Taking No Chances: Lender Monitoring and Corporate Acquisitions

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November 28. 2020

Abstract

Using mergers between firms' existing lenders as shocks to monitoring incentives and bargaining power, I find that intensified lender monitoring significantly reduces borrowers' public takeover activities. The effect is driven by mergers involving lead lenders, and becomes stronger for less bank-dependent firms with more risk-taking tendencies. Lender mergers reduce not only acquisitions that are value-destroying to shareholders but also value-enhancing ones. Deals that do happen on average create no additional shareholder value and target cash-rich firms with stable incomes. These results suggest that lender monitoring mitigates agency concerns, yet also leads to over-conservative firm behavior.

Keywords: Creditor Governance, Lender Monitoring, Mergers and Acquisitions

JEL Codes: G21, G30, G34

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I am gratefule for valuable comments from Miguel Antón, Tobias Berg, Yongqiang Chu, Vicente Cuñat, Christian Eufinger, Daniel Ferreira, Mireia Gine, Moqi Groen-Xu, Scott Guernsey, Dirk Jenter, Clemens Otto, Daniel Paravisini, Stefano Sacchetto, Pedro Saffi, Tao Shu, Sascha Steffen, Karin Thorburn, Yongxiang Wang, Hongda Zhong, seminar participants at LSE, IESE, and 28th Finance Forum PhD Day.

1 Introduction

The empirical corporate governance literature largely focuses on the influence of shareholders and corporate boards. Creditors are traditionally viewed as passive bystanders outside of payment default states .[1] More recent empirical studies show that creditors influence corporate policies far before payment default states, mainly due to loan covenants. Creditors affect firm policies by imposing covenants in loan contracts (Nini et al., 2009), by renegotiating covenants (Denis and Wang, 2014), and by gaining control rights after covenant violations (Chava and Roberts, 2008; Nini et al., 2012; Ferreira et al., 2018). Yet there is little evidence on whether lender monitoring changes managerial behavior beyond the effect of contractual provisions.

There are both theoretical reasons and some empirical evidence that creditors monitor their borrowers on a regular basis.[2] Even with collateral and covenants, lenders can be incentivized to monitor in order to update information and be able to make more efficient resolution decisions (Rajan and Winton, 1995). Because monitoring is mostly unobservable, existing studies often use shares retained by lead lenders to indirectly examine monitoring intensity (Sufi, 2007; Ivashina, 2009). More recently, Gustafson et al. (2020) provides direct empirical evidence of active lender monitoring including borrower meetings, site visits, and demanding information on a monthly, even daily basis. It is, however, unclear whether lender monitoring only serve as information acquisition, or whether it also has real effects on borrowers. This paper tests whether an increase in monitoring incentives and bargaining power for lenders disciplines borrowers and constrains their risk-taking. I focus on corporate acquisition behavior, one of the most important corporate actions that can significantly change a firm's risk profile (Furfine and Rosen, 2011).

[1] See, for example, theoretical models in (Townsend, 1979; Gale and Hellwig, 1985; Hart and Moore, 1998) on creditor involvement at payment default. Schleifer and Vishny (1997) and Nini et al. (2012) point out that empirical evidence on creditor governance remains limited compared to that on shareholder governance, despite an extensive theoretical discussion in the literature.

[2] There is an extensive theoretical literature discussing the comparative advantage of banks in monitoring, see, e.g. Diamond (1984); Fama (1985); Diamond (1991).

A key empirical challenge for studying the effect of lender monitoring on firm behavior is that banks might self-select into lending to firms with smaller managerial agency costs. I address this challenge by using a difference-in-difference (DiD) design based on lender mergers following Chu (2019). Bank mergers are often driven by consolidation in the financial sector in response to regulatory and technology shocks (Harford, 2005) or by business strategy reasons such as market penetration (Jayaraman et al., 2002). In addition, merging lenders in the DiD sample are large banking conglomerates lending to a substantial number of firms. They are unlikely to pursue mergers based on factors related to particular firms in their portfolios. Thus, I view the resulting increase in lender concentration as exogenous to the investment behavior of individual borrowers.

When a firm's two existing lenders merge, the newly merged lender has a more concentrated stake in the borrower and, hence, stronger incentives to monitor (Holmstrom and Tirole, 1997; Sufi, 2007). Gustafson et al. (2020) documents that monitoring intensity is indeed strongly asscoiated with the lead lender's skin in the game. Increased lender concentration can also ease coordination and mutual free-riding problems among different creditors, and therefore equip the new lender with more bargaining power when negotiating with managers and shareholders.[3] Should the enhanced monitoring generates actionable information on managerial risk taking, the merged lender can exert influence on management through renegotiations of loan terms (Chu, 2019; Gustafson et al., 2020) or the threat of cutting off future financing (Almeida et al., 2011). As a result, the increase in the lender's skin in the game should foster intensified lender monitoring that disciplines borrower managers from taking risky actions.

My baseline results show that lender mergers reduce borrowers' propensity to pursue a public takeover by 52% during the following three years. M&A is an often used setting to study investor monitoring of managerial risk-taking (Chen et al., 2007; Kempf et al., 2016). Acquisitions are substantial discretionary investments that managers can alter in response to

[3]See, for example, Gertner and Scharfstein (1991); Bernardo and Talley (1996); Bris and Welch (2005); Brunner and Krahnen (2008) who show that creditor dispersion allows managers to expropriate uncoordinated creditors.

monitoring pressure. Such investments are largely observable and reflect conflicts of interests between managers and investors. Furfine and Rosen (2011) show that mergers increase the acquirer's default risk, outweighing the potential benefit of asset diversification. Public takeovers, in particular, are much larger in size, more visible to creditors, and generally associated with managerial discretion (Jensen, 1986; Morck et al., 1990; Masulis et al., 2007).

The effect of lender mergers on acquisitions is solely driven by mergers involving a lead lender. This is consistent with lead lenders being the "informed lenders" with the responsibility of conducting due diligence and monitoring on the borrower for other participants in the loan syndicate (Sufi, 2007). The effect becomes more significant when the increase in lead lender skin in the game is more substantial. It is also stronger for firms with greater risk-taking tendencies, less subject to bank scrutiny, and when creditors are less coordinated. This evidence supports the idea that lender monitoring is intensified by the creation of a larger lead lender, causing borrowers to be better disciplined.[4]

The baseline results survive a battery of robustness tests that reinforce the validity of the DiD design. The dynamic effects of lender mergers and a falsification test indicate that the results are not driven by pre-existing trend differences between treated and control firms. I include firm$\times$bank merger fixed effect, and I select control firms from firms borrowing from either, but not both of the merging banks. These specifications help mitigate concerns that acquisition decisions are affected by bank mergers through confounding factors such as changes in banks' monitoring technology or higher financing costs due to increased bank market power. Additional tests also rule out that the results are driven by changes in ownership structure induced by bank mergers, as some banks also hold equity of their borrowers. The results are robust to selecting control firms outside of firms borrowing from either of the merging banks, with the propensity-score-matching approach. The results also remain consistent after exclud-

[4]Lender mergers do not have a significant effect on acquisitions with private targets, which is likely due to the fact that private takeovers are often much smaller in size, less observable in terms of target characteristics, associated with less overpayment, and more value creation for the acquirer firm (Chang, 1998; Fuller et al., 2002; Officer, 2007; Harford et al., 2012). See further discussion in Section 2.3.

ing the three largest bank mergers in the sample, ruling out the concern that the findings are driven by a few particularly large mergers.

I test several additional alternative explanations for my results. For example, the control firms might rely less on the merging banks relative to the treated firms and hence, might be less subject to a financing shock from increased market power of the merged lender. I do not find evidence of increased borrowing costs or decreased credit availability for loan contracts obtained by treated firms before and after the treatment. Another possibility is that my results simply capture the merged lender's improved bargaining power in renegotiations. I show that lender mergers between only loan participants, who do not monitor but can still affect the renegotiation process, do not have an effect on the borrowers' acquisition activities. I further find that the treatment effect does not differ if the firm is in violation of a loan covenant. The tests combined indicate that these alternative explanations are unlikely to drive my results.

Importantly, I find that lender mergers reduce not only shareholder value-destroying public takeovers (bad deals), but also value-enhancing ones (good deals) during the following three years, by 45% and 58% respectively. Deals that do happen following lender mergers tend to target firms with more cash holdings and lower cash flow volatility. However, these deals on average do not create shareholder value or synergies. I then provide additional suggestive evidence that managers also cut leverage following lender mergers. This again supports the conjecture that lender mergers lead to intensified lender monitoring that mitigates managerial risk-taking. Overall, the results of my analyses suggest that increased lender concentration due to lender mergers leads to intensified lender monitoring which disciplines managerial risk-taking. However, this enhanced monitoring leads to over-conservative decisions to forgo good investment opportunities, and to undertake risk-reducing takeovers, which mainly appeal to creditors.

This paper is closely related to two recent studies. Chu (2019) employs the same DiD design based on bank mergers to study how increased lender concentration affects the debt overhang problem. His study shows that lender mergers can ease the loan renegotiation process and bet-

ter allow capital investment when the borrower has good growth opportunities. My findings complement his study by showing that lead lenders can leverage such mitigation of the coordination problem to gain more bargaining power and better discipline borrower managers. Becher et al. (2020) investigate how creditors influence borrower acquisition activities after they gain control rights at the stage of technical default (covenant violation). My paper differs from and complements their study by showing that creditors can affect acquisition decisions even before any control right shift through more intense monitoring. Cross-sectional analyses confirm that my results are more likely to be driven by intensified lender monitoring instead of improved renegotiation efficiency.

The findings of this study contribute to the corporate governance literature, particularly research studying creditors' role in governance. Previous evidence of creditor influence on managerial decisions is mainly in the context of loan covenants (Nini et al., 2009; Chava and Roberts, 2008; Nini et al., 2012; Ferreira et al., 2018; Becher et al., 2020). My findings complement the recent evidence on non-covenant-based lender monitoring (Gustafson et al., 2020), and suggest that lender monitoring can mitigate managerial agency cost yet also lead to overconservative firm policies for shareholders in some occasions. Notably, while previous studies based on the covenant setting often find that creditor control after covenant violations creates value for shareholders, my results suggest enhanced lender monitoring can have mixed implications for them.

My results also add empirical evidence to the theoretical literature debating the costs and benefits of creditor dispersion. Easing the coordination problem among dispersed creditors can equip creditors with more bargaining power when negotiating with managers and shareholders (Gertner and Scharfstein, 1991; Bernardo and Talley, 1996; Bris and Welch, 2005; Brunner and Krahnen, 2008). Yet this coordination problem can also act as a disciplinary device committing managers to repayment, since workout success is less probable in strategic default (Bolton and Scharfstein, 1996; Zhong, 2020). Hence, easing this problem can actually weaken managers' repayment incentives. My findings provide evidence supporting the former argument,

that is, increased lender concentration mitigates managerial agency costs and benefit creditors.

This paper also contributes to the M&A literature by providing further evidence of creditor influence on acquisition decisions and outcomes. Many prior studies find evidence of shareholder influence on corporate acquisitions through, for example, based on ownership forms (Matvos and Ostrovsky, 2008; Harford et al., 2011), voting (Becht et al., 2016), portfolio inattention (Kempf et al., 2016), etc. Focusing on creditors, Acharya et al. (2011) show that firms are more likely to take diversifying acquisitions that are risk-reducing and shareholder value-destroying in countries with stronger creditor protection. I add to their findings by showing that, when creditors have stronger monitoring incentives and bargaining power, they can have similar effects on borrowers' takeover activities.

2 Theoretical Background and Testable Hypotheses

2.1 Creditor Governance

How do creditors get involved in corporate governance? Prior studies argue that state-contingent allocation of control rights to creditors is an efficient way to mitigate agency problems (Aghion and Bolton, 1992; Dewatripont and Tirole, 1994). Recent empirical evidence indeed show that creditors can extend their influence far before payment default state and have real effects on firm policies by imposing covenants on loan contracts (Nini et al., 2009). Creditors can also gain control rights from shareholders and directly influence firm policies when the firm violates a loan covenant (Chava and Roberts, 2008; Nini et al., 2012; Ferreira et al., 2018). A covenant violation is a technical default that happens quite frequently and the violating firm is often far from actual financial distress. After a violation, creditors gain informal control rights by being able to leverage their rights to terminate or accelerate the loan and exert influence on firm policy changes. Denis and Wang (2014) show that creditor control rights can extend to even before any covenant violation through covenant renegotiation.

Beyond the loan covenant setting, how do lenders conduct monitoring? There is an extensive theoretical discussion on banks' comparative advantage in monitoring (Fama, 1985; Diamond, 1984, 1991). Even covenants can often require creditors to monitor in order to become better informed and able to make more efficient resolution decisions at technical default states (Rajan and Winton, 1995). Gustafson et al. (2020) show direct empirical evidence that lenders conduct active monitoring through borrower meetings, site visits, as well as demanding information on a monthly and even daily basis from their borrowers. This evidence suggests that lenders do monitor and potentially have real effects on their borrowers' behavior even beyond using contractual provisions such as covenants. The aim of this paper is to investigate whether an exogenous increase in a lender's incentives and power to monitor can have a disciplinary effect on its borrowers' managers, deterring them from taking risky actions.

2.2 Lender Concentration and Managerial-Risk Taking

When a firm borrows from multiple lenders, these lenders are likely to encounter the collective action problem because different lenders can have different interests. As a result, lenders may disagree on the best course of actions and cause coordination failure at the default state. Such coordination difficulties also reduce the firm's expected liquidation value. A dispersed lender base can indicate that creditors have less bargaining power when negotiating with the manager or shareholders (Bris and Welch, 2005). Dispersed creditors are unable to be proactive because they cannot easily coordinate. Managers can in turn expropriate wealth from such uncoordinated creditors (Gertner and Scharfstein, 1991; Bernardo and Talley, 1996).

Empirically, while Brunner and Krahnen (2008) show that having a larger pool of creditors reduces the probability of workout success, Chu (2019) provides evidence that consolidation of a firm's lender base through a merger between its two existing lenders can ease this problem. As a result, the newly merged lender gains more bargaining power over its borrowers in negotiations, which should better discipline managers from expropriating creditors. Furthermore, this increase in bargaining power should also enable the newly merged lender to better carry

out monitoring activities identified by Gustafson et al. (2020) ranging from borrower meetings, site visits, to demanding frequent information. Risk-taking actions by managers should in turn become more exposed to creditors.

In addition to enhanced bargaining power to monitor, another important factor stemming from lender mergers is the increased incentive to monitor for the newly merged lender. Holmstrom and Tirole (1997) argue that lenders with monitoring responsibilities (lead lenders) often have the incentive to shirk because monitoring is costly and monitoring behavior is unobservable. Sufi (2007) and Ivashina (2009) show that when such lenders own more shares of the loans, this problem is mitigated. Lin et al. (2012) provide additional empirical evidence that lead arrangers form more concentrated syndicate structures to facilitate enhanced due diligence and monitoring efforts amid the borrowers' agency problems. The newly merged lender created by a merger between the firm's two existing lenders ends up with more at stake in this borrower. As a result, this new lender should be more incentivized to exert monitoring efforts.

In sum, lender mergers should lead to the creation of a larger lead lender with more incentive and power to monitor, resulting in an increased disciplinary effect on the borrower's risk taking behavior. The borrower's manager is more exposed and under intensified scrutiny when making corporate decisions. This increase in lender monitoring also enhances the governance role of loan covenants, because the new lender is able to acquire more information and make more efficient decisions during covenant renegotiation or post-violation policy interventions.

The enhanced monitoring hypothesis: *The increase in loan share held as the lead lender leads to stronger bargaining power and incentives for the newly merged lender to monitor, reducing managerial risk taking.*

2.3 Managerial Risk-Taking - The Corporate Acquisition Setting

Corporate acquisitions have been frequently employed as an ideal setting to study the effect of corporate governance on managerial risk taking (Masulis et al., 2007; Chen et al., 2007; Kempf et al., 2016). Acquisitions are largely observable and reflect conflicts of interests between man-

agers and investors. Such investments are often associated with aggressive or self-interested managerial actions (Jensen, 1986; Morck et al., 1990; Masulis et al., 2007). Mergers increase default risk, outweighing the potential benefit of asset diversification (Furfine and Rosen, 2011). Billett et al. (2004) show that bondholders of the acquiring firms on average react negatively to the acquisition announcements, indicating that most acquisitions are also value-destroying to acquirer creditors. Even if the acquisition is value enhancing, such a substantial investment with uncertainties should be unfavorable to creditors. Finally, unlike the much stickier capital and R&D expenditures, acquisitions are substantial discretionary investments that managers can alter in response to monitoring pressure. The enhanced monitoring hypothesis predicts that intensified lender monitoring induced by lender mergers should thereby better discipline managers and lead to a reduction in acquisitions.

However, not all acquisitions have the same value implications. Harford et al. (2012) show that a key source of value destruction of acquisitions by entrenched managers is the avoidance of private targets. The empirical evidence of negative announcement returns to acquisitions has also been based predominantly on public takeovers. Prior research has shown that private takeovers are generally associated with value creation. Acquiring firms are less likely to overpay for private targets due to the illiquidity discount (Officer, 2007). Additionally, acquiring firms can also be giving in to more intense monitoring when acquiring private targets (Chang, 1998; Fuller et al., 2002). Such private takeovers are likely to create new outside blockholders which can lead to increased monitoring effectiveness. The willingness of private target shareholders to take on the blocks also signals the market with favorable information on the acquiring firms. Therefore, creditors are actually more likely to benefit from private takeovers. Private takeovers are also often much smaller in size and thus associated with a lower level of risk. As a result, the effect of lender mergers on managerial risk-taking should be mainly associated with public takeovers.

3 Research Design

A key empirical challenge of studying the real effect of lender monitoring on corporate actions is that creditors can choose to lend to a firm because it has creditor-friendly policies. To address this reverse causality problem, I follow Chu (2019) and employ a difference-in-difference (DiD) design using mergers between large banks. The merging banks are large conglomerates lending to a substantial amount of firms and the number of affected firms in this design are often very small in comparison. Such banks are unlikely to merge because they want to be able to better monitor a few of their mutual borrowers. In addition, bank mergers are often driven by changes in deregulation in the financial sector (Harford, 2005) or business strategy reasons (Jayaraman et al., 2002). Therefore, the increase in lead lender skin in the game induced by bank mergers is likely to satisfy the exclusion condition. After a firm's two existing lenders merge, the newly merged lender has increased incentives to exert monitoring efforts because it now has a more concentrated debt investment in the firm (Sufi, 2007; Ivashina, 2009). Additionally, this increase in concentration mitigates the coordination problem, equipping the newly merged lender with more bargaining power to monitor and influence managerial decisions. As a result, this design should also meet the relevance condition.

The primary data sources of this paper come from LPC DealScan, Compustat, and SDC Platinum M&A. I first follow Schwert (2018) to aggregate DealScan lenders at the parent level and identify changes in lender ownership due to bank mergers.[5] As in Schwert (2018), only lenders with at least 50 loans or at least $10 billion in loan volume in the overall DealScan-Compustat sample are included. I then proceed to use the bank mergers identified from Schwert's sample to identify treated firms. Borrower firms in DealScan can be matched to GVKEYs using the link table provide by Chava and Roberts (2008). I require a treated firm to be borrowing from both the merging lenders through loan(s) initiated before and continuing beyond the merger year. While Chu (2019) focuses his analyses on mergers between lenders within the same syndicate,

[5] I thank Michael Schwert, as well as Sudheer Chava and Michael Roberts for making their link tables available.

my analyses include both within-syndicate and cross-syndicate mergers as long as the lenders are both lending to the treated firm. This is because I am interested in the overall concentration of the firm's lender base.[6]

To select control firms, I first exclude all the identified treated firms from the pool of potential control firms. I require the control firms to be borrowing from either of the merging lenders to control for unobservable lender characteristics as in Chu (2019). This helps isolate the potential that the results could be driven by certain banks having specific selection standards for borrowers, or changes in access to financing from the larger new lender. For example, the newly merged lender may have stronger market power and command higher borrowing costs, leading to a decrease in affected firms' acquisition activities. In this way even the control firms are affected by the bank mergers. Controlling for this aspect allows me to focus on the effect of a reduction in number of lenders induced by the mergers, rather than the effect of the mergers. I then match the control firms to each treated firm based on two-digit SIC industry classifications and whether they belong to the same tercile of asset size, market-to-book, and three-year average acquisition spending[7] sorted for the treatment year within the Compustat sample (Hong and Kacperczyk, 2010; Derrien and Kecskés, 2013). I follow Chu (2019) and use a (-3, +3) six-year window around the merger year, with the merger year discarded for clean identification. Furthermore, I exclude mergers that occur after 2007. Lewellen and Lowry (2020) show that including financial institution mergers during the crisis period can contaminate the DiD, because the treatment effect based on such mergers might be capturing how firms respond to the financial crisis. The final DiD sample consists of 24 large bank mergers with 17 involving at least one lead lender, as shown in Table A.1.

I focus on mergers involving a lead lender because lead lenders in syndicated loans bear the

[6]For example, if lender A is lending to firm i through one syndicated loan and lender B is doing so through another, a new lender C created by the merger of A and B will still have increased incentive to monitor because it has more at stake than A or B alone. It also has more bargaining power as it can hold up renegotiation in either of the loans.

[7]Acquisition expenditures divided by total assets averaged across the treatment year and the previous two years.

responsibilities of monitoring the borrowers (Sufi, 2007; Ivashina, 2009). Loan participants, as uninformed lenders, rely on lead lenders (informed lenders) for due diligence and monitoring of the borrower firms. Lead lenders have the incentives to shirk on their responsibilities since monitoring is costly (Holmstrom and Tirole, 1997). Such evidence from the existing literature predicts that lender mergers involving lead lenders should have a more pronounced effect on public takeovers, if the effect comes from intensified lender monitoring. A merger with another lead lender or loan participant of loans to the same firm increases the lead lender's stake in the firm and therefore induces more monitoring effort from the newly merged lender. Lenders with lead arranger credit in the loan facilities as indicated in DealScan are classified as lead lenders of the corresponding loans, and participants otherwise. I assign a firm as treated if the bank merger involves at least one lead lender of its currently existing loans (originated before and continue beyond the bank merger year).

As presented in Figure 1, the mergers are spread out over the period of 1992 to 2004. There is a cluster of mergers in the late 1990s which is likely due to the deregulation in the financial service industry. The weight of the treated firms in the merging banks' combined portfolio of total borrowers ranges from 0.3% to 6.1%, with an average of 2%. This provides support to the exogeneity assumption that banks are unlikely to merge based on the fundamentals of 2% of the firms in their combined lending portfolio. However, 2% in the overall portfolio should still be significant enough to attract attention for the newly merged lender to allocate resources to monitor following the merger.[8]

The data source of firm takeover activities comes from the SDC Platinum M&A database. I classify a takeover as public (private) takeover if SDC reports the target as a public (private) target. A deal is only kept if the acquirer owns less than 50% of the target prior to deal announcement and is seeking to own more than 50% of the target. My baseline analysis focuses on whether increased lender concentration affects the firm's takeover activity. The DiD specifi-

[8] In Table 6 I exclude the three largest mergers and obtain similar results.

cation is as follows:

$$Takeover_{i,t} = \beta_1 Treat_{i,k} \times Post_{i,k,t} + \beta_2 Post_{i,k,t} + \delta X_{i,t-1} + \alpha_{i,k} + \alpha_{j,t} + \epsilon_{i,t} \quad (1)$$

where $Takeover_{i,t}$ is either a dummy variable that equals one (Acquisition Dummy) if firm i announces an acquisition during year t, or the natural logarithm of one plus the number of acquisitions firm i announces during year t (Acquisition Activity). $Treat_{i,k}$ is a dummy variable that equals one if firm i is classified as treated in merger k, and zero otherwise. $Post_{i,k,t}$ is a dummy variable that equals one if the fiscal year t is after the year in which merger k occurs. $\alpha_{i,k}$ is the firm$\times$merger fixed effects. Including firm$\times$merger fixed effects instead of simply firm fixed effects is important because it helps isolate unobservable merger-specific characteristics that might contaminate the identification. $\alpha_{j,t}$ is the industry$\times$year fixed effects, where j is the 2-digit SIC industry to which firm i belongs. Because firm takeover activities can be closely related to industry-specific boom and bust, it is important to control for time-varying industry trends. Finally, I include a set of firm controls $X_{i,t-1}$ from the prior year end that can influence the firm's propensity to pursue an acquisition, including market-to-book, cash holdings, leverage, firm size (the logarithm of total assets), profitability (ROA), annual stock return. I cluster standard errors at the firm level to account for heteroskedasticity and serial correlation.

In follow-up analyses, I further analyze the acquisitions that do get announced under intensified lender monitoring. Acquisition deal level data is obtained from the SDC M&A database. I analyze deals with public U.S. targets to explore target characteristics. Another key variable to examine in this step is the value creation of the acquisition under increased lender concentration. The typical way to measure this in the M&A literature is using cumulative abnormal return (CAR) to the acquirer around the announcement date of the deal. The CAR reflects market reaction to the acquisition. Following prior literature, I compute the CAR using the market model for a (-1, +1) window around deal announcement, with an estimation window of (-200, -60) prior to the announcement date. Synergies (%) are calculated following Harford et

al. (2011) as the CAR (-1,+1) of the value weighted portfolio of the acquirer and target, with target adjusted for toehold (combined abnormal increase in market value). I repeat the regression using Equation 1 with acquirer CAR or synergies as the dependent variable. Table 1 provides summary statistics of firm level and deal level variables used in the main empirical analyses of this paper. Detailed variable definitions are included in the appendix Table A.2.

4 Empirical Results

4.1 Baseline DiD Results - Lender Mergers and the Propensity to Pursue Acquisitions

Table 2 reports the results of the baseline analysis following Equation 1. I conduct the analysis for both public and private takeovers. The results indicate that lender mergers only have a significant effect on the affected firms' public takeover activities and do not significantly influence their private takeover activities. The $Treat \times Post$ variable has a significant and negative effect on public takeovers across all specifications. In column (1), I include firm and merger fixed effects separately. The *Treat* variable has a coefficient in this case because a firm can be affected by multiple bank mergers in the same year. The small and insignificant coefficient indicates that there is no significant difference between treated and control firms before the bank mergers. Column (3) presents the baseline result including all firm level controls as well as firm$\times$merger fixed effect and industry$\times$year fixed effect. After the merger between two of its existing lenders, a firm's probability of pursuing an acquisition of a public target is decreased by 6.2%. This effect is of great economic magnitude. Panel A of Table 1 shows that the sample average of the public takeover dummy is 0.12, indicating that a lender merger can reduce a firm's propensity to conduct a public takeover by 52%. This effect remains robust to the use of a continuous dependent variable in Column (4), acquisition activity. The result also holds when using alternative acquisition measures in Table A.3, i.e. acquisition spending and that as

a percentage of total assets.

The overall results in Table 2 suggest that lender mergers can significantly reduce a firm's public takeover activities whereas they do not have a significant impact on the firm's private takeover decisions. This can be related back to the previous discussion on the difference between acquisitions of public and private targets in Section 2. Public takeovers are often associated with managerial agency costs, while private takeovers have been shown to generally create value and less linked to agency concerns (Chang, 1998; Fuller et al., 2002; Harford et al., 2012). Their often much smaller deal sizes also represent a much lower level of uncertainty risk to creditors relative to public takeovers. A firm's public takeover activities are much more likely to draw its creditors' monitoring attention. Overall, baseline results presented in Table 2 support the enhanced monitoring hypothesis, which conjectures that lender mergers lead to enhanced lender monitoring that disciplines managers from pursuing risky actions, even outside of the default state and beyond contractual provisions.

4.2 Lender Heterogeneity - Lender Monitoring or Renegotiation Efficiency?

To further test whether the effect of lender mergers on public takeovers comes from stronger lender monitoring, I explore mergers between lenders with different roles. The baseline analysis in Table 2 focuses on bank mergers involving a lead lender. Lead lenders are informed lenders who conduct monitoring of the borrowers whereas loan participants do not monitor and rely on lead lenders for due diligence. While mergers involving only loan participants can improve renegotiation efficiency, as shown in Chu (2019), it should not affect lender monitoring. I first repeat the analysis with the full bank merger sample. I then create a sample of mergers involving only loan participants. Panel A of Table 3 presents the results of repeating the regression of Equation 1 with all bank mergers and Panel B presents the results for the participant merger sample.

As expected, the treatment effect of lender mergers is mainly driven by mergers involving lead lenders. In the all bank merger sample, the $Treat \times Post$ variable shows signs of a decrease

in public takeover activities yet does not have enough statistical significance. This is likely due to the effect from lead lender mergers being diluted by that from loan participant mergers. The $Treat \times Post$ variable has close to zero point estimate in the loan participant merger sample, indicating that mergers involving only loan participants have no effect on the borrower's acquisition activities.

These results support the notion that increased lender concentration induced by lender mergers leads to intensified lender monitoring. Such strengthening of lender monitoring appears to be driving the baseline results. If the results are equally significant for both lead lender mergers and participant mergers, the relationship between lender mergers and borrower public takeovers could also be driven by the renegotiation efficiency channel identified by Chu (2019). Although loan participants are uninformed lenders who do not monitor the borrower firm, they have the ability to hold up renegotiation. A merger between two loan participants increases the incentive for the newly merged participant lender to ensure the safety of its now more concentrated loan investment. One could argue that by leveraging this ability, such newly merged participant lenders can influence the lead lenders and even directly the borrower firms' managers on corporate decisions. The results based on mergers involving only participants suggest that this alternative channel is unlikely to be driving the results. Therefore, these tests indicate that the baseline results are more likely to be related to intensified lender monitoring rather than improved renegotiation efficiency.

4.3 Intensity of Treatment

One important assumption of the relevance condition in the DiD experiment is that the marginal increase in the lead lender's skin in the game should be meaningful enough, for the treatment effect identified to be explained by an increase of monitoring incentive and bargaining power. Unfortunately, there is a data limitation issue when it comes to using DealScan data. A significant portion of observations on within-syndicate loan share allocated to each lender have missing values. Therefore, I cannot construct a continuous treatment measure for the full base-

line DiD sample to capture the level of increase in lead lender skin in the game. As a solution, I construct a conditional DiD sample using loans with non-missing lender share values. Ivashina (2009) shows that there is no systematic difference between those with and without loan share information. I identify treated firms using lender mergers involving a lead lender based on these loans, and match them to control firms following the same procedure as in the construction of the baseline sample.

I first repeat the baseline DiD analysis with this conditional sample to check for consistency in the treatment effect. Columns (1) to (3) in Table 4 show that the results remain similar in this smaller sample. I then construct a continuous treatment variable, Δ Lead Share, which measures the percentage of increase in the lead lender's loan investment in the treated firm. The increase in lead lender skin in the game is 63% for the average treated firm in this sample. Δ Lead Share is set to zero for treated firms during the pre-treatment period and controls firms for the full period. Columns (4) to (6) report the results of running the DiD analysis with this continuous treatment measure instead of the discrete treatment variable. The results indicate that the treatment effect indeed strengthens as the increase in lead lender skin in the game becomes more significant. This evidence provides further support to the notion that the baseline results are driven by the increase in monitoring incentive.

4.4 Robustness of Baseline Results

4.4.1 Dynamic Effects of Lender Mergers

Because I aim to obtain causal estimates of the effect lender mergers have on corporate risk taking, it is important to check for the robustness of the difference-in-difference design to rule out potential identification threats. The first threat is that the baseline results can be driven by pre-existing trend differences between treated and control firms. I conduct a dynamic analysis

of the effect of lender mergers by estimating the following:

$$Takeover_{i,t} = \sum_{n=-3}^{n=3} \beta_n Treat_{i,k} \times Year^n + \delta X_{i,t-1} + \alpha_{i,k} + \alpha_{j,t} + \epsilon_{i,t} \quad (2)$$

The same firm level controls and fixed effects from Equation 1 are included. The *Treat* variable is interacted with year dummies across the (-3, +3) DiD period. If the baseline results are indeed driven by the treatment effect of lender mergers, I expect to see no significant effect when n is less than zero. The results of this analysis are presented in Table 5 and plotted in Figure 2. Results on the table indicate that there is close to zero and statistically insignificant effect of lender mergers on public takeover activities before the merger year. The negative effect starts to materialize in Year 2 after the merger year. The plots show that the treatment effect hovers around zero during the pre-treatment period, providing visual support that the treatment effect in the baseline results are unlikely to be affected by pre-existing trend differences between treated and control firms.

4.4.2 Falsification Tests

To further address the concern over pre-existing trend differences, I follow Chu (2019) and conduct a diagnostic falsification test. I create fictional mergers that occur four years before the actual mergers. I then repeat the same analysis in the baseline DiD with such fictional mergers with the same treated and control firms during a (-3, +3) window. If the baseline results are indeed driven by pre-existing trend differences, $Treat \times Post$ will also have a significant and negative coefficient in this falsification test. Based on results presented in Panel A of Table 6, this is not the case because the coefficient of $Treat \times Post$ is close to zero and statistically insignificant across all specifications. Overall, the results of both the dynamic analysis and falsification test suggest that the parallel trend condition is likely to be satisfied.

4.4.3 Controlling for Potential Confounding Factors from Bank Mergers

Another identification threat to the DiD design is that the results are capturing the effects of other variables affected by the treatment. Large banks often have asset management divisions that hold equity shares of their borrower firms. The merger between the firm's two existing lenders can create a new blockholder if both merging parties also hold its shares. Blockholders have been shown to be able to monitor and scrutinize managerial actions (Schleifer and Vishny, 1997; Chang, 1998). The merger can also lead to an increase in ownership concentrated in the top 5 institutional shareholders. Therefore, the $Treat \times Post$ Variable can be actually capturing the effect of an additional blockholder or increased top ownership concentration on the firm's decision to pursue public takeovers.

Additionally, there is an emerging literature studying when the firm's creditors simultaneously hold its equity shares. Anton and Lin (2020) show that such dual holders can align shareholder creditor interests and better monitor against managerial discretion, leading to a mitigation of overinvestment, particularly in acquisitions. A merger between the firm's two existing lenders can also lead to an increase in the number of dual holders. Financial institution mergers have indeed been also used by dual holder studies for identification (Chu, 2018; Anton and Lin, 2020). The $Treat \times Post$ Variable can thus be capturing the effect of an increase in shareholder-creditor incentive alignment facilitated by an increased dual holder presence.

Finally, common ownership can be another shareholder governance variable biasing the estimates of the treatment effect. Common ownership means when a firm's large shareholders also hold shares across its industry peers. Prior studies have shown that common ownership equips such shareholders with industrywide information and governance expertise, allowing them to monitor the managers more effectively (Kang et al., 2018). Lin (2020) further shows that such monitoring leads to improved credit conditions, using financial institution mergers as the source of exogenous variation in common ownership. As a result, it is necessary to disentangle the potential increase in common ownership induced by the treatment in the baseline DiD.

To address these concerns, I first repeat the baseline analysis controlling for the number of blockholders, top 5 institutional ownership, the number of dual holders, and common ownership,[9] both separately and together, alongside with institutional ownership. Panel B of Table 6 presents the results of these regressions. These potential confounding factors do not appear to bias the point estimate of the main variable of interest across all specifications. Top 5 ownership concentration does show a significant and positive effect on public takeover propensity yet it does not affect the coefficient of the *Treat* $\times$ *Post* variable. In Panel C I further examine whether the treatment in this DiD design has any influence on these four types of ownership. The results indicate that there is no significant change in the number of blockholders, top 5 ownership concentration, the number of dual holders, or common ownership following the treatment in this DiD. These tests help rule out the possibility that the treatment effect of bank mergers could be capturing factors other than increased lender concentration.

4.4.4 Alternative Windows

I then conduct another set of robustness checks on the event windows used in the DiD. I first use a shorter (-2, +2) window then extend the window to the longer (-4, +4). The results are reported in Panel A of Table 7. The results remain robustly significant in both the shorter and longer windows. However, *Treat* $\times$ *Post* has a negative coefficient that is only significant at the 5% level. *Treat* $\times$ *Post* in the (-4, +4) window has stronger statistical significance. These results suggest that the real effect of enhanced lender monitoring takes time to materialize, consistent with the trend in Figure 2 and Table 5.

4.4.5 Robustness of the Bank Merger Sample

The underlying assumption for the exclusion restriction of the DiD design is that banks do not merge in order to obtain more concentrated positions in borrower firms with less managerial

[9] Common ownership is calculated following Harford et al. (2011) as $CO_j^{HJL} = \sum_{k=1}^{K} \sum_{i=1}^{I} w_k \frac{\beta_{ij}\beta_{ik}}{\beta_{ij}+\beta_{ik}}$, where $i = 1, \ldots, I$ is the set of shareholders of firm j, β_{ij} is the ownership share of shareholder i in firm j, and β_{ik} is the ownership share of shareholder i in firm k.

risk-taking. To further ensure that this is a valid assumption, I conduct two additional sets of robustness sets with alternative bank merger samples in Panel B of Table 7. First I exclude the three bank mergers with the largest numbers of affected firms. Column (1) and (2) show that the relationship between lender mergers and public takeover remains robust after excluding such mergers, suggesting that my results are not driven by a few particularly large mergers. Additionally, I limit the sample to include bank mergers that occured in the late 1990s and early 2000s (1995-2002). This sample corresponds to a period of two major banking deregulation events, the Riegle-Neal Interstate Banking and Branching Efficiency Act of 1994 and the Gramm-Leach-Bliley Act of 1999. Such mergers are more likely to occur due to the deregulation of the U.S. financial sector (Harford, 2005), and less likely to be driven by factors related to individual firms. Results reported in column (3) and (4) show that the relationship again holds for this sample.[10]

Finally, one concern over the relevance of the DiD design is that it is contingent on the merging lenders still holding their loan share at the time of merging. It is possible that in some occasions both of the merging banks have already sold their shares of the loans to the treated firm. In this case there will be no increase in the incentive to monitor. Since there is no available data on loan sales in the DealScan LPC database used in the DiD analysis, this is the limitation of my study, which has to rely on the assumption that most of the merging banks still hold on to their loan shares of the treated firms at the time of merging. Nevertheless, banks are less likely to sell their loan shares from revolver loans due to the lines of credit they keep with the borrowers. I therefore construct a sample of lender mergers through revolver loans and repeat the DiD analysis. Furthermore, Irani and Meisenzahl (2017) show that there is a very low activity level in the U.S. secondary syndicated loan market from early to mid 1990s. I restrict my bank merger sample to only the five mergers from 1992 to 1995 and repeat the

[10]In untabulated results, I further use the sample of bank mergers that occurred outside of this deregulation period, i.e. two in 1992, one in 1994, and two in 2004, and repeat the analysis. The result remains economically and statistically significant using this sample. This additional test helps address the concern that potential unobserved factors related to the regulatory changes, other than lender mergers, could be driving the results.

baseline analysis. The results of these two tests are reported in Table A.4. They indicate that the results remain similar in these samples, mitigating the concern over traded loan shares.

4.4.6 Alternative Matching Method for Control Firms

The last set of robustness check I conduct is to address the concern that control firms are also affected in the baseline DiD design. It is important to control for potential impact on access to debt financing due to lender mergers, or bank-specific borrower selection standards, which is why I select control firms from firms borrowing from one of the merging banks. However, one could argue that control firms also being affected by the treatment can add noise to capturing the difference between treated and control firms during the post period. In this section, I select control firms from a pool of firms that are not borrowing from any merging banks in the bank merger year, based on the propensity score matching method. I perform the matching based on the list of firm control variables used in the baseline regression, as well as the logarithm of the number of lenders (DealScan) the firm is borrowing from in the bank merger year, and the three year average acquisition spending as in the baseline DiD control selection. I then select the three firms with the closet propensity scores to each treated firm as the control firms. Table A.5 presents the results of repeating Equation 1 and the dynamic analysis of Equation 2 using this new sample. The results are consistent with those in Table 2 and 5.

4.5 Alternative Explanations

Although I control for the potential financing channel to some extent by selecting control firms from borrowers of either but not both of the merging banks, one could still argue that unlike the treated firms, control firms rely less on the merging banks. As a result, they have better access to other lenders if the newly merged lender commands a higher borrowing cost with increased market power. Therefore, debt financing becomes more costly mainly for the treated firms. Another possibility is that the newly merged lender could decide to reduce future credit to the treated firms in order to reduce exposure to a now more concentrated set of borrowers.

This is unlikely since the treated firms consist of a small fraction of the combined portfolio of the merging banks, as indicated by Figure 1.

Nevertheless, I directly test whether this financing channel is driving my results. Both arguments predict that after a lender merger, it becomes more difficult for the treated firms to access new debt financing compared to the control firms, limiting their ability to pursue public takeovers. In Table 8, I examine loan facilities originated to the sample firms involving the merging banks before and after mergers. The results indicate that the treated firms actually enjoy cheaper borrowing costs from the newly merged lenders following bank mergers, as well as longer maturity. When looking at all new loans issued to the sample firms, instead of only those involving the merging banks, there is again no sign of an increase in borrowing costs or a decrease in credit availability. Therefore, my findings are unlikely to be related to the potential change in access to debt financing following lender mergers.

Another possible explanation to the baseline results can be derived from the improved renegotiation efficiency idea. Becher et al. (2020) show that after a covenant violation, lenders tend to tighten restrictions on the borrower's acquisition activities through loan renegotiation. One could argue that since the newly merged bank is a lead lender with a more concentrated loan share, it is in a better position to push for such restrictions during the post-violation renegotiation. As a result, my analysis may be only capturing a cross section of the findings of Becher et al. (2020). I obtain covenant violation data from Amir Sufi's website and test whether the treatment effect of my DiD design differs for firms in violation. Table 9 reports the results. Columns (2) and (5) suggest that a borrower firm is less likely to pursue a public takeover if it had a covenant violation in the previous year, similar to the findings of Becher et al. (2020). Columns (3) and (6) show that there is no significant difference in the treatment effect for firms in violation, compared to those that are not. This test indicates that my baseline results are unlikely to be mainly related to renegotiation efficiency after covenant violations.

4.6 Heterogeneity of the Treatment Effect

4.6.1 Bank Dependency and Creditor Coordination

The enhanced monitoring hypothesis predicts that the treatment effect should be more pronounced when creditors are more dispersed and have less bargaining power. Firms establish a solid reputation and "graduate" from bank debt to public debt (Diamond, 1991; Sufi, 2007). Monitoring then becomes "less necessary". However, such lack of bank monitoring attention can leave more room for managerial agency problems. Lin et al. (2013) show that when managerial agency problem is more severe, managers can choose public debt over private debt to avoid bank scrutiny. I follow Sufi (2007) and Schwert (2018) to classify firms as non-bank-dependent if they have an S&P credit rating, and unrated firms as bank-dependent. Colla et al. (2013) show that unrated firms are more likely to specialize in one type of debt while rated firms have more heterogeneous debt structures. Non-bank-dependent firms can often diversify away from bank scrutiny and thus have more uncoordinated creditors.

Brunner and Krahnen (2008) show that managers tend to expropriate wealth from uncoordinated creditors. The enhanced monitoring hypothesis conjectures that lender mergers can mitigate this problem by easing the coordination failure problem and disciplining managers from exploiting creditors. I follow Gormley and Matsa (2011) and calculate debt HHI as a proxy for creditor bargaining power, as well as an indicator of whether the firm has one type of debt being over 90% of its total debts. I then split the sample using these three proxies based on *ex ante* information in the year before the lender merger. A firm is classified into the low debt HHI subsample if its debt HHI in the year before the treatment event year is below sample median. Panel A of Table 10 presents the results of these subsample tests.

Consistent with the prediction of the enhanced monitoring hypothesis, the results suggest that the effect of lender mergers on firm public takeover activities is mainly driven by firms that are less bank-dependent and when creditors have less bargaining power due to coordination difficulties. The treatment has no significant effect on firms that are bank-dependent and when

creditors are more concentrated. This evidence provides further support to the notion that the effect is associated with intensified lender monitoring, which disciplines managerial risk-taking in firms that are usually less subject to bank scrutiny.

4.6.2 Managerial Risk-Taking

With the enhanced monitoring hypothesis, I expect to see the effect of lender mergers to be stronger for firms with more managerial risk-taking tendencies. With increased incentives for the newly merged bank to engage in monitoring and its stronger bargaining power in renegotiation, managers with more discretion should be under more pressure to cut down risk-taking actions such as public takeovers. I first use R&D expenditures as a proxy for managers' willingness to take on risky projects (Coles et al., 2006). I split the sample based on whether the firm had any R&D expenses in the year before the event year. I then split the sample based on firms' cash flow risk, i.e. whether a firm's operating income volatility is above or below sample median in the year before the event year. Finally, Managers in firms with high cash flows but poor growth opportunities are more likely to pursue empire-building acquisitions (Jensen, 1986). I use a score based on free cash flow and market-to-book to measure a firm's overinvestment tendency. I first rank firms' ROA and market-to-book ratios into sample deciles. The deciles are then scaled by ten into scores from zero to one. The overinvestment score is the average between the ROA score and one minus the market-to-book score. A firm with high profitability and poor growth opportunities are more likely to overinvest.

Panel B of Table 10 shows the results of these subsample tests. The results indicate that firms with more risk-taking tendencies are more likely to experience a decrease in public takeover activities, supporting the notion that increased pressure from intensified lender monitoring better disciplines managers who would have otherwise taken more risky actions. However, these results have to be interpreted carefully as being suggestive, since the measures are imperfect proxies and some of the interaction terms are not statistically significant at the conventional level.

Overall, the subsample tests based on bank dependency and managerial risk-taking lend further support to the enhanced monitoring hypothesis. Based on prior literature, managers in firms less subject to bank scrutiny are likely to exploit uncoordinated creditors. A more consolidated lender base induced by bank mergers mitigates this problem by creating stronger creditor pressure with increased monitoring incentives and strengthened bargaining power, disciplining managers from risk-taking actions.

4.7 Lender Mergers and Acquisition Quality

Not all acquisitions are unfavorable to creditors, even in the case of public takeovers. Some positive NPV acquisitions can benefit creditors, although some can also increase firm risk and hurt creditors. In this section, I examine whether the quality of the acquisitions, i.e. shareholder value creation, matters in lenders' monitoring of their borrowers' acquisition decisions. I follow prior literature (Harford et al., 2011) to define acquisitions with positive cumulative abnormal returns (CARs) as value-enhancing deals (good deals) and those with negative CARs as value-destroying deals (bad deals). The results are reported in Table 11.

I find that lender mergers not only decrease bad public takeovers but also those that are value-enhancing to shareholders. This evidence suggests that managers tend to refrain themselves from not just bad deals but public takeovers *per se* when their creditors gain more incentives and bargaining power to monitor. The empirical evidence in the literature indeed shows that acquirer creditors generally react negatively to public takeovers (Billett et al., 2004). This sheds light on why creditors appear to be against public takeovers in general. The recent work of Becher et al. (2020) show that after a covenant violation, creditors use their control rights to prevent bad deals but do not significantly affect good deals. My findings are more in line with the notion that intensified lender monitoring leads to a stronger disciplinary effect on the manager, leading to over-conservative firm policies amid heightened creditor pressure. Managers are less likely to take any chances by pursuing public takeovers which are usually costly and associated with high uncertainties.

I then examine public takeovers that do get announced following lender mergers. Acharya et al. (2011) show that in an environment with strong creditor rights, firms tend to engage in acquisitions that are appealing to creditors but value-destroying to shareholders. This is an often used way by managers to reduce firm risk and "play it safe" (Amihud and Lev, 1981; Morck et al., 1990; Gormley and Matsa, 2011, 2016). I test both the target characteristics and value creation (acquirer CAR and synergies) of such deals in the DiD framework as below:

$$Y_{i,\mu,t} = \beta_1 Treat_{i,k} \times Post_{i,k,t} + \beta_2 Post_{i,k,t} + \gamma Z_\mu + \delta X_{i,t-1} + \alpha_{i,k} + \alpha_{j,t} + \epsilon_{i,t} \tag{3}$$

where Y is the outcome variable, μ denotes the acquisition deal and Z_μ is a set of deal characteristic controls including whether it is an all stock deal, an all cash deal, has a competing bidder, a tender or hostile offer, and the relative deal size. I classify the deal as a diversifying deal if the acquirer and the target have different macro industry descriptions in the SDC database. The sample consists of completed deals with U.S. public targets. I include industry$\times$merger as well as announcement year fixed effects. The results are reported in Table 12 and 13.

Column (1) to (3) show that public takeovers following lender mergers target firms with more cash holdings. The relationship is robust to controlling for deal and acquiring firm characteristics. Furthermore, column (5) to (7) indicate that target firms in such deals also tend to have lower cash flow volatility. In columns (4) and (8), I further include firm fixed effect to control for firm-specific components that can affect acquisitions. As pointed out by Golubov et al. (2015), some firms are extraordinary acquirers with persistent acquisition performance. The results remain significant for both target characteristics, despite a sharp decrease in the number of observations. Treated firms tend to also finance these deals with less cash and more equity (Table A.6). These results support the notion that managers tend to pursue acquisitions that are appealing to creditors following lender mergers.

Meanwhile, Table 13 suggests that deals that do get announced amid intensified lender monitoring do not appear to create additional shareholder value. Lender mergers have no sig-

nificant effect on announcement returns or deal synergies. In fact, the coefficient for *Treat* × *Post* is negative for both acquirer return and deal synergies when including firm fixed effect, even though it is still statistically not significant. This can be due to the mixed implications of such deals to shareholders. While these deals are unlikely to be empire-building or wasteful acquisitions, they are also less likely to be targeting for future growth such as certain technologies. Instead, these acquisitions target cash cow firms with stable incomes, for risk-reducing purposes under creditor pressure (Amihud and Lev, 1981; Morck et al., 1990; Gormley and Matsa, 2011, 2016). Overall, results in this section suggest that while lender monitoring mitigates managerial agency costs, it can also lead to over-conservative firm policies that focus on risk-reducing and forgo growth opportunities.

4.8 Financial Policies Following Lender Mergers

For an extension of my empirical analyses on investment conservatism amid intensified lender monitoring induced by lender mergers, I examine how managers respond in financial policies. The choice of leverage policy has been often associated with managerial risk-taking (Coles et al., 2006). An increase in firm leverage puts existing creditors' debt investment in the firm at more risk. Being exposed to more creditor scrutiny following a lender merger, I expect the manager to also be more conservative in the choice of financial policies. I repeat the baseline DiD regression with book leverage ratio as the outcome variable. For the list of control variables, I omit leverage and add tangibility as well as Z score.

Results presented at Table 14 suggest that managers indeed cut leverage following lender mergers. In the dynamic analysis, the results support that leverage starts to decline only after the treatment. The coefficients are close to zero and have minimal statistical significance in the years before treatment. Column (1) indicates that managers on average cut leverage by 1.6% of total assets in the three years following a lender merger. The unconditional sample mean leverage is 0.31, implying a 5% decrease in debt financing. Note that in the sample of less bank dependent firms (column (3) to (4)), i.e. firms with S&P credit ratings, the treatment effect is

more significant both statistically and economically. Cross-sectional analyses have indicated that such firms are likely to be affected the most by lender mergers.

5 Conclusion

In this paper, I use bank mergers as a source of exogenous increase in firms' lead lender shares. Increases lead lender shares lead to stronger lender monitoring incentives (Sufi, 2007) and bargaining power (Bolton and Scharfstein, 1996; Bris and Welch, 2005). I find that lender mergers reduce the affected borrowers' public takeover activities, with the effect driven by mergers involving a lead lender. Additional analyses suggest that the effect becomes stronger for firms subject to more managerial risk-taking and less bank scrutiny, as well as when creditors are less coordinated. I further find that such intensified lender monitoring reduces not only bad public takeovers but also those that are value-enhancing to shareholders. Deals that do get announced under such heightened creditor pressure target cash rich firms with low cash flow volatility, while creating no additional shareholder value.

These findings are consistent with the main conjecture of this paper, which is that an increase in lead lender share leads to stronger incentives and bargaining power for lender monitoring, forming a disciplinary effect on borrower managers. With intensified lender monitoring, borrower managers are under more scrutiny and thus are less inclined to take risky actions such as pursuing public takeovers. Managers tend to also take risk-reducing actions that are appealing to creditors. These results show additional evidence of how creditors impact managerial decisions, beyond channels through contractual specifications such as loan covenants (Chava and Roberts, 2008; Nini et al., 2009, 2012; Ferreira et al., 2018; Becher et al., 2020). This paper further adds to the findings of Acharya et al. (2011) by showing another channel through which creditors influence firm acquisition decisions and the consequences of this influence. Creditor influence in corporate governance is able to better discipline managers, yet it does not always benefit shareholders.

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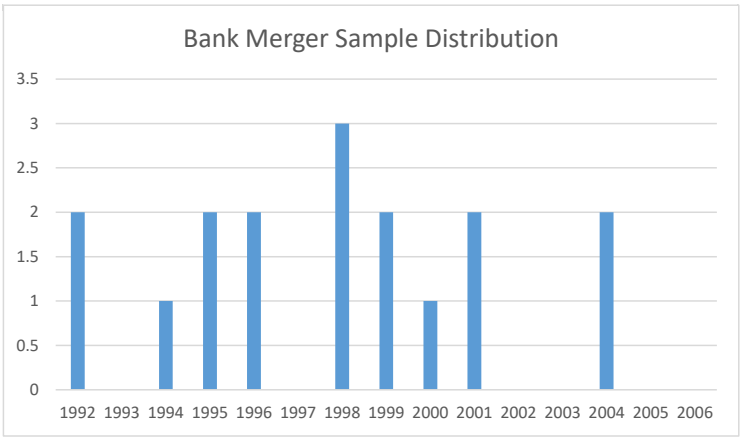
6 Figures

Figure 1. Bank merger distribution and treated firm weight in bank portfolio.

(A) Figure 1.1. This figure shows the distribution of the 17 sample bank mergers used in the analysis over the period of 1992 to 2004. Details of the mergers can be referred to Appendix Table A.1. The full bank merger sample includes 24 mergers from 1992 to 2006. The sample used for baseline analyses is restricted to bank mergers involving at least one lead arranger of the treated firm.



(B) Figure 1.2. This figure shows the treated firms in each bank merger (No.1 to 17) as a percentage of the total borrower firms of the two merging banks. Details of the mergers can be referred to Appendix Table A.1.

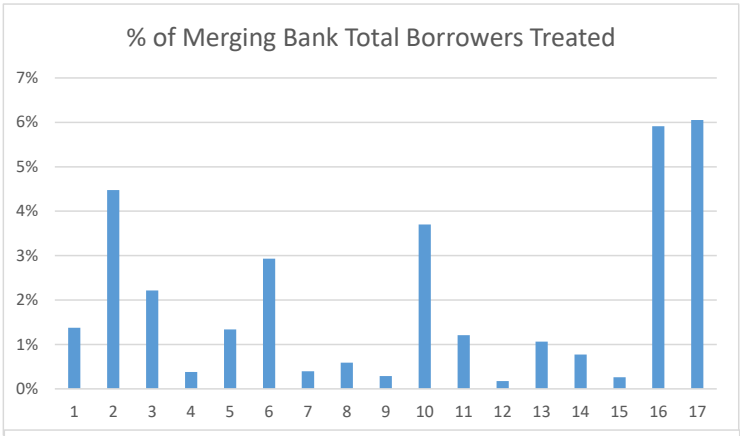
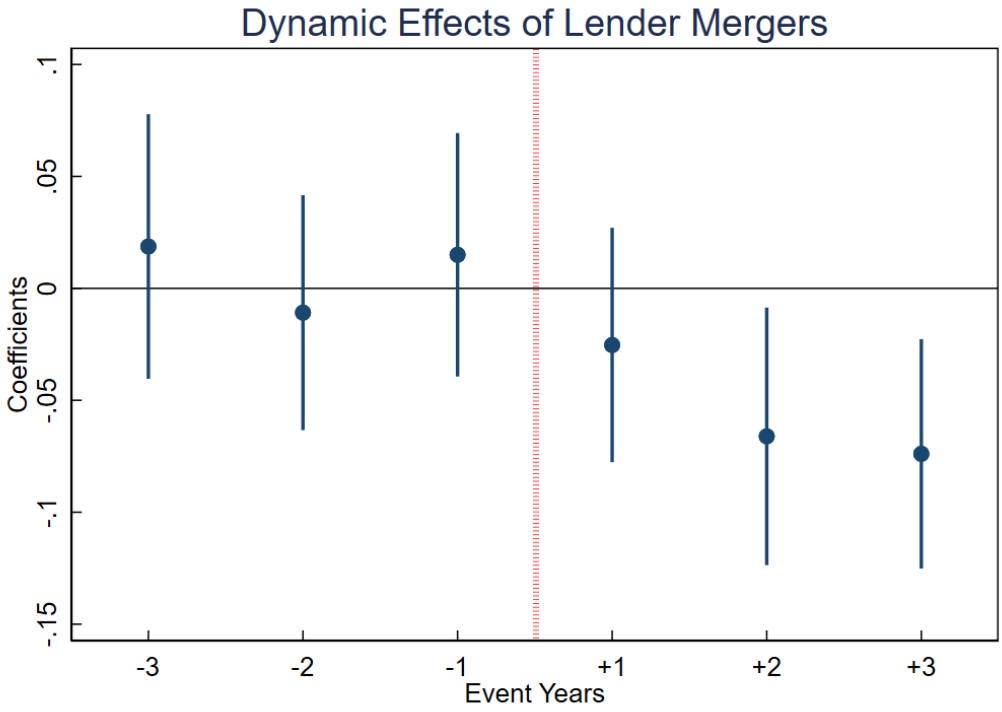


Figure 2. Public takeover activity surrounding lender mergers.

This figure plots the coefficients of Table 5 based on Equation 2. It shows the dynamic treatment effect of lender mergers before and after the merger year. A firm is considered treated if at two of its existing lenders merge, with at least one of them being involved with the firm with lead arranger credit. Control firms are borrower firms of either but not both of the merging banks, matched to the treated firms based on industry, size, market-to-book, and three-year average acquisition expenditures. The event year (merger year) is discarded in the analysis for each bank merger.



7 Tables

Table 1. Summary Statistics.
This table provides summary statistics of the variables used in the analyses. Detailed variable definitions can be referred to Appendix A.2

Panel A: Firm Level Variables						
Variables	N	Mean	SD	5th pct	Median	95th pct
Acquisition dummy (public takeover)	6,732	0.12	0.32	0.00	0.00	1.00
Acquisition activity (public takeover)	6,732	0.09	0.26	0.00	0.00	0.69
Acquisition dummy (private takeover)	6,732	0.30	0.46	0.00	0.00	1.00
Acquisition activity (private takeover)	6,732	0.29	0.50	0.00	0.00	1.39
Market-to-book	6,720	1.37	0.79	0.58	1.16	2.97
Cash	6,732	0.06	0.08	0.00	0.03	0.23
Leverage	6,732	0.31	0.18	0.03	0.30	0.64
Size	6,732	7.83	1.44	5.62	7.71	10.24
ROA	6,732	0.14	0.07	0.05	0.14	0.27
Stock Return	6,689	0.16	0.45	-0.45	0.10	0.95

Panel B: Acquisition Deal Level Variables						
Variables	N	Mean	SD	25th pct	Median	75th pct
Acquirer CAR (%)	522	-0.79	6.50	-4.16	-0.75	2.60
Synergies (%)	522	1.86	6.28	-1.49	1.39	5.13
Target Cash Holdings	522	16.84	2.57	15.63	17.05	18.44
Target Cashflow Volatility	443	0.036	0.048	0.008	0.016	0.041
Relative Size	516	0.17	0.31	0.01	0.05	0.17
Diversifying	522	0.48	0.50	0.00	0.00	1.00
Competed	522	0.07	0.25	0.00	0.00	0.00
All cash	522	0.31	0.46	0.00	0.00	1.00
All stock	522	0.23	0.42	0.00	0.00	0.00
Tender offer	522	0.24	0.43	0.00	0.00	0.00
Hostile	522	0.02	0.16	0.00	0.00	0.00

Table 2. Lender Mergers and the Propensity to Pursue Acquisitions.

This table presents the baseline difference-in-difference (DiD) regression of acquisition dummy (acquisition activity) using Equation 1, in Section 4.1. In the public (private) analysis acquisition dummy (acquisition activity) is computed with only acquisitions on public (private) targets. All control variables are lagged one year. Detailed variable definition can be referred to Appendix A.2. All non-log control variables are winsorized at the 1% and 99% level. Industry fixed effects are at the 2-digit SIC level. Standard errors are clustered at the firm level. T-statistics are displayed in parentheses. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	Public Takeovers				Private Takeovers			
	Acquisition Dummy (1)	Acquisition Dummy (2)	Acquisition Dummy (3)	Acquisition Activity (4)	Acquisition Dummy (5)	Acquisition Dummy (6)	Acquisition Dummy (7)	Acquisition Activity (8)
Treat×Post	-0.059***	-0.059***	-0.062***	-0.047***	-0.024	-0.025	-0.024	-0.030
	(-3.781)	(-3.787)	(-3.923)	(-3.662)	(-0.989)	(-1.035)	(-0.999)	(-1.197)
Treat	0.009				0.016			
	(0.693)				(0.804)			
Post	0.008	0.006	0.017	0.027	0.045	0.038	0.052	0.052
	(0.275)	(0.203)	(0.547)	(1.091)	(1.301)	(1.074)	(1.476)	(1.425)
Market-to-book			0.023	0.016			0.014	0.021
			(1.519)	(1.427)			(0.744)	(0.933)
Cash			0.262**	0.225**			0.191	0.242
			(2.295)	(2.504)			(1.150)	(1.372)
Leverage			-0.166***	-0.114***			-0.279***	-0.343***
			(-3.106)	(-2.641)			(-3.673)	(-4.049)
Size			-0.026	-0.015			0.040*	0.056**
			(-1.215)	(-0.619)			(1.946)	(2.284)
ROA			0.299**	0.235*			0.472**	0.445**
			(2.022)	(1.942)			(2.566)	(2.323)
Stock Return			0.036***	0.038***			0.064***	0.065***
			(2.710)	(3.176)			(3.584)	(3.817)
Constant	0.121***	0.125***	0.285	0.163	0.277***	0.285***	-0.057	-0.175
	(7.817)	(7.923)	(1.562)	(0.839)	(15.56)	(16.06)	(-0.321)	(-0.852)
N	6,634	6,633	6,586	6,586	6,634	6,633	6,586	6,586
Firm FE	Yes	No	No	No	Yes	No	No	No
Merger FE	Yes	No	No	No	Yes	No	No	No
Firm×Merger FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adjusted R-squared	0.181	0.162	0.177	0.227	0.276	0.270	0.279	0.358

Table 3. Lender Mergers and the Propensity to Pursue Acquisitions - All Lender Mergers and Lender Mergers Involving Only Loan Participants.

This table presents of the analysis in Table 2 with alternative bank merger samples. In Panel A I use all bank mergers available instead of only mergers involving a lead arranger. In Panel B I use mergers involving only loan participant lenders as treatments. The same list of controls from Table 2 are included. In the public (private) analysis acquisition dummy (acquisition activity) is computed with only acquisitions on public (private) targets. All control variables are lagged one year. Detailed variable definition can be referred to Appendix A.2. All non-log control variables are winsorized at the 1% and 99% level. Industry fixed effects are at the 2-digit SIC level. Standard errors are clustered at the firm level. T-statistics are displayed in parentheses. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: All Bank Mergers

	Public Takeovers				Private Takeovers			
	Acquisition Dummy (1)	Acquisition Dummy (2)	Acquisition Dummy (3)	Acquisition Activity (4)	Acquisition Dummy (5)	Acquisition Dummy (6)	Acquisition Dummy (7)	Acquisition Activity (8)
Treat×Post	-0.011	-0.011	-0.019*	-0.013	-0.013	-0.014	-0.019	-0.023
	(-0.968)	(-0.966)	(-1.696)	(-1.492)	(-0.784)	(-0.812)	(-1.120)	(-1.250)
Treat	0.005				0.001			
	(0.533)				(0.062)			
Post	-0.010	-0.010	-0.009	0.000	-0.019	-0.026	-0.025	-0.008
	(-0.598)	(-0.540)	(-0.527)	(0.027)	(-0.903)	(-1.200)	(-1.180)	(-0.366)
Controls	No	No	Yes	Yes	No	No	Yes	Yes
N	12,222	12,222	12,158	12,158	12,222	12,222	12,158	12,158
Firm FE	Yes	No	No	No	Yes	No	No	No
Merger FE	Yes	No	No	No	Yes	No	No	No
Firm×Merger FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.206	0.174	0.189	0.234	0.299	0.281	0.287	0.364

Panel B: Bank Mergers Involving Only Loan Participants

	Public Takeovers				Private Takeovers			
	Acquisition Dummy (1)	Acquisition Dummy (2)	Acquisition Dummy (3)	Acquisition Activity (4)	Acquisition Dummy (5)	Acquisition Dummy (6)	Acquisition Dummy (7)	Acquisition Activity (8)
Treat×Post	0.016	0.015	0.005	0.003	0.007	0.005	-0.003	-0.012
	(1.208)	(1.190)	(0.367)	(0.267)	(0.358)	(0.298)	(-0.164)	(-0.581)
Treat	-0.002				-0.017			
	(-0.173)				(-1.207)			
Post	-0.024	-0.023	-0.023	-0.009	-0.042*	-0.051**	-0.052**	-0.027
	(-1.241)	(-1.166)	(-1.206)	(-0.619)	(-1.754)	(-2.099)	(-2.148)	(-1.069)
Controls	No	No	Yes	Yes	No	No	Yes	Yes
N	10,103	10,103	10,045	10,045	10,103	10,103	10,045	10,045
Firm FE	Yes	No	No	No	Yes	No	No	No
Merger FE	Yes	No	No	No	Yes	No	No	No
Firm×Merger FE	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.213	0.180	0.194	0.243	0.299	0.280	0.286	0.365

Table 4. Continuous Treatment - Conditional Sample.

This table presents of the DiD analysis on public takeover activities using a conditional sample. I identify treated firms based on lender mergers through loans with non-missing lender share value in DealScan. Columns (1) to (3) repeat the analysis in Table 2. Columns (4) to (6) use a continuous treatment variable, Δ Lead Share, instead of a discrete treatment variable. Δ Lead Share is the percentage increase in the lead lender's loan investment in the firm after the lender merger. The same list of control variables from Table 2 are included. All control variables are lagged one year. Detailed variable definition can be referred to Appendix A.2. All non-log control variables are winsorized at the 1% and 99% level. Industry fixed effects are at the 2-digit SIC level. Standard errors are clustered at the firm level. T-statistics are displayed in parentheses. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	Public Takeover					
	Acquisition Dummy (1)	Acquisition Dummy (2)	Acquisition Activity (3)	Acquisition Dummy (4)	Acquisition Dummy (5)	Acquisition Activity (6)
Treat×Post	-0.059***	-0.061***	-0.055***			
	(-2.587)	(-2.677)	(-3.053)			
Δ Lead Share				-0.080**	-0.082**	-0.074***
				(-2.326)	(-2.411)	(-2.822)
N	3,645	3,625	3,625	3,645	3,625	3,625
Controls	No	Yes	Yes	No	Yes	Yes
Firm×Merger FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.152	0.167	0.225	0.152	0.167	0.225

Table 5. Dynamic Treatment Effects of Lender Mergers.

This table presents the dynamic difference-in-difference (DiD) regression of acquisition dummy (acquisition activity) using Equation 2. Results for year dummies are not reported for brevity. The same list of controls from Table 2 are included. All control variables are lagged one year. Detailed variable definition can be referred to Appendix A.2. All non-log control variables are winsorized at the 1% and 99% level. Industry fixed effects are at the 2-digit SIC level. Standard errors are clustered at the firm level. T-statistics are displayed in parentheses. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	Public Takeovers	
	Acquisition Dummy (1)	Acquisition Activity (2)
Treat×Year-3	0.019	0.017
	(0.621)	(0.684)
Treat×Year-2	-0.011	-0.004
	(-0.406)	(-0.180)
Treat×Year-1	0.015	0.022
	(0.541)	(1.068)
Treat×Year+1	-0.025	-0.011
	(-0.949)	(-0.524)
Treat×Year+2	-0.066**	-0.050**
	(-2.257)	(-2.170)
Treat×Year+3	-0.074***	-0.045**
	(-2.832)	(-2.140)
Controls	Yes	Yes
N	7,700	7,700
Firm×Merger FE	Yes	Yes
Industry×Year FE	Yes	Yes
Adj. R-squared	0.194	0.235

Table 6. Robustness Check - Falsification Test and Confounding Factors.

Panel A of this table presents a falsification test of the baseline DiD analysis on public takeovers. The test is based on the same treated and control firms using fictional mergers which occur four years before the actual merger event years. Panel B repeat the baseline DiD analysis on public takeovers controlling for different ownership measures. *NumBlockholders* is the logarithm of one plus number of blockholders in the firm at the fiscal year end. *Top 5 Ownership Concentration* is the sum of ownership percentage held by the firm's top 5 institutional shareholders at the fiscal year end. *Number of Dualholders* is the number of dual holders in the firm at the fiscal year end. Common ownership is calculated following Harford et al. (2011) as described in Section 4.4. The same list of control variables from Table 2 are included. All control variables are lagged one year. Detailed variable definition can be referred to Appendix A.2. All non-log control variables are winsorized at the 1% and 99% level. Industry fixed effects are at the 2-digit SIC level. Standard errors are clustered at the firm level. T-statistics are displayed in parentheses. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: Falsification Test Using Fictional Merger Events

	Public Takeovers			
	Fictional Mergers in Year T-4 with Same Treated and Control Firms			
	Acquisition Dummy (1)	Acquisition Dummy (2)	Acquisition Dummy (3)	Acquisition Activity (4)
Treat×Post	0.000	-0.001	-0.000	-0.007
	(0.019)	(-0.061)	(-0.003)	(-0.438)
N	6,405	6,404	6,081	6,081
Controls	No	No	Yes	Yes
Firm FE	Yes	No	No	No
Merger FE	Yes	No	No	No
Firm×Merger FE	No	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.158	0.136	0.142	0.156

Panel B: Controlling for Institutional Investor Governance, Block Ownership, Dual Ownership, and Common Ownership in Baseline DiD

	Acquisition Dummy (Public Takeovers)			
	(1)	(2)	(3)	(4)
Treat×Post	-0.061***	-0.062***	-0.062***	-0.062***
	(-3.888)	(-3.910)	(-3.952)	(-3.911)
Number of Blockholders	-0.006			
	(-0.325)			
Number of Dualholders		0.006		
		(0.659)		
Top 5 Ownership Concentration			0.190**	
			(2.433)	
Common Ownership				0.182
				(0.482)
Institutional Ownership	0.057	0.046	0.014	0.031
	(1.103)	(1.012)	(0.280)	(0.533)
N	6,586	6,586	6,586	6,586
Controls	Yes	Yes	Yes	Yes
Firm×Merger FE	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.177	0.177	0.179	0.177

Panel C: Impact of Baseline DiD Events on Number of Blockholders, Dualholders, Ownership Concentration, and Common Ownership

	Number of Blockholders (1)	Top 5 Ownership (2)	Number of Dualholders (3)	Common Ownership (4)
Treat×Post	0.006	-0.001	-0.069	-0.000
	(0.273)	(-0.168)	(-0.251)	(-0.145)
N	6,586	6,586	6,586	6,586
Controls	Yes	Yes	Yes	Yes
Firm×Merger FE	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.743	0.698	0.821	0.930

Table 7. Robustness Check - Alternative Windows and Alternative Bank Merger Samples.

Panel A of this table presents the baseline difference-in-difference (DiD) regression of acquisition dummy (acquisition activity) using Equation 1 for various event windows, in Section 4.4. Panel B shows results of the DiD regression based on alternative bank merger samples. The three largest bank mergers in the sample in terms of affected firms are NationsBank/BankAmerica, Bank of America/FleetBoston, and JP Morgan Chase/Bank One. They are excluded in the analysis for column (1) and (2) of Panel B. Column (3) and (4) show results of analysis using only bank mergers occurring during 1995 to 2002, a period corresponding to two major banking deregulation events, the Riegle-Neal Interstate Banking and Branching Efficiency Act of 1994 and the Gramm-Leah-Bliley Act of 1999. The same list of control variables from Table 2 are included. All control variables are lagged one year. Detailed variable definition can be referred to Appendix A.2. All non-log and non-dummy control variables are winsorized at the 1% and 99% level. Industry fixed effects are at the 2-digit SIC level. Standard errors are clustered at the firm level. T-statistics are displayed in parentheses. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: Alternative Event Windows

	Public Takeover			
	(-2, +2)		(-4, +4)	
	Acquisition Dummy (1)	Acquisition Activity (2)	Acquisition Dummy (3)	Acquisition Activity (4)
Treat×Post	-0.041**	-0.034**	-0.046***	-0.035***
	(-2.231)	(-2.349)	(-3.265)	(-3.190)
N	4,987	4,987	9,603	9,603
Controls	Yes	Yes	Yes	Yes
Firm×Merger FE	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.191	0.226	0.160	0.197

Panel B: Robustness of Bank Merger Sample

	Public Takeover			
	Exclude 3 Largest Bank Mergers		Using Only 1995-2002 Bank Mergers	
	Acquisition Dummy (1)	Acquisition Activity (2)	Acquisition Dummy (3)	Acquisition Activity (4)
Treat×Post	-0.086***	-0.067***	-0.071**	-0.053**
	(-2.730)	(-2.630)	(-2.518)	(-2.311)
N	2,428	2,428	3,067	3,067
Controls	Yes	Yes	Yes	Yes
Firm×Merger FE	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.138	0.206	0.166	0.221

Table 8. Alternative Explanation: Effect on the Access to Debt Financing - Loan Terms of Contracts Originated Before and After Lender Mergers.

This table presents the loan facility level difference-in-difference (DiD) regression of loan terms on lender mergers. Loan spread is the logarithm of the all-in-drawn spread. Loan size is the logarithm of the facility amount. Loan maturity is the logarithm of the facility maturity. Columns (1) to (6) use the sample of only loans involving the merged lenders originated during the (-3, +3) period around the lender mergers. Columns (7) to (12) use the sample of all loans originated to the sample firms during the DiD period. Secured is a dummy variable that equals one if the loan facility is secured. Performance pricing is a dummy variable that equals one if the loan facility contains a performance pricing clause. All firm control variables are lagged one year. Detailed variable definition can be referred to Appendix A.2. All non-log control variables are winsorized at the 1% and 99% level. Industry fixed effects are at the 2-digit SIC level. Standard errors are clustered at the firm level. T-statistics are displayed in parentheses. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	Loans Involving the Merged Lender						All Loans					
	Loan Spread		Loan Size		Loan Maturity		Loan Spread		Loan Size		Loan Maturity	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Treat×Post	-0.072*	-0.068*	0.001	-0.002	0.080***	0.086***	-0.020	-0.017	-0.010	-0.006	0.070**	0.071**
	(-1.875)	(-1.871)	(0.014)	(-0.041)	(2.793)	(3.082)	(-0.533)	(-0.474)	(-0.153)	(-0.112)	(2.475)	(2.561)
Post	0.056	0.049	0.188*	0.191*	0.040	0.050	0.086	0.087	0.222*	0.217**	0.023	0.032
	(0.803)	(0.737)	(1.751)	(1.905)	(0.766)	(0.986)	(1.344)	(1.476)	(1.957)	(1.998)	(0.430)	(0.623)
Loan Size	-0.135***	-0.130***			0.063***	0.066***	-0.187***	-0.185***			0.026*	0.029**
	(-6.032)	(-5.578)			(4.060)	(4.180)	(-10.38)	(-10.03)			(1.816)	(2.045)
Loan Maturity	0.007	0.012	0.212***	0.217***			0.045**	0.054***	0.102*	0.114**		
	(0.318)	(0.561)	(3.794)	(3.901)			(2.370)	(2.786)	(1.781)	(2.004)		
Loan Spread			-0.588***	-0.586***	0.009	0.017			-0.858***	-0.875***	0.053**	0.065***
			(-8.502)	(-7.920)	(0.319)	(0.562)			(-12.40)	(-12.08)	(2.431)	(2.884)
Secured	0.390***	0.363***	0.041	0.046	0.018	0.020	0.410***	0.385***	0.023	0.029	0.067	0.066
	(7.810)	(7.111)	(0.401)	(0.450)	(0.454)	(0.509)	(10.06)	(9.261)	(0.218)	(0.276)	(1.556)	(1.523)
Performance Pricing	-0.092***	-0.089***	0.184***	0.205***	0.053**	0.050*	-0.108***	-0.103***	0.309***	0.320***	0.054**	0.055**
	(-3.740)	(-3.955)	(3.019)	(3.486)	(2.051)	(1.951)	(-4.348)	(-4.264)	(5.935)	(6.418)	(2.452)	(2.524)
Market-to-book		-0.092***		-0.013		0.037		-0.072***		-0.076		0.068**
		(-3.611)		(-0.296)		(1.369)		(-2.733)		(-1.514)		(2.428)
Cash		0.224		0.230		-0.009		0.070		0.072		0.204
		(0.951)		(0.509)		(-0.034)		(0.310)		(0.150)		(0.766)
Leverage		0.528***		0.353		-0.011		0.541***		0.429*		-0.132
		(2.911)		(1.468)		(-0.092)		(3.851)		(1.880)		(-1.284)
Size		-0.059		0.416***		-0.018		0.010		0.308***		0.009
		(-1.248)		(5.804)		(-0.455)		(0.227)		(3.661)		(0.234)
ROA		-0.919***		1.524***		0.081		-1.038***		0.864		0.007
		(-2.801)		(2.874)		(0.294)		(-3.151)		(1.366)		(0.027)
Stock Return		-0.044		0.080		-0.001		-0.054*		0.057		0.003
		(-1.508)		(1.647)		(-0.032)		(-1.949)		(1.180)		(0.097)
Constant	6.951***	7.410***	21.34***	17.56***	2.274***	2.272***	7.872***	7.796***	22.72***	20.07***	2.817***	2.567***
	(16.14)	(15.60)	(62.33)	(21.90)	(6.187)	(5.006)	(24.10)	(17.87)	(63.21)	(22.94)	(8.645)	(5.883)
N	4,746	4,690	4,746	4,690	4,746	4,690	6,521	6,451	6,521	6,451	6,521	6,451
Firm×Merger FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan Purpose FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Loan Type FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
S&P Rating FE	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Adj. R-squared	0.863	0.869	0.664	0.671	0.751	0.751	0.819	0.826	0.599	0.604	0.694	0.695

Table 9. Alternative Explanation: Treatment Effect on Firms in Covenant Violation.

This table presents the baseline difference-in-difference (DiD) regression of acquisition dummy (acquisition activity) using Equation 1, with cross-sectional variation based on covenant violation. Covenant violation data is obtained from Amir Sufi's website. Violation is a dummy variable that equals one if the firm had a covenant violation during the previous year. The same list of control variables from Table 2 are included. All continuous control variables are lagged one year. Detailed variable definition can be referred to Appendix A.2. All non-log control variables are winsorized at the 1% and 99% level. Industry fixed effects are at the 2-digit SIC level. Standard errors are clustered at the firm level. T-statistics are displayed in parentheses. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	Public Takeover					
	Acquisition Dummy			Acquisition Activity		
	(1)	(2)	(3)	(4)	(5)	(6)
Treat×Post	-0.058***	-0.058***	-0.057***	-0.042***	-0.042***	-0.042***
	(-3.184)	(-3.207)	(-3.136)	(-2.947)	(-2.968)	(-2.901)
Treat×Post×Violation			-0.029			-0.017
			(-0.232)			(-0.187)
Violation		-0.052	-0.003		-0.041	-0.007
		(-1.451)	(-0.031)		(-1.641)	(-0.119)
Treat×Violation			-0.018			-0.009
			(-0.191)			(-0.133)
Post×Violation			-0.067			-0.050
			(-0.692)			(-0.731)
Post	-0.085	-0.083	-0.083	-0.043	-0.042	-0.041
	(-0.677)	(-0.661)	(-0.657)	(-0.494)	(-0.477)	(-0.471)
N	3,970	3,970	3,970	3,970	3,970	3,970
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm×Merger FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.178	0.178	0.177	0.223	0.223	0.222

Table 10. Bank Dependency and Managerial Risk-Taking.

This table presents the baseline difference-in-difference (DiD) regression of acquisition dummy (acquisition activity) using Equation 1, for subsamples split by different cross-sectional variables. In Panel A, columns (1) and (2) split the sample based on whether the firm had an S&P credit rating in the year before the treatment year. Columns (3) and (4) split the sample based on whether the firm's debt HHI was above or below sample median in the year before the treatment year. Columns (5) and (6) split the sample based on whether the firm had one debt type greater than 90% of its total debt in the year before the treatment year. In Panel B, columns (1) and (2) split the sample based on whether the firm had any R&D expenses in the year before the treatment year. Columns (3) and (4) split the sample based on whether the firm's cash flow volatility was above or below sample median in the year before the treatment year. Columns (5) and (6) split the sample based on whether the firm's overinvest score in the previous year is above or below sample median. The same list of controls from Table 2 are included. Detailed variable definition can be referred to Appendix A.2. All non-log continuous control variables are winsorized at the 1% and 99% level. Industry fixed effects are at the 2-digit SIC level. Standard errors are clustered at the firm level. T-statistics are displayed in parentheses. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

Panel A: Bank Dependency

	Acquisition Dummy (Public Takeover)					
	S&P Rating		Debt HHI		One Debt Type >90% of Total	
	Yes	No	High	Low	Yes	No
	(1)	(2)	(3)	(4)	(5)	(6)
Treat×Post	-0.079***	-0.004	-0.020	-0.090***	-0.012	-0.074***
	(-4.153)	(-0.143)	(-0.826)	(-4.122)	(-0.369)	(-4.112)
N	4,642	1,789	3,133	3,111	1,750	4,714
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm×Merger FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.194	0.184	0.164	0.205	0.098	0.213
P-Value of Difference	0.049		0.053		0.135	

Panel B: Managerial Risk-Taking

	Acquisition Dummy (Public Takeover)					
	R&D		Cash Flow Volatility		Overivnest Tendency	
	Yes	No	High	Low	High	Low
	(1)	(2)	(3)	(4)	(5)	(6)
Treat×Post	-0.085***	-0.040*	-0.071***	-0.034	-0.074**	-0.035
	(-3.303)	(-1.911)	(-2.924)	(-1.575)	(-2.285)	(-1.549)
N	3,096	3,379	3,175	3,156	2,434	3,695
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm×Merger FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.197	0.158	0.190	0.183	0.158	0.213
P-Value of Difference	0.228		0.227		0.157	

Table 11. Value-Destroying and Value-Enhancing Deals.

This table presents the baseline difference-in-difference (DiD) regression of acquisition dummy (acquisition activity) using Equation 1, for public takeover deals with positive acquirer CAR (value-enhancing) and negative acquirer CAR (value-destroying), respectively. The same list of controls from Table 2 are included. All control variables are lagged one year. All control variables are lagged one year. Detailed variable definition can be referred to Appendix A.2. All non-log control variables are winsorized at the 1% and 99% level. Industry fixed effects are at the 2-digit SIC level. Standard errors are clustered at the firm level. T-statistics are displayed in parentheses. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	Value-Destroying Deals			Value-Enhancing Deals		
	Acquisition Dummy (1)	Acquisition Dummy (2)	Acquisition Activity (3)	Acquisition Dummy (4)	Acquisition Dummy (5)	Acquisition Activity (6)
Treat×Post	-0.033***	-0.033***	-0.021**	-0.032***	-0.036***	-0.028***
	(-2.659)	(-2.661)	(-2.172)	(-2.884)	(-3.149)	(-3.301)
Post	-0.007	-0.002	0.001	0.027	0.034	0.029
	(-0.268)	(-0.070)	(0.068)	(1.066)	(1.314)	(1.530)
N	6,633	6,586	6,586	6,633	6,586	6,586
Controls	No	Yes	Yes	No	Yes	Yes
Firm×Merger FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.099	0.107	0.126	0.115	0.124	0.135

Table 12. Public Takeovers Following Lender Mergers - Target Characteristics.

This table presents the difference-in-difference (DiD) regression of target characteristics for public takeover deals with U.S. targets following lender mergers, using Equation 3, in Section 4.7. All firm control variables are lagged one year. Detailed variable definition can be referred to Appendix A.2. All non-log and non-dummy continuous variables are winsorized at the 1% and 99% level. Industry fixed effects are at the 2-digit SIC level. Standard errors are clustered at the firm level. T-statistics are displayed in parentheses. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	Target Cash Holdings				Target Cash Flow Volatility			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Treat×Post	1.145***	0.890**	0.926**	0.917**	-0.025*	-0.029*	-0.028*	-0.041*
	(2.644)	(2.233)	(2.277)	(2.062)	(-1.786)	(-1.879)	(-1.800)	(-1.713)
Treat	-0.257	-0.364	-0.404		0.005	0.005	0.004	
	(-0.598)	(-0.943)	(-1.078)		(0.499)	(0.506)	(0.357)	
Post	0.286	0.398	0.306	-0.300	0.013	0.023	0.023	0.003
	(0.403)	(0.576)	(0.433)	(-0.302)	(0.734)	(1.225)	(1.203)	(0.155)
All Stock		1.119***	1.168***	1.219		0.015	0.016	-0.011
		(2.949)	(2.878)	(1.581)		(1.314)	(1.356)	(-0.506)
All Cash		0.032	-0.033	-0.072		0.007	0.007	0.012
		(0.108)	(-0.114)	(-0.155)		(0.730)	(0.705)	(0.864)
Competing		1.422***	1.482***	2.139***		0.048	0.049	0.067
		(2.847)	(2.914)	(2.647)		(1.477)	(1.541)	(1.564)
Diversifying		-0.891**	-0.813**	-0.412		0.017*	0.019**	0.019
		(-2.392)	(-2.152)	(-0.959)		(1.825)	(2.051)	(1.347)
Tender Offer		-0.554	-0.364	-0.914		-0.006	-0.008	-0.075**
		(-1.357)	(-0.852)	(-1.164)		(-0.565)	(-0.706)	(-2.573)
Hostile		1.297	1.115	1.225		-0.005	-0.006	-0.000
		(1.320)	(1.118)	(0.634)		(-0.318)	(-0.342)	(-0.007)
ROA			-2.281	4.957			-0.026	-0.056
			(-0.823)	(0.622)			(-0.314)	(-0.370)
Market-to-Book			0.129	0.009			-0.004	-0.019
			(0.738)	(0.022)			(-0.735)	(-1.053)
Leverage			0.001	1.446			-0.050	-0.113
			(0.001)	(0.519)			(-1.439)	(-1.425)
Market Capitalization			0.536**	0.268			0.002	-0.003
			(2.299)	(0.390)			(0.454)	(-0.174)
Annual Stock Return			-0.220	0.056			-0.005	0.003
			(-0.747)	(0.099)			(-0.589)	(0.223)
Constant	16.71***	16.88***	12.10***	13.19*	0.034***	0.015	0.019	0.157
	(43.62)	(42.56)	(5.812)	(1.871)	(3.331)	(1.194)	(0.410)	(0.890)
N	454	448	446	293	372	367	364	230
Industry×Merger FE	Yes	Yes	Yes	No	Yes	Yes	Yes	No
Firm×Merger FE	No	No	No	Yes	No	No	No	Yes
Announcement Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Relative Size Decile Dummies	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Adj. R-squared	0.143	0.246	0.299	0.289	0.075	0.111	0.112	0.103

Table 13. Public Takeovers Following Lender Mergers - Shareholder Value Creation.

This table presents the difference-in-difference (DiD) regression of deal value creation for public takeover deals with U.S. targets following lender mergers, using Equation 3, in Section 4.7. Following Harford et al. (2011), CAR(-1,+1) is calculated using the market model with an estimation window of (-200, -60) prior to the announcement date. Synergies(%) is CAR (-1,+1) of the value weighted porfolio of the acquirer and target, with target adjusted for toehold. All firm control variables are lagged one year. Detailed variable definition can be referred to Appendix A.2. All non-log and non-dummy continuous variables are winsorized at the 1% and 99% level. Industry fixed effects are at the 2-digit SIC level. Standard errors are clustered at the firm level. T-statistics are displayed in parentheses. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	Acquirer CAR (-1, +1)					Synergies				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Treat×Post	0.007	0.005	0.004	0.005	-0.015	0.005	0.000	0.001	0.002	-0.017
	(0.510)	(0.330)	(0.322)	(0.398)	(-0.758)	(0.384)	(0.017)	(0.050)	(0.119)	(-0.998)
Treat	-0.003	0.001	0.000	-0.003		-0.002	0.005	0.005	0.004	
	(-0.297)	(0.116)	(0.024)	(-0.279)		(-0.185)	(0.474)	(0.463)	(0.350)	
Post	-0.011	-0.008	-0.009	-0.004	-0.018	-0.004	0.000	-0.000	0.003	-0.020
	(-0.583)	(-0.462)	(-0.536)	(-0.255)	(-0.835)	(-0.250)	(0.023)	(-0.024)	(0.185)	(-0.967)
All Stock		0.022*	0.022	0.020	0.028*		0.013	0.012	0.014	0.018
		(1.753)	(1.637)	(1.459)	(1.788)		(1.081)	(0.982)	(1.119)	(1.363)
All Cash		0.021**	0.018*	0.015	0.010		0.006	0.005	0.010	-0.005
		(2.289)	(1.881)	(1.509)	(0.698)		(0.579)	(0.480)	(0.951)	(-0.356)
Competing		-0.005	-0.008	0.001	0.031		0.012	0.009	-0.000	0.025
		(-0.240)	(-0.336)	(0.024)	(1.274)		(0.503)	(0.369)	(-0.013)	(1.097)
Diversifying		0.011	0.014	0.011	0.002		0.008	0.010	0.013	0.008
		(1.224)	(1.406)	(1.082)	(0.137)		(0.838)	(0.988)	(1.292)	(0.669)
Tender Offer		0.026***	0.026***	0.022**	0.033*		0.019	0.016	0.019	0.024
		(2.762)	(2.735)	(2.163)	(1.979)		(1.646)	(1.382)	(1.537)	(1.541)
Hostile		-0.008	-0.010	-0.003	-0.046		0.007	0.007	0.002	-0.046
		(-0.401)	(-0.475)	(-0.139)	(-1.485)		(0.256)	(0.270)	(0.0911)	(-1.592)
Acquirer ROA			0.064	0.076	-0.263			0.106	0.162*	-0.101
			(0.611)	(0.688)	(-1.409)			(1.148)	(1.690)	(-0.541)
Acquirer Market-to-Book			0.000	-0.001	0.011			-0.000	-0.002	0.010
			(0.007)	(-0.145)	(0.843)			(-0.067)	(-0.394)	(0.854)
Acquirer Leverage			-0.002	0.009	-0.011			-0.008	0.005	-0.007
			(-0.071)	(0.237)	(-0.118)			(-0.264)	(0.147)	(-0.0938)
Acquirer Market Capitalization			0.001	0.005	0.009			-0.008	-0.010	-0.019
			(0.210)	(0.692)	(0.781)			(-1.417)	(-1.518)	(-1.409)
Acquirer Annual Stock Return			0.011	0.007	0.012			0.004	0.005	0.009
			(1.107)	(0.760)	(0.759)			(0.456)	(0.587)	(0.715)
Target ROA				0.034	0.042				0.004	-0.008
				(1.125)	(1.177)				(0.160)	(-0.224)
Target Market-to-Book				0.002	0.001				-0.000	-0.000
				(0.645)	(0.185)				(-0.086)	(-0.076)
Target Leverage				-0.004	0.018				-0.024	0.008
				(-0.171)	(0.566)				(-1.101)	(0.285)
Target Market Capitalization				-0.008**	-0.011***				0.005	0.003
				(-2.276)	(-2.792)				(1.592)	(0.808)
Target Annual Stock Return				0.002	0.012				0.000	0.010
				(0.199)	(1.386)				(0.025)	(1.076)
Constant	-0.005	-0.031**	-0.055	-0.044	-0.029	0.018**	0.000	0.056	0.037	0.166
	(-0.492)	(-2.450)	(-1.015)	(-0.824)	(-0.233)	(1.982)	(0.000)	(1.199)	(0.735)	(1.137)
N	454	448	446	439	287	454	448	446	439	287
Industry×Merger FE	Yes	Yes	Yes	Yes	No	Yes	Yes	Yes	Yes	No
Firm×Merger FE	No	No	No	No	Yes	No	No	No	No	Yes
Announcement Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Relative Size Decile Dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.043	0.104	0.096	0.119	0.297	0.058	0.096	0.086	0.098	0.213

Table 14. Financial Policies Following Lender Mergers.

This table presents the difference-in-difference (DiD) regression of financial policy changes in Section 4.8. Book leverage is the leverage ratio as total debt divided by total assets. Column (3) to (4) focus on the sample of firms with S&P credit ratings based on Compustat. Results for year dummies used for dynamic analyses are omitted for brevity. All firm control variables are lagged one year. Detailed variable definition can be referred to Appendix A.2. All non-log control variables are winsorized at the 1% and 99% level. Industry fixed effects are at the 2-digit SIC level. Standard errors are clustered at the firm level. T-statistics are displayed in parentheses. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	Book Leverage			
	Full Sample		Firms with S&P Ratings	
	(1)	(2)	(3)	(4)
Treat×Post	-0.016**		-0.018**	
	(-2.159)		(-2.154)	
Post	-0.010		0.002	
	(-0.956)		(0.205)	
Treat×Year-3		0.004		-0.001
		(0.460)		(-0.098)
Treat×Year-2		0.003		0.008
		(0.325)		(0.849)
Treat×Year-1		0.004		0.007
		(0.739)		(1.058)
Treat×Year+1		-0.008		-0.012*
		(-1.422)		(-1.903)
Treat×Year+2		-0.017**		-0.013*
		(-2.253)		(-1.653)
Treat×Year+3		-0.011		-0.015
		(-1.226)		(-1.639)
Market-to-Book	0.006	0.006	-0.006	-0.008
	(1.107)	(1.137)	(-0.988)	(-1.375)
Cash	-0.036	-0.038	-0.062	-0.043
	(-0.863)	(-0.966)	(-1.289)	(-0.945)
Size	-0.002	-0.002	-0.007	-0.005
	(-0.273)	(-0.235)	(-0.661)	(-0.486)
ROA	0.236***	0.210***	0.280***	0.232***
	(2.845)	(2.641)	(3.044)	(2.594)
Stock Return	-0.023***	-0.023***	-0.025***	-0.021***
	(-4.607)	(-4.818)	(-4.582)	(-4.539)
Tangibility	-0.024	-0.018	-0.057	-0.069
	(-0.480)	(-0.346)	(-0.925)	(-0.985)
Z Score	-0.059***	-0.056***	-0.060***	-0.055***
	(-6.590)	(-6.808)	(-6.882)	(-7.078)
N	6,113	7,151	4,137	4,865
Firm×Merger FE	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.771	0.785	0.810	0.819

A Appendix

Table A.1. List of Bank Mergers Used.

The original list of bank mergers used include 24 mergers from 1992 to 2006. Testing the enhanced monitoring hypothesis requires restricting the sample to only mergers involving a lead arranger. As in Schwert (2018), I focus on a similar sample requiring each lender included to have at least 50 loans or at least $10 billion in loan volume in the overall DealScan-Compustat sample (ending 2012). This list provides the final sample used with this selection standard. Detailed identification of treatment and control firms can be referred to Section 3.

Merger No	Acquirer	Target	Merger Year	Treated Firms	Control Firms
1	BankAmerica	Secutiry Pacific National Bank	1992	5	9
2	Chemical Bank	Manufacturer Hanover	1992	15	20
3	BankAmerica	Continental Bank	1994	12	26
4	Fleet	Shawmut	1995	1	1
5	First Chicago	NBD	1995	7	24
6	Chemical Bank	Chase	1996	21	54
7	NationsBank	Boatmen's National Bank	1996	3	24
8	Bank One	First Chicago	1998	5	12
9	First Union	CoreStates Bank	1998	2	12
10	NationsBank	BankAmerica	1998	55	139
11	Fleet	Bank Boston	1999	14	44
12	Deutsche Bank	Bankers Trust	1999	1	2
13	JP Morgan	Chase	2000	10	37
14	Wachovia	First Union	2001	8	46
15	US Bancorp	Firstar Bank	2001	1	2
16	Bank of America	FleetBoston	2004	106	201
17	JP Morgan	Bank One	2004	71	132

Table A.2. Variable Definitions.

Variables	Description
Treat	A dummy variable that equals one if the firm is borrowing from both merging lenders, with the loan(s) originated before and continue(s) beyond the merger year.
Post	A dummy variable that equals one if the sample year is after the corresponding merger year.
Acquisition Dummy	A dummy variable that equals one if the firm announces any acquisition during the year.
Acquisition Activity	The logarithm of one plus the number of acquisitions announced by the firm during the year.
Public Takeover	The acquisition activity is classified as a public takeover if the target is publicly listed.
Private Takeover	The acquisition activity is classified as a private takeover if the target is a private firm.
Size	The logarithm of total assets.
Leverage	The sum of debt in current liabilities and long term debt divided by total assets.
Market-to-Book	The sum of debt in current liabilities, long term debts, preferred stocks, deferred taxes, and market value, divided by total assets.
ROA	Return on assets as operating income divided by total assets.
Cash	Cash and marketable securities divided by total assets.
Stock Return	The cumulative annual return of the firm's stock.
Institutional Ownership	Percentage of shares outstanding held by institutional investors.
Overinvest Tendency	ROA and market-to-book are ranked into sample deciles. The overinvestment score is the average betwen ROA deciles scaled by ten, and one minus market-to-book deciles scaled by ten. A score of one indicates strong tendency to overinvest and a score of zero indicates low likelihood to overinvest.
Debt HHI	Calculated from Compustat: $(dclo/sum\_debt)^2 + (dlto/sum\_debt)^2 + (dd/sum\_debt)^2 + (dn/sum\_debt)^2 + (ds/sum\_debt)^2 + (dcvt/sum\_debt)^2$, where missing values for dclo, dlto, dd, dn, ds, and dcvt are replaced with zeros and sum_debt = dclo + dlto + dd + dn + ds + dcvt. (Gormley and Matsa, 2016)
Z Score	Firm distance to default measure. Z=1.2×(working capital/total assets)+1.4×(retained earnings/total assets)+3.3×(EBIT/total assets)+0.6×(shareholder equity/debt)+1.0×(sales/total assets).
Acquirer CAR	The cumulative abnormal return calculated using the market model for a (-1, +1) window around acquisition announcement, with an estimation window of (-200, -60) prior to the announcement date.
Diversifying	A dummy variable that equals one if the merging firms have different macro industry descriptions.
Competed	A dummy variable that equals one if the acquisition has a competing acquirer.
Completed	A dummy variable that equals one if the acquisition is eventually completed.
All Cash	A dummy variable that equals one if the acquisition is paid only with cash.
All Stock	A dummy variable that equals one if the acquisition is paid only with equity.
Relative Size	Deal transaction value as reported by SDC, divided by acquirer total assets in the prior year.
Target Cash Holdings	The log of one plus dollar value cash and marketable securities held by target firm at the prior fiscal year end.
Cash Flow Volatility	Standard deviation of the past eight earning changes to average total assets over the past eight quarters.

Table A.3. Alternative Measures for Public Takeover Activities.

This table presents the baseline difference-in-difference (DiD) regression of acquisition dummy (acquisition activity) using Equation 1, in Section 4.1 with alternative measures for public takeover activities. Acquisition spending is the sum of transaction value of public takeover deals announced by the firm in the given year, as reported by SDC. The measure used in columns (1) and (2) is acquisition spending scaled by total assets of the prior fiscal year end. The measure used in columns (3) and (4) is the logarithm of one plus acquisition spending. The same list of control variables from Table 2 are included. All control variables are lagged one year. Detailed variable definition can be referred to Appendix A.2. All non-log control variables are winsorized at the 1% and 99% level. Industry fixed effects are at the 2-digit SIC level. Standard errors are clustered at the firm level. T-statistics are displayed in parentheses. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	Public Takeover			
	Acquisition Spending/Total Assets		Log(1+Acquisition Spending)	
	(1)	(2)	(3)	(4)
Treat×Post	-0.016*	-0.019**	-0.304***	-0.318***
	(-1.681)	(-1.970)	(-2.902)	(-3.011)
N	6,633	6,586	6,633	6,586
Controls	No	Yes	No	Yes
Firm×Merger FE	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes
Adj. R-squared	0.140	0.173	0.185	0.204

Table A.4. Bank Mergers Through Revolver Loans and Bank Mergers in Early to Mid 1990s.

This table presents the baseline difference-in-difference (DiD) regression of acquisition dummy (acquisition activity) using Equation 1, with alternative bank merger samples. Columns (1) to (3) use an alternative bank merger sample based on revolver loans. Banks are less likely to sell their loan shares from revolver loans in the secondary market. Columns (4) to (6) use a bank merger sample based on the five bank mergers from 1992 to 1995, a period with a very low level of secondary syndicated loan market activities as documented by Irani and Meisenzahl (2017). More details about the mergers can be referred to Table A.1. The same list of control variables from Table 2 are included. All control variables are lagged one year. Detailed variable definition can be referred to Appendix A.2. All non-log continuous control variables are winsorized at the 1% and 99% level. Industry fixed effects are at the 2-digit SIC level. Standard errors are clustered at the firm level. T-statistics are displayed in parentheses. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	Public Takeover					
	Lender Mergers Through Revolver Loans			Using Only Mergers in Early to Mid 1990s		
	Acquisition Dummy (1)	Acquisition Dummy (2)	Acquisition Activity (3)	Acquisition Dummy (4)	Acquisition Dummy (5)	Acquisition Activity (6)
Treat×Post	-0.059***	-0.061***	-0.045***	-0.125**	-0.127**	-0.0989***
	(-3.625)	(-3.773)	(-3.503)	(-2.549)	(-2.603)	(-2.708)
N	6,375	6,329	6,329	681	671	671
Controls	No	Yes	Yes	Yes	Yes	Yes
Firm×Merger FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.160	0.175	0.225	0.059	0.070	0.101

Table A.5. Robustness Check - Alternative Matching Method for Control Firms.

This table presents the results of repeating Equation 1 with a different method of selecting control firms. For the pool of potential firms, I first exclude all firms borrowing from any merging banks in the bank merger year. I perform a propensity-score-matching based on the list of firm control variables in Table 2, as well as the logarithm of the number of lenders (DealScan) the firm is borrowing from in the bank merger years, and the three year average acquisition spending. The three firms with the closest propensity scores to each treated firm are selected as control firms. The same list of controls from Table 2 are included. All control variables are lagged one year. Detailed variable definition can be referred to Appendix A.2. All non-log control variables are winsorized at the 1% and 99% level. Industry fixed effects are at the 2-digit SIC level. Standard errors are clustered at the firm level. T-statistics are displayed in parentheses. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	Public Takeovers					
	Acquisition Dummy (1)	Acquisition Dummy (2)	Acquisition Dummy (3)	Acquisition Activity (4)	Acquisition Dummy (5)	Acquisition Activity (6)
Treat×Post	-0.032**	-0.032**	-0.032**	-0.024**		
	(-2.392)	(-2.348)	(-2.329)	(-2.208)		
Treat	-0.000					
	(-0.011)					
Treat×Year-3					-0.004	-0.001
					(-0.166)	(-0.063)
Treat×Year-2					-0.006	-0.006
					(-0.252)	(-0.323)
Treat×Year-1					-0.016	-0.005
					(-0.708)	(-0.265)
Treat×Year+1					-0.016	-0.009
					(-0.725)	(-0.505)
Treat×Year+2					-0.045*	-0.035*
					(-1.884)	(-1.894)
Treat×Year+3					-0.061***	-0.043**
					(-2.675)	(-2.329)
Controls	No	No	Yes	Yes	Yes	Yes
N	9,795	9,795	9,784	9,784	11,423	11,423
Firm FE	Yes	No	No	No	No	No
Merger FE	Yes	No	No	No	No	No
Firm×Merger FE	No	Yes	Yes	Yes	Yes	Yes
Industry×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Adj. R-squared	0.167	0.155	0.167	0.194	0.177	0.201

Table A.6. Public Takeovers Following Lender Mergers - Payment Method.

This table presents the difference-in-difference (DiD) regression of payment method for public takeover deals with U.S. targets following lender mergers. The same list of deal and firm controls from Table 12 are included. All firm control variables are lagged one year. In addition, offer price to target stock price premium - 1 week prior to deal announcement is included as a control. Detailed variable definition can be referred to Appendix A.2. All non-log and non-dummy continuous variables are winsorized at the 1% and 99% level. Standard errors are clustered at the firm level. T-statistics are displayed in parentheses. ***, **, and * indicate p-values of 1%, 5%, and 10%, respectively.

	% Paid with Cash (1)	All Cash Payment (2)	% Paid with Stock (3)	All Stock Payment (4)
Treat×Post	-13.69	-0.002	15.98*	0.206**
	(-1.367)	(-0.022)	(1.851)	(2.114)
Deal Characteristics	Yes	Yes	Yes	Yes
Firm Characteristics	Yes	Yes	Yes	Yes
N	266	266	266	266
Firm×Merger FE	Yes	Yes	Yes	Yes
Announcement Year FE	Yes	Yes	Yes	Yes
Relative Size Decile Dummies	Yes	Yes	Yes	Yes
Adj. R-squared	0.655	0.483	0.612	0.446