Capital Flows and the Real Effects of Corporate Rollover Risk

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ABSTRACT

What are the real costs of reversals in international capital flows? In this paper, I exploit plausibly exogenous variation in firms' exposure to rollover risk to identify a causal liquidity channel at play during sudden stop episodes. Using a panel of firms across 39 countries, I show that firms with higher exposure (as measured by the share of long-term debt maturing over the next year) reduce investment ten percentage points more than non-exposed firms following sudden stops in capital flows. The impact is persistent: exposed firms experience lower investment, lower employment and lower assets than non-exposed firms even three years after the initial shock. In robustness tests, I show that the results are specific to sudden stop episodes in that they do not hold in periods without sudden stops, and they hold across sudden stop episodes regardless of whether the sudden stop takes place during large economic contractions.

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1 Introduction

The costs and benefits of volatile international capital flows remain a key issue for policymakers and the object of a large literature. The global financial crises brought the issue to the forefront once again, showing that capital flow volatility can affect advanced and emerging economies alike. Policymakers' shifting views on capital flows are best exemplified by the new IMF's 'Institutional View on the Liberalization and Management of Capital Flows.'[1] Despite policy changes, concrete empirical evidence on the costs and transmission channels of sudden stops remains scarce.

Identifying a causal effect of capital flows at the aggregate level is notoriously hard as capital flows are endogenous. Capital flows respond to expectations of future macroeconomic developments and so, any observed relationship between capital flows and subsequent economic performance could be explained by reverse causality or omitted variables. Thus, the empirical macro literature has focused on highlighting how large reversals in capital flows *correlate* or *predict* large macroeconomic events such as financial crises or large contractions in economic activity. Despite this progress, empirically identifying the causal effects of sudden stops and showing the channel through which these effects operate remains a challenge.

In this paper, I establish a causal liquidity channel at play during sudden stops in capital flows, by exploiting plausibly exogenous cross-sectional variation in the level of firm exposure to liquidity shocks. More specifically, I exploit heteregoneity in firms' need to rollover long-term debt during sudden stops to show that firms with higher refinancing needs contract investment more than similar firms that do not need to rollover long-term debt. The exogeneity of this measure of exposure to rollover risk allows me to estimate the causal effect of being exposed to liquidity shocks during a sudden stop episode.

My identification strategy rests on finding exogenous variation in firms' exposure to rollover risk and on identifying episodes of large contractions in capital flows. Following Almeida, Campello, Laranjeira, and Weisbenner (2011), I measure exposure to rollover risk using the share of long-term debt maturing over the next twelve months.

[1] IMF (2012) argues that "For countries that have to manage the macroeconomic and financial stability risks associated with inflow surges or disruptive outflows, a key role needs to be played by macroeconomic policies, including monetary, fiscal, and exchange rate management, as well as by sound financial supervision and regulation and strong institutions. In certain circumstances, capital flow management measures can be useful."

The main advantage of this measure is that it is largely predetermined at the moment of debt issuance (potentially years in advance) and as such is less affected by recent developments. This is a key advantage in that it allows me to get around the issue of endogeneity that confounds most alternative measures of rollover risk. Usual measures of exposure to rollover risk, such as leverage, short-term debt, or maturity mismatches are problematic as they are correlated with other firm characteristics that might explain a firm's response to a shock. For instance, Barclay and Smith Jr (1995) shows that the decision to issue short-term debt vs. long-term debt is correlated with firm characteristics such as size and profitability. The measure of exposure I use circumvents this endogeneity issue: even if firms optimally pick their share of long-term debt, the amount of long-term debt that is maturing over the next twelve months can be considered exogenous.

In order to test the effect of high exposure to rollover risk, I identify periods with large contractions in capital flows. Following Forbes and Warnock (2012, 2020), my methodology centers around identifying periods in which the drop in capital flows is large relative to the recent path of capital flows in the given country. In the main results, I focus on sudden stop episodes identified using data on gross debt portfolio inflows. This is the debt portfolio component of foreign flows into a country.

My identification strategy does not rely on finding exogenous shocks to capital flows but rather, on exploiting exogenous cross-sectional exposure to plausibly endogenous shocks to capital flows. The identification assumption is then that any aggregate level shocks that could cause large capital flows do not differentially affect exposed and non-exposed firms in a systematic way. This is a plausible assumption: consider two similar firms financing themselves with 5-year bonds. Firm X issued its bonds four years ago, and hence has a large portion of debt to refinance next year, but firm Y issued its bonds two years ago, making its refinancing needs for next year much smaller. The identification assumption is that whatever the cause of the capital outflows is, the shock does not affect firms X and Y differently, other than for the fact that firm X has higher refinancing needs than firm Y. This is the causal liquidity channel I identify in this paper.

The main finding of this paper is that firms with higher exposure (as measured by the share of long-term debt maturing over the next year) reduce investment ten percentage points more than non-exposed firms following sudden stops in capital flows.

This result is highly statistically significant and holds across a number of alternative specifications: different sets of fixed-effects as well as different sets of controls do not significantly affect the results. Moreover, the impact is persistent: exposed firms experience lower investment, lower employment and lower assets than non-exposed firms even three years after the initial shock. I find these results in a large cross-country panel of firms over the period 1980-2019. The sample is composed of over 700,000 firm-year observations in a panel of 39 advanced and emerging economies.

The main contribution of this paper is that it identifies a causal liquidity channel that amplifies the real effects of financial frictions during sudden stop episodes. Three aspects of the setting in which I find these results are relatively new to the literature and thus, worth noting. First, by using gross inflow data (as opposed to net inflows), I identify a number of sudden stop episodes that would have not been identified using net inflows data. This result suggests that large reversals in investment by foreigners have real effects even if at the aggregate level 'retrenchment' behavior by local investors makes up for some of the contraction in credit supply.

Second, I find a large effect of financial frictions in a set of episodes that, to a large extent, are not accompanied by large macroeconomic events. This is because the episodes that I identify are not defined by current or subsequent GDP contractions, but instead, the only requirement is a large drop in capital flows. That is, even though I identify periods with large outflows of foreign capital, in most cases these periods do not come hand-in-hand with financial crises, large devaluations, or even large disruptions in economic activity. This is a significant departure from most papers that study the effects of large credit supply shocks on the cross-section of firms. These studies usually focus on large credit events like the global financial crisis and hence, their results come from periods of large economic disruption. In fact, my results hold if I exclude from the sample periods with large recessions.

Third, by the nature of my dataset, my results identify a negative real effect of credit frictions on very large, mostly public firms. This is a somewhat new result considering that studies that explore the cross-sectional effects of financial frictions usually find that larger firms are the least impacted. For instance, Chodorow-Reich (2014) explores the effects of a large credit supply shock on employment and finds that smaller, more financially constrained firms, suffer the most.

In robustness tests, I run a number of placebo tests to show that the results are specific to sudden stop episodes: they do not hold in periods without sudden stops, and they hold across sudden stop episodes regardless of whether the sudden stop takes place during large economic contractions. I also explore alternative definitions of my measure of exposure to rollover risk and show how these changes affect the main results.

My results have clear policy implications. First, they contribute to the body of evidence on the real negative effects of capital flow reversals that has led to the implementation of capital flow management measures. In this sense, my results provide further justification for measures that aim to reduce volatility in capital flows. For instance, policies that incentivize capital flows that pursue long-term investments over portfolio flows that are considerable more fickle.

Second, and more importantly, my results have policy implications in terms of highlighting the importance of active maturity management as a way for firms to hedge against large reversals in capital flows. Thus, policies oriented to incentivize active maturity management that leads to spreading maturities over time and minimizes the likelihood of 'being caught' with large portions of debt coming due at the time of a sudden stop can have large real benefits. Policies aimed at reducing firms' exposure to rollover risk could be added to the standard toolkit of macroprudential tools that attempt to reduce firms and banks exposure to sudden changes in credit conditions.

This paper is related to various strands of literature. First, and foremost, it relates to the international macro literature on the costs of volatile capital flows. Studies such as Calvo, Leiderman, and Reinhart (1993), Kaminsky, Lizondo, and Reinhart (1998), and Reinhart and Rogoff (2009) discuss the effects of volatile capital flows on financial stability. More recently, Forbes and Warnock (2012, 2020), and Cavallo, Powell, Pedemonte, and Tavella (2015) have highlighted the importance of studying how the effects of capital flows vary by the type of flow.

As I discuss later in the paper, many of the sudden stop episodes I identify come in 'waves' and seem to be driven by global financial conditions. In that sense, my paper also relates to the more recent literature on the Global Financial Cycle (GFC) and the importance of global factors in driving local credit and business cycles. Rey (2015) discusses the existence of a GFC in capital flows, asset prices, and credit growth and the effect this has on countries' monetary policy independence. Miranda-Agrippino and Rey (2015a) discusses the importance of US monetary policy as a driver of the GFC, and Miranda-Agrippino and Rey (2015b) studies the importance of the GFC as a driver of world assets returns.

On the theory side, the costs of volatile capital flows are understood in the context of pecuniary and/or aggregate demand externalities, as summarized by Korinek (2020). On the one hand, studies such as Krugman (1999) and Caballero and Simsek (2018) highlight the pecuniary externalities created by the balance sheet effects of capital flow volatility. For instance, in the context of large currency depreciations, borrowers do not internalize that by repaying debt to foreign debtors they are putting additional pressure on the local currency. On the other hand, Farhi and Werning (2016) and Korinek and Simsek (2016) highlight aggregate demand externalities that arise from the fact that foreign and local investors have different propensities to consume. Thus, when wealth is transferred to foreign agents during periods with large capital ouflows, aggregate demand decreases.

This paper also contributes to the corporate finance literature that attempts to identify exogenous heterogeneity in the level of firms' exposure to credit shocks. Almeida et al. (2011) introduces the idea of using the share of long-term debt coming due in the following year to identify exposure to rollover risk. The paper finds that US firms with large shares of debt coming due right after the 2007 credit supply shock contracted investment substantially more than firms with low shares of debt coming due. In the context of capital flow volatility in emerging markets, Bleakley and Cowan (2010) studies the impact of short-term debt and maturity mismatches but finds no significant effects on firm investment following sudden stops.

In the context of firms' foreign currency exposure, Bleakley and Cowan (2008) finds no effect on investment while Aguiar (2005), Kim, Tesar, and Zhang (2015), and Verner and Gyöngyösi (2020) find significant reductions in firm investment by firms with more foreign currency exposure.

At a more general level, this paper also relates to a number of papers that study the effect of large credit booms on firm investments and productivity. Giroud and Mueller (2018) shows that increases in firms' borrowing are associated with boom-bust cycles: growth in the short run but declines in the medium run. Kalemli-Ozcan, Laeven, and Moreno (2018) look at European firm investment following the financial crises and the role of debt overhang and short-term debt in explaining the sluggish recovery. In the context of capital account liberalizations, Larrain and Stumpner (2017) shows that credit booms lead to improvements in capital allocation as capital constrained firms get access to finance. On the other hand, in the context of declining interest rates in Spain in the 2000's Gopinath, Kalemli-Özcan, Karabarbounis, and Villegas-Sanchez (2017) shows that increased credit supply leads to an increase in capital misallocation as only large, unproductive firms can take advantage of the extra supply of credit.

Although my results are partial-equilibrium results, recent work by Herreno (2020) shows that cross-sectional effects, in the context of bank lending to firms, survive aggregation in general equilibrium.

The rest of the paper is structured as follows: Section 2 discusses the data sources. Section 3 explains how I define and identify sudden stops and exposure to rollover risk. Section 4 presents the main results of the paper. Section 5 shows robustness tests. Section 6 discusses the policy implications of my results and section 7 concludes.

2 Data

The main data source is annual balance sheet information obtained from Worldscope. This is a panel of firms across a large number of countries during the period 1980-2019. The initial dataset contains nearly 1.5 million firm-year observations, but I perform a number of refinements that bring the total number of firm-year observations to around 700,000. I discuss these refinements in more detail below but the main adjustments I make are: (i) I drop all financial firms and (ii) I drop all countries for which I do not have enough cross-sectional variation.

I complement balance-sheet data from Worldscope with three other sources of firm-level data: data on primary bond issuances from SDC Platinum New Issuances, more detailed data on firms' capital structure from Capital IQ Capital Structure, and daily data on stock prices from Compustat IQ Daily.

SDC Platinum data provides information on key aspects of bond issuances such as proceeds, yields, maturities, ratings, and currency of the bond, as well as firm identifiers that allow me to match bond issuance data with financial information on issuing firms. Since many firms issue more than one bond in a given year, I aggregate bond issuances at the yearly level. This makes my unit of observation the firm-year. The main use for this data is that it provides information on bond maturities at the time of issuance which allows me to build predetermined proxies for when firms should have large portions of long-term debt coming due.

Capital IQ Capital Structure provides additional data on each firm's debt structure. For instance, I obtain information on which percentage of a firm's debt is bank debt. I use this information to test if firms that are more reliant on bank debt are more exposed to sudden stops in bank capital flows.

Firm-level data is complemented with a number of standard global and country-level macroeconomic variables obtained mostly from the BIS, the IMF's BoP, and the IMF's IFS. The key country level variables in my analysis are measures of capital flows obtained from the IMF's International Financial Statistics BPM6 standards. Quarterly data is dissaggregated by type of flow (e.g. bank flows vs. portfolio flows) but also by whether the flows are originated by foreigners or local investors. I discuss these distinctions in more detail in the 'Sudden Stop' section.

2.1 Descriptive Statistics

The main results of the paper are based on a balanced sample of firms. Once I define the set of events, I build seven-years event windows around the events (three years before and three years after). For each event, I only include firms for which I have data on the main variables of interest for all seven years in the event window. This refinement allows me to exclude young firms (firms that were not in the sample before the start of the event) and, more importantly, firms that disappear from the sample following the event. That is, the effects I discuss are those identified only on surviving firms.

All balance-sheet data is provided in nominal terms in local currency. I inflate all values to 2010 values by dividing each variable by the consumer price index with base 2010.

Table 6 presents summary statistics for the main variables of interest. Investment, cash flow, and cash holdings are scaled by dividing by the lag of total assets, log (size) is the log of total assets. Other variables of interest are the the share of debt that is long-term debt, and the share of long-term debt that is due in the next twelve months (the current portion of long-term debt).

Table 6 is divided in four panels. Panel A presents summary statistics for all firms in the sample. Panel B restricts the sample to the 'balanced sample' which is the sample of firms for which I have data for all years in the event window. That is, firms for which I have data in the three years before and the three years after each of the sudden stops episodes I identify. Panel C restricts the sample to treatment firms which are the firms with a large share of long-term debt coming due in the next twelve months. Panel D presents summary statistics only for control firms.

As Table 6 shows, treatment and control firms are relatively similar along all dimensions except for the share of current long-term debt. This is expected as this is the variable used to split firms into treatment and control. While treatment firms have an average of 36% of long-term debt coming due, control firms only have 12%. It is important to note that the summary statistics include all observations in the seven year window around the sudden stop. Thus, even though treatment-control status is defined depending on whether firms have more or less than 20% of long-term debt coming due the year of the sudden stops, it is still possible that these firms have more (or less) than 20% of debt coming due in the remaining years of the event window. That explains why some ‘treatment’ firms have less than than 20% of debt in some of the years.

3 Variable Definitions

The identification strategy is based around exploiting how plausibly exogenous exposure to rollover risk affects firms’ real outcomes following sudden stop episodes. In this section I discuss how I define and measure exposure to rollover risk and how I identify sudden stop episodes.

3.1 Exposure to Rollover Risk

Can we identify exogenous variation in exposure to rollover risk? The challenge with usual measures of exposure to credit supply shocks, such as leverage, short-term debt, or maturity mismatches is that they are likely correlated with other firm characteristics that might explain a firm’s response to a shock. For instance, Barclay and Smith Jr (1995) shows that the decision to issue short-term debt vs. long-term debt is correlated with firm characteristics such as size and profitability.

Using endogenous measures of exposure to credit supply shocks to measure the effect of exposure on firm real outcomes might then lead to biased estimates. For instance, if firms that rely more on short-term debt are also riskier firms that are in general more affected by changes in credit conditions, using short-term debt as a proxy for exposure would bias the results upwards. That is, we would interpret the decline in firm real activity to be *caused* by the level of short-term debt while in reality part of the observed effect is due to the fact that the firm is riskier and in general more sensitive to changes in aggregate conditions.

To get around the issue of endogeneity, I use the share of long-term debt maturing over the next twelve months. This measure, introduced by Almeida et al. (2011) has the advantage that it is largely predetermined at the moment of debt issuance, potentially years in advance, and as such it is less affected by the firm's recent performance.

The idea behind this measure is that it captures an aspect of firms' financing needs while abstracting from *decisions* that are correlated with the future performance of the firm. This is due to the fact that the specific timing of long-term debt maturities coming due is to a large extent predetermined at the moment of debt issuance. Hence, whether or not a firm has a substantial portion of its long-term debt coming due in a given year is to a large extent random and uncorrelated with other firm characteristics that might predict how the firm would react to an aggregate shock.

It is very important to note that long-term debt maturing in a given year is an entirely different concept than short-term debt. As noted above, the decision to issue short-term debt versus issuing long-term debt is endogenous and very correlated with other determinants of firm investment. As such, firms that rely more on short-term debt are in fact noticeably different than firms that rely more on long-term debt.

However, whether or not a firm that has issued long-term term debt in the past (potentially many years ago) has a large portion of that debt coming due in a given year is to a large extent random and hence uncorrelated with other firm characteristics.

To be more specific, I build a measure of the share of long-term debt coming due over the next twelve months by using two variables from Worldscope. I first compute total long-term debt by adding up 'Current Portion of Long Term Debt' (which includes all long-term debt payments that need to be made over the next twelve months) and 'Long-Term Debt' (which includes all payments with maturities longer than twelve months). I then compute the ratio of the current portion of long-term debt to total long-term debt. In my main specification, to make the analysis and interpretation of the results simpler, I construct a dummy that is equal to one for firm-years that have more than 20% of their long-term debt coming due over the next twelve months. In robustness tests, I discuss how the results depend on the cutoffs for the categorical variable.

3.2 Sudden Stops

In order to test the effect of high exposure to rollover risk, one needs to identify periods with large contractions in capital flows. Identifying these periods involves making two main decisions: first, which type of capital flows should we focus on? Gross or net? All flows or portfolio flows? And second, how do we define large contractions in capital flows?

Which type of capital flows do we care about? In this paper, I focus on identifying large drops in gross debt portfolio inflows. These are defined as the debt portfolio component of 'the net of foreign purchases of domestic assets and foreign sales of domestic assets.' That is, I use the debt component of portfolio flows by foreigners. There are two main reasons to focus on this component of flows. First, debt portfolio flows are those that most directly impact firms' ability to finance themselves in capital markets and, as such, are the most likely to directly affect firms' ability to rollover debt. Moreover, debt flows are the most 'fickle', as discussed by Caballero and Simsek (2018) and Korinek (2018), and hence using debt flows allows me to identify a sudden stop episode as early as possible.

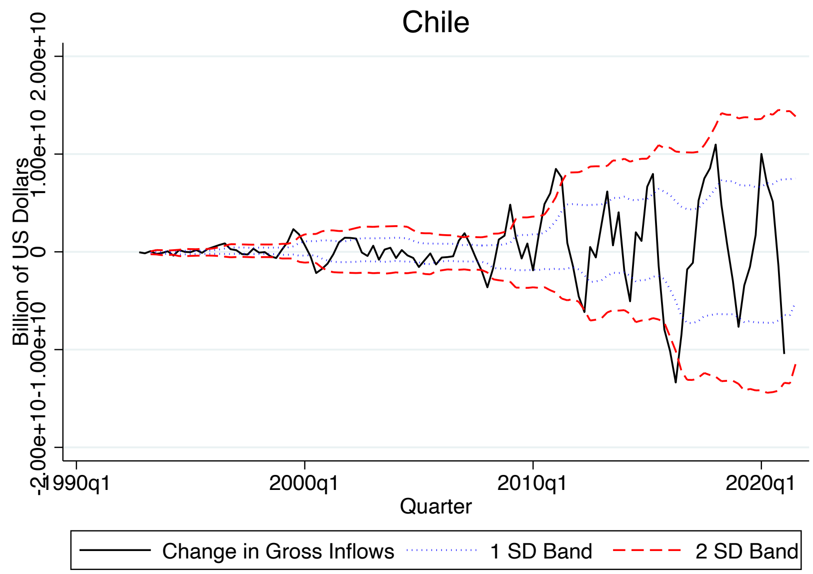
Additionally, the reason to use gross inflows, as opposed to net flows, is twofold. First, focusing on the behavior of foreign investors might alleviate concerns about the endogeneity of sudden stops. Second, there is increasing evidence on the importance of focusing in gross inflows when trying to understand sudden stop dynamics, as discussed in Forbes and Warnock (2020) and Cavallo et al. (2015).

How do I identify periods with large contractions in inflows? I identify periods in which the drop in capital flows is large relative to the recent path of capital flows in the given country, following Forbes and Warnock (2012, 2020). More specifically, starting with the corresponding quarterly series of capital flows from the IMF's IFS, I first compute 4-quarter moving sum of inflows and then compute the change in the sum with respect to four quarters ago. This produces a time series of year-on-year changes in capital flows for each quarter. For each quarter, I then compute the 5-year rolling mean and standard deviation. Figure 1 plots these series for the case of Chile.

I identify the start of a sudden stop episode as the first quarter in which the change in flows drops one standard deviation below the series mean, provided that it then drops to two standard deviations below the mean. This definition is intended to capture very large changes (changes that deviate two standard deviation from the mean) but attempts to identify the episode as soon as it starts. This is precisely the main difference between my definition and that of Forbes and Warnock (2020): while that paper identifies entire sudden stop 'episodes' that can last many years, I only focus on the year in which the sudden stop starts. The reason for this is that my goal is to capture the effect of exposure to rollover risk at the time of a large, unexpected, contraction in inflows. My measure of exposure to rollover risk two or three years into a sudden stop episode might be contaminated by firms' decisions during the episode. That is, the claim that my measure of exposure is exogenous is less grounded if we look at exposed firms after a prolonged period of outflows. This is because my measure might capture firms' differential ability to issue debt with longer maturities during a sudden stop episode.

Figure 1 shows how the procedure works in the example of Chile. The black line tracks year-on-year changes in capital flows. For a drop to qualify as a sudden stop episode it must cross the bottom red line (the 2-standard deviation band). However, for those episodes, I identify the start of the episode as the quarter in which the blue line (the one standard deviation band) is first crossed. The graph also highlights the importance of using 5-year rolling windows to identify episodes. As capital flows become more volatile, a larger change is needed for the episode to qualify as a sudden stop.

Figure 1. Surge and Stop Episodes

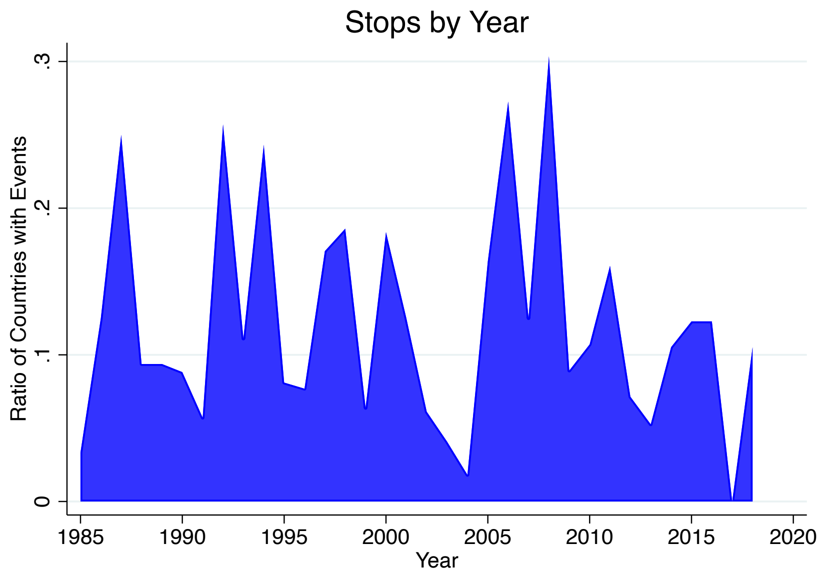


Notes: This figure presents an example of how the methodology used to identify sudden stop episodes works. The solid black line is the time series for the year-on-year change in capital flows. The red lines represent the 2-standard deviations below and above the mean of changes in capital flows. Sudden stop episodes are those in which the black line crosses the bottom red line. I identify the beginning of a sudden stop episode as the first period in which the black line drops one standard deviation below the mean of the series (when it crosses the blue line).

Figure 2 displays the prevalence of sudden stop episodes across time. Sudden stop episodes come in waves with the highest prevalence being in the years around the global financial crisis when 30% of the countries in the sample experience a sudden stop episode. Other well known waves take place in the early 1990's (countries in Latin America), the late 1990's (Asian crisis) and in the post-global financial crises period (following the taper tantrum).

Even though Figure 2 shows that sudden stops do come in waves, the figure also highlights the fact that sudden stop episodes are spread out through time. In all years, there are at least a number of countries suffering a sudden stop episode. Table 5 provides the full list of country-years identified as sudden stops in my baseline regressions.

Figure 2. Prevalence of Sudden Stop Episodes



Notes: This figure displays the prevalence of sudden stop episodes across time. Sudden stop episodes come in waves, with the highest prevalence being in the years around the global financial crisis, when 30% of the countries in the sample experience a sudden stop episode. Other well known waves take place in the early 1990's (countries in Latin America), the late 1990's (Asian crisis) and in the post-crisis period following the taper tantrum.

4 Main Results

4.1 Main Specification

To estimate the effect of being exposed to rollover risk during sudden stop episodes, I run a difference-in-difference regression. The main dependent variable of interest is the log of firm investment. The independent variable is my measure of firm exposure to rollover risk interacted with a post-treatment dummy. The coefficient on this interaction will then capture the differential effect of being exposed to rollover risk during sudden stop episodes on firm investment. The specification then takes the form:

$$I_{i,t} = \beta_1 \times POST_{c,t} \times Exposure_{i,t=0} + \gamma POST_{c,t} \times \mathbf{X}_{i,t=-1} + \alpha_i + \alpha_{c,t} + \epsilon_{i,t}, \qquad (1)$$

where c is the country, i is the firm, and t is a time variable that defines the year relative to the year in which the sudden stop episode occurs: $t = 0$. $POST_{c,t} = 1$ the year after the start of the sudden stop and 0 the year before. $I_{i,t} = log(Capex_{i,t})$ is the log of annual capital expenditures. For my baseline results, I define my measure of $Exposure_{i,t}$ as a dummy equal to 1 if long-term debt maturing over the next 12 months is more than 20% of total long-term debt. In robustness tests, I show the results using different cutoffs and also using the continuous variable. I include firm-event fixed effects α_i to account for any remaining firm heterogeneity and also $Country \times Year$ fixed effects $\alpha_{c,t}$ aimed at capturing any remaining macro shocks. Finally, I include a set of firm-level controls $X_{i,t-1}$ identified in the corporate finance literature as major determinants of corporate investment. In all specifications, I include controls for cash holdings to total assets, cash flows to total assets, the log of total assets, and total long-term debt to total assets.

Table 1 presents the results of the previous regression for β_1, the coefficient that captures the differential effect of being exposed to rollover risk during sudden stop episodes.

Table 1: Effect of Exposure on Investment

	(1)	(2)	(3)	(4)	(5)
$POST_{c,t} \times Exposure_{t=0}$	-0.0896***	-0.0853***	-0.0996***	-0.105***	-0.0738**
	(0.0319)	(0.0328)	(0.0318)	(0.0327)	(0.0308)
Observations	24,180	24,180	24,180	24,180	24,180
$Country \times Year\ FE$	Yes	No	Yes	No	Yes
$Firm \times Event\ FE$	Yes	Yes	No	No	Yes
Controls	Yes	Yes	Yes	Yes	No

Notes: This table shows the results of running specification 1 for the coefficient β_1. Column (1) has the results for the main specification, including firm fixed effects, $Country \times Year$ fixed effects, and controlling for size, cash flows, cash holdings, and the long-term debt to total assets. Column (2) does not include $Country \times Year$ fixed effects. Column (3) does not include firm-event fixed effects. Column (4) does not include any fixed effects. Column (5) does not include the controls. Standard errors are clustered at the firm level.
The results show that firms that are exposed to rollover risk (as measured by the percentage of long-term debt maturing over the next year) contract investment around 9 percentage points more than non-exposed firms following a sudden stop.

There is a large, economically and statistically significant effect of exposure to rollover risk during sudden stop episodes. The 2-year change in investment indicates that exposed firms reduce investment nine percentage points more than non-exposed firms following a sudden stop. This differential amounts to exposed firms reducing investment around three times as much as non-exposed firms: on average exposed firms contract investment by 14.6% while non-exposed firms reduce investment by 5.7%.

It is important to note that this effect is only the differential effect between treatment and control firms. Any remaining aggregate level effects are absorbed by the country-year fixed effects. These aggregate level effects might explain why, on average, non-exposed firms reduce investment by 5.7%.

Columns 2-4 show that the inclusion/exclusion of different sets of fixed effects has little effect on the size of the coefficient. Column 5 confirms the importance of including the set of controls previously discussed. Not including factors just at the size or liquidity condition of firms does confound the results and leads to both smaller and less statistically significant coefficients (only statistically significant at the 5% level).

4.2 Pre-trends and Persistence

As with any difference-in-difference setting, the validity of the results depends on exposed and non-exposed firms showing parallel trends in the period before the sudden stops. In order to test for pre-trends as well as to study the persistence of the effects found in the previous section, I run the following specification:

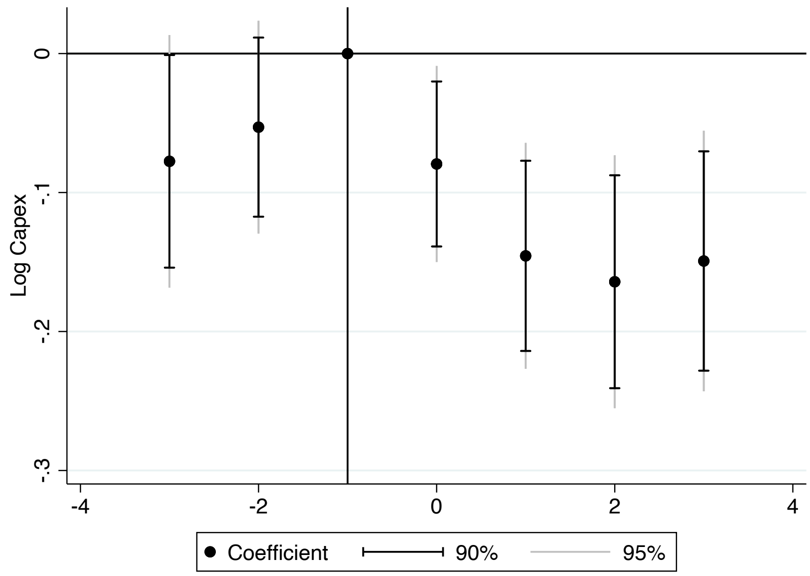
$$I_{i,t} = \sum_{\tau \neq t-1} \beta_\tau I\left(1\, if\; t = \tau\right) Exposure_{i,t=0} + \sum_{\tau \neq t-1} \gamma_\tau I\left(1\, if\; t = \tau\right) X_{i,t=-1} + \alpha_i + \alpha_{c,t} + \epsilon_{i,t} \tag{2}$$

where as before $t = 0$ is the year of the sudden stop and $\tau = -3, -2, 0, 1, 2, 3$. That is, the specification takes $t = -1$ as the base year and β_τ tracks the difference-in-difference coefficient for a number of years around the base year.

Figure 3 plots the coefficients, as well as the confidence interval for all seven years around the sudden stop episode. The first important result to notice from the graph is the presence of parallel trends in investment before the sudden stop. As Figure 3 shows the coefficients for years $t = -3$ and $t = -2$ are indistinguishable from zero.

In terms of the post-event effects, the coefficient for $t = 0$ is around 5%. This shows that there is an initial impact on the investment of exposed firms that starts the year of the sudden stop. However, as the coefficients for $t = 1, 2, 3$ show, the effect on investment is larger starting in year $t = 1$ and persists for at least the three years following the sudden stop episode.

Figure 3. Pre-trends and Persistence



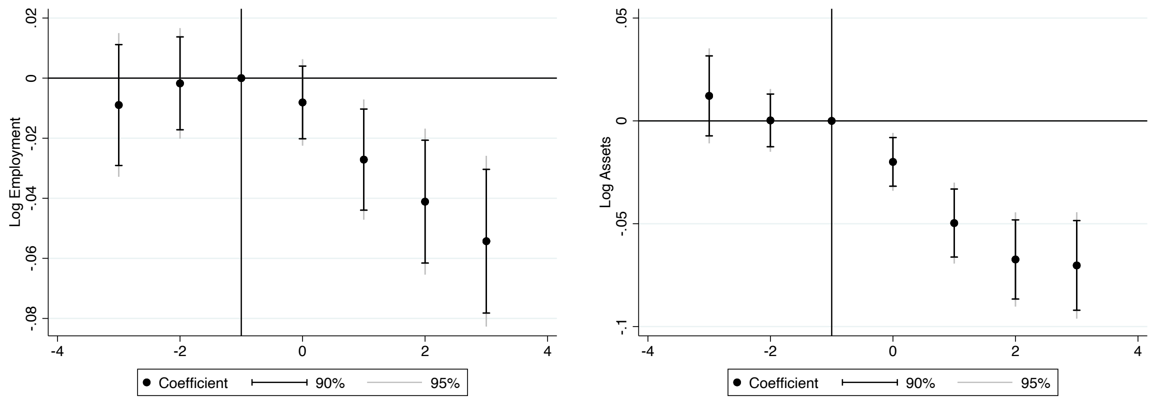
Notes: This figure shows the results of running specification 2. Each dot represents the coefficient of running the difference-in-difference estimation of the change in the log of investment of exposed firms vs. non-exposed firms, using year $t = -1$ as the baseline and year $t = 0$ as the year of the sudden stop. The results for years $t = -3$ and $t = -2$ show that there are no different pre-trends before treatment and control firms. The results for years $t = 1, 2, 3$ show that there is a large and persistent effect of being exposed to rollover risk during a sudden stop episode.

The persistence of the results shows that exposed firms remain at a lower level of investment (compared to non-exposed firms) years after the initial shock. That is, the large reduction in investment following a year when a large portion of long-term debt was coming due is not simply a transitory contraction that gets reversed immediately after the firm pays back or rolls over its debt.

4.3 Other Firm Outcomes

The effects on investment discussed in the previous section extend to a number of other firm real outcomes. Figure 4 replicates the exact same methodology and results of Figure 3 but for the log of employment and the log of total firm assets. The results confirm a similar pattern than that found for capital expenditures.

Figure 4. Effect of Exposure on Employment and Asset Growth



Notes: This table shows the results of running specification 2. Each dot represents the coefficient of running the difference-in-difference estimation of the change of the log of employment (or log assets) of exposed firms vs. non-exposed firms, using year $t = -1$ as the baseline and year $t = 0$ as the year of the sudden stop. The results for years $t = -3$ and $t = -2$ show that there are no different pre-trends. The results for years $t = 1, 2, 3$ show that there is a large and persistent effect of being exposed to rollover risk during a sudden stop episode.

Exposed firms reduce employment between three to five percentage points more than non-exposed firms during sudden stop episodes. The effect seems to be somewhat more delayed than the effect seen in investment. First, the effect the year of the shock is statistically indistinguishable from zero. Second, the effect seems to get larger over time. While the point estimate for years one and two is around 3%, the point estimate for year three is 5%. The results are consistent with the notion that investment is a more immediate adjustment variable for firms with financing constraints and employment adjusts more slowly.

The results for the log of total assets paint a similar picture. The effect the year of the shock is around two percentage points but it increases to as much as eight percentage points three years after the sudden stop. That is, exposed firms reduce total assets by eight percentage points more than non-exposed firms do following a sudden stop episode.

4.4 Does the Timing of Exposure Matter?

In the main results, I identify firms as exposed or not-exposed by their level of exposure the year of the beginning of the sudden stop episode. How do firms with large exposure in the years following the beginning of the episode fare? Figures 8 and 9 explore this issue.

First, Figure 8 shows that using the baseline definition of exposure on years $t = 0$ or $t = 1$ leads to statistically significant results. That is, firms exposed on year 0 contract investment more than firms not exposed on year 0, while the same is true for year 1. However, this result does not extend to years 2 and 3.

Figure 9 delves deeper into the issue by introducing a number of alternative treatment vs. control definitions. A number of results are worth noting. First, definition 2 shows that there is not statistical difference in the change in investment between firms exposed on year $t = 0$ (and not exposed on year $t = 1$) and firms exposed on year $t = 1$ (but not on year $t = 0$). Similar results are obtained in definitions 3 and 4.

However, the most interesting result seems to be that of definition 5. This definition sets as treatment firms those with exposure at year $t = 0$ and as controls those exposed at least once in years $t = 1, 2, 3$ but not exposed on year $t = 0$. The difference in investment is statistically significant showing that being exposed at the exact time of the beginning of the sudden stop is more costly than being exposed in the following years. A possible explanation for this is that firms that have a large amount of debt coming due in the following years have time to react and, thus they do not need to contract investment as much as those firms that get caught having to rollover debt the year the sudden stop episode begins.

5 Robustness Tests

I split my robustness tests into two categories. First, I study issues related to my measure of sudden stops and then I explore concerns related to my measure of exposure to rollover risk.

5.1 Sudden Stops

5.1.1 Role of Sudden Stops

Are the results found in the previous section specific to sudden stop episodes? One potential concern about my results is that they capture some general *cost* associated with having large portions of long-term debt coming due and hence are unrelated to sudden stops. For instance, this would be the case in a world of large financial frictions in which firms that need to repay debt always find it costly to extend maturities and hence often need to reduce investment.

In order to address this issue, I construct placebo tests built around the idea of testing whether my results hold in years without sudden stops. I identify a number of country-years with no sudden stops within a seven-year even window as placebo years and perform the same analysis as before. I find no statistically significant difference between the investment growth of exposed and non-exposed firms in placebo years. This result is notable considering my definition of placebo years. I have only imposed the requirement that there is no sudden stop within the event window. This allows for the possibility of including events with substantial drops in capital flows but just not large enough to satisfy my definition of a sudden stop.

Additionally, to more formally estimate the differential effect of exposure on event years, I run the following specification:

$$\begin{aligned} I_{i,t} = \beta_1 \times POST_{c,t} \times SS_{c,t} \times Exposure_{i,t} + \beta_2 \times POST_{c,t} \times Exposure_{i,t} \\ + \gamma POST_{c,t} \times \mathbf{X}_{i,t-1} + \alpha_i + \alpha_{c,t} + \epsilon_{i,t}. \end{aligned} \tag{3}$$

I run this regression on a pool that combines event years (years with sudden stops) with placebo years (event windows where there is no sudden stops). As before the dummy $POST_{c,t} = 1$ identifies the before-after years for all events (including the placebo years), while the $SS_{c,t} = 1$ identifies the actual years with sudden stops. In this setting, β_2 captures the effect of exposure in all years (sudden stop and placebo years), while β_1 captures the differential effect of being exposed to rollover risk during sudden stop episodes vs. being exposed to rollover risk in non-event years.

Table 2 presents the results of the previous specification. Exposed firms contract investment by four and a half percentage points more than non-exposed firms in all events (whether the event is a sudden stop or it is a placebo event). However, the effect is persistent, and gets larger, only on years with sudden stops.

Table 2: Effect of Exposure on Investment, Sudden Stops vs. Placebo Years

	(1)	(2)	(3)	(4)
	t	$t+1$	$t+2$	$t+3$
$POST_{c,t} \times Exposure_{t=0} \times SS_{c,t}$	0.00724	-0.0647***	-0.0975***	-0.0556**
	(0.0149)	(0.0154)	(0.0172)	(0.0217)
$POST_{c,t} \times Exposure_{t=0}$	-0.0453**	-0.0129	0.0234	-0.0256
	(0.0139)	(0.0143)	(0.0155)	(0.0185)

Notes: This table shows the results of running specification 3. Each Column presents the difference-in-difference coefficient using different horizons but always $t = -1$ as the baseline. The regression pools a set of event years (years with sudden stops) and a set of placebo event years (years with no sudden stops). The bottom row shows the coefficients for all years while the top row shows the differential effect of being exposed on years with sudden stops.
The results show that there is an initial drop in investment for exposed firms the year of the shock (t) for all years, both event and placebo event years. However, as the top row shows, the effect is persistent, and gets larger, only in years with sudden stops.

The results in Table 2 confirm that exposure to rollover-risk leads to lower investment only during sudden stop episodes. That is, exposure does not seem to be permanently costly to firms during normal years. This result is consistent with the idea that during normal years, firms have an easier time rolling over long-term debt and hence, they do not need to reduce investment.

Table 3 presents more direct evidence of the fact that rollover risk is a major factor by looking at the effects on the level of long-term debt. If firms can perfectly rollover maturing debt, we should see no impact of exposure on the stock of long-term debt. More importantly, the impact should not be any different on years with sudden stops vs. placebo years.

The bottom row of Table 3 shows that exposed firms reduce the stock of long-term debt in years with large portions for long-term debt coming due, more relative to firms that do not have large shares coming due. This is expected as not all firms are able or willing to roll over all their maturing debt. However, the difference between exposed and non-exposed firms becomes smaller and statistically insignificant by year three after the event. That is, in placebo years, exposed firms initially reduce their stock of debt (i.e. they pay at least a portion of their maturing debt) but rapidly go back to the initial level of debt.

The situation is drastically different when we look at years with sudden stops (the top row of Table 3). After years with sudden stops, exposed firms reduce long-term debt by twelve percentage points more than non-exposed firms (on top of the eighteen percentage point contraction that affects exposed firms in all years regardless of whether there is a sudden stop or not). More importantly, this difference only gets larger in the subsequent years, jumping to around twenty percentage points.

Table 3 confirms the large role played by rollover risk around sudden stop episodes. When firms in a country are exposed during a sudden stop episode, they cannot rollover their debt and are forced to reduce long-term debt. Firms have a lower stock of debt even three years after the sudden stop. More importantly, this result is specific to years with sudden stops: exposed firms in placebo years initially reduce their stock of debt but they rebuild it quite rapidly.

Table 3: Effect of Exposure on Long-Term Debt, Sudden Stops vs. Placebo Years

	(1)	(2)	(3)
	$t+1$	$t+2$	$t+3$
$POST_{c,t} \times Exposure_{t=0} \times SS_{c,t}$	-0.127***	-0.213***	-0.198***
	(0.0128)	(0.00982)	(0.0204)
$POST_{c,t} \times Exposure_{t=0}$	-0.182***	-0.0536**	-0.0148
	(0.0163)	(0.0160)	(0.0299)

Notes: This table shows the results of running specification 3 but for the level of long-term debt instead of investment. Each Column presents the difference-in-difference coefficient using different horizons but always $t = -1$ as the baseline. The regression pools a set of event years (years with sudden stops) and a set of placebo event years (years with no sudden stops). The bottom row shows the coefficients for all years while the top row shows the differential effect of being exposed on years with sudden stops.

As expected, the results show an initial effect on long-term debt for all years: placebo years and sudden stop years. However, after the initial mechanical delevering, firms go back to the previous level of debt in the placebo years. The effect is only permanent, and it gets larger, for years with sudden stops.

5.1.2 Sudden Stops and Retrenchment

As discussed above I have defined sudden stops from data in gross inflows. That is, my measure of sudden stops focuses on flows by foreigners and ignores behavior by local investors. Part of the rationale for this decision, is that netting out flows by foreign and domestic investors leads to missing substantial swings in flows by foreign investors. This is because some episodes in which foreign investors pull out capital are accompanied by large retrenchment episodes: episodes in which local investors bring back capital into their own country.

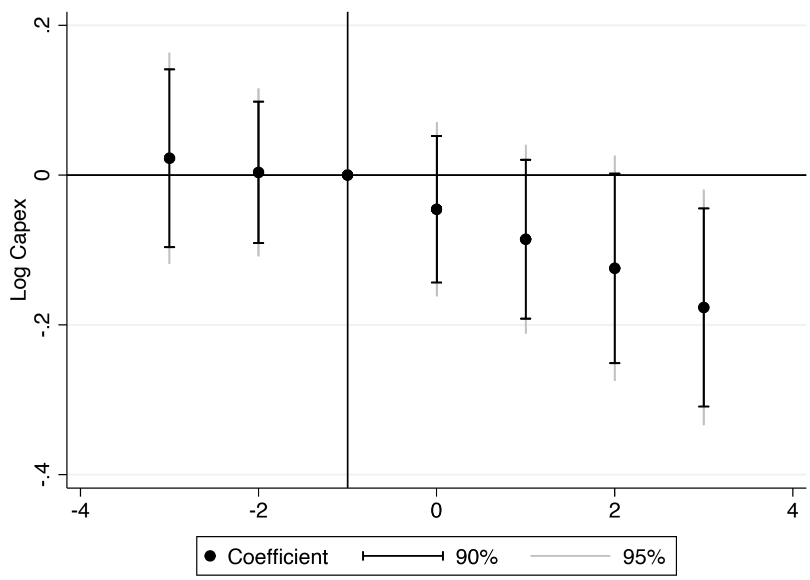
This is in stark contrast with the traditional literature on sudden stops which usually focuses on net inflows.[2] The main reason why studies tend to focus on net flows (other than data limitations) is that if foreign and domestic capital are perfect substitutes, episodes in which outflows by foreigners are balanced out by inflows by domestic investors should not have significant effects on credit conditions.

[2]The main exceptions being Forbes and Warnock (2012, 2020).

However, that is not what my analysis finds. I find that in the large set of events, sudden stops in foreign inflows have effects on firm real outcomes, regardless of what is happening to flows by domestic investors. In fact, as Figure 5 shows, my results hold to some extent even when I restrict my sample of events to events in which the sudden stop in foreign flows is accompanied by a surge in inflows my domestic investors (retrenchment episodes are defined in a symmetric manner as to stop episodes.

It is important to note that this is the most benign version of a sudden stop: domestic investors fully compensate for the fall in foreign inflows. The fact that the results hold, to some extent, even in these 'benign' episodes, suggest that foreign and domestic capital are not perfect substitutes and that domestic investors do not fully fill the liquidity vacuum generated by foreign investors leaving the country.

Figure 5. Effect of Exposure on Investment, Sudden Stops With Retrenchment



Notes: This Figure shows the results of running specification 1 for the coefficient β_1 but restricting the sample of events only to those sudden stops that are accompanied by a retrenchment episode (a surge in inflows by domestic investors).

5.1.3 Does the type of flow matter?

As discussed above, the main results use the debt-portfolio component of flows to identify sudden stops. However, as Figure 7 shows, the type of flow used does not seem to be consequential. The reason for this seems to be that all types of flows crash during sudden stops, and thus, using any of them leads to identifying a very similar set of events. Note, however, that this is not to say that the correlation between all types of flows is high. Instead, it suggests that during periods in which foreign investors contract capital flows of one type of flows, they do it for all types of flows.

5.1.4 Macro Effects

A usual concern in studies that look at the effect of credit events is that other 'macro effects' could be affecting exposed and non-exposed firms differently and hence the observed results are due to reasons other than rollover risk. This is substantially less of a concern in my context as most of the episodes I identify do not seem to be associated with substantial disruptions in economic activity. In fact, out of the ninety-one sudden stops I identify in my main specification: only thirteen experience negative GDP growth the year of the sudden stop (twenty-five the year following the sudden stop) and only two have a GDP contraction of at least 2% the year of the sudden stop (sixteen the year following the sudden stop). Refining the sample to exclude years with GDP contractions or large devaluations yields very similar results.

To more formally test the hypothesis that other 'macro effects' are driving my results I conduct two tests: first, I run my main specification excluding years with large macroeconomic events. Second, I run my specification for country-years with GDP contractions but without sudden stops. The results of both tests are consistent with the notion that sudden stops in capital flows are indeed the drivers of my results.

5.2 Exposure to Rollover Risk

Is my measure of exposure to rollover risk exogenous? In this section, I explore a set of tests designed to explore whether my measure captures exogenous variation in exposure to rollover risk.

5.2.1 Confounding Factors

Is my measure of exposure to rollover risk simply capturing relatively time-invariant firm level differences? One potential concern with my measure of rollover risk is that it could be capturing the fact that some firms simply have a long-term debt maturity structure such that they often find themselves exposed to rollover risk. If some firms are more likely to engage in this kind of behavior than others, my measure could be capturing time-invariant differences between firms.

If this is indeed the case, firms would have relatively persistent levels of exposure to rollover risk. That is, firms would either frequently be exposed or frequently be not-exposed. In such a scenario, past measures of exposure should predict how a firm reacts to a sudden stop. I test for this by replicating the results of my main specification, but instead of using exposure at the time of the shock, I use the firm's exposure k years before the shock.

Table 4 presents the results of that test for different lags of the measure of exposure. As the results show, past exposure at different horizons does not predict how a firm would respond to a sudden stop shock. Table 4 provides evidence that my measure of exposure is not persistent and that the main driver of how a firm responds to a shock is the level of exposure at the specific time of the sudden stop.

Table 4: Effect on Investment Using Past Exposure

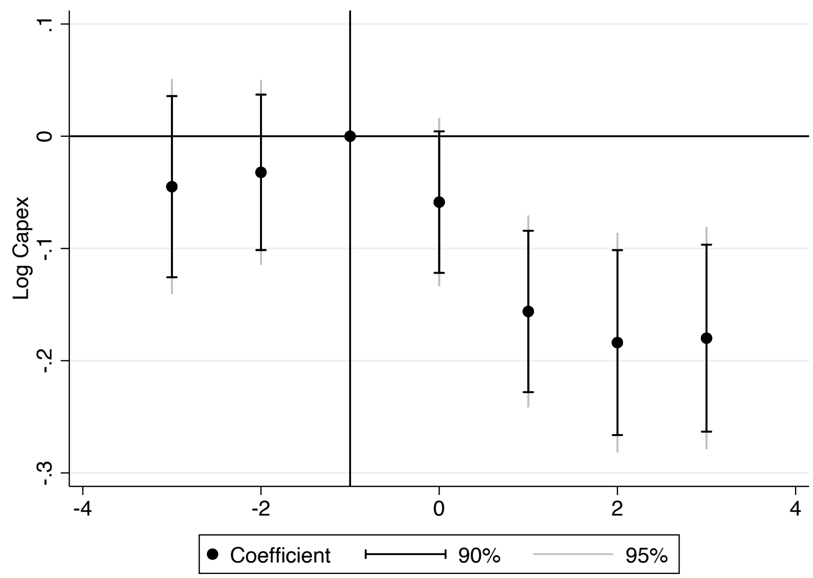
	(1)	(2)	(3)	(4)	(5)	(6)
$POST_{c,t} \times Exposure_{i,t-k}$	-0.0896***	-0.0322	-0.0480	-0.0629	-0.0420	-0.0494
	(0.0319)	(0.0333)	(0.0325)	(0.0452)	(0.0458)	(0.0442)
		k=2	k=3	k=4	k=5	k=6
$Country \times Year\ FE$	Yes	Yes	Yes	Yes	Yes	Yes
$Firm \times Event\ FE$	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table shows the results of running the main specification but using the level of firm exposure with a lag of k years. That is, instead of using the level of exposure the year of the sudden stop, I use the level of exposure k years before the sudden stop. Each column presents the difference-in-difference coefficient using different lags of the exposure variable.

An additional test to measure if the results are just driven by time-invariant differences across firms is to conduct my tests dropping firms that do not exhibit significant variation in the level of exposure within the time window around the sudden stop. Figure 6 shows the results of my main test excluding from the sample all firms that were not exposed in any of the seven years around the sudden stop.

Excluding all firms that do not have exposure in any of the seven years in the event window does not substantially affect the results.

Figure 6. Effect of Exposure on Investment Excluding Firms With No Exposure



Notes: Th table shows the results of running specification 2 but excluding firms that are not exposed thoughout the event window. Each dot represents the coefficient of running the difference-in-difference estimation of the change of the log of investment of exposed firms vs. non-exposed firms, using year $t = -1$ as the baseline and year $t = 0$ as the year of the sudden stop. The results for years $t = -3$ and $t = -2$ show that there are no different pre-trends. The results for years $t = 1, 2, 3$ show that there is a large and persistent effect of being exposed to rollover risk during a sudden stop episode.

5.2.2 Maturity Management

Is maturity management a problem for my identification? A potential concern about my measure of rollover risk is that active maturity management at the firm level could make it endogenous. That is, it could be the case that better CEO's decide to proactively extend maturities before they are due and hence, these firms would be less likely to 'get caught' by a sudden stop with a large share of long-term debt maturing.

Active maturity management could be an issue for my specification if, and only if, active maturity management correlates with other firm-level variables that might explain how well a firm would respond to a sudden stop.

For instance, if two identical firms differ only in that one of their CEO's extends maturities frequently and hence their firm is less likely to be exposed at any given time, this is not a problem for my specification, my results still capture the cost of rollover risk.

It is a problem for my identification if smart CEO's extend maturities and also make other decisions that reduce exposure to sudden stops. For instance, it could be the case that risk-averse CEO's extend maturities but also engage in other precautionary policies such as increasing cash holdings or reducing the total level of debt. In my main specification, I control for some of these observable hedges and my main results are not affected.

However, adding controls does not fully rule out other potential hedges that I cannot control for because they are unobservable. Three sets of results seem to suggest that maturity management is not a problem in my context. First, Almeida et al. (2011) shows that the structure of maturities is predetermined: firms do not seem to be extending maturities. Second, Xu (2018) shows that investment grade firms do very little maturity management. Third, my own preliminary results from primary issuance data seem to suggest that maturity at issuance predict my balance sheet measure of exposure quite well. That is, looking at maturity dates at the moment of issuance seems to predict when firms will be exposed to rollover risk very well. This would not be the case if firms were constantly extending the maturity of their long-term debt.

6 Policy Implications

The results discussed above on the effects of exposure to rollover risk during sudden stop episodes have clear policy implications both in terms of capital flow management and firms' maturity management.

First, in terms of capital flow management, my results identify a very specific and causal liquidity channel through which large movements in capital flows get transmitted to the real economy. Thus, my results provide further justification for measures that aim to reduce volatility in capital flows.

Second, my results highlight the importance of maturity structure at the firm level. There are at least two aspects of firms' maturity decisions that could be influenced by policy and deserve further attention. First, policymakers should pay more attention, and possibly regulate firms' maturity decisions at issuance. If, as some of my results suggest, firms do shorten the maturity of their long-debt issuances during capital flow booms, these decisions could be exposing them to higher rollover risk if/when the capital flow boom ends. This is the case because the shorter the maturities are, the more likely it is that a firm is exposed to rollover risk in a given year.

Moreover, from the perspective of the policy maker, there might be reasons to incentivize maturity management. As previously discussed, discontinuities in the timing of firms' maturities can have large real effects. Thus, policies oriented to incentivizing active maturity management that leads to spreading maturities over time and minimizes the likelihood of 'being caught' with large maturities coming due at the time of a sudden stop can have large real benefits.

Policies oriented at reducing firms' exposure to rollover risk could be added to the standard toolkit of macroprudential tools that aim at reducing firms' and banks' exposure to sudden changes in credit conditions.

7 Conclusion

Empirically identifying the costs of volatile international capital flows at the aggregate level remains a substantial challenge for the international macroeconomics literature. In this paper, I bring firm-level data for a large cross-country panel of firms to measure the costs of sudden stops at the firm level. Exploiting an exogenous discontinuity in the maturity structure of firms' long-term debt, I am able to identify a causal liquidity channel at play during sudden stop episodes. This liquidity channel amplifies the real costs of aggregate credit supply shocks.

I find that exposure to rollover risk (as measured by the share of long-term debt maturing over the following twelve months) leads to economically and statistically significant drops in investment. Exposed firms contract capital expenditures by ten percentage points more than non-exposed firms following sudden stop episodes.

My results extend to other firm outcomes such as total debt, employment, and total assets. More importantly, the effects are persistent: three years after the sudden stop, exposed firms remain at the lower levels of investment observed the year after the shock.

A number of robustness tests show results consistent with the idea that credit drying up during the sudden stop is the main driver of the results: the results do not hold in periods without sudden stops, but they do hold in periods with sudden stops but no discernible slow down in economic activity.

Three aspects of my results are somewhat new and hence a contribution to the literature. First, my results highlight the importance of studying gross inflows instead of using net flows. Second, my results hold for a large number of sudden stop episodes even in the absence of any significant slowdown in economic activity. This is a significant departure from most papers on credit frictions that find large effects during episodes of large macroeconomic disruption. Third, by the nature of my dataset, my results identify a negative real effect of credit frictions on very large, mostly public firms. This is a relatively new result considering that studies that explore the cross-sectional effects of financial frictions usually find that larger firms are the least impacted.

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8 Additional Tables

Table 5: Sudden Stop Episodes

Nation	Year	Nation	Year	Nation	Year
Argentina	2000	Germany	1987	Peru	2006
Argentina	2008	Germany	1994	Peru	2014
Australia	1987	Germany	2006	Philippines	1997
Australia	2007	Greece	2006	Philippines	2007
Australia	2016	Indonesia	1997	Poland	2006
Austria	1987	Indonesia	2011	Portugal	1989
Austria	1996	Ireland	1987	Portugal	2000
Austria	2008	Ireland	2007	Portugal	2010
Brazil	1993	Ireland	2016	Romania	2009
Brazil	2008	Israel	1998	Russia	2013
Brazil	2015	Israel	2005	South Africa	2008
Canada	1987	Italy	1987	South Korea	1997
Canada	1994	Italy	1994	South Korea	2005
Canada	2011	Italy	2006	South Korea	2016
Chile	1998	Japan	1988	Spain	1990
Chile	2007	Japan	2001	Spain	2007
Chile	2015	Japan	2008	Sweden	1992
Chinese Taipei	2004	Malaysia	2008	Sweden	2000
Colombia	1997	Mexico	1994	Sweden	2008
Colombia	2008	Mexico	2006	Switzerland	2005
Colombia	2015	Mexico	2014	Thailand	1995
Croatia	2014	Netherlands	1989	Thailand	2006
Denmark	1986	Netherlands	2000	Thailand	2013
Denmark	1994	Netherlands	2008	Turkey	1998
Denmark	2008	New Zealand	1991	Turkey	2007
Denmark	2016	New Zealand	2008	UK	1989
Finland	1986	Norway	1987	UK	1997
France	1993	Norway	1999	UK	2008
France	2002	Norway	2008		
France	2010	Peru	1998		

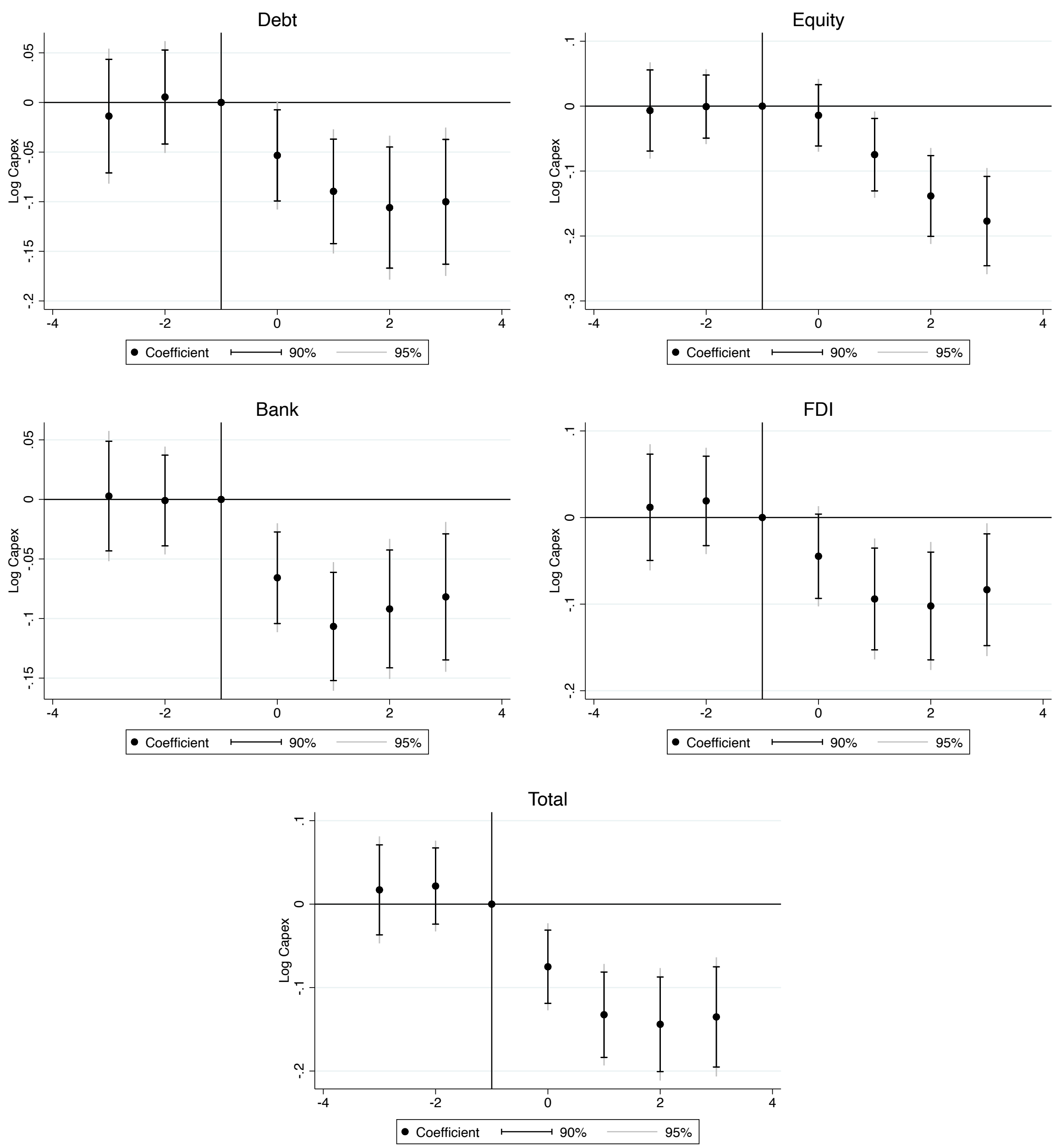
Sudden Stop Episodes: This table provides the list of sudden stops identified in the baseline regressions. Sudden stops are identified as described in section 3.2, using data on gross debt portfolio inflows. Sudden stop episodes can last for multiple years. In the table, I identify the first year of each episode as my event. The table includes only those episodes that are included in the baseline regressions (i.e. episodes for which I have enough firm-level data). This excludes a number of episodes at the beginning of the sample.

Table 6: Firm-Level Summary Statistics

Panel A: All Firms					
Variable	Obs	Mean	St. Dev	Min	Max
Investment/Assets	534,564	0.07	0.12	0.00	0.84
Log(Size)	623,493	16.17	3.45	7.48	24.08
Cashflow/Assets	481,472	0.03	0.32	-2.21	0.58
Cash / Assets	571,611	0.22	0.36	0.00	2.67
LTD / Total Debt	502,281	0.17	0.19	0.00	0.85
Current LTD / LTD	503,282	0.22	0.28	0.00	1.00
Panel B: Firms in Balanced Panel					
Variable	Obs	Mean	St. Dev	Min	Max
Investment/Assets	175,534	0.07	0.11	0.00	0.84
Log(Size)	190,113	16.68	3.75	7.48	24.08
Cashflow/Assets	152,928	0.02	0.32	-2.21	0.58
Cash / Assets	186,129	0.20	0.33	0.00	2.67
LTD / Total Debt	146,838	0.17	0.18	0.00	0.85
Current LTD / LTD	146,967	0.21	0.26	0.00	1.00
Panel C: Treatment Firms in Balanced Panel					
Variable	Obs	Mean	St. Dev	Min	Max
Investment/Assets	39,299	0.06	0.08	0.00	0.84
Log(Size)	40,618	17.96	3.65	7.48	24.08
Cashflow/Assets	38,259	0.07	0.20	-2.21	0.58
Cash / Assets	40,189	0.15	0.23	0.00	2.67
LTD / Total Debt	48,530	0.16	0.17	0.00	0.85
Current LTD / LTD	48,555	0.36	0.27	0.00	1.00
Panel D: Control Firms in Balanced Panel					
Variable	Obs	Mean	St. Dev	Min	Max
Investment/Assets	49,338	0.06	0.11	0.00	0.84
Log(Size)	51,048	17.31	3.87	7.48	24.08
Cashflow/Assets	47,829	0.04	0.30	-2.21	0.58
Cash / Assets	50,402	0.22	0.35	0.00	2.67
LTD / Total Debt	82,137	0.18	0.20	0.00	0.85
Current LTD / LTD	82,228	0.12	0.21	0.00	1.00

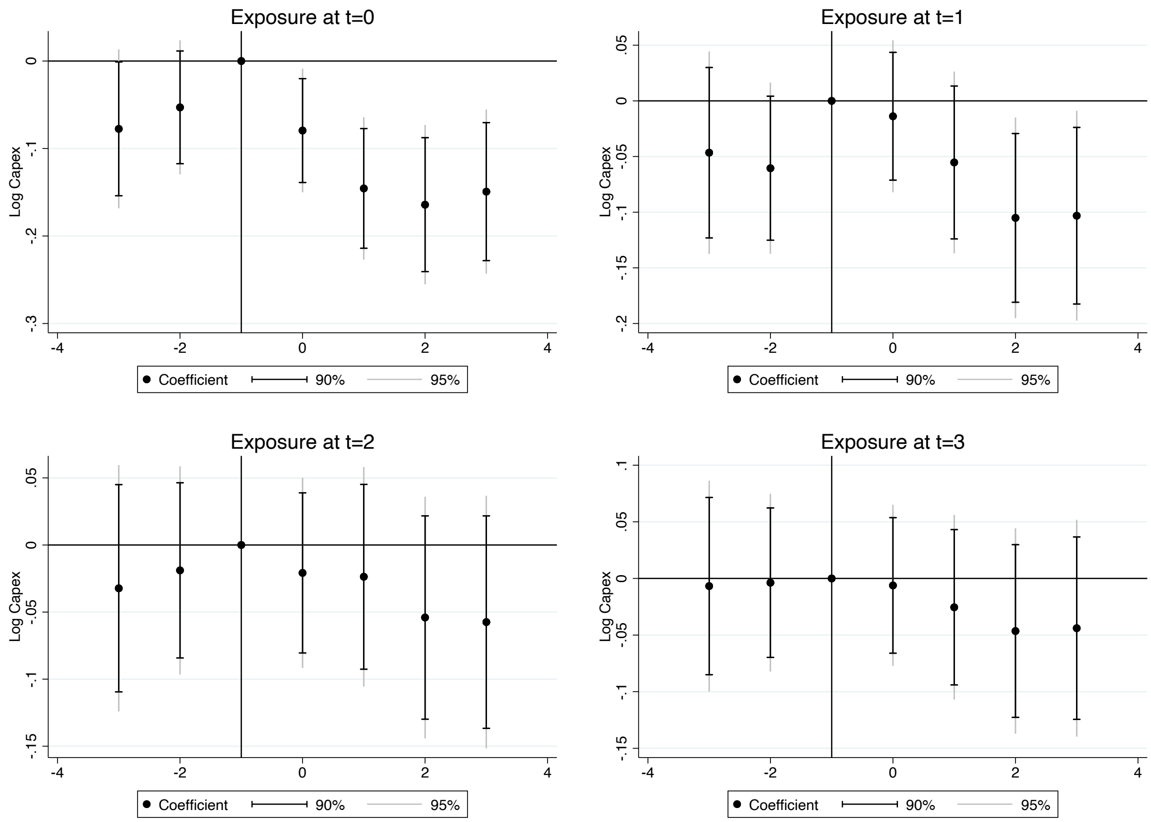
Notes: This table provides descriptive statistics for the firm-level variables in the sample. Investment, cash flows, and cash holdings are scaled by the previous year assets. Panel A includes all firm-years in the sample. Panel B includes firms in the balanced panel that are included in the main regressions. Panel C restricts the sample to treatment firms (firms with more than 20% of long-term term maturing over the next twelve months). Panel D includes only control firms.

Figure 7. Effect On Investment During Sudden Stops of Each Type of Flow



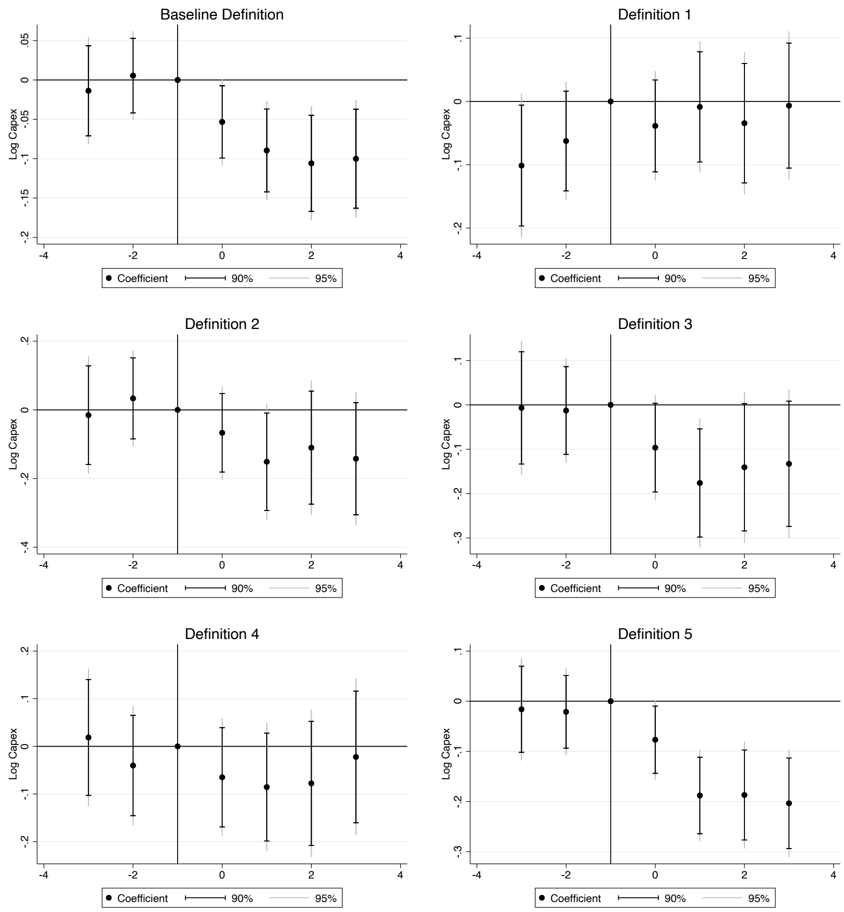
Notes: The figure shows the results of running specification 2 while varying the type of flow used to define sudden stop episodes. Each graph plots the effect for a different of flow: debt, equity, bank, foreign direct investments, and total flows. Each dot represents the coefficient of running the difference-in-difference estimation of the change of the log of investment of exposed firms vs. non-exposed firms, using year $t = -1$ as the baseline and year $t = 0$ as the year of the sudden stop. The results for years $t = -3$ and $t = -2$ show that there are no different pre-trends. The results for years $t = 1, 2, 3$ show that there is a large and persistent effect of being exposed to rollover risk during a sudden stop episode. The results show that the largest effects are observed during sudden stops in portfolio flows (debt and equity).

Figure 8. Effect On Investment by Year of Exposure



Notes: The figure shows the results of running specification 2 while varying the definition of exposure. Each graph plots the effect of being exposed in a different year. Each dot represents the coefficient of running the difference-in-difference estimation of the change of the log of investment of exposed firms vs. non-exposed firms, using year $t = -1$ as the baseline and year $t = 0$ as the year of the sudden stop. The results for years $t = -3$ and $t = -2$ show that there are no different pre-trends. The results for years $t = 1, 2, 3$ show that there is a large and persistent effect of being exposed to rollover risk during a sudden stop episode. Being exposed in year $t = 1$ also leads to a decline in investment, although the statistical significance of the results is lower. Being exposed in years $t = 2$ or $t = 3$ does not lead to statistically significant differences between exposed and non-exposed firms.

Figure 9. Effect On Investment - Different Treatment-Control Definitions



Notes: This figure shows the results of running specification 2 while varying the definition of the treatment-control groups. Each dot represents the coefficient of running the difference-in-difference estimation of the change of the log of investment of exposed firms vs. non-exposed firms, using year $t = -1$ as the baseline and year $t = 0$ as the year of the sudden stop. The results for years $t = -3$ and $t = -2$ show that there are no different pre-trends. The results of the baseline definition show that there is a large and persistent effect of being exposed to rollover risk during a sudden stop episode.

Baseline Definition: Exposed at $t = 0$ vs. not-exposed at $t = 0$.

Definition 1:

Exposed at least once in years $t = 0, 1, 2, 3$ vs. not-exposed at $t = 0$

Definition 2:

Exposed at $t = 0$ and not-exposed at $t = 1$ vs. not-exposed at $t = 0$ and exposed at $t = 1$.

Definition 3:

Exposed at $t = 0$ and not-exposed at $t = 2$ vs. not-exposed at $t = 0$ and exposed at $t = 2$.

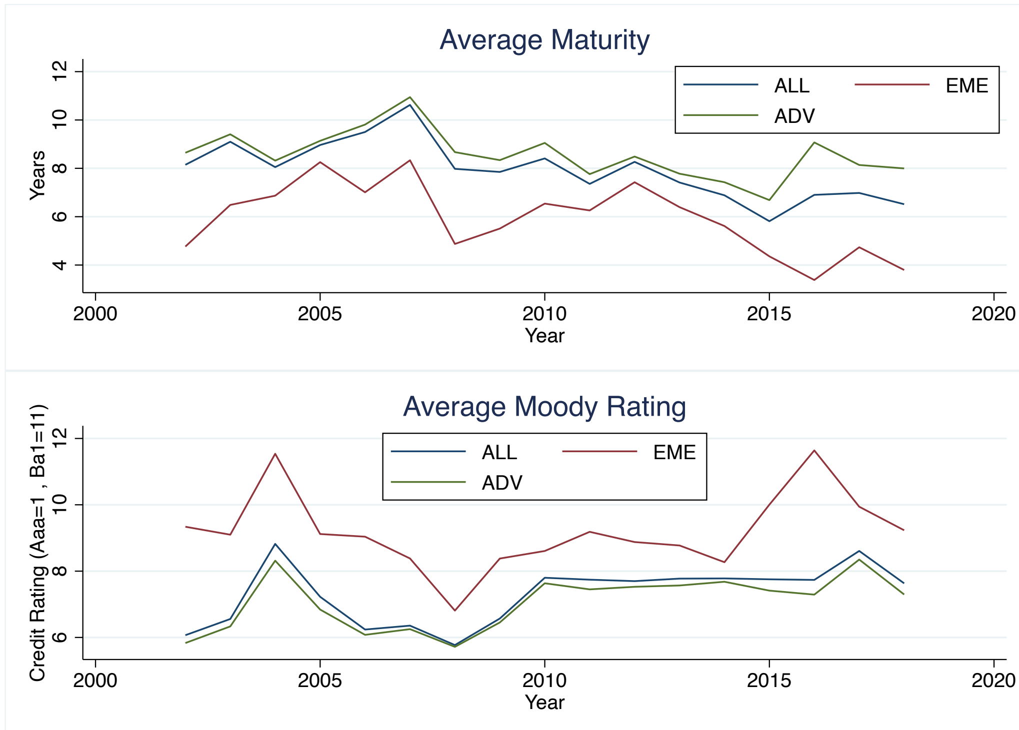
Definition 4:

Exposed at $t = 0$ and not-exposed at $t = 3$ vs. not-exposed at $t = 0$ and exposed at $t = 3$.

Definition 5:

Exposed at $t = 0$ vs. not-exposed at $t = 0$ and exposed at least once in years $t = 1, 2, 3$.

Figure 10. Issuance Trends



Notes: The graphs plot average credit ratings and maturities of bonds in my sample of international bond issuances by non-financial corporates from SDC Platinum.

The pre-global financial crisis expansion period saw extensions in maturities. Bond issuances by firms in emerging countries went from average maturities of five years in 2002 to eight years in 2007.

The post-global financial crisis expansion period seems to be experiencing the opposite pattern. Average maturity was around seven years in 2012 but dropped to three years in 2016.

Shortening maturities might exacerbate rollover risk as discussed in my main results. If firms issue shorter long-term debt, they are more likely to need tod refinance their debt in any given year.

The bottom panel plots average credit ratings. The changes on average maturities do not seem to be explained by changes in the credit composition of issuers.

Table 7: Correlation Between Country Inflows and Country and Firm Issuance

	(1)	(2)	(3)	(4)
	$ln(Proceeds_{c,t})$	$ln(Proceeds_{i,t})$	$ln(1 + Proceeds_{i,t})$	$Issuer_{i,t}$
$ln(Inflows_{c,t})$	0.140***	0.0685***	0.378**	0.0200**
	(0.0445)	(0.0150)	(0.164)	(0.00872)
Observations	1,068	12,477	90,844	90,844
R-squared	0.812	0.738	0.267	0.249
Country FE	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes
Firm FE		Yes	Yes	Yes

Notes: This table shows the correlation between country inflows and country-level and firm-level debt issuance in a regression of the form:

$$Proceeds_{c,t} = \beta_1 Country\, Inflows_{c,t} + FE + \epsilon_{c,t}$$

Column 1 regresses total country proceeds on debt inflows. Debt inflows are not just a transference of existing debt from local to foreign investors, debt issuance is higher during periods of high inflows. Columns 2 and 3 show firm-level regressions (firm-level proceeds): firm issuances go up with country inflows. Column 4 has a dummy equal to one when a firm issues debt in a given year, showing the extensive margin at work: firms are more likely to issue when inflows are high.

Table 8: Effect on Investment by Importance of Long-Term Debt in Firm's Financing

	(1)	(2)	(3)	(4)
	All	$< 10\%$	$10\% - 50\%$	$> 50\%$
$POST_{c,t} \times Exposure_{t=0}$	-0.0896***	0.0394	-0.159***	-0.379*
	(0.0319)	(0.0475)	(0.0399)	(0.215)
$Country \times Year\ FE$	Yes	Yes	Yes	Yes
$Firm \times Event\ FE$	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes

Notes: This table shows the results of running specification 1 for the coefficient β_1 for different subsamples of firms depending on the firm's reliance on long-term debt. Column 1 shows results for the full sample, Column 2 only for firms where long-term debt is less than 10% of total debt. Column 3 only for firms where long-term debt is between 10 and 50% of total debt and Column 4 only for firms where long-term debt is more than 50% of total debt.

The results show that the main results of the paper are indeed driven by firms for which long-term debt is a substantial source of financing. For firms with low shares of long-term debt (Column 2), having a large share of long-term debt coming due does not predict an effect of firm investment following the sudden stop.

Table 9: Effect of Exposure on Investment by Reliance on Bank Debt

	(1)	(2)	(3)
	All	< 75%	> 75%
$POST_{c,t} \times Exposure_{t=0}$	-0.152***	-0.191**	-0.128**
	(0.0476)	(0.0783)	(0.0588)
$Country \times Year\ FE$	Yes	Yes	Yes
$Firm \times Event\ FE$	Yes	Yes	Yes
Controls	Yes	Yes	Yes

Notes: This table shows the results of running specification 1 for the coefficient β_1 for different subsamples of firms depending on the firm's reliance on bank debt. Column 1 shows results for the full sample, Column 2 only for firms for which bank debt is less than 75% of their total debt. Column 3 only for for firms for which bank debt is more than 75% of their total debt.

The results show that the effect is indeed larger for firms that rely more on capital markets (Column 2). However, Column 3 shows that the effect is significant even for firms that rely largely on bank debt.