Capital Flows and the Real Effects of Corporate Rollover Risk

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ABSTRACT

How do shocks to international capital flows get transmitted to the real economy? In this paper I exploit plausibly exogenous variation in firms' exposure to rollover risk to identify a causal channel. Using a panel of firms across a large number of countries, I show that firms with higher exposure -as measured by the share of long-term debt maturing over the next year- reduce investment 10 percentage points more than non-exposed firms following sudden stops in capital flows. I also show that the results are persistent -exposed firms have lower investment even 3 years after the shock and that they extend to other firm outcomes, such as employment and total assets. Additionally, 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.

1 Introduction

Large reversals in international capital flows are costly and have been associated with financial instability. Although this used to be a concern mostly for emerging markets the global financial crises showed that capital flow volatility is a concern for advanced and emerging economies alike. Changes in policymakers attitudes towards capital flows are best exemplified by the IMF changing its stance on the benefits of financial liberalization and advocating for capital flows management measures in IMF (2012). In parallel, the theoretical literature has made substantial progress in highlighting the various externalities capital flows create: Caballero and Simsek (2018), Farhi and Werning (2016), and Korinek (2018). Despite this progress in identifying some of the costs of volatile capital flows, concrete empirical evidence on the transmission channels of sudden stops remains scarce.

Identifying a causal effect of capital flows at the aggregate level is notoriously hard as capital flows flows are endogenous. 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 seek to establish a causal effect of sudden stops in capital flows, by exploiting plausibly exogenous cross-sectional variation in the level of firm exposure to credit supply 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 and other real outcomes more than similar firms that do not need to rollover long-term debt. If my measure of exposure to rollover risk is indeed exogenous, my results can be interpreted as the causal effect of being exposed to credit supply shocks during a sudden stop episode.

My identification strategy rests then in finding exogenous variation in firms' exposure to rollover risk and in identifying episodes of large contractions in capital flows. Following Almeida, Campello, Laranjeira, and Weisbenner (2009) I measure exposure

The main contribution of this paper is that it brings firm-level data, and exploits a discontinuity in firms' long-term debt maturity structure, to answer an important question of the international macroeconomics literature. Besides establishing the main result, which is that sudden stops have real effects on investment, this paper adds a number of interesting facts to the literature. First, it highlights the importance of using gross inflows -as opposed to net inflows- to define sudden stops. Using gross inflows identifies a number of sudden stop episodes that would have not been identified using net inflows data. This result suggests that large reversals in the investment by foreigners have real effects even if at the aggregate level 'retrenchment' behavior by local investors makes up for some of the losses 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. 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 period of large economic 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 somewhat a new result considering that studies that explore the cross-section 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.

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 at reducing volatility in capital flows.

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 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.

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), Forbes and Warnock (2020), and Cavallo, Powell, Pedemonte, and Tavella (2015) have highlighted the importance of studying how the effects of capital flows vary by the time 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 my 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 independance. 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 further putting 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. (2009) introduces the idea of using the share of long-term debt coming due in the following year as 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 (2008) explores the role of firms' currency mismatches while Bleakley and Cowan (2010) studies the impact of short term debt. Both studies find no effects of being exposed to depreciations or large sudden stops.

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 firm's 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 large credit supply leads to an increase in capital misallocation as only large unproductive firms can take advantage of the extra supply of credit.

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

My main source of data is annual balance sheet information obtained from Worldscope. This is a panel of firms across a large number of countries in 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 800,000. 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 firms in countries for which I don't have enough observations to have enough cross-sectional variation.

From this dataset I obtain all the main variables of interest for my analysis: investment, debt, and long-term debt, long-term debt maturing over the next year, among many others.

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 day 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 us 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, making 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, and hence, it allows me to build predetermined proxies for when firms should have large maturities 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. Quaterly 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 Stops' section.

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 that such as size and profitability.

To get around the issue of endogeneity, I use the share of long-term debt maturing over the next 12 months. This measure, introduced by Almeida et al. (2009) 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 relies on the timing of maturities

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. Three 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 most likely to affect firms' ability to rollover debt. Moreover, debt flows are the most 'fickle' as discussed Caballero and Simsek (2018), and Korinek (2018), and hence it allows me to identify a sudden stop episode as early as possible. Additional, the reason to use gross inflows, as opposed to net, is twofold. First, focusing on the behavior of foreign investors might alleviate concerns about the endogeneity of sudden stops. And 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).

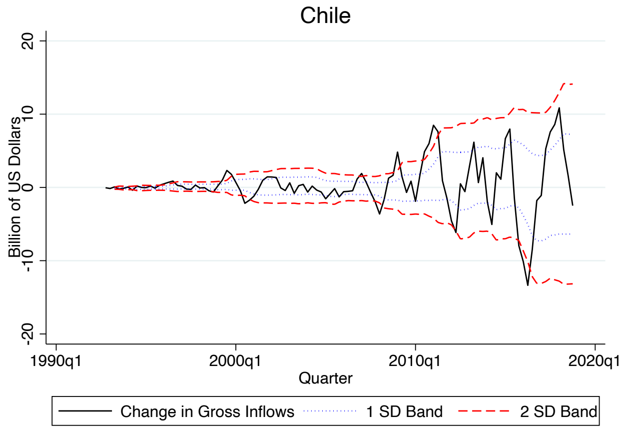
How do I identify periods with large contractions in inflows? I identify periods in which the drop in capital flows is large relative to the recent path of capital flows in the given country, following Forbes and Warnock (2012) and Forbes and Warnock (2020). More specifically, starting with the corresponding 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 4 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 deviation below the mean. This definition is intended to capture very large changes -changes that deviate 2-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 2 or 3 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 capturing 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 1-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



Note: The figures 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.

3.3 Capital Flows and Maturity Decisions

How do firms' maturity decisions at debt origination vary with capital flows?

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:

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

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: $t = 0$. $POST_{t,c} = 1$ 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: Diff-in-Diff Estimator of the Effect of Exposure on Investment

	(1)	(2)	(3)	(4)	(5)
$POST_{t,c} \times Exposure_{t=0}$	-0.0960***	-0.0973***	-0.0989***	-0.107***	-0.0775**
	(0.0341)	(0.0349)	(0.0340)	(0.0348)	(0.0326)
Observations	22,024	22,024	22,024	22,024	24,002
CountryYearFE	Yes	No	Yes	No	Yes
FirmEvent FE	Yes	Yes	No	No	Yes
Controls	Yes	Yes	Yes	Yes	No

Note: The 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 10 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 10 percentage points more than that of non-exposed firms. This differential amounts to exposed firms reducing investment around 3 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%.

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

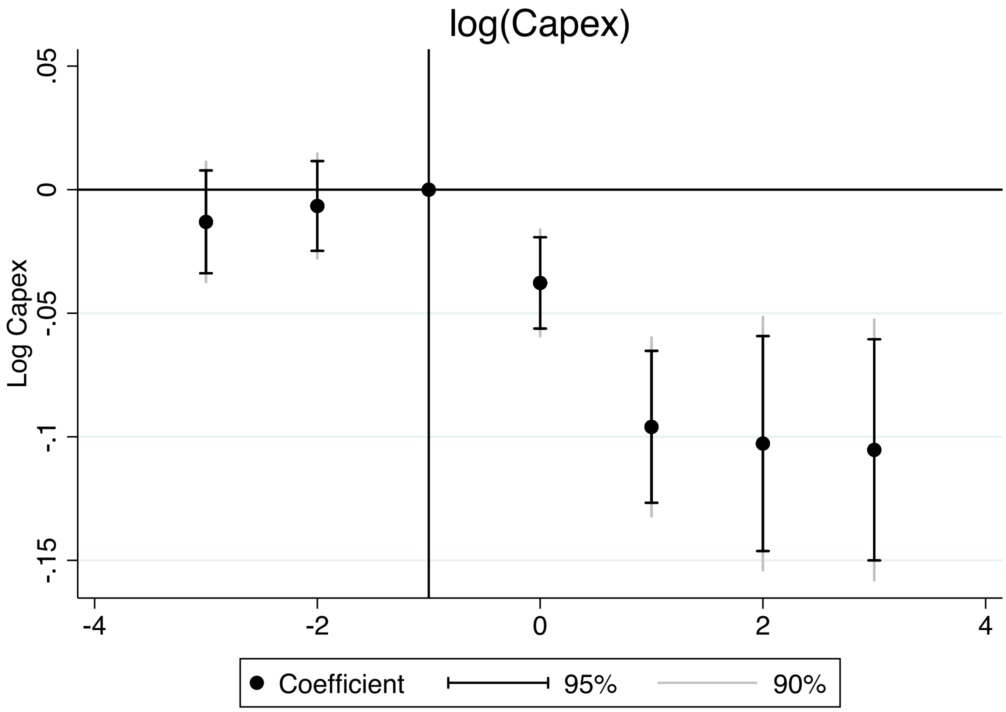
As with any difference-in-difference setting, the validity of the results depends on exposed and non-exposed firms showing parallel trends in the period before the sudden stops. In order to test for pre-trends as well as to study the persistence of the effects found in the previous section I run the 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 2 plots the coefficients, as well as the confidence interval for all seven years around the sudden stop episode. The first importance result to notice from the graph is the presence of parallel trends in investment before the sudden stop. As figure 2 the coefficients for years $t = -3$ and $t = -2$ are indistinguishable from 0.

Figure 2. : Pre-trends and Persistence



Note: The table shows the results of running specification 3. 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.

In terms of the post-event effects, the coefficient for $t = 0$ is around 4% showing that there is initial impact on the invesment 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 persistent for at least the 3 years following the sudden stop episode.

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. This would be the case for instance 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 7-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.

Additional 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 the 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 4.5 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 only persistent -and gets larger- on years with sudden stops.

Table 2: Diff-in-Diff Estimator SS vs Normal Years

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

Note: The 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 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 only persistent, and gets larger, in years with sudden stops.

5.1.2 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 141 sudden stops I identify in my main specification: only 21 experience negative GDP growth the year of the SS (34 the year following the SS) and only 7 have a GDP contraction of at least 2% the year of the SS (20 the year following the SS). 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 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.

5.1.3 Idiosyncratic vs. Global Sudden Stops

5.2 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?

Table 3: Diff-in-Diff Estimator Using Past Exposure

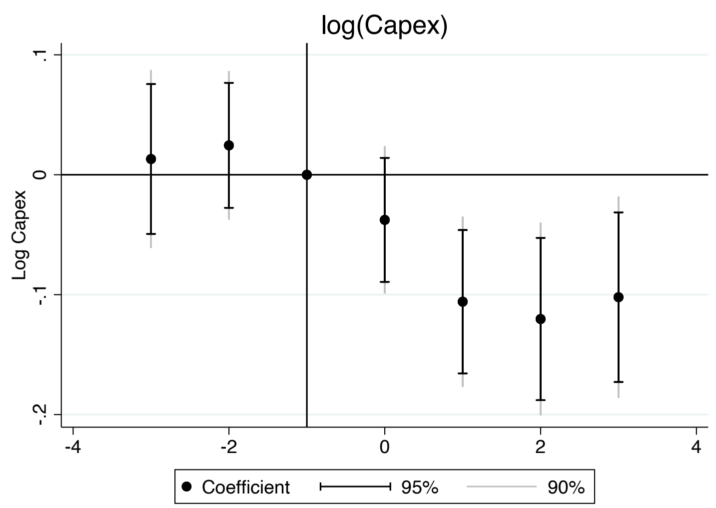
VARIABLES	(1) $log(Capex_{t+1})$	(2) $log(Capex_{t+1})$	(3) $log(Capex_{t+1})$	(4) $log(Capex_{t+1})$	(5) $log(Capex_{t+1})$	(6) $log(Capex_{t+1})$	(7) $log(Capex_{t+1})$
$POST_{t,c} \times Exposure_{t-k}$	-0.0960***	-0.0313	-0.0603*	-0.0534	-0.0224	-0.0326	-0.0460
	(0.0341)	(0.0358)	(0.0352)	(0.0477)	(0.0494)	(0.0473)	(0.0493)
	k=0	k=-2	k=-3	k=-4	k=-5	k=-6	k=-7
CountryYearFE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
FirmEvent FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Is my measure of exposure persistent and simply capturing other differences between firms?

I test by using exposure in previous years as my treatment variable.

Results indicate that only exposure the year of the sudden stop causes significant effects.

Excluding firms with no variation in the measure of expose leads similar results



Excluding all firms that do not have exposure in any of the 7 years in the event window does not substantially affect the results.

5.2.2 Maturity Management

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 channel through which large movements in capital flows affect get transmitted to the real economy. Large boom-bust cycles in capital inflows

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, policy makers should pay more attention, and possibly regulate firms' maturity decisions at issuance. This is because 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 when/if the capital flow boom ends. This is the case because the shorter maturities are, the more likely a firm is exposed to rollover a 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

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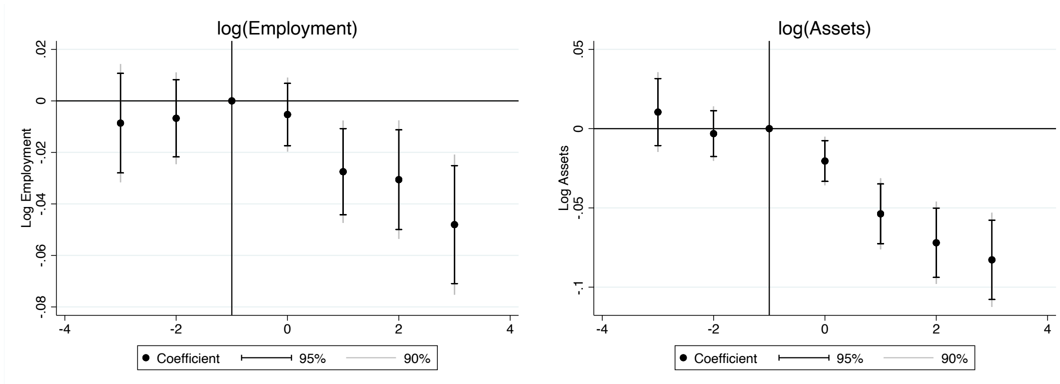
Appendix

Figure 3. : Employmeny and Assets Growth



- Exposed firms contract employment 3-5% *more* than non-exposed firms.
- Exposed firms contract total assets 5-8% *more* than non-exposed firms.

Table A1: Diff-in-Diff Estimator For Change in Total Long-Term Debt SS vs Normal Years

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

- Exposure has a large effect for all years (stops and non-event years).
- There is an additional effect on years with sudden stops
- More importantly, the effect becomes larger with time, following sudden stops while it goes to zero in non-event years.