M&A under financing frictions: evidence from credit supply shortfalls*

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Abstract

Despite empirical evidence showing that firms' investments decrease during periods of credit supply shortfalls, little is known about how firms can eventually circumvent such financing frictions, thereby attenuating the adverse effects of negative credit supply shocks. In this paper, we show that firms can relieve financing frictions during banking crisis periods by selling equity stakes to outside investors. We examine Mergers and Acquisitions (M&A) transactions worldwide between 1990-2019 and the outcomes of targeted firms ex-post the deal. By exploring cross-sectional variation in the supply of credit induced by banking crises, we find that firms that have higher levels of expiring debt maturities in the year of the credit shock are more likely to become targets in M&A deals. Moreover, we find strong evidence that target firms invest more and issue more debt after the deal relative to other financially constrained firms that did not undergo such transactions. Our results remain robust after controlling for alternative explanations and show that M&As can work as leeway to relieve financing frictions in periods when credit supply frictions are more prevalent.

Keywords: M&A; Financial Constraints, Financing Decisions, Corporate Investsment.

J.E.L. codes: C11, C52, G11, G17, F31

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1 Introduction

Why some firms are more constrained than others in their ability to fund profitable investment opportunities and smooth out negative credit market shocks? This question traces back to the assumptions embedded in Modigliani and Miller (1959), where perfect capital markets implied that all profitable investment opportunities should be undertaken. In practice, absent from frictionless financial markets, standard models of firm investment under financing frictions shed light on the transmission of negative credit supply shocks into the real economy (Stiglitz and Weiss, 1981; Holmstrom and Tirole, 1997; Jaffee and Russell, 1976). Together with financing frictions, negative shocks to the supply of external finance should dampen firms' future investments if firms are unable to fund profitable investment opportunities with internal resources (Duchin et al., 2010).

In line with such theoretical predictions, the financial crisis of 2008 has motivated a series of empirical studies aimed at identifying the transmission of negative credit supply shocks into the real economy – *i.e,* the *credit supply channel* –, where corporate investment for non-financial firms significantly declined following the onset of the crisis (Almeida et al., 2012; Duchin et al., 2010). Moreover, the *supply-driven* nature of this crisis, which is largely attributed to being generated outside of the real sector, is also a key feature to understand how large contractions in the supply of credit might affect firms' future outcomes through the transmission of banks' weak balance sheet positions.

As shown in a recent survey presented in Campello et al. (2010), results from more than 1,050 Chief Financial Officers (CFOs) from the U.S, Europe, and Asia show that during the 2008 financial crisis, constrained firms have deeper cuts in capital spending and employment levels, burned through more internal resources, drew more on credit lines, and were more prone to liquidate assets to fund their operations. Additionally, these firms were also more likely to foregone profitable investments, as 86% of the CFOs of financially constrained firms said that investment in profitable projects was bypassed due to a lack of funding.

A fundamental question that arises in this setting is: how do firms withstand such shocks? Although there is much attention from the empirical literature on the transmission of credit supply shocks to firms' future outcomes, less is known regarding the channels by which firms can alleviate such adverse effects. As a piece of evidence on this issue, Garcia-Appendini and Montoriol-Garriga (2013) show that, during contractions in the supply of credit by financial lenders, *between-firm* credit provision among supplier-client parties increases: as compared to *ex-ante* cash-poor firms, more liquid suppliers extended trade

credit to other corporations, consistent with supplied firms providing liquidity insurance downstream.

In this paper, we investigate an alternative leeway in which firms can attenuate the adverse effects of negative credit supply shocks: by selling equity stakes through Mergers and Acquisitions (M&A). transactions. Several papers have emphasized the interplay between firms' financial constraints and the occurrence of M&A as a way to boost firms' ability to access funding, either by direct equity placements or through better access to capital markets. For example, using a sample of European majority acquisitions, Erel et al. (2015) show that financially constrained firms are more likely to be targeted in such transactions, and that *ex-post* deal occurrence, these firms are able to relieve their financing constraints on several dimensions, such as increasing levels of future investments, debt, and a lower dependence upon internal resources. Similarly, other papers, such as Ouimet (2012); Liao (2014), show that, among other motivations, target firms' financial constraints are positively related to the occurrence of minority acquisitions. Finally, Khatami et al. (2015) shows that, in terms of acquisition gains, acquisitions involving financially constrained targets yield positive abnormal results for both parties. Overall, empirical evidence is in line with the argument that such transactions are able to increase the ability of target firms to access resources, either through an internal reallocation of assets (*i.e,* internal capital markets) or a better a access to external markets, by decreasing the firms' information asymmetry to outside market participants (Hertzel and Smith, 1993).

As expected, identifying a causal relationship between firms' financing constraints and the occurrence of M&As has several empirical caveats. First, as financial constraints are not directly observable, relying on indirect proxies for firms' internal cash dependence, such as dividend payouts (Fazzari et al., 1988) and some indexes of financial constraints, such as the KZ Index ((Kaplan and Zingales, 1997), the WW Index (Whited and Wu, 2006) and the SA Index (Hadlock and Pierce, 2010) may capture several firms' dimensions other than financial constraints, such as unobservable investment opportunities. Additionally, despite the advances in the related literature in providing more reliable measures of firms' financial constraints, Farre-Mensa and Ljungqvist (2015) highlights that most of the indirect proxies used proposed in the literature do not adequately capture the degree to which firms are financially constrained in situations where these firms should behave *as if* they were financially constrained, such as episodes of decrease in the supply of credit.

Besides correctly capturing firms' financial constraints, interpreting the differential *ex-post* outcomes following deal occurrence for firms that were targets of such transactions as causal is also problematic. Several papers highlighted the potential *ex-post* that might

occur to target and acquirer firms following M&A transactions, such as changes in governance levels and gains from operational synergies. If this is true, a causal interpretation of the *ex-post* effects following M&A episodes as due to *liquidity* motives is likely to be not accurate. To that matter, as selection and sorting of firms into targets and acquirers may drive the effects due to deal unobservables, a simple comparison between targeted and non-targeted firs is likely to capture other mechanisms previously discussed in the literature that are not directly related to firms' financial constraints being relieved after deal occurrence.

To overcome these empirical issues and shed light on how firms can attenuate the transmission of credit supply shocks, we exploit the differential exposure of firms to credit supply shocks occurring in their countries' headquarters to analyze deal future outcomes. Following Almeida et al. (2012), we explore exogenous variation in long-term debt maturity due in years when target firms were experiencing a banking crisis in their home countries in *differences-in-differences* approach to analyze whether financially constrained firms are more likely to be targeted in M&A operations. As such, our identification strategy aims to insulate the estimated effects from the aforementioned selection on unobservables problem happening both in the sorting of firms into financially constrained and unconstrained and from the potential confounders on deal *ex-post* performance.

Our results show that, during years when target firms were experiencing a banking crisis in their home countries, results show that firms with higher portions of expiring debt maturities are 15% more likely to be targeted in acquisitions. Our results are robust to different specifications of firms' expiring debt maturities as well as remain unchanged even after employing a wide set of fixed effects. In several additional results, we show that this increase in the likelihood is not explained by past differences between exposed/non-exposed firms, and concentrates in the exact period when a banking crisis hits the targets' headquarters. Overall, our results are consistent with the argument that, during periods of credit supply shocks, access to debt markets is crucial may be valuable for firms' to mitigate the effects of uncertainty on corporate policies, such as precautionary savings and investments (Favara et al., 2021).

Despite the increased likelihood of being acquired for financially constrained firms around banking crises, we cannot, directly from these results, claim that these acquisitions were exclusively motivated by liquidity needs. As thoroughly discussed in Eckbo (2014); Ouimet (2012), apart from financing motivations, acquisitions can arise as a solution for circumventing contracting issues, as well as reducing agency costs by enhancing corporate governance practices. To the extent that these motivations are not mutually exclusive, one

can argue whether the *ex-post* deal outcomes for the target firms behave in line with the predictions highlighted in the theoretical models of firm investment under information asymmetry (Holmstrom and Tirole, 1997).

As stated in Erel et al. (2015), when access to capital markets is imperfect, value maximization will lead managers to adopt financial policies that seek to guarantee that the most important investments will be effectively put in place. To that matter, such rationale allows us to draw predictions on how some key firm fundamentals should behave *as if* financial constraints were relieved. To shed even further light on the liquidity motivations behind such transactions, we focus on the subset of firms with expiring debt maturities at the onset of a banking crisis and compare the *ex-post* outcomes of targeted firms to the outcomes of firms that *have not* engaged in any deal throughout the studied period. Overall, not only our estimates are consistent with targeted firms relieving their financing constraints *ex-post* deal occurrence during crisis periods, but also they seem to be persistent over a window of three years after the shock. Importantly, our results do not appear to present any pre-trends between targeted and non-targeted firms over a window of three years before a banking crisis, which further reinforces our argument that confounding factors are not driving our results.

As a first piece of evidence, our results on the *ex-post* differentials show that targeted firms were able to attenuate the adverse effects of the decrease in the supply of credit by more than their counterparts: although firms, on average, experienced a drop in -6.9% in asset levels, targeted firms were able to absorb 5.8% of this shock, or 84% less than their counterparts. Similarly, as managers of financially constrained firms might hold cash and liquid assets as a precautionary motive for insuring against credit supply shortfalls (Ferreira and Vilela, 2004), although firms' cash holding levels decreased almost 14% after the shock, the effect is much less pronounced for targeted firms, as they were able to offset 9.4% of this effect, indicating that non-targeted firms were forced to rely more on cash buffers to withstand the negative credit supply conditions.

Moreover, since credit market frictions might oblige firms to foregone economically viable investment projects, if M&A transactions have the potential to mitigate financing frictions, one should expect financially constrained targets to present a higher growth in debt levels. Our results confirm that this is the case: targeted firms increase long-term debt levels 11% more, on average, relative to otherwise similar firms, with increasing effects that amount to almost 20%, on average, three years after the shock. Interestingly, results are not statistically significant while looking at short-term debt levels, which are often related to firms' current operations, and therefore are more prone to be affected by

confounding channels that also change after the deal has taken place. Overall, these results help to alleviate concerns about the specific channel that is driving *ex-post* differentials.

To shed even further on the mechanism behind our findings, we employ several robustness checks and additional analyses to rule out potential confounding factors that might also be related to the *ex-post* effects of M&A. Our results remain robust to different specifications of the financing constraints variables, different sets of controls, placebo tests around banking crisis years. Moreover, when replicating our results in a subsample of firms that *were not* exposed to liquidity issues - *i.e,* those with lower expiring debt maturities -, we do not find any significant results. Moreover, when comparing only firms that were targeted in M&A transactions during banking crises, but varying on their degree of exposure due to expiring debt maturities, we find higher long-term debt growth levels for the subset of financially constrained firms, with positive and statistically significant effects.

Overall, we find strong evidence that targeted financially constrained firms invest more, issue more debt, and reduce the dependence on cash holdings *ex-post* deal occurrence as compared to those that have not engaged in such deals. Taken together, these results highlight the role of the M&A channel in relieving firms' financing constraints during credit supply shortfalls.

Our results contribute to several strands of the literature. First, we contribute to the growing literature on understanding the credit supply channel and its spillovers to the real economy. The relevance and the consequences of the transmission of credit-supply shocks to the real economy have been previously documented in several studies. For example, previous papers have documented the adverse effects of the transmission of the recent financial crisis on firms future investments and profitability (Duchin et al., 2010; Cingano et al., 2016; Chava and Purnanandam, 2011; Almeida et al., 2012), capital market dislocations (Jang, 2017), stock valuation (Carvalho, 2015; Tong and Wei, 2011), corporate payouts (Bliss et al., 2015), lending patterns (De Haas and Van Horen, 2013; Schnabl, 2012; Giannetti and Laeven, 2012), and liquidity provision Garcia-Appendini and Montoriol-Garriga (2013). More specifically, some studies have emphasized the nature of the credit supply shock to the severity of the transmission to the real economy by contrasting different transmission channels, such as banking crises (Levine et al., 2016; Giesecke et al., 2014; Iyer et al., 2014; Kroszner et al., 2007), bank resolution (Beck et al., 2021), bond market crises (Giesecke et al., 2014), as well as highlighting heterogeneous effects on firms' outcomes depending on shareholder protection laws (Levine et al., 2016).

To that matter, as most of these studies focus on specific market events, such as the

recent financial crisis of 2007-2008, our study adds valuable insights to this literature by exploring the role of banking crises over the cross-section of countries during 1990-2019. For example, when analyzing the adverse effects of the 2008 subprime crisis, studies such as (Almeida et al., 2012; Duchin et al., 2010) show consistent evidence for the negative effect of the credit supply shortfall on the U.S listed firms' investment behavior *ex-post* crisis period. Overall, although there is a more consolidated understanding of the transmission of negative credit supply shocks in the real economy, less is known regarding how firms can eventually circumvent or, at least, attenuate such shocks. Our results show that, in these situations, financially constrained firms are more likely to engage as targets in M&A transactions. As such, our paper complements the findings of (Garcia-Appendini and Montoriol-Garriga, 2013) on the trade-credit channel and highlights M&A as an alternative channel for financially constrained firms to smooth out negative credit supply shocks induced through credit supply shortfalls.

Furthermore, our work also adds up to the growing literature on the study of the determinants and consequences of M&A transactions. While some authors seek to understand characteristics of more aggregate phenomena, such as merger waves (Xu, 2017; Andriosopoulos and Yang, 2015; Martynova and Renneboog, 2008a), several authors have focused on understanding the specific motivations behind M&A transactions, such as governance spillovers (Martynova and Renneboog, 2008b), bonding and certification effects (Burns et al., 2007), internal capital markets (Doukas and Kan, 2008), product-market relationships (Allen and Phillips, 2000), among others. On the other hand, other authors have documented the effects of such transactions on firms' market valuation (Dos Santos et al., 2008; Francis et al., 2008), minority shareholders' returns (Croci and Petmezas, 2010), and ownership concentration (Bhaumik and Selarka, 2012).

To that point, our work contributes to the specific strand of this literature that analyzes the interplay between corporate liquidity and M&A activity. Almeida et al. (2011) provide a theoretical framework to analyze the acquisition of distressed firms by liquid firms in the same industry, even in the absence of operational synergies. As such, these "liquidity mergers" would emerge as a way to reallocate assets to solvent firms and avoid inefficient liquidation. Their empirical results corroborate with the model's prediction, with liquidity mergers occurrence more in industries with higher asset specificity, but transferable across firms. Additionally, while some studies have documented a relationship between MA characteristics and liquidity provision by focusing on the acquirer side (Yang et al., 2019), other studies have documented such relationship from the target's perspective (Erel et al., 2015; Liao, 2014; Khatami et al., 2015; Masulis and Simsir, 2018).

For example, Erel et al. (2015) use a sample of European acquisitions and find that majority acquisitions are an effective way by which financially constrained firms can relieve financing frictions and foster corporate investment. Their results show that target firms decrease their degree of financial constraints in several measures *ex-post* the deal occurrence. However, as a full integration between the target and the acquirer firm is also potentially related to operational synergies - which in turn can affect the likelihood of a deal outcome-, it is difficult to assess the relative importance of financial constraints motive to the observed increases in investment. As a way to partly overcome such difficulty, Liao (2014) uses a panel of minority block acquisitions from 1990 to 2009 and shows a positive relationship between minority acquisitions and financial constraints. Her results indicate that not only target firms are, in general, financially constrained, but also that minority acquisitions are related to increasing stock prices at announcement dates, as well as increases in future investments after the deal occurrence. Considering a time-span of two years following the acquisition, 27% (9%) of the target firms issue new equity (debt), raising 27% (24%) of their market capitalization.

Notwithstanding, none of the previous studies is conducted under a situation of pronounced decline in the supply of credit. Crucially, to the extent that unobserved investment opportunities are heterogeneous across deals, the lack of a clean identification strategy casts doubt on the endogeneity of the relationship between deal occurrence and investment opportunities, which can severely undermine a causal interpretation of the effect (Farre-Mensa and Ljungqvist, 2015). In this sense, by focusing on periods marked by banking crises, our study complement previous findings Liao (2014); Erel et al. (2015); Ouimet (2012) on financial constraints and M&A activity not only by explicitly considering a situation of special interest on the behavior of financially constrained firms – *i.e,* when credit market imperfections are more prevalent and credit supply has decreased but also by employing an identification strategy that is less affected by the presence of unobserved confounders that might also explain deal activity and firms' *ex-post* outcomes.

Overall, although several papers have emphasized the relevance of credit supply shocks and, in special, the specific role that banking crises have on the transmission of these shocks to the real economy, less is known about firms can eventually attenuate these adverse effects. To that point, our work extends the understanding of M&A as a potential channel to alleviate firms' liquidity needs induced by banking crises.

The remainder of this paper is structured as follows. Section 2 describes the data sources and sampling procedures used to construct our dataset. After that, Section 3 provides a detailed discussion on how to measure financial constraints and the empirical

caveats related to assessing the causal effects in our framework, as emphasizing the role of our identification strategy as a way to circumvent some of the empirical issues found in previous studies. Section 4 and Section 5 provides a discussion of the results and some robustness checks. Finally, Section 6 concludes and provide directions for future research.

2 Data and Sampling Procedures

We construct an extensive database at the *firm-year* level by using three main sources of data: deal information, target firms' financials, and country characteristics. In the next subsections, we provide a thorough description of the main steps to construct the dataset used in our empirical analysis.

2.1 Deal Activity Data

As our starting point, we collect transaction data from *Securities Data Company Platinum (SDC Platinum)*, the industry's standard for information on M&A activity, maintained by *Refinitiv*. *SDC Platinum* provides broad coverage of detailed information on historical transactions for listed and non-listed firms worldwide, such as acquirer and target information, percentage of acquired and final shares by the acquirer firm, payment method, among others. Specifically, we collect all transactions from 1990 to 2019 in which the deal status was defined as "completed" and remove any duplicated transactions, yielding 928,48 deals.

Next, we remove all deals in which i) target and acquirer firm belong to the same ultimate parent (67,237); and ii) deals where the target firm belongs to the financial industries and the utilities sector[1] (178,458), leaving us with 683,053 unique deals.

2.2 Firms' Financials and Country-Level Financial Development Data

Along with the transactions collected in *SDC Platinum*, we collect target firms' financials in *COMPUSTAT US/Global* by matching on firms' Ticker, SEDOL and CUSIP identifiers. As such, for all the deals in our sample, we collect firm-year information regarding key target firm fundamentals, such as Cash-Flows, Assets, Profits, Short and Long-Term Debt, among others. Importantly, we drop all deals in which we are not able to recover target

[1]Specifically, we drop all observations for firms that belong to the following 2-digit SIC codes: 45, 49, 60, 61, 63, 64, 65, 66, 67, 92, 94, 95, 96, 97, and 99.

firm financials from *COMPUSTAT* using Ticker, CUSIP, or SEDOL codes (622,393), leaving us with 60,660 deals.

To be able to compare targeted and non-targeted firms, we merge our data with the universe of *COMPUSTAT* firms that *have not* entered in any M&A transaction as targets during the sample period, collecting their financials across the same sample period. This procedure yields an (unbalanced) panel of firm-year level information regarding targeted and non-targeted firms in M&A activity during 1990 and 2019.

Finally, with our firm-year data, we merge target and acquirer country-year level information regarding firm countries' financial development characteristics using the *Global Financial Development Data (GFDD)*, an extensive dataset developed by *The World Bank Group* that comprises financial and market characteristics for almost 210 economies. More specifically, this dataset comprises several measures of (1) the size and prevalence of financial institutions and markets, (2) the degree to which individuals can and do use financial services, (3) the efficiency of financial intermediaries and markets in intermediating resources and facilitating financial transactions, and (4) the stability of financial institutions and markets.

After applying all the filters regarding key financial indicators – *e.g,* negative values for Assets, Debt ratios, Leverage, as well as negative/greather than one for PPE/Assets, Cash Holdings/Assets, etc –, we reach a final sample of 58,814 deals, with 962,073 firm-year observations comprising targeted/non-targeted firms. Based on this sample, all firms' financial variables were winsorized on an yearly basis at the 1^{th} and 99^{th} percentiles. A detailed description of the main variables used in the baseline specifications is presented in Table 1. Before formally describing the empirical strategy employed in the study, in the next subsection we provide detailed descriptive information regarding several relevant facts in our sample.

2.3 Summary Statistics

As it can be seen from Table 2, firms that have engaged in minority acquisitions as targets are somewhat different in these years from the rest of the sample. In general, these firm-year pairs are slightly bigger, present higher levels of *Cash Holdings*, *Short-Term Debt* and *Sales Growth*, while also presenting lower levels of *Cash Flow* generation and *Long-Term Debt*. The average firm in our sample - weighted by the number of firm-year observations - has an Asset size of approximately 77 billion USD in nominal values. This value is considerably *higher* than the median firm in Compustat: among all years, the average firm in

our sample is equivalent, in terms of *Total Assets*, to the 85^{th} percentile of the *Total Assets* distribution for that year. This possibly reflects the fact that firms that engage in minority acquisitions - or that are headquartered in countries with higher deal activity - tend to be bigger than those that do not have well-developed capital markets in their home countries, which can coincide with the coverage from *SDC Platinum*. While we acknowledge that our effects are not representative of the overall population of firms, due to its representativeness in terms of size, we hypothesize that a better *ex-post* deal performance for financially constrained firms in our sample would possibly imply significant aggregate effects for the economy.

Finally, despite the fact that some of the firms' financial characteristics are statistically different among target/non-target firms, the magnitudes of such differences are generally modest. Notwithstanding, since a naive comparison between these groups is unfeasible, the next section will provide a detailed description of the empirical strategy adopted in the study to tease out the relationship between financing constraints and deal outcomes.

3 Methodology

3.1 Measures of Financial Constraints

As in most of the Corporate Finance issues, analyzing the effects of financing constraints on firms' future behavior is not straightforward, as a series of unobservable characteristics may be related to the firms' degree of financial constraints and, at the same time, determine their future outcomes. Importantly, simply absorbing firm-invariant confounders through firm fixed effects is unlikely to be sufficient, as there are concerns that time-varying, unobservable investment opportunities, which are observable to the firms' but unobservable to the econometrician, may be driving the results. Due to this reason, analyzing the effects of target firms' financial constraints on their decision to sell minority stakes has several empirical caveats.

First, as financial constraints are not directly observable, empirical attempts to analyze the willingness of firms to foregone positive NPV projects generally use indirect proxies related to firms' investment sensitivity. Since the seminal paper by Fazzari et al. (1988), several attempts to effectively measure the degree of firms' financial constraints have been applied, although there is no consensus regarding which is the best proxy for identifying a firms' investment sensitivity to financing conditions. While some measures focus on only one specific dimension of financial constraints (*e.g*, size, dividend payout, ratings) -

Fazzari et al. (1988); Carpenter and Guariglia (2008), indexes of financial constraints that spans several dimensions of firms' financial decisions have been widely applied, such as the *KZ* (Kaplan and Zingales, 1997; Lamont et al., 2001), the *WW* (Whited and Wu, 2006) and the *SA* (Hadlock and Pierce, 2010) indexes.

Despite the lack of a general agreement with regards to these measures of financial constraints, several recent papers adopt some of these measures by splitting the sample into terciles/quintiles of the distribution, assigning firms into groups of high/low degree of financial constraints (Khatami et al., 2015; Liao, 2014). However, recent work by Farre-Mensa and Ljungqvist (2015) shows that although widely applied in the literature, such measures do not adequately identify firms that behave as if they were financially constrained. By defining two testable assumptions regarding the behavior of financially constrained firms in debt and equity markets, the authors show that none of these measures adequately classifies financially constrained firms as if they were in fact constrained.

To overcome some of these limitations, we depart from the aforementioned studies and follow Almeida et al. (2012) by using the value of long-term debt due in one year as a measure of financial constraints. Being applied to analyze the differential effects of credit supply shocks on financially constrained firms during the recent financial crisis, this approach has several advantages over the preexisting proxies for financial constraints, as it represents a situation in which firms with higher levels of maturing debt suffer more from exogenous variations in the supply of bank loans (Farre-Mensa and Ljungqvist, 2015). As such, the rationale behind considering the long-term debt maturity as a measure of financing constraints is that firms with a higher of long-term debt maturing *at the onset of the crisis* crisis were forced to adjust their decisions in a more pronounced way than otherwise similar firms that were not *ex-ante* exposed to higher levels of maturing debt in such a short time-span.

Notwithstanding, there are potential endogeneity issues arising from using this measure as a proxy for firms' financing constraints if the level of maturing debt in a specific year is determined by managerial forward-looking behavior seeking to choose the best maturity period to maximize the firm's value. We argue that this concern is unlikely to hold in our setting. First, as the *timing* of maturity is arguably exogenous, any potential source of endogeneity coming from factors unrelated to financial constraints should also explain the firms' decisions to issue debt maturing at that exact period. If it were the case, then firms should be able to time the market and optimally decide the maturity date, which is unlikely to hold in a setting marked by firms being unable to comply with debt payments accordingly.

Given all of the above, we define $Maturing_{i,t}$ as the firm's i Long-Term Debt maturing in one year normalized by its Total Long-Term Debt in period t. To define our measure of financial constraints, we follow closely Almeida et al. (2012) and define $FC_{i,t}$ as a dummy variable that assigns 1 (one) if the firm's long-term maturing debt (normalized by total long-term debt is higher than the *industry-year* median, and zero otherwise. To the point that Almeida et al. (2012) define a stricter measure of firms' financing constraints by assigning 1 (one) if the firm's normalized long-term maturing debt is higher than 20%, we argue that as the empirical setting presented in Almeida et al. (2012) was focused on understanding the effects of the 2007-2008 financial crisis, such an absolute measure can fail to capture differences between financially constrained and unconstrained firms over time and across different countries[2].

That being said, even though the timing of maturity is arguably exogenous to several firm characteristics that might correlate with future outcomes, one might still be concerned about the relevance of this measure to proxy for financing constraints. To that matter, given that our sample is comprised only of listed firms, is it reasonable to assume that a firm that has a high portion of its long-term debt in its balance-sheet maturing in the current period would be restricted from pursuing its investments upfront? Put another way, would this firm have any difficulty in simply rolling over debt and extending its maturity? If this is the case, then a high portion of maturing debt would be a weak instrument for the degree of firms' financial constraints.

Even though it is unlikely that listed firms with expiring debt would have any difficulties in extending their maturities, a different situation emerges in situations where credit market imperfections are more prevalent. When considering periods of credit supply shortfalls, such as banking crises, successfully rolling over debt may not be an option, even for listed firms. In this situation, with a higher level of maturing debt that due *exactly* in periods of negative credit supply shocks, firms may anticipate the lack of financial resources and be forced to withdrawn future investments.

To capture the idea that maturing debt may affect firms' investments during adverse credit supply conditions, we use the *Systemic Banking Crises Database*, discussed in Laeven and Valencia (2018) and available on *GFDD* database, and highlight deals in years when banking crises were in place in the target firms' country headquarters. Drawing upon Laeven and Valencia (2018)'s study, we define $BankCrisis_{m,t}$ as a dummy variable that assigns 1 (one) if country m was suffering from a banking crisis in year t.

[2]In unreported tables, we run econometric estimations of your baseline regressions and find qualitatively similar results when defining our financing constraints variable as in Almeida et al. (2012).

More specifically, a banking crisis is defined as systemic if *two* conditions are met: first, there are significant signs of financial distress in the banking system, as indicated by significant bank runs, losses in the banking system, and/or bank liquidations; second, there are significant banking policy intervention measures in response to significant losses in the banking system. As such, the first year that *both* criteria are met is considered as the year when the crisis starts becoming systemic (*i.e*, $BankCrisis_{m,t} = 1$). Relatedly, the end of a crisis is defined as the year before both real GDP growth and real credit growth are positive for at least two consecutive years (*i.e*, $BankCrisis_{m,t} = 0$).

3.2 Baseline Specification

In our main specification, we employ a *differences-in-differences* approach to analyze the differential effect on the likelihood of being targeted in an acquisition for financially constrained firms during periods of credit supply shortfalls induced by banking crises. For that, we run a linear probability model (LPM) regression model with the following specification:

$$\begin{aligned} Deal_{i,t} = \beta_1 \times BankCrisis_{m,t} + \beta_2 \times FC_{i,t} + \beta_{DD} \times (BankCrisis_{m,t} \times FC_{i,t}) \\ + \gamma_1' Controls_{i,t-1} + \gamma_2' (Controls_{i,t-1}{}' BankCrisis_{m,t}) + \alpha_i + \alpha_t + \varepsilon_{i,t}, \end{aligned} \tag{1}$$

where $Deal_{i,t}$ is a dummy variable that assigns 1 (one) if firm i was target of an acquisition in year t, and zero otherwise, $FC_{i,t}$ is a dummy variable that equals one if firm i is considered financially constrained, and $BankCrisis_{i,t}$ is defined as before. Even though the timing of debt maturity is arguably exogenous to firms' behavior, we employ a wide set of fixed and time-varying covariates to control for possible confounders that may drive the likelihood of selling equity. More specifically, we control for firm fixed effects (α_i and α_t, respectively), and include a vector of one-year lagged firms' financial characteristics to control for possible confounding variation in our results. More specifically, we include *Cash Holdings*, *Cash Flow*, *Leverage*, and *Property, Plant and Equipment*, all normalized by *Total Assets*, *Size*, as measured by the natural logarithm of assets, and *Sales Growth*. In our full specification, we also include *pre post* trends for $Controls_{i,t-1}$ to capture any effect coming from the deterioration of firms' fundamentals.

In this framework, β_{DD} measures the differential effect of a firm having a significant portion of its long-term debt maturing in the same year when their headquarter country

is suffering from a banking crisis. As the previous discussion makes clear, we expect β_{DD} to be *positive*, as firms with debt maturing in periods where credit market imperfections are more prevalent might seek to attenuate the adverse effects of such shocks, either by private placements or certifying investment opportunities to outside investors.

4 Results and Discussion

4.1 Differential effects for the occurrence of M&A transactions during banking crises

Table 3 presents the results of the *differences-in-differences* estimation highlighted in Equation (1). In all specifications, the interaction term, $FC \times BankCrisis$, is positive and statistically significant, which indicates that firms with expiring debt maturities are more likely to be targeted in M&A transactions during banking crises. Our results hold even when considering different specifications for classifying the degree of firms' financing constraints, $FC_{i,t}$, such as terciles and quintiles of $Maturing_{i,t}$ distribution, as well as a continuous, standardized version of $Maturing_{i,t}$[3]. All specifications include clustered standard errors at the firm level.

It is interesting to compare our estimates presented in Columns (1)-(4) to understand the effect of including different sets of controls in our baseline specification. While in Columns (1)-(2), where we include no controls/only firm-level covariates and year fixed effects, respectively, banking crises, in general, seem to have a significant effect on M&A activity, in general. However, if M&A activity (from the target's perspective) is generally located in countries that are less likely to suffer from banking crises, then *BankCrisis* is also measuring some sorting of firms into different locations[4]. As shown, the inclusion of firm fixed effects, presented in Column (3), amplifies the effect of the *BankCrisis* during banking crises by more than twice the magnitude presented in Column (2).

Notwithstanding, it could also be the case that firms headquartered in countries with a higher propensity to suffer from a banking crisis also suffer more from declining stock prices during downturns. As our sample is comprised of listed firms only, a potential concern with respect to the first three specifications is the fact that firms' fundamentals

[3]In unreported tables, we run the same specifications presented in Table 3 for terciles, quintiles, and a continuous version of $Maturing_{i,t}$. All results hold, quantitatively and qualitatively.

[4]For example, drawing on an institutional analysis framework, if a country's investor protection system is poor, this can be related to a low degree of M&A activity and at the same time to lower economic development that ultimately leads to a higher likelihood of being hit by a banking crisis.

may deteriorate in the event of a banking crisis, driving stock prices down and ultimately affects M&A activity due to firms exposure to hostile takeovers.

To address such concern, we also include in our specifications interactions between firms' financials and *BankCrisis*, seeking to capture any effect that comes through firms' decreasing fundamentals. In Column (4), we present our most preferred specification, which includes one-year-lagged firm's financials and their interactions with the banking crisis variable, as well as firm and time fixed effects. Interestingly, expiring debt maturities, *individually*, do not seem to be related to firms' propensity to sell minority stakes anymore. That is, after including controls for pre/post trends on banking crisis for firms' fundamentals, the effect of $FC_{i,t}$, which is positive and statistically significant in specifications (1)-(3), vanishes. In other words, absent from any relevant change in firms' fundamentals *ex-post* a banking event, the level of expiring debt maturity does not seem to drive the likelihood of firms to be targeted in acquisitions [5]. Interestingly, to evaluate the extent to which the change in firms' fundamentals during downturns is relevant to explain deal activity, it is worth noticing the substantial increase in the Adjusted R^2 upon the inclusion of $Control \times BankCrisis$ terms, which potentially indicates the relevance of deteriorating firms' fundamentals during periods of negative credit supply shocks.

More importantly, during periods of banking crises, although firms are overall less likely to engage as targets in M&A transactions, there is an *increase* in the likelihood for financially constrained firms, as shown by the interaction term, $FC \times BankCrisis$, which is positive and statistically significant in all specifications. More importantly, the *net* effect (the sum of the coefficients FC and $FC \times BankCrisis$) for financially constrained during banking crises is positive and statistically significant in Column (4), which indicates that such effects more than compensate for the decrease in M&A activity during adverse credit supply conditions. Not only statistically significant, the estimates for $FC \times BankCrisis$ presented in Table 3 are also of economic significance: as the unconditional (conditional) average occurrence of a deal in our sample is 0.036 (0.046), estimates from Column (4) imply an increase of 13.8% (10.3%) on the likelihood of an acquisition. These effects are sizable, especially when considering a situation often marked by a stark increase in financial market volatility.

[5] For example, if due to the banking crisis, firm's fundamentals deteriorate severely as to drive stock prices down, this firm is more prone to be target of a hostile takeover from an acquirer seeking to explore a decrease in the target's stock prices. To be able to insulate our estimation from relevant changes in firm's fundamentals, in Column (4) we include *pre* and *post* trends for firms' financials by interacting them with $BankCrisis_{i,t}$.

4.2 Robustness Checks

The results from the last subsection highlight that firms with a higher portion of expiring debt maturities in years that coincide with banking crises are more prone to sell equity stakes, even after considering a wide set of covariates and fixed effects to control for potential confounders. In this section, we provide additional evidence that our results are robust to different specifications of the estimation procedure, timing patterns, and outliers.

4.2.1 Industry-wise and Country-wise trends

First, we address potential endogeneity concerns related to omitted time-varying confounders, which we present in Table 4. For example, one may be concerned that our results are specific to industries with intense M&A activity during periods that coincide with banking crises, as industries characterized by low asset redeployability may be more leveraged and, at the same time, more prone to M&A activity during adverse credit conditions. If this is the case, then the inclusion of industry-year fixed effects should absorb all within industry-year variability that explains merger activity. As shown in Column (1) of Table 4, the inclusion of time-varying industry effects does not qualitatively change the results.

Additionally, it could also be the case that country-specific factors are driving our results. For example, if some countries with higher levels of corporate debt implemented institutional in response to the potential adverse effects of banking crises, then controlling for time-varying, country specific factors, should absorb all the variability that is within country-year specific and relevant to explain merger activity. As shown in Column (2), this is also unlikely to be the case, as the estimated coefficients are still in line with the baseline estimations presented in Table 3. In Columns (3)-(6), we run different specifications using combinations of firm, industry-year, and country-year fixed effects, finding no evidence that industry-specific and country-specific trends are responsible for explaining the surge in M&A activity during credit supply shortfalls for financially constrained firms.

4.2.2 Timing around banking crises

Another concern related to the results presented in Table 3 relates to the specific *timing* of banking crises. Our identification strategy implicitly assumes that the timing of banking crises reflects *supply-side* shocks that affect the firms' set of financing options. Notwith-

standing, another possible concern is that the timing of banking crises experienced by firms in our sample may be confounded by other relevant time-varying country-level factors that are present even in the absence of banking crisis and that, in turn, could generate supply-side contractions. In special, one might be concerned that our effects are ultimately *demand*, and not *supply*, driven. If this is the case, then our identification may be capturing *demand-side* factors that correlates with firms' unobservable investment opportunities.

Moreover, since our measure of credit supply shocks relies on a series of events that happened during 1990 to 2019, a potential concern with this approach is that results might be driven by some specific event - *i.e,* the rise in uncertainty associated with the 2008 financial crisis - rather than a common component of distinct banking crisis episodes related to the supply of credit. If this is true, then the presented estimates may be confounded with time-specific drivers not related to negative credit supply shocks.

To address both of these issues, we re-estimate a dynamic version of Equation (1) by running the following regression:

$$\begin{aligned} Deal_{i,t} = \beta_1 \times FC_{i,t} + \sum_{t=-3}^{+3} \beta_t \times \{1[Rel.Year_{m,t} = t] \times FC_{i,t}\} \\ + \gamma_1^{'} Controls_{i,t-1} + \gamma_2^{'} (Controls_{i,t-1}{}^{'} BankCrisis_{m,t}) \\ + \alpha_i + \alpha_t + \varepsilon_{i,t}, \end{aligned} \tag{2}$$

where $Rel.Year_{m,t}$ is the relative year with respect to the banking crisis, and all the other variables defined as before. As Table 5, Column (1) shows, the positive and statistically significant results found in Table 3 are driven mainly by the *exact* year when a banking crisis hits the economy, and do not seem to be related to preexisting events. As such, these results reinforce that the surge in the likelihood is related to the specific timing of banking crises, and not to other potential preexisting trends, such as demand-induced trends, that might also affect the likelihood of a deal. Furthermore, the increase in the likelihood of a transaction seems to be concentrated at the onset of the crisis, when credit supply conditions are more deteriorated and do not seem to survive over time, as shown by the non-statistically significant results for periods that are ahead of the exact year of the banking crisis.

Moreover, Column 2(3) expands the set of controls by adding country-year (industry-year) fixed effects as a way to party control for time-varying unobservables at the country(industry) level. As such, we guarantee that our effects are not driven by a specific

country and/or industry that is more exposed to an episode of severe credit supply contractions. Together, these results provide additional evidence that our results do reflect common characteristics of banking crises with regard to the adverse effects implied by severe credit supply constraints.

4.3 Assessing *ex-post* deal effects

The results discussed in the last subsection provide evidence that although the overall level of M&A tends to decrease during periods of banking crises, such transactions are more likely to target firms with a high level of expiring debt maturities. Along with a set of additional results, we show that our results are likely to arise due to *liquidity* issues from the *target* firms' perspective, and not through other motivations, such as stock price devaluation through the deterioration of firms' fundamentals. Moreover, the increased likelihood of a financially constrained firm to be targeted in a M&A deal seems to be concentrated in the exact year of the banking crisis, with statistically insignificant results for periods before or after the country's headquarter is considered to suffer from a banking crisis. Put another way, our results translate the fact that firms with higher levels of expiring debt maturities during credit supply contractions found difficulties in accessing credit markets to successfully roll over on debt, and opt to act as targets in M&A transactions as a way to alleviate such frictions.

Despite this evidence, it is not clear from these results *how* selling equity stakes would eventually help firms to mitigate such financing constraints. Assuming that managers are optimally choosing how to allocate between the available funding sources, understanding *why* such transactions are happening with these firms is of primary importance. To shed light on this issue, as well as to provide even further evidence of the motivations that justify the increased likelihood of selling equity stakes presented in Table 3, one must assess what happens *after* deal occurrence. This subsection provides several results to address these points.

To that matter, if expiring maturing debt levels are binding to firms' financing capacity, then firms that have actively engaged in selling equity stakes should suffer*less* from the adverse consequences of credit supply than otherwise similar firms. Likewise, in the presence of positive investment opportunities, if these firms do not have their financial capacity constrained, they should also be *less* likely to cut investment levels *ex-post*. Even though we find evidence that financially constrained firms do engage in equity selling during banking crisis periods, we cannot infer if it actually eases firms' financial con-

straints, and what is the *channel* that translates *e.g*, a direct equity placement or an increase in borrowing capacity from outside lenders.

Like identifying a *causal* relationship between the target's financial constraints and the likelihood of selling equity stakes, assessing the *ex-post* effects of deal occurrence is empirically challenging. Fundamentally, selection on unobservables may drive the decision of some firms to sell equity stakes in a given period and, at the same time, influence their future outcomes. For example, if targeted firms simply tend to have better prospects than non-targeted firms during banking crisis periods, a naive regression estimate of firms' future outcomes on deal occurrence is likely to yield biased estimates. Moreover, there is vast literature on the *ex-post* effects of M&As. To the extent that our effect captures a reduced-form of the transaction effect for the target firm, one must also be able to highlight the contribution of the *ex-post* differentials that is attributable to the liquidity problem by insulating them from any other potential effect induced by M&A transactions previously discussed in the literature.

If credit rationing is indeed the mechanism behind the surge in acquisitions, we should expect targeted firms to behave differently *ex-post* deal occurrence relative to other constrained firms during a banking crisis. As such, to investigate the *ex-post* effects related to liquidity-induced deals, we analyze the subsample of target/non-targeted firms that with higher levels of expiring maturing debt maturities in banking crisis years. In this sense, for the case of targeted firms, we consider only the firms that were exposed to banking crises and have become targets in the *same* year. To shed light on what happens *ex-post* deal occurrence for these financially constrained firms during crisis periods, we run the following specification:

$$log(Y_{i,t}) = \beta_1 \times Deal_i + \beta_2 \times After_{i,t} + \beta_{DD} \times (Deal_i \times After_{i,t}) + \alpha_i + \alpha_t + \epsilon_{i,t}, \qquad (3)$$

where $Deal_i = 1$ if a firm has been targeted (in any moment of the sample), and $After_{i,t} = 1$ for periods after the firm's i headquarter country has been classified as being suffering from a banking crisis – here, we consider the "zero" period as $After_{i,t} = 1$[6].

Additionally, to get a better understanding of the timing of the *ex-post* effects, we also employ a dynamic version of Equation (3) by running:

[6]For example, if a firm has its headquarter country classified as a suffering from a banking crisis in 2008, $After_{i,2008} = 1$

$$log(Y_{i,t}) = \beta_1 \times Deal_i + \sum_{t=-3}^{3} \gamma_t \{1[Rel.Year = t]\}$$
$$+ \sum_{t=-3}^{3} \beta_t \{1[Rel.Year = t] \times Deal_i\} + \alpha_i + \alpha_t + \epsilon_{i,t}, \quad (4)$$

where $Rel.Year_{i,t}$ is the relative year with respect to the banking crisis period, considering the year exactly before – *i.e*, $Rel.Year_{i,-1}$ as the baseline year, and all other variables defined as before. We measure the differential effect of selling equity stakes in crisis years for financially constrained firms on future outcomes such as *Assets*, *Cash Holdings*, *Property, Plant and Equipment (PPE)*, and *CAPEX*, as well as debt measures, such as *Long-Term* and *Short-Term* Debt.

Table 6 presents the estimates for firms' *ex-post* outcomes around banking crisis periods using the industry-year median of *Maturing* as the criteria for classifying firms into financially constrained/unconstrained groups, while Figure 1 presents a graphical illustration of the dynamic-specification regression presented in Equation (4) by plotting β_t for each outcome over a window of ± 3 years.

Interestingly, the coefficients on $Deal \times After$ presented in Table 6 are in line with target firms alleviating their financial constraints *ex-post* deal occurrence. More specifically, even though firm firms' growth in *Assets* , on average, drops significantly during banking crisis periods, targeted firms are able to attenuate these adverse effects substantially: as shown in the first Column of 6, while the average drop in *Assets* is 7%, targeted firms can accommodate 5.8% of this drop. Moreover, these firms also show to be able to accommodate the drawdown in *Cash Holdings* levels significantly better than their counterparts, while also being able to attenuate the drop in and *Property, Plant and Equipment* growth levels. Interestingly, while long-term debt levels seem to drop substantially during the credit supply shock, on average, short-term debt levels also increase substantially, although not fully compensating for the decrease. However, targeted firms are able to partly compensate for the drop in long-term debt levels, possibly by extending their debt maturities.

Shedding even further light on the results of Table 6, Figure 1 provides intuitive evidence on the *timing* of such *ex-post* effects. As we can see from the coefficient plots, *Total Assets*, *Cash Holdings* and *Long-Term* debt seem to respond immediately to the deal, with persistent and increasingly higher effects throughout the next three years. On the other hand, while *PPE* levels shown in Table 6 seem to increase, on average, for targeted firms,

the last plot of Figure 1 shows that the dynamics of the differential in growth levels is concentrated after two to three years following the banking crisis. Overall, none of the firms' outcomes seem to be related to pre-crisis differences among targeted/non-targeted firms, which alleviates concerns about our effects being driven by firms' unobservable characteristics.

5 Confounding Factors and Other Mechanisms

Together, the results from the last subsection are illustrative of target firms being able to attenuate the credit supply shock with a higher portion of long-term debt, while also tapping into internal resources to a lesser extent than their counterparts. Importantly, when looking at a longer horizon, these firms also seem to invest in tangible assets to a higher extent than non-targeted firms, which also reinforces the liquidity hypothesis motivating the occurrence of such deals. Importantly, none of the differentials seem to be related to *ex-ante* differences between targeted/non-target characteristics, as shown by the statistically insignificant coefficients pre-crisis coefficients in all plots, providing even further evidence that our effects are not related to firms' unobservables that could otherwise affect the likelihood of firms engaging in M&A transactions as targets.

To the extent the estimated effects are not *demand-driven* – *i.e,* not correlated to target firms' unobservable characteristics, such as investment opportunities –, one can interpret these findings as evidence that firms that engaged in M&A as targets did not need to adjust their behavior and foregone their investments due to the expiring debt maturity coinciding with the credit supply shock. However, these set of results does not overcome the empirical difficulties arising when insulating *ex-post* deal effects described before from other potential M&A consequences. As argued before, M&A transactions can be related to other changes in target firms' characteristics – such as managerial ability, economies of scale and scope, among others – that could also drive the *ex-post* differential results described before, even in the absence of any liquidity issue.

To that matter, we provide several complementary that enhance our understanding of the potential underlying mechanisms that drive the *ex-post* differential effects. Along with the analysis of the acquisition-induced results on *ex-post* target firms' fundamentals, presented in Table 6, Figure 1, we provide several cross-section heterogeneity tests to explore whether the extent of our results varies in the cross-section in ways that are consistent with our hypothesis and therefore further buttress the liquidity interpretation. In the next subsections, we provide detailed and convincing evidence that our estimated effects of the

deal *ex-post* differentials relate to the *liquidity* channel, and not due to any other channel that might affect firms' future outcomes in the presence of M&As.

5.1 Comparing across Financially Unconstrained firms

A first concern related to our results on the *ex-post* firms' fundamentals is that they reflect distinct M&A motivations other than liquidity. For example, the literature on M&A has suggested several other motivations for firms to engage in transactions that also predict increases in firm performance. If this is the case, then our regressions are likely to reflect the effect of *other* changes occurring within the target firm, such as operational synergies, managerial turnover, among others.

To be able to insulate our results from other M&A-related motivations, we claim that, if anything, such effects should also manifest when comparing firms with lower levels of expiring maturity during banking crisis periods. Based on that, we repeat the analyses presented in Figure 1 considering only the subset of financially unconstrained firms – *i.e*, those that have presented lower levels of expiring debt maturities when being hit by a banking crisis in their headquarters.

The results, shown in Table 7, provide interesting insights with respect to the concern that *ex-post* effects unrelated to the liquidity motive biasing our results. First, contrasting with the case of financially constrained firms, unconstrained firms, on average, did not present any fundamental change in *Asset* Levels, *Cash Holdings*, *PPE*, or *CAPEX* when being hit by a banking crisis, which is consistent with the interpretation that liquidity problems are not binding to this subset of firms. More importantly, the differential *ex-post* effects for targeted firms are statistically insignificant for almost all firms' outcomes. In special, unconstrained target firms do not present any different dynamic for debt levels, either short or long-term. Together, the contrasting results for financially constrained an unconstrained firms provide convincing evidence that other M&A-related motivations are unlikely to explain the *ex-post* differential effects for financially unconstrained firms.

5.2 Change of control, Managerial Turnover, and Operational Synergies

Also related to different motivations for M&As other than liquidity issues, another potential concern is that our *ex-post* results merely reflect potential synergy gains between the target and acquirer firms, as well as managerial improvements induced by the change of control. For example, equity ownership between customer-supplier relationships can

arise as way to bond trading parties together through reducing the adverse effects from contractual incompleteness and financial frictions (Fee et al., 2006). Additionally, if these transactions also trigger changes in corporate control, firms' fundamentals *ex-post* deal could also reflect changes induced by managerial turnover and/or any changes that are attributable to ownership.

As shown in additional results, however, when looking at differences between minority and majority acquisitions, such motivations are unlikely to be driving our results. Representing block equity purchases that do not exceed 50% of the target firms' total equity, minority acquisitions represent partial integration strategies between acquirers and targets[7]

Importantly, minority acquisitions represent a distinct organizational choice in terms of integration strategies, which can, under some circumstances, provide a more efficient allocation of incentives (Ouimet, 2012). For example, minority acquisitions can facilitate integration and mitigate incomplete contracting between independent firms when property rights are blurry, such as research and development (R&D) activities (Fee et al., 2006). Aside from other governance and contracting motives, minority acquisitions may as well have first-order importance for firms' *financing* motives. Since financial constraints hinges upon asymmetric information between the firm and its potential lenders, any gains from a decrease in the degree of information asymmetry may improve the firms' capability to contract new debt at economically viable contract terms. In this sense, partially integrating with a better-informed party may have the potential to relieve target firms' financial constraints, either *directly* or *indirectly* (Ouimet, 2012).

More specifically, for financially constrained firms, block equity transactions with a more informed party can provide capital directly to the issuing firm by equity private placement, reducing the potential dilution effect on the actual shareholders (Myers and Majluf, 1984). Moreover, by the same information asymmetry argument, minority acquisitions can also act indirectly as a *certification device* from the target's investment opportunities to the capital market or other capital providers (Hertzel and Smith, 1993; Wruck, 1989). As such, if our results are reflecting changes in corporate control, we should expect the effects to be stronger for majority acquisitions – that is, those where the acquirer firm has more than 50% of the total voting shares of the target firm – especially for the case of

[7] In practical terms, one in every seven firms was a target of a minority acquisition between 1990 and 2009 (Liao, 2014): overall, there have been more than 40,000 completed deals during the period, summing up to more than $2 trillion in constant 2008 U$ dollars. Despite its occurrence, the study of minority acquisitions and their *ex-post* performance effects has been carried out by relatively few authors (Liao, 2014; Fee et al., 2006; Kang and Kim, 2008), as compared to studies on the grounds of full integrations.

debt levels.

To investigate this point, we rerun Equation (4) by subgroups according to the final equity stake position held by the acquirer. As shown in Table 8, *ex-post* differentials for both long and short-term debt are positive and statistically significant only when considering the subsample of minority acquisitions, showing negative and statisfically insignificant effects for the case of majority acquisitions. Notwithstanding, when looking at future investment levels, the *opposite* situation emerges: majority acquisitions present positive and statistically significant results for increases in *CAPEX* and *PPE*, whereas minority acquisitions do not appear to affect future investment levels.

To shed even further light on this issue, we divide our regression sample into deciles of the distribution of *Final Equity* held by the acquirer firm and run the same specification within subsamples of equity bins. The coefficients for the *ex-post* differentials, plotted in Figure 2, shows that there is a *non-monotonic* relationship between the size of the final equity stake held by the acquirer firm and their future growth in fundamentals after the deal has taken place. Interestingly, the effect seems to increase *within* the subsample of minority acquisitions according to the final size of the acquirers' position in the target firm, but decays abruptly for majority acquisitions, showing statistically insignificant results for almost all outcomes. While increases in *CAPEX* seems to be only relevant for the case of majority acquisitions, the second panel of Figure 2 shows that these effects are driven by deals with the largest equity stakes held by the acquirer, while presenting statistically insignificant results for all other subsamples.

Crucially, the results for *Long-Term Debt* shows a clearer picture of the potential channel by which *ex-post* differentials manifest in our results: the effects seem to increase on the size of the acquirer final stake held in the target firm, with positive and statistically significant results irrespective of the decile, but decays smoothly for majority acquisitions. All in all, these results move away from a change of control explanation for the *ex-post* differentials found before. As such, if there are other channels other than liquidity issues, we should expect these effects to be stronger for the case of majority acquisitions, as a formal change of control arguably facilitates firm reorganization.

Finally, one may still be concerned about *selection*: if firms that are targeted are different in unobservables from those that are not targeted, then our results may merely reflect differences that are attributable to variables that are not observed by the econometrician, such as differences in investment opportunities. As such, another dimension that can add to our results is to compare only firms that were targets during banking crises, varying across the distribution of expiring debt maturities. For that, we run the following regres-

sion:

$$log(Y_{i,t}) = \beta_1 \times FC_{i,t} + \beta_2 \times After_{i,t} + \beta_{DD} \times (FC_{i,t} \times After_{i,t}) + \alpha_i + \alpha_t + \epsilon_{i,t}, \quad (5)$$

where all variables are defined as before. In this setting, any unobservables that are likely to be related to selection of firms into M&As as targets is properly controlled for, as we are only comparing firms were effectively targeted during banking crises, but varying their exposure to the credit supply shock, The results, shown in Table 9, shows that the future debt levels for financially constrained firms increase approximately 17% more, relative to unconstrained targeted firms. Interestingly, none of the other outcomes seems to differ across different levels of expiring debt maturities, which also reinforces our interpretation of the liquidity channel.

5.3 How do target firms relieve their financial constraints?

The results from the last two subsections provide several additional results that reinforce the interpretation of our results as a consequence of liquidity issues: firms with higher expiring debt maturities, when faced with severe credit supply conditions, opt to sell equity stakes to other parties in M&A transactions as a way to relieve their financial constraints. To that point, our additional results reinforce that potential endogeneity issues arising from selection on unobservables, as well as other mechanisms that are likely to be triggered during M&A episodes, are unlikely to explain the patterns presented before.

Notwithstanding, even though these results are indicative of target firms selling equity stakes due to *liquidity* reasons, they tell little about the *how* they effectively relieve their financial constraints *ex-post*. Therefore, we now shed light on the specific mechanisms by which firms, when selling equity stakes, are able to boost their lending capacity and avoid postponing future investments due to credit shortages.

First, it is interesting to understand how our results vary within domestic and cross-border acquisitions. For example, (Rossi and Volpin, 2004) analyzes the determinants of cross-border and domestic acquisitions by focusing on differences in laws and regulations across countries and find that target firms in cross-border acquisitions are generally from countries with poorer investor protection, suggesting that M&As can play a governance role by imposing higher governance standards from acquirer countries to the target firm. Relatedly, Aggarwal et al. (2011) analyze institutional investor ownership around 23

countries from 2003-2008 and find that institutional shareholders can improve firm-level governance. As such, firms with higher institutional ownership have increased ability act effectively monitor the managers, which translates into a higher probability of firing poorly-performing Chief Executive Officers (CEOs).

To the extent that these results corroborate our findings, Table 10 presents the *ex-post* differential results for financially constrained firms that underwent M&As as targets separately the domestic and cross-border subsamples. As the results show, *ex-post* differentials are stronger for cross-border relative to domestic acquisitions, which goes in line with the findings on Rossi and Volpin (2004) and Aggarwal et al. (2011) that cross-border acquisitions may be useful for target firms to bond on better investor protection at the country-level.

More specifically, while the *ex-post* differentials are generally positive both for domestic and cross-border acquisitions, the *net* effect for cross-border firms after a deal has taken place during the banking crisis period is *positive*. That is, cross-border targeted firms have presented growth in their fundamentals (*Assets*, *Cash Holdings*,*Total Debt*, and *CAPEX*) even in the presence of a negative credit supply shock, while domestic targeted firms, at best, were able to attenuate the adverse effects induced by the crisis, which is expected as the acquirers of domestic deals are also suffering from a banking crisis in the time of the transaction.

6 Conclusion

This paper provides evidence that M&A transactions can serve as leeway for financially constrained to smooth out negative credit supply shocks during banking crisis periods. Our results show that while deal activity substantially decreases during periods marked by credit supply contractions, firms with expiring debt maturities constrained firms are approximately 15% more likely to sell minority stakes during such periods. These effects are robust to the definition of financial constraints, and remain significant even after including a wide set of controls, fixed effects, and is also qualitatively similar across a wide range of tests and alternative specifications.

Importantly, conditional on selling equity stakes to outside investors, we find strong evidence that targeted financially constrained firms invest more, issue more debt, and reduce the dependence on cash holdings after the deal. These effects are statistically and economically significant. Our results not only are suggestive of such transactions as an effective way to relieve firms' financing constraints during periods of a market decline in the supply of credit but highlights a *certification effects* by acquirer firms as a mechanism that drives this change, given that the results on the *ex-post* differentials are driven by cross-border minority acquisitions.

While we recognize the potential avenues that can still be explored towards the understanding of the interplay between M&A transactions and corporate liquidity, our results extend the findings on the credit supply channel (Almeida et al., 2012; Duchin et al., 2010), providing evidence for the relevance of M&A as a liquidity device when credit market imperfections are more prevalent, complementing the earlier literature on the firm's financial policies during periods of banking crises (Garcia-Appendini and Montoriol-Garriga, 2013).

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7 Tables and Figures

Table 1: Variable names, definitions and sources.

This table presents a detailed description of the main variables used in the study. All information is collected through the sources indicated in Section 2.

Name	Variable	Definition
Deal information (Source: SDC Platinum)		
Deal Occurrence	*Deal*	A dummy variable that assigns 1 (one) if a firm has engaged in a minority acquisition as target, and zero otherwise.
Deal Stake Acquired (%)	*DealStake*	The percentage of shares bought by the acquirer firm of target's outstanding shares.
Financial Data (Source: Compustat Global and Compustat US)		
Cash Holdings/Assets	CH	Cash and Short-Term Investments over Total Assets
PPE/Assets	PPE	Property, Plant and Equipment over Total Assets
Debt/Assets	DA	Total Debt over Total Assets
Long-Term Debt Ratio	$LTDebt$	Long-Term Debt over Total Debt
Short-Term Debt Ratio	$STDebt$	Short-Term Debt over Total Debt
Size	*Size*	Natural Logarithm of Total Assets
Sales Growth	SG	Yearly growth in gross revenues.
Dividend Yield	*Yield*	Dividend paid over share price.
Dividend Dummy	*Dividend*	A dummy variable that assigns one if $Yield > 0$.
Sales Growth	SG	Yearly growth in gross revenues.
Sales Growth	SG	Yearly growth in gross revenues.
Maturing Debt	*Maturing*	The firm's proportion of long-term debt due in one year.
Financially Constrained	FC	A dummy variable that assigns 1 (one) if the firm's proportion of long-term debt due in one year is greater than the industry-year median (Fama French Industry Classification).
Country-level data (Source: Global Financial Development Database (The World Bank)		
Banking Crisis	*BankCrisis*	A dummy variable that assigns 1 (one) if a banking crisis has emerged in a firm's headquarter country in a given year, and zero otherwise, following Laeven and Valencia (2018).

Table 2: Summary Statistics for the Regression Sample

This table presents the summary statistics for the sample used in the baseline specifications described in Section 4. "Deal Sample" is a binary variable that assigns "Yes" if a firm was involved in an M&A operation as a target during the sample period, and zero otherwise. The remaining columns refer to average means, standard deviations, minimum value, first quartile, median, third quartile, and the maximum of each subsample. Finally, *p-val* denotes the p-value for the *Welch t-test* of the differences between the two subsamples. Our sample comprises 58,814 unique cross-border and domestic M&A transactions. All variable definitions are presented in Table 1.

Descriptive Statistics - Targeted and non-targeted Firms									
	Deal Sample	μ	σ	**Min**	**Q1**	**Median**	**Q3**	**Max**	*p-val*
Size	Yes	7.29	3.10	−0.89	5.11	7.08	9.33	15.68	<0.001
	No	6.76	3.20	−0.89	4.59	6.68	8.84	15.68	
Cash Holdings/Assets	Yes	0.14	0.15	0.00	0.03	0.09	0.19	0.83	<0.001
	No	0.14	0.15	0.00	0.03	0.09	0.19	0.83	
PPE/Assets	Yes	0.32	0.23	0.00	0.13	0.28	0.47	0.94	<0.001
	No	0.31	0.23	0.00	0.13	0.27	0.46	0.94	
Debt/Equity	Yes	0.87	2.03	−13.02	0.15	0.50	1.12	14.67	<0.001
	No	0.73	1.84	−13.02	0.11	0.41	0.96	14.67	
Cash-Flow/Assets	Yes	0.01	0.11	−0.69	−0.02	0.00	0.03	0.69	<0.001
	No	0.01	0.10	−0.69	−0.01	0.00	0.03	0.69	
Long-Term Debt Ratio	Yes	0.52	0.35	0.00	0.18	0.56	0.84	1.00	<0.001
	No	0.47	0.35	0.00	0.11	0.48	0.80	1.00	
Short-Term Debt Ratio	Yes	0.48	0.35	0.00	0.16	0.44	0.82	1.00	<0.001
	No	0.53	0.35	0.00	0.20	0.52	0.89	1.00	
Debt/Assets	Yes	0.29	0.26	0.00	0.11	0.25	0.40	3.15	<0.001
	No	0.29	0.31	0.00	0.10	0.23	0.39	3.15	
Dividend Yield	Yes	0.16	0.36	−1.62	0.00	0.07	0.24	3.01	<0.001
	No	0.18	0.37	−1.62	0.00	0.10	0.25	3.01	
Dividend Dummy	Yes	0.63	0.48	0.00	0.00	1.00	1.00	1.00	<0.001
	No	0.69	0.46	0.00	0.00	1.00	1.00	1.00	
Sales Growth	Yes	0.30	3.01	−1.00	−0.05	0.07	0.24	1,126	0.007
	No	0.27	3.81	−1.00	−0.05	0.07	0.22	1,126	
Bank Crisis	Yes	0.08	0.28	0.00	0.00	0.00	0.00	1.00	<0.001
	No	0.09	0.28	0.00	0.00	0.00	0.00	1.00	

Table 3: *Differences-in-Differences* estimates - Maturing $\geq$ Industry-Year Median

This table presents the estimation results of the Linear Probability Model (LPM) specification, presented in Equation (1) and described in Section 3. The dependent variable, $Deal_{i,t}$, is a binary variable that assigns 1 if firm i was involved in an M&A transaction as a target in period t, and zero otherwise. FC_i is a binary variable that assigns 1 if firm i is considered financially constrained in period t *i.e,* $Maturing_{i,t} \geq$ Industry-Year median, and zero otherwise. *BankCrisis* assigns 1 if the observation is measured in an year when a banking crisis hits the firm's headquarters. presented in Table 1. Standard errors are in parenthesis. *, **, *** denote statistical significance at 10, 5, and 1 percent, respectively.

	Deal Dummy (1 if Deal in that year, zero otherwise)				
	(1)	(2)	(3)	(4)	(5)
FC	0.002^{***}	0.003^{***}	0.003^{***}	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
BankCrisis	-0.005^{***}	-0.005^{***}	-0.012^{***}	-0.004^{**}	-0.008^{**}
	(0.001)	(0.001)	(0.001)	(0.002)	(0.004)
FC $\times$ *BankCrisis*	0.004^{**}	0.004^{*}	0.006^{***}	0.005^{**}	0.005^{**}
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Controls?	No	✓	✓	✓	✓
Year fixed effects	No	✓	✓	✓	✓
Firm fixed effects	No	No	✓	✓	✓
Controls $\times$ Crisis?	No	No	No	No	✓
S.E Clustering	Firm	Firm	Firm	Firm	Firm
Observations	601,784	504,815	504,815	504,815	504,815
R^2	0.0001	0.0003	0.002	0.086	0.086
Adjusted R^2	0.0001	0.0003	0.001	0.011	0.011
Note:					*p<0.1; **p<0.05; ***p<0.01

Table 4: *Differences-in-Differences* estimates - Alternative sets of controls

This table presents the estimation results of alternative specifications of the Linear Probability Model (LPM) presented in Equation (1) and described in Section 3. In Column (1), we include industry-year fixed effects to control for time-varying unobserved heterogeneity at the industry-level by using the Fama-French Industry Classification. In Column (2), we include country-year fixed effects to control for time-varying unobserved heterogeneity at the country-level. Column (3) includes both sets of time-varying controls presented in (1) and (2). Column (4) adds firm-fixed effects to Column (1) specification, and Column (5) adds firm-fixed effects to Column (2) specification. Finally, Column (6) includes all sets of alternative controls (firm fixed effects, industry-year fixed effects, and country-year fixed effects). Therefore, the remaining variation in column (6) captures firm-year level variation that is not related to any of the covariates and/or controls. All variable definitions and specifications are similar to Table 3. Standard errors are in parenthesis. *, **, *** denote statistical significance at 10, 5, and 1 percent, respectively.

	Deal Dummy (1 if Deal in that year, zero otherwise)					
	(1)	(2)	(3)	(4)	(5)	(6)
BankCrisis	-0.017^{***}	-	-	-0.010^{**}	-	-
	(0.004)			(0.004)		
FC	0.003^{***}	0.002^{**}	0.001^{**}	0.001	0.001	0.001^{*}
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
FC × *BankCrisis*	0.006^{***}	0.006^{***}	0.006^{***}	0.005^{**}	0.005^{**}	0.005^{**}
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Fixed Effects	Ind. × Year	Country × Year	(1+2)	Firm + Ind. × Year	Firm + Country × Year	All Controls
S.E Clustering	Firm	Firm	Firm	Firm	Firm	Firm
Observations	498,406	504,815	498,406	498,406	504,815	498,406
R^2	0.006	0.014	0.018	0.089	0.095	0.098
Adjusted R^2	0.004	0.010	0.012	0.012	0.016	0.017
Note:						*p<0.1; **p<0.05; ***p<0.01

Table 5: *Differences-in-Differences* Estimates - Deal Likelihood Around Banking Crises

This table presents the estimation results of the dynamic effects, linear probability model (LPM) specification, presented in Equation (2) and described in Section 3. The dependent variable, $Deal_{i,t}$, is a binary variable that assigns 1 if firm i was involved in an M&A transaction as a target in period t, and zero otherwise. FC_i is a binary variable that assigns 1 if firm i is considered financially constrained in period t *i.e,* $Maturing_{g_{i,t}} \geq$ Industry-Year median, and zero otherwise. $BankCrisis_{\pm t}$ refers to the relative year of the banking crisis that occurred in firm's i country headquarters - *i.e,* how many leads or lags relative to a banking crisis year in his headquarters firm i, in period t, is. In all specifications, we present only the interaction terms with the financially constrained status, FC. Column (1) presents the estimation results of Equation (2) using firm and year fixed effects. Column (2) includes country-year fixed effects to account for unobserved, time-varying heterogeneity at the country-level, and Column (3) includes country-year fixed effects to account for unobserved, time-varying heterogeneity at the industry-level, according to the Fama French Industry Classification. Covariates definitions are presented in Table 1. Standard errors are in parenthesis. *, **, *** denote statistical significance at 10, 5, and 1 percent, respectively.

	Deal Dummy (1 if Deal in that year, zero otherwise)		
	(1)	(2)	(3)
$BankCrisis_{-3} \times FC$	-0.010	-0.031^{*}	-0.009
	(0.010)	(0.016)	(0.010)
$BankCrisis_{-1} \times FC$	0.012	0.006	0.008
	(0.012)	(0.017)	(0.012)
$BankCrisis_{0} \times FC$	0.023^{**}	0.028^{*}	0.024^{**}
	(0.012)	(0.016)	(0.012)
$BankCrisis_{+1} \times FC$	-0.002	0.018	0.005
	(0.015)	(0.023)	(0.015)
$BankCrisis_{+2} \times FC$	-0.016	-0.017	-0.014
	(0.014)	(0.022)	(0.015)
$BankCrisis_{+3} \times FC$	0.014	0.015	0.007
	(0.009)	(0.013)	(0.010)
Controls	Firm + Year	Firm + Country-Year	Firm+Industry-Year
Controls x Crisis?	✓	✓	✓
S.E Clustering	Firm	Firm	Firm
Observations	67,531	50,395	66,670
R^2	0.636	0.661	0.651
Adjusted R^2	-0.009	-0.005	-0.004
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 6: *Ex-Post* Deal Occurrence Effects for Financially Constrained Firms

This table presents the estimation results of the *ex-post* deal effects specification, presented in Equation (3) and described in Section 3, considering only the subsample of firms that were exposed to a banking crisis during the studied period and classified as financially constrained in the crisis year. $Deal_i$ is a binary variable that assigns 1 if firm i belongs to the subsample of firms that have were targeted in an M&A transaction during any period of the study, and zero otherwise. $After$ assigns 1 if the observation is measured in the same year of after the banking crisis hits the firm's headquarters. Dependent variable definitions are presented in Table 1. Standard errors are in parenthesis. *, **, *** denote statistical significance at 10, 5, and 1 percent, respectively.

	Target Firm Fundamentals					
	log(Assets)	log(Cash Holdings)	log(LT Debt)	log(ST Debt)	log(PPE)	log(CAPEX)
	(1)	(2)	(3)	(4)	(5)	(6)
Deal	1.340**	1.358**	1.340*	1.388**	1.371**	0.940
	(0.665)	(0.662)	(0.696)	(0.678)	(0.661)	(0.677)
After	−0.069***	−0.131***	−0.816***	0.573***	−0.059**	−0.199***
	(0.020)	(0.034)	(0.045)	(0.041)	(0.023)	(0.040)
Deal × *After*	0.058***	0.094***	0.117***	0.051	0.046*	0.029
	(0.020)	(0.034)	(0.045)	(0.038)	(0.025)	(0.040)
Year fixed effects	✓	✓	✓	✓	✓	✓
Firm fixed effects	✓	✓	✓	✓	✓	✓
S.E Clustering	Firm	Firm	Firm	Firm	Firm	Firm
Observations	74,243	73,822	68,537	72,318	73,965	60,471
R^2	0.977	0.945	0.925	0.935	0.975	0.943
Adjusted R^2	0.974	0.939	0.916	0.928	0.972	0.935
Note:						*p<0.1; **p<0.05; ***p<0.01

Figure 1: *Ex-Post* Deal Occurrence Effects for Financially Constrained Firms - Dynamic Effects Specification

This figure presents the *ex-post*, dynamic effects estimations of THE *ex-post* deal effects, presented in Equation (4) and described in Section 3, considering only the subsample of firms that were exposed to a banking crisis during the studied period and classified as financially constrained in the crisis year. In each panel, the interaction term coefficient of the firms' *ex-post* crisis fundamentals regression on deal occurrence, $Deal_i \times Rel.Year_{i,t}$, is plotted on a black point, with the solid black line denoting the 95% confidence interval. Dependent variable definitions are presented in Table 1.

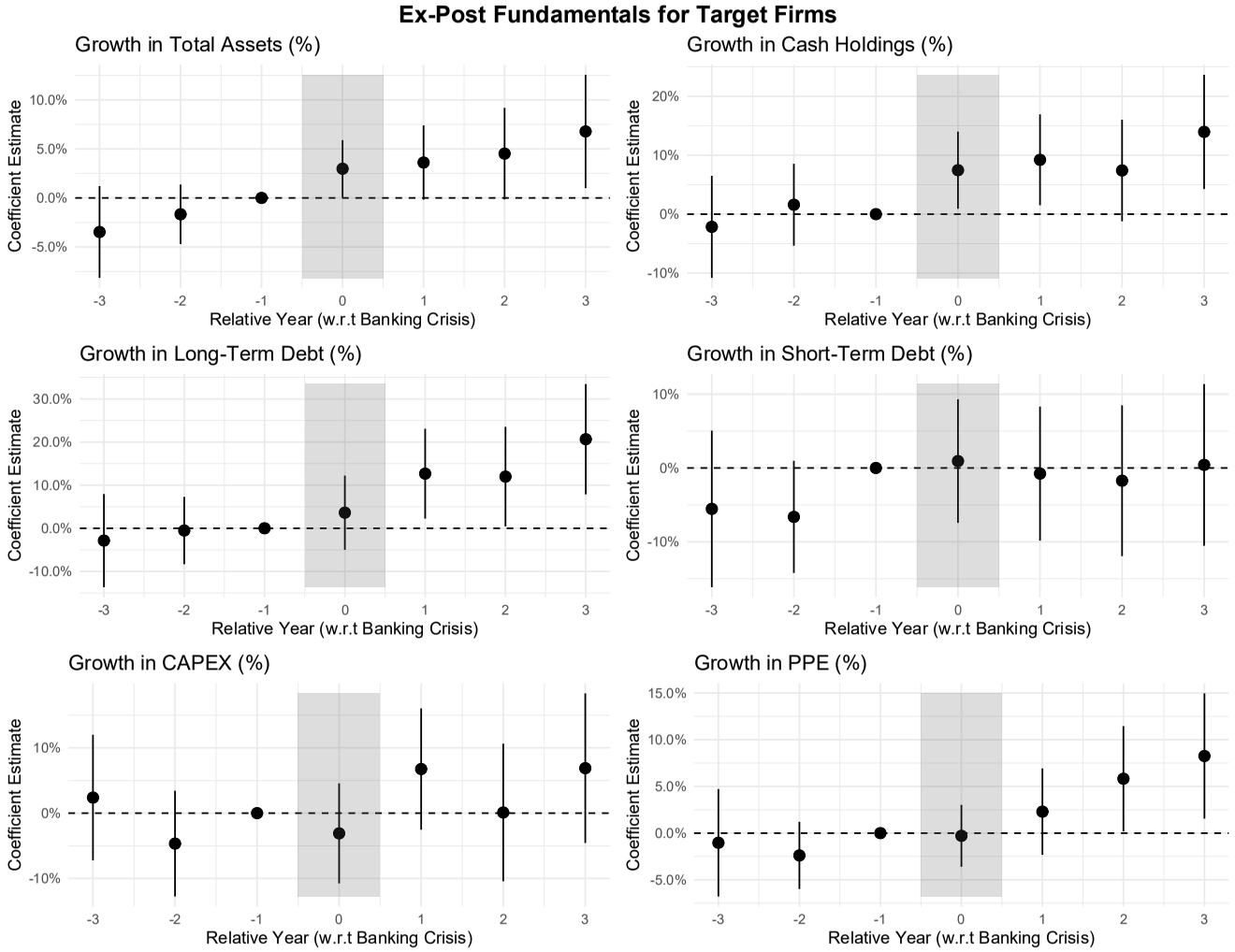

Table 7: *Ex-Post* Deal Occurrence Effects for Financially Unconstrained Firms

This table presents the estimation results of *ex-post* deal effects, presented in Equation (3) and described in Section 3, considering only the subsample of firms that were exposed to a banking crisis during the studied period and classified as financially unconstrained in the crisis year. Similar to Table 6, $Deal_i$ is a binary variable that assigns 1 if firm i belongs to the subsample of firms that have were targeted in an M&A transaction during any period of the study, and zero otherwise. $After$ assigns 1 if the observation is measured in the same year of after the banking crisis hits the firm's headquarters. Dependent variable definitions are presented in Table 1. Standard errors are in parenthesis. *, **, *** denote statistical significance at 10, 5, and 1 percent, respectively.

	Target Firm Fundamentals					
	log(Assets)	log(Cash Holdings)	log(LT Debt)	log(ST Debt)	log(PPE)	log(CAPEX)
	(1)	(2)	(3)	(4)	(5)	(6)
$Deal$	0.312	0.321	0.529	0.247	0.306	0.286
	(0.318)	(0.322)	(0.327)	(0.351)	(0.320)	(0.265)
$After$	0.034*	−0.047	0.607***	−0.395***	0.055***	0.001
	(0.017)	(0.031)	(0.042)	(0.045)	(0.021)	(0.033)
$Deal \times After$	0.014	0.050*	0.008	0.035	0.001	0.014
	(0.018)	(0.029)	(0.041)	(0.040)	(0.022)	(0.031)
Year fixed effects	✓	✓	✓	✓	✓	✓
Firm fixed effects	✓	✓	✓	✓	✓	✓
S.E Clustering	Firm	Firm	Firm	Firm	Firm	Firm
Observations	73,625	73,236	70,927	68,820	73,369	64,925
R^2	0.975	0.940	0.914	0.915	0.974	0.940
Adjusted R^2	0.973	0.934	0.904	0.905	0.971	0.933
Note:						*p<0.1; **p<0.05; ***p<0.01

Table 8: *Ex-Post* Effects - Majority and Minority Deals

This table presents the estimation results based on different subsamples for minority and majority acquisitions of the *ex-post* deal effects, presented in Equation (3) and described in Section 3, for firms that were exposed to a banking crisis during the studied period and classified as financially constrained in the crisis year. *Panel A* presents the estimation results for acquisitions where the acquirer final equity stake is less than 49.99% of the target - *i.e, minority acquisitions*, whereas *Panel B* presents the same set of results for acquisitions where the acquirer owns more than 50% of the target's equity stake *i.e, majority acquisitions*. Similar to table 6, $Deal_i$ is a binary variable that assigns 1 if firm i belongs to the subsample of firms that have were targeted in an M&A transaction during any period of the study, and zero otherwise. $After$ assigns 1 if the observation is measured in the same year of after the banking crisis hits the firm's headquarters. Dependent variable definitions are presented in Table 1. Standard errors are in parenthesis. *, **, *** denote statistical significance at 10, 5, and 1 percent, respectively.

	Panel A: Only Minority Acquisitions (Final Equity by the Acquirer is less than 49.99%						
	log(Assets)	log(Cash Holdings)	log(LT Debt)	log(ST Debt)	log(Total Debt)	log(CAPEX)	log(PPE)
Deal	1.511**	1.524**	1.524*	1.619**	1.539**	1.105	1.564**
	(0.770)	(0.767)	(0.796)	(0.782)	(0.764)	(0.805)	(0.763)
$After$	−0.068***	−0.142***	−0.790***	0.528***	−0.067***	−0.220***	−0.053**
	(0.020)	(0.036)	(0.048)	(0.044)	(0.021)	(0.043)	(0.023)
$Deal \times After$	0.061***	0.109***	0.186***	0.073*	0.057**	0.018	0.038
	(0.023)	(0.040)	(0.052)	(0.042)	(0.023)	(0.046)	(0.028)
Year fixed effects	✓	✓	✓	✓	✓	✓	✓
Firm fixed effects	✓	✓	✓	✓	✓	✓	✓
S.E Clustering	Firm	Firm	Firm	Firm	Firm	Firm	Firm
Observations	52,639	52,361	48,630	51,451	52,633	41,873	52,437
R^2	0.977	0.946	0.928	0.937	0.977	0.943	0.975
Adjusted R^2	0.974	0.939	0.919	0.929	0.974	0.934	0.972
	Panel B: Only Majority Acquisitions (Final Equity by the Acquirer is greater than 50%						
	log(Assets)	log(Cash Holdings)	log(LT Debt)	log(ST Debt)	log(Total Debt)	log(CAPEX)	log(PPE)
Deal	0.647	0.739	0.658	0.531	0.728	−0.483	0.635
	(0.869)	(0.851)	(0.972)	(0.902)	(0.865)	(0.763)	(0.881)
$After$	−0.054***	−0.087***	−0.799***	0.631***	−0.043**	−0.156***	−0.040**
	(0.018)	(0.030)	(0.041)	(0.038)	(0.017)	(0.035)	(0.020)
$Deal \times After$	0.053**	0.075**	−0.015	−0.009	0.016	0.068*	0.058**
	(0.021)	(0.035)	(0.049)	(0.042)	(0.020)	(0.041)	(0.027)
Year fixed effects	✓	✓	✓	✓	✓	✓	✓
Firm fixed effects	✓	✓	✓	✓	✓	✓	✓
S.E Clustering	Firm	Firm	Firm	Firm	Firm	Firm	Firm
Observations	35,832	35,512	32,598	34,727	35,828	29,415	35,614
R^2	0.977	0.946	0.926	0.934	0.978	0.940	0.976
Adjusted R^2	0.973	0.935	0.910	0.921	0.974	0.926	0.972

Note: *p<0.1; **p<0.05; ***p<0.01

Figure 2: *Ex-Post* Effects Based on the Final Equity Stake Held by the Acquirer Firm (in Deciles)

This figure presents the *ex-post* deal effects estimations, presented in Equation (4) and described in Section 3, considering different subsamples of according to the final equity stake held by the acquirer firm in the target firm. For all transactions, we divide the final equity stake of the target firm held by the acquirer in deciles and run the specification described in Equation (4). For each panel, the interaction term coefficient of the firms' *ex-post* crisis fundamentals regression on deal occurrence, $Deal_i \times After_{i,t}$, is plotted on a black point, with the solid black line denoting the 95% confidence interval. Dependent variable definitions are presented in Table 1.

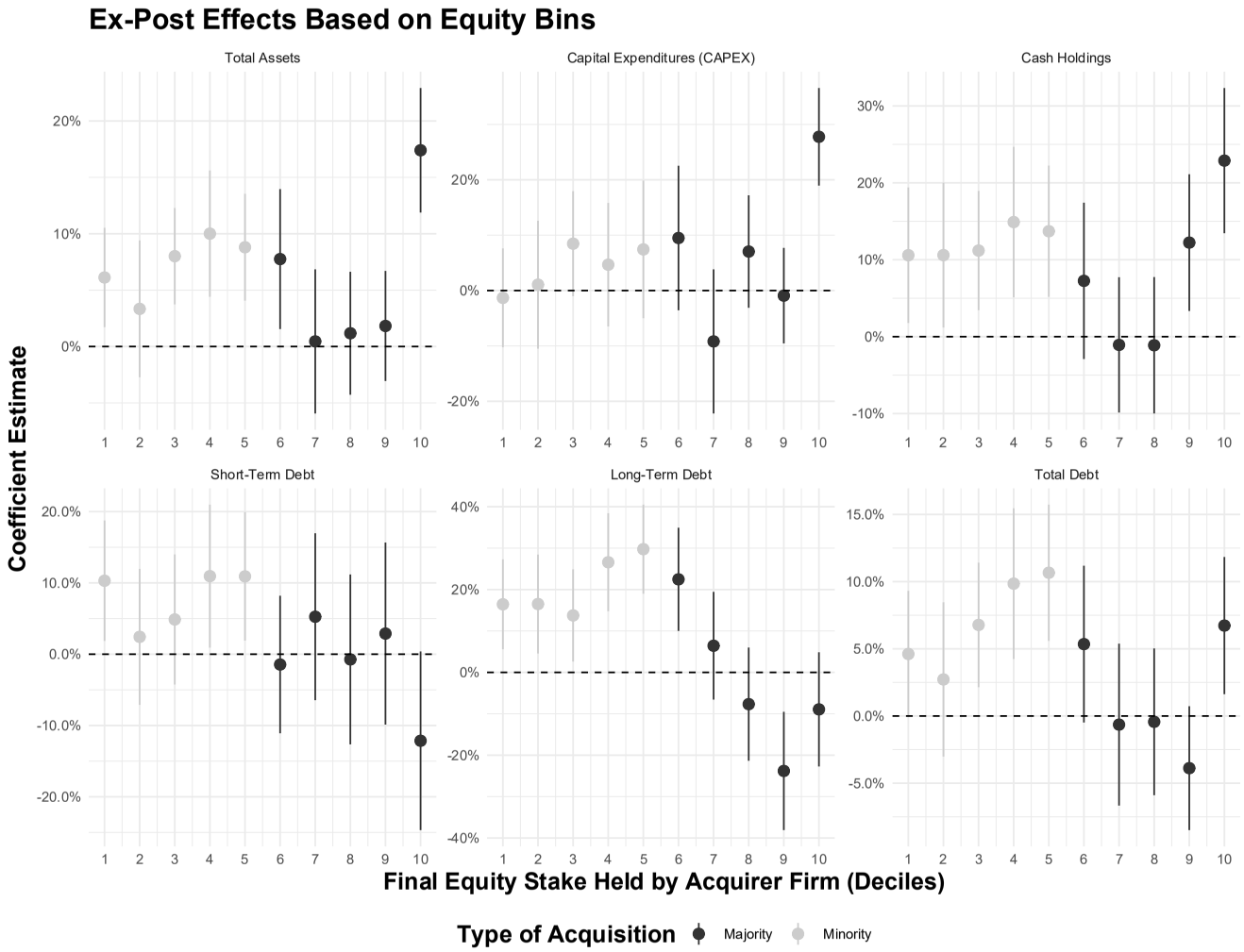

Table 9: *Ex-Post* Effects - Using only the deal subsample in banking crises

This table presents the estimation results of the *ex-post* deal effects specification, presented in Equation (3) and described in Section 3, considering only the subsample of targeted firms that were exposed to a banking crisis during the studied period, irrespective of its financial constraints status. FC_i is a binary variable that assigns 1 if firm i is considered financially constrained in period t *i.e*, $Maturing_{i,t} \geq$ Industry-Year median, and zero otherwise. $After$ assigns 1 if the observation is measured in the same year of after the banking crisis hits the firm's headquarters. Dependent variable definitions are presented in Table 1. Standard errors are in parenthesis. *, **, *** denote statistical significance at 10, 5, and 1 percent, respectively.

	Deal Dummy (1 if Deal in that year, zero otherwise)				
	log(Assets)	log(Cash Holdings)	log(Total Debt)	log(CAPEX)	log(PPE)
	(1)	(2)	(3)	(4)	(5)
FC	-0.107	-0.134	-0.227^{***}	-0.157^{*}	-0.134^{*}
	(0.066)	(0.120)	(0.076)	(0.083)	(0.070)
$After$	-0.066	-0.103	0.045	-0.419^{***}	-0.057
	(0.123)	(0.139)	(0.136)	(0.144)	(0.131)
$FC \times After$	0.084	0.119	0.173^{*}	-0.041	0.072
	(0.086)	(0.120)	(0.093)	(0.100)	(0.088)
Year fixed effects	✓	✓	✓	✓	✓
Firm fixed effects	✓	✓	✓	✓	✓
S.E Clustering	Firm	Firm	Firm	Firm	Firm
Observations	8,197	8,162	8,197	7,745	8,184
R^2	0.907	0.866	0.902	0.892	0.913
Adjusted R^2	0.900	0.855	0.894	0.883	0.906
Note:					*p<0.1; **p<0.05; ***p<0.01

Table 10: *Ex-Post* Effects - Cross-border and Domestic Deals

This table presents the estimation results based on different subsamples for domestic and cross-border acquisitions of the *ex-post* deal effects, presented in Equation (3) and described in Section 3, for firms that were exposed to a banking crisis during the studied period and classified as financially constrained in the crisis year. *Panel A* presents the estimation results for acquisitions where the acquirer's and target's headquarters are in the same country - *i.e, domestic acquisitions*, whereas *Panel B* presents the same set of results for acquisitions where the acquirer's headquarter differs from the target's *i.e, cross-border acquisitions*. Similar to table 6, $Deal_i$ is a binary variable that assigns 1 if firm i belongs to the subsample of firms that have were targeted in an M&A transaction during any period of the study, and zero otherwise. $After$ assigns 1 if the observation is measured in the same year of after the banking crisis hits the firm's headquarters. Dependent variable definitions are presented in Table 1. Standard errors are in parenthesis. *, **, *** denote statistical significance at 10, 5, and 1 percent, respectively.

	Panel A: Only Domestic Deals					
	log(Assets)	log(Cash Holdings)	log(Long-Term Debt)	log(Short-Term Debt)	log(Total Debt)	log(CAPEX)
Treated	1.527*	1.557**	1.578*	1.623**	1.094	1.580**
	(0.789)	(0.787)	(0.830)	(0.788)	(0.807)	(0.782)
$After$	−0.057***	−0.143***	−0.786***	0.562***	−0.175***	−0.045**
	(0.018)	(0.032)	(0.042)	(0.038)	(0.039)	(0.022)
Treated × $After$	0.034*	0.076**	0.082*	0.035	0.005	0.021
	(0.020)	(0.035)	(0.045)	(0.037)	(0.042)	(0.025)
Year fixed effects	✓	✓	✓	✓	✓	✓
Firm fixed effects	✓	✓	✓	✓	✓	✓
S.E Clustering	Firm	Firm	Firm	Firm	Firm	Firm
Observations	58,040	57,671	53,376	56,557	45,972	57,796
R^2	0.978	0.947	0.929	0.938	0.942	0.977
Adjusted R^2	0.975	0.940	0.919	0.930	0.932	0.974
	Panel B: Only Cross-border Deals					
	log(Assets)	log(Cash Holdings)	log(Long-Term Debt)	log(Short-Term Debt)	log(Total Debt)	log(CAPEX)
Treated	0.952	0.963	0.946	0.989	0.756	0.975
	(0.796)	(0.787)	(0.813)	(0.824)	(0.782)	(0.791)
$After$	−0.072***	−0.085**	−0.835***	0.590***	−0.211***	−0.060**
	(0.021)	(0.036)	(0.051)	(0.046)	(0.039)	(0.023)
Treated × $After$	0.123***	0.141***	0.202***	0.089*	0.084*	0.109***
	(0.029)	(0.049)	(0.063)	(0.052)	(0.048)	(0.036)
Year fixed effects	✓	✓	✓	✓	✓	✓
Firm fixed effects	✓	✓	✓	✓	✓	✓
S.E Clustering	Firm	Firm	Firm	Firm	Firm	Firm
Observations	34,230	33,982	31,398	33,294	28,780	34,042
R^2	0.975	0.944	0.925	0.932	0.944	0.974
Adjusted R^2	0.971	0.935	0.912	0.921	0.934	0.970

Note: *p<0.1; **p<0.05; ***p<0.01