, coupon changes, the share of the haircut in net present value, and whether the restructuring was preemptive or implemented post-default.

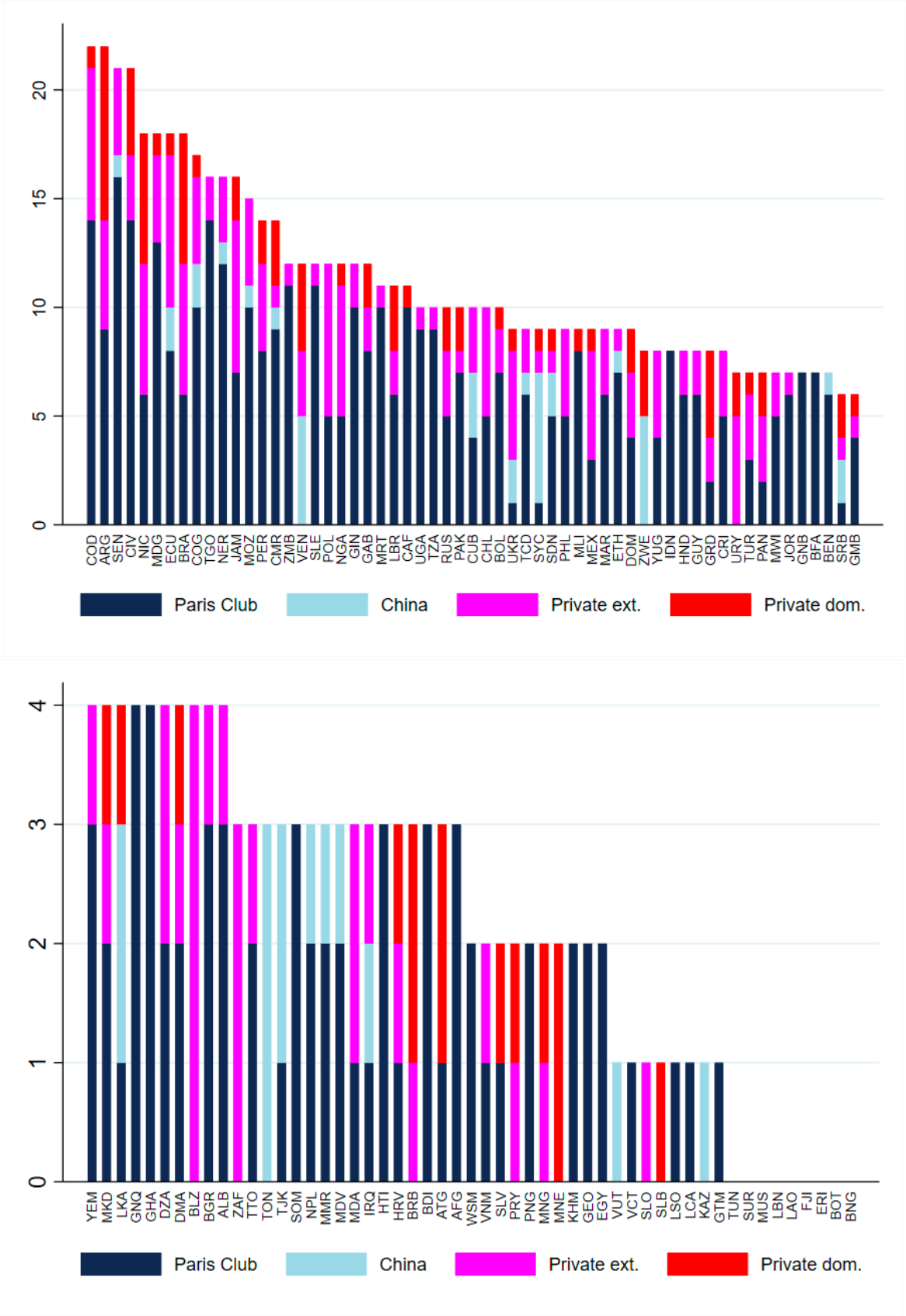
A2. Debt restructuring: additional statistics

Figure A1: Temporal Evolution of Debt Relief Treatments - by Creditors



Notes: Y axis reports the number of sovereign debt restructuring episodes, by type of creditors. Only debt restructurings involving nominal debt relief are considered. Data for Paris Club debt restructuring come from Cheng et al. (2018) and were completed with data from Horn et al. (2022). Data for Chinese debt restructurings come from Horn et al. (2022). Data for external private creditors restructurings are retrieved from Asonuma and Trebesch (2016) database while data for restructuring on domestic-law debt are from Erce et al. (2022).

Figure A2: Top Defaulters - Ranking by Number of Sovereign Debt Restructurings



A3. WBES selection: trimming rules and final sample

Sample trimming To observe DR impacts on the growth of sales, we need within-country variation in firm-level data. Therefore, we only consider countries with at least two waves of WBES data, *i.e.* for which we have two sets of firm-level data at different points in time. In addition, we focus our analysis on developing countries and exclude developed economies for which WBES data are available.[9] These initial exclusions reduce the sample from 159 countries to 105, with 175,221 observations. Next, we apply additional filters to align with the universe of inference described in the WBES sampling methodology which correspond to all formal private sector businesses with at least one percent of private ownership and of at least five employees. Consequently, we drop fully state-owned enterprises, enterprises that have never been formally registered, and those with fewer than five employees. Additionally, we exclude firm affiliates located in the same country with no separate financial statements, as the unit of analysis is a business entity with its own set of financial statements.. We also exclude cooperative or collective establishments that are not within the universe of inference. Finally, we discard observations flagged by interviewers as "not truthful" regarding opinions, perceptions, or as containing "arbitrary and unreliable numbers" related to firm metrics such as production, productivity, or number of employees. These additional filters reduce the sample size to 161,913 firms across 105 countries.

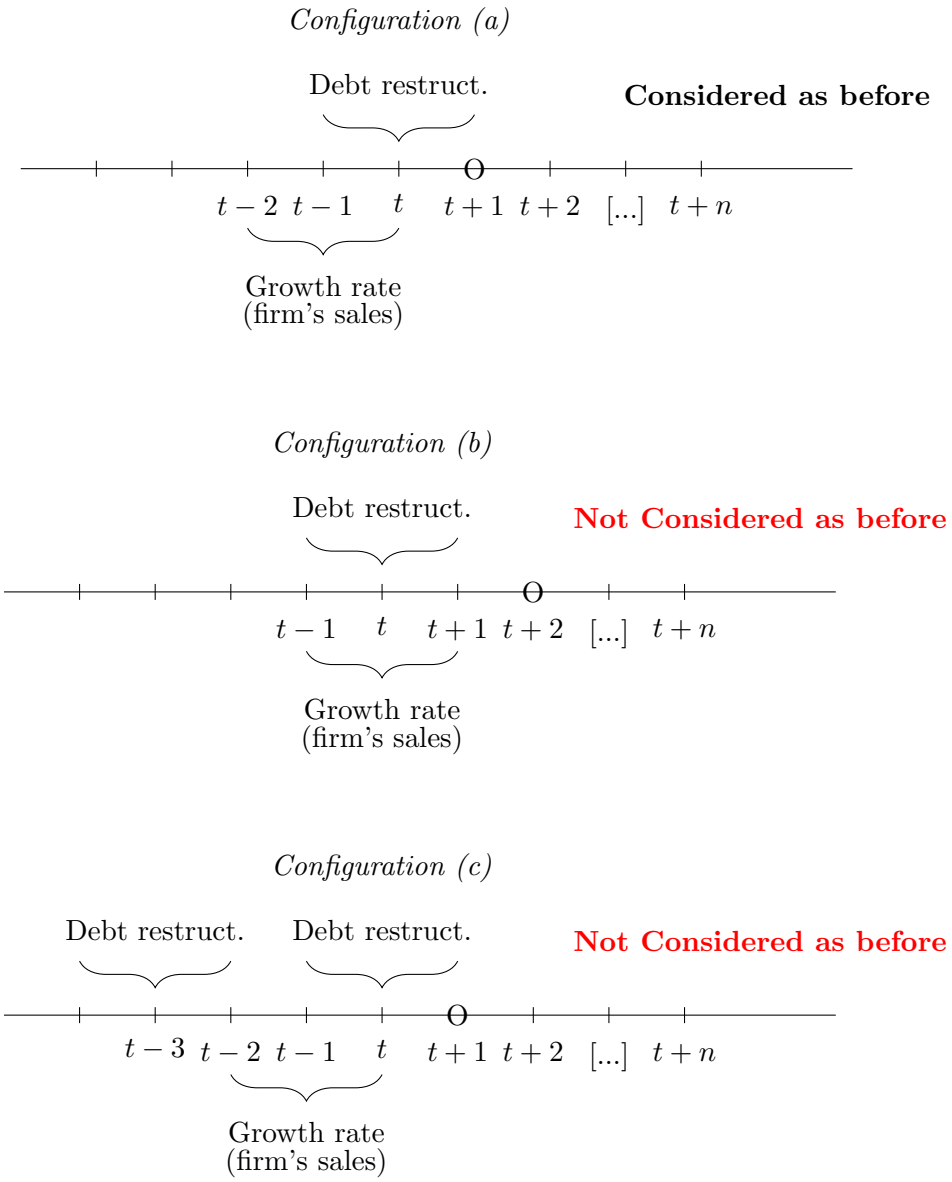
Selection of WBES waves Examining the impact of DR on the performance of formal private firms requires observing firms' outcome before and after their country's government benefited from sovereign debt restructuring. This allows for comparing the before-and-after difference with what occurred during the same periods in other, preferably similar, countries that did not benefit from such restructuring (yet, or never will). This forms the difference-in-difference setting framework often used to evaluate the impact of policies or reforms. In our context, the definition of periods before and after debt restructuring is constrained by *i)* the year of the WBES and *ii)* the year of the occurrence of sovereign debt restructuring. Even though the year of the WBES is t, the period over which we compute firms' growth rate of sales is from $t-1$ to $t-3$. This allows us to consider a WBES as a 'before debt-restructuring' observation if the restructuring occurs the same year as the WBES.

In addition, we applied specific criteria regarding the selection of WBES to be included in the analysis. Since we are interested in the impact of debt restructuring, we consider as 'before-restructuring' observations, WBES conducted before the year of the restructuring or one year after at the latest. This approach allows us to capture firms' sales growth rates with the last point used for calculating the growth rate being, again at the latest, the year of the restructuring, as we expect to observe some effects in the aftermath of the restructuring. Configurations (a) and (b) in Figure A3 in the appendix illustrate the different scenarios in which we select WBES data and classify it as a 'before-restructuring' observation. As some countries experienced multiple debt restructurings during the study period, we ensure that the year of the selected WBES should not be preceded by a debt restructuring over the last

[9] We thus drop from the sample WBES waves for Antigua and Barbuda (2010), Belize (2010), Congo Rep. (2009), Djibouti (2013), Dominica (2010), Fiji (2009), Gabon (2009), Grenada (2010), Guinea-Bissau (2006), Micronesia (2009), Papua New Guinea (2015), Seychelles (2023), St. Vincent and Grenadines (2010), Tonga (2009), Trinidad and Tobago (2010), and Vanuatu (2009).

5 years. This requirement provides two years free from debt restructuring before the period over which the sales growth rate is calculated (i.e. between $t-3$ and $t-1$). Configuration (c) in A3 illustrates a scenario where we exclude WBES because of the temporal proximity of a previous debt restructuring episode.

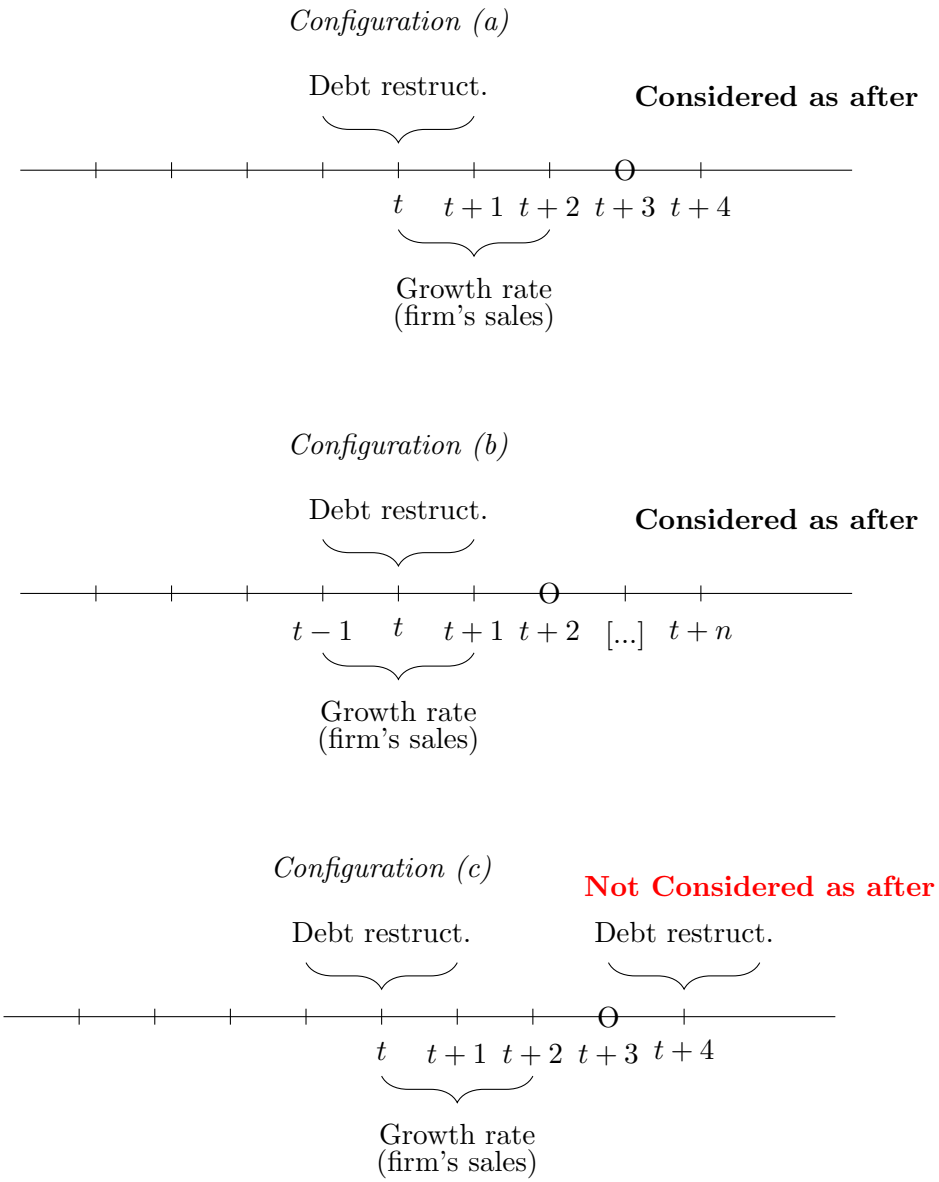
Figure A3: Defining 'before-restructuring' WBES (O = WBES year)



Similarly, for 'post-restructuring' observation (see Figure A4), we restrict the sample to WBES where the survey year is at least two years post-restructuring, allowing the final point for growth rate calculation to be after the restructuring. We also exclude WBES that meet this requirement but are too close to a new debt restructuring, ensuring a gap of at least two years between the last point of the growth rate and a subsequent restructuring.

Figure 2 shows the impact of these rules on WBES selection. For some countries, although

Figure A4: Defining 'post-restructuring' WBES (O = WBES year)



multiple WBES are available from the early 2000s onwards, the year of WBES and the multiple debt restructuring they encountered prevent us to consider them in the analysis. This applies for Argentina for instance which recorded sovereign debt restructurings every 3 years on average, thus preventing to have 'before' and 'post-restructuring' observations free from additional debt restructurings. Ultimately, we retain most of the WBES (see WBES selected in the Figure 2) for countries having experienced debt restructuring since the early 2000s, which therefore constitute the 'treatment' group. In addition to these WBES, we consider all other surveys in countries that did not experience debt restructurings over the study period, and that make up for the 'never-treated' control group.

Table A1: Sample Composition

Coutries with debt restructuring over the study period

Country	*N*	*year of survey*	*Country*	*N*	*year of survey*	*Country*	*N*	*year of survey*
Afghanistan	332	2008, 2014	Ghana	1,253	2007, 2013, 2023	Nepal	1,275	2009, 2013, 2023
Barbados	250	2010, 2023	Guinea	196	2006, 2016	Nicaragua	550	2010, 2016
Benin	226	2009, 2016	Honduras	420	2010, 2016	Nigeria	2,648	2007, 2014
Bolivia	698	2006, 2010, 2017	Indonesia	3,159	2009, 2015, 2023	Pakistan	2,026	2007, 2013, 2022
Bosnia & Herz.	1,087	2009, 2013, 2019, 2023	Jordan	531	2013, 2019	Paraguay	1,087	2006, 2010, 2017, 2023
Burundi	327	2006, 2014	Kazakhstan	1,446	2009, 2013, 2019	Peru	2,644	2006, 2010, 2017, 2023
Cameroon	608	2009, 2016	Kenya	1,879	2007, 2013, 2018	Rwanda	739	2011, 2019, 2023
Central Afr. Rep.	104	2011, 2023	Kyrgyz Rep.	877	2009, 2013, 2019, 2023	Samoa	194	2009, 2023
Chad	401	2009, 2018, 2023	Lesotho	362	2009, 2016, 2023	Senegal	717	2007, 2014
Congo, DR.	454	2010, 2013	Liberia	250	2009, 2017	Sierra Leone	443	2009, 2017, 2023
Cote d'Ivoire	825	2009, 2023	Madagascar	799	2009, 2013, 2022	Tajikistan	378	2008, 2013
Dominican Rep.	464	2010, 2016	Malawi	390	2009, 2014	Tanzania	1,109	2006, 2013, 2023
Ecuador	1,048	2006, 2010, 2017	Mali	391	2007, 2010, 2016	Togo	326	2009, 2016, 2023
El Salvador	1,703	2006, 2010, 2016, 2023	Moldova	880	2009, 2013, 2019	Uganda	879	2006, 2013
Ethiopia	832	2011, 2015	Mongolia	956	2009, 2013, 2019	Ukraine	1,469	2008, 2013, 2019
Gambia, The	295	2018, 2023	Montenegro	401	2009, 2013, 2019, 2023	Yemen	395	2010, 2013
Georgia	1,224	2008, 2013, 2019, 2023	Myanmar	912	2014, 2016	Zambia	1,265	2007, 2013, 2019

Coutries with no debt restructuring over the study period

Country	*N*	*year of survey*	*Country*	*N*	*year of survey*	*Country*	*N*	*year of survey*
Albania	557	2007, 2013, 2019	Eswatini	304	2006, 2016	Philippines	2,49	2009, 2015, 2023
Angola	345	2006, 2010	Guatemala	1,001	2006, 2010, 2017	Romania	2,259	2009, 2013, 2019, 2023
Armenia	732	2009, 2013, 2020	India	12,836	2014, 2022	South Africa	1,868	2007, 2020
Azerbaijan	519	2009, 2013, 2019	Kosovo	270	2013, 2019	Suriname	319	2010, 2018
Bangladesh	2,931	2007, 2013, 2022	Laos	1,117	2009, 2012, 2016, 2018	Thailand	591	2004, 2016
Belarus	819	2008, 2013, 2018	Lebanon	860	2013, 2019	Timor-Leste	372	2009, 2015, 2021
Bhutan	436	2009, 2015	Macedonia, FYR	1,162	2009, 2013, 2019, 2023	Tunisia	884	2013, 2020
Botswana	917	2006, 2010, 2023	Mauritania	284	2006, 2014	Turkey	2,277	2008, 2013, 2019
Cambodia	683	2016, 2023	Mauritius	548	2009, 2023	Uruguay	971	2006, 2010, 2017
Colombia	2,954	2006, 2010, 2017, 2023	Mexico	3,132	2006, 2010, 2023	Uzbekistan	1,239	2008, 2013, 2019
Costa Rica	529	2010, 2023	Morocco	2,227	2004, 2013, 2019, 2023	Vietnam	2,264	2009, 2015, 2023
Croatia	1,561	2007, 2013, 2019, 2023	Namibia	438	2006, 2014	West Bank & G.	809	2013, 2019, 2023
Egypt	5,46	2004, 2013, 2016, 2020	Panama	431	2006, 2010			

A4. Robustness Tests

Figure A5: TWFE Estimates - Sample Dependence Test

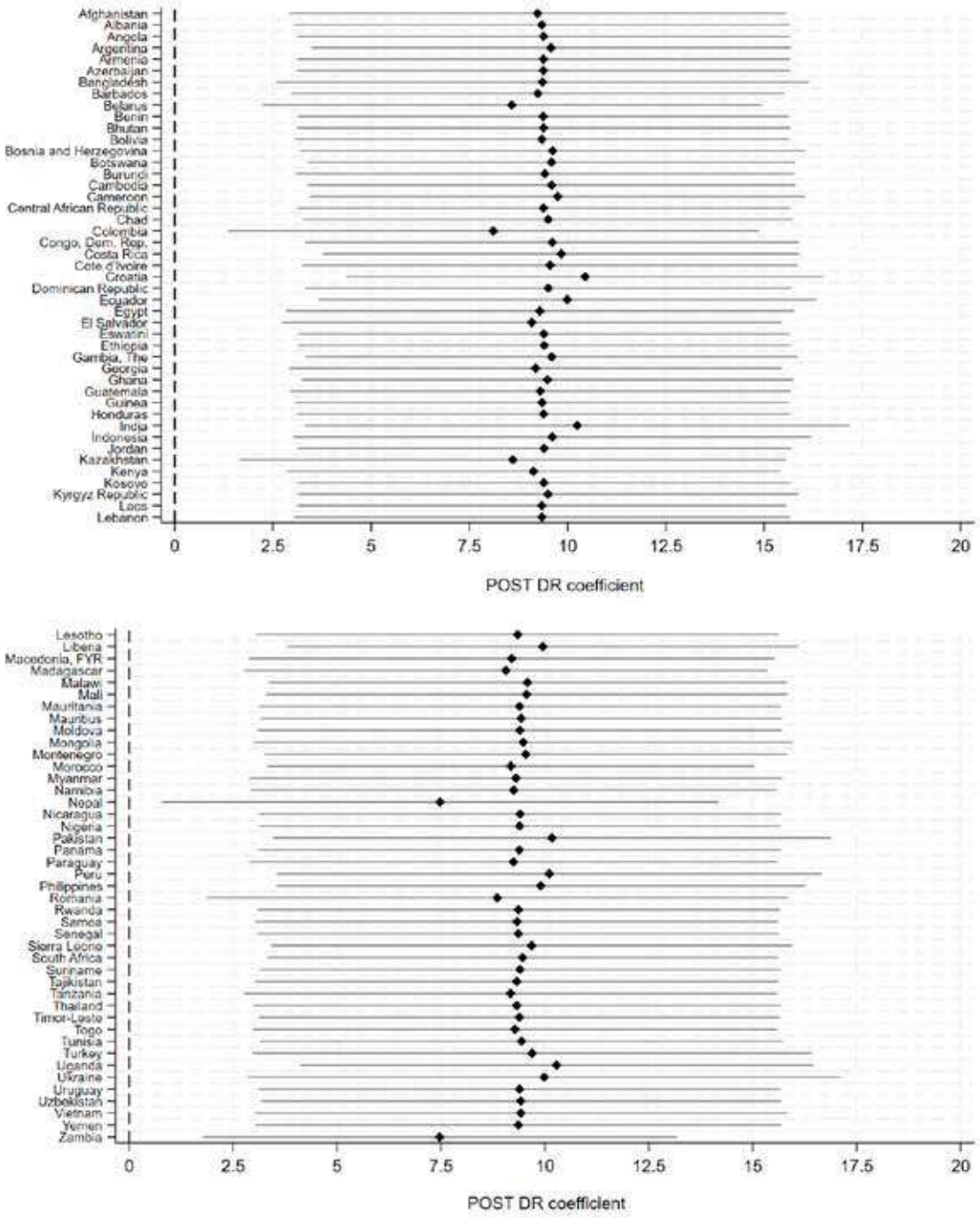


Table A2: Removing Firm with Growth Rate and Debt Restructuring the Same Year

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.:	$Sales\_Growth_{i,k,j,(t-1;t-3)}$					
Panel A- DID TWFE						
$POST\_DR_{j,(t-1;t-3)}$	5.64	7.72*	9.24**	5.66	8.56**	9.80***
	(3.49)	(3.90)	(3.68)	(3.60)	(3.86)	(3.66)
Dep. var. (mean)	5.075	5.075	5.075	5.075	5.075	5.075
#	89,072	89,072	89,072	89,072	89,072	89,072
Panel B- DID Imputation						
$POST\_DR_{j,(t-1;t-3)}$	8.53***	7.34***	5.79***	9.31***	8.52***	7.97***
	(3.09)	(2.84)	(2.14)	(3.28)	(3.08)	(2.54)
Dep. var. (mean)	8.147	8.147	8.147	8.117	8.117	8.117
#	64,376	64,376	64,376	64,286	64,286	64,286
Panel C - DID Imputation						
$POST\_DR_{j,(t-1;t-3)}$	10.90**	9.33*	8.05*	11.84**	10.76**	10.34***
	(5.09)	(4.84)	(4.16)	(4.93)	(4.32)	(3.68)
$POST\_DR_{j,(t-1;t-3)}$	-10.58	-4.80	2.22	2.19	9.69	12.92
	(13.06)	(13.96)	(15.44)	(11.14)	(11.87)	(12.92)
$PTREND^{[-7/-12]}_{j,(t-1;t-3)}$	4.56	5.76	9.71	12.33	14.15	16.03*
	(10.17)	(10.24)	(10.35)	(9.01)	(8.95)	(9.19)
Dep. var. (mean)	8.147	8.147	8.147	8.117	8.117	8.117
#	64,376	64,376	64,376	64,286	64,286	64,286
Control		✓	✓		✓	✓
svy-year-FE	✓	✓	✓			
indus.-FE	✓	✓	✓			
svy_year x indus. FE				✓	✓	✓

Note: Robust standard errors in parentheses clustered at the country-level. Estimates are obtained using WBES sampling weights and all regressions include country fixed effects. *, **, *** denote significance at the 10, 5, and 1% level, respectively.

Figure A6: Distribution Density around Debt Restructuring Episodes

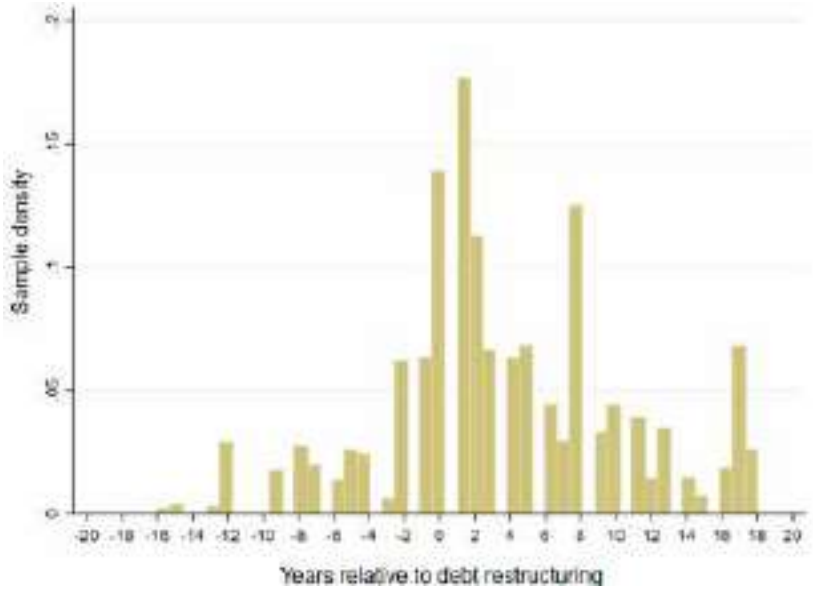


Table A3: Shortening the Timeframe Around Debt Restructuring Episodes

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.:	$Sales\_Growth_{i,k,j,(t-1;t-3)}$					
	DID TWFE			*DID Imputation*		
	Years dropped relative to sovereign debt restructuring year					
	>7	>6	>5	>7	>6	>5
$POST\_DR_{j,(t-1;t-3)}$	8.98***	8.76***	10.22***	7.94***	7.94***	8.31***
	(3.40)	(3.25)	(3.13)	(2.52)	(2.52)	(2.51)
#	80,481	79,515	78,306	65,593	65,593	65,460
Dep. var. (mean)	4.899	4.904	4.905	8.395	8.395	8.299
	<-7	<-6	<-5	<-7	<-6	<-5
$POST\_DR_{j,(t-1;t-3)}$	8.65***	9.23***	9.19**	7.51***	9.03***	10.97***
	(3.22)	(3.47)	(3.65)	(2.36)	(2.70)	(4.05)
#	89,618	89,196	88,818	61,289	60,590	59,089
Dep. var. (mean)	4.842	4.670	4.657	7.828	7.750	7.619
	<-7 & >7	<-6 & >6	<-5 & >5	<-7 & >7	<-6 & >6	<-5 & >5
$POST\_DR_{j,(t-1;t-3)}$	7.84**	8.60**	10.69***	7.51***	9.03***	11.72***
	(3.39)	(3.39)	(3.47)	(2.36)	(2.70)	(4.19)
#	77,881	76,493	74,906	61,289	60,590	58,956
Dep. var. (mean)	4.802	4.618	4.604	7.828	7.750	7.511

Note: Robust standard errors in parentheses clustered at the country-level. Estimates are obtained using WBES sampling weights and all regressions include controls, country and survey-year × industry fixed effects. *, **, *** denote significance at the 10, 5, and 1% level, respectively.

Table A4: Restricting "never-treated units" to debt distress countries

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. var.:	$Sales\_Growth_{i,k,j,(t-1;t-3)}$					
	DID-TWFE			*DID Imputation*		
$POST\_DR_{j,(t-1;t-3)}$	6.78*	9.55***	10.53***	10.62***	6.37***	4.92**
	(3.45)	(3.36)	(3.51)	(2.05)	(2.00)	(1.99)
#	63,543	63,543	63,543	36,122	36,122	36,122
Dep. var. (mean)	6.591	6.591	6.591	9.355	9.355	9.355
Control firm-level		✓	✓		✓	✓
Control country-level[a]			✓			✓

Note: Robust standard errors in parentheses clustered at the country-level. Estimates are obtained using WBES sampling weights and all regressions include country and survey-year × industry fixed effects. [a] Population ($POP_{j,(t-3;t-5)}$) is excluded from the set of control variables. Including population leads to significant drop in statistical significance as it drastically change the distribution of the population variable in the reduced sample. *, **, *** denote significance at the 10, 5, and 1% level, respectively.

A5. Transmission channels: intensity measures and additional results

Table A5: Structural dependence by industry

	Intensity in:					
	Institutions	External Finance	Public Utilities	Transport & Construct.	Finance	Human Capital
Manufacturing industries						
Basic metals	-0.17	0.00	0.03	2.78	0.03	0.20
Chemicals & chem. products	-0.14	1.09	0.49	2.24	0.46	8.68
Coke & refined petrol. prod.	-0.22	0.24	5.35	20.5	0.47	1.12
Computer & electronic product	-0.09	0.72	0.39	9.63	0.57	0.98
Fabricated metal prod., exc. mach. & equip.	-0.11	0.24	0.08	19.9	0.13	0.54
Food & beverages products	-0.19	0.07	0.01	0.11	0.02	2.53
Leather products	-0.19	-0.37	0.05	1.92	0.05	1.54
Machinery & equipment	-0.06	0.45	0.62	7.02	0.15	0.64
Motor vehicles & transp. equipment	-0.09	0.43	0.14	4.55	0.45	0.36
Plastic & non-metal products	-0.08	2.68	0.14	26.0	0.34	3.01
Textiles & garments	-0.15	0.51	0.05	1.92	0.05	1.54
Woods, paper, & furniture	-0.22	0.16	0.03	17.5	0.58	1.88
Other furniture	-0.08	-0.04	0.10	5.07	0.46	8.68
Printing & publishing	-0.14	0.48	0.47	1.67	8.25	5.54
Other manufacturing	-0.10	2.10	0.10	5.07	0.46	8.68
Services industries						
Accommodation & food services			0.90	1.90	2.70	1.80
Construction			0.40	0.20	0.40	0.10
Information & communication			0.70	1.90	4.90	2.30
Professional activities			1.60	3.80	6.70	3.50
Transport & storage			2.70	13.30	2.40	2.10
Wholesale trade & retail			0.60	6.10	0.40	1.30
Wholesale trade & repair, motor Vehicles			0.20	2.20	1.60	1.10
Other services			0.30	2.00	2.10	1.60
Median	-0.14	0.43	0.30	3.80	0.46	1.60

Note: Structural intensity data in institutions come from Levchenko (2007), while these related to external financing have been retrieved from Rajan and Zingales (1998). The remaining structural intensity indices have been computed with data from the World Input-Output Database (Timmer et al., 2015).

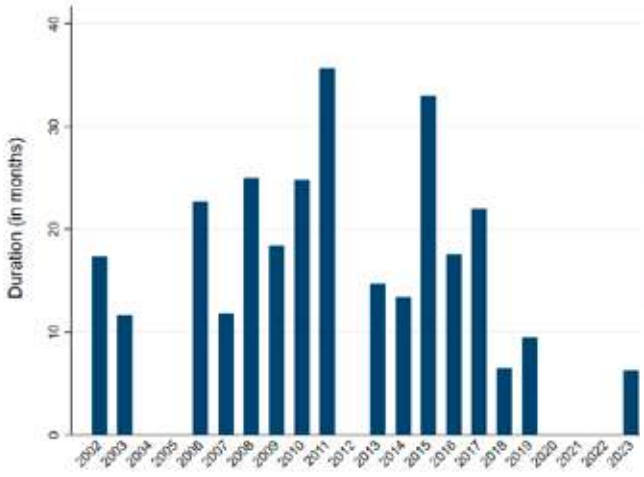
Table A6: Two-Way Fixed Effects Estimates - Industry Heterogeneity Results

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dep. var.:	$Sales\_Growth_{i,k,j,(t-1;t-3)}$							
	Whole Sample		Manufacturing only		Whole Sample			
Source:			Levchenko	R&J	WIOD			
Intensity in:			Institutions	External. Finance	Public Utilities	Transport & Construct.	Finance	Human Capital
			$INTENSITY = 1$ if $\geq$ median intensity; 0 otherwise					
$POST\_DR_{j,(t-1;t-3)}$	9.20***							
	(2.79)							
$POST\_DR_{j,(t-1;t-3)}$		7.88***						
		(2.60)						
$POST\_DR_{j,(t-1;t-3)}$		2.07						
$\times SERVICES_{(0/1)}$		(1.38)						
$POST\_DR_{j,(t-1;t-3)}$			5.30*	1.66	7.24***	10.77***	5.84**	6.30**
			(3.03)	(3.89)	(2.65)	(2.98)	(2.68)	(2.76)
$POST\_DR_{j,(t-1;t-3)}$			0.61	4.74*	2.69**	-4.08**	4.57***	3.62**
$\times INTENSITY_{(0/1)}$			(0.73)	(2.45)	(1.34)	(1.82)	(1.59)	(1.63)
Joint-sig. (p-val)		0.003	0.155	0.021	0.002	0.001	0.000	0.002
#	98,619	96,474	47,952	47,952	90,073	90,073	90,073	90,073
Dep. var. (mean)	4.549	4.357	3.717	3.717	4.729	4.729	4.729	4.729

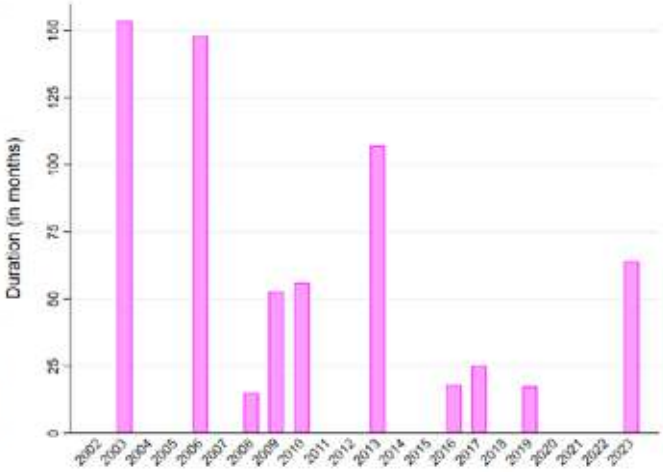
Note: Robust standard errors in parentheses clustered at the country-level. Estimates are obtained using WBES sampling weights and all regressions include country and survey year×industry fixed effects as well as country and firm-level controls. *, **, *** denote significance at the 10, 5, and 1% level, respectively.

A6. Frictions in debt restructuring: additional statistics

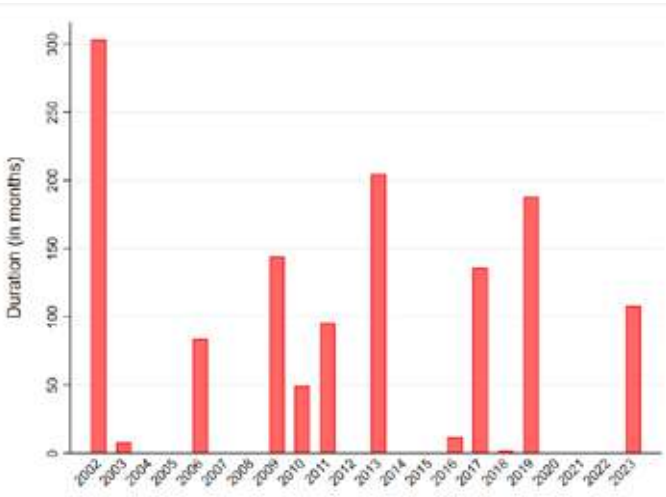
Figure A7: Duration of debt restructuring process - by creditors (yearly average)



a) Paris Club creditors



b) External private creditors



c) Domestic debt creditors

Notes: Figures report the average of the duration for annual debt restructuring, by creditors for which we could retrieve such information *i.e.* Paris Club creditors (in dark blue), private external creditors (in magenta), and domestic debt creditors (in red). Average are computed on a yearly basis and at the country level (so the number of firms surveyed in each country does not influence the mean values).

“ Sur quoi la fondera-t-il l’économie du monde qu’il veut gouverner ? Sera-ce sur le caprice de chaque particulier ? Quelle confusion ! Sera-ce sur la justice ? Il l’ignore. ”

Pascal

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