Debt renegotiation and debt overhang: Evidence from lender mergers

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Abstract

This paper studies whether debt renegotiation mitigates debt overhang and improves investment efficiency. Using mergers between lenders participated in the same syndicated loans as natural experiments that exogenously reduce the number of lenders and thus make renegotiation easier, I find that firms affected by the mergers experience more loan renegotiations and increase capital expenditure investment. I also find that the effect is stronger for firms with higher Q, suggesting improved investment efficiency. Further evidence suggests that the effect concentrates on loans without performance pricing provisions and unsecured loans, providing further support that lender mergers improves investment efficiency for firms suffering from debt overhang *ex ante*.

Keywords: Debt Overhang, Renegotiation, Syndicated Loan, Underinvestment, Investment Efficiency

JEL Code: G21, G23, G32, G34, G35

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1 Introduction

Myers (1977) argues that risky debt may cause firms to forego positive net present value investment opportunities because shareholders have to share the returns from such investment with creditors, a phenomenon known as debt overhang. Myers (1977) also recognizes that a necessary condition for debt overhang is frictions that can prevent *ex post* debt renegotiation. If shareholders and creditors can renegotiate *ex post* without much cost, shareholders can get concessions from creditors and commit to invest. As Myers (1977) puts it, renegotiation can lead to "an arrangement in which creditors accept less than the face amount of their securities in exchange for the owner's commitment to put up funds for future investment." In fact, Roberts and Sufi (2009) and Roberts (2015) find that many debt renegotiations occur outside of financial distress and bankruptcy and often lead to additional credit and reduced interest rates, which is consistent with the idea that creditors give concessions to clear the way for future investment.

The idea that debt renegotiation can resolve the debt overhang problem is formally modeled in Aivazian and Callen (1980) and Gertner and Scharfstein (1991), who show that debt renegotiation mitigates underinvestment and restores investment efficiency. More recently, Arnold and Westermann (2015) show that covenant renegotiation may resolve agency cost of debt, including debt overhang. The theory, as is well developed and easily understood, however, has never been directly tested.

One challenge to empirically test the effect of renegotiation on debt overhang is often the lack of exogenous variation in the ability to renegotiate debt contracts. On one hand, debt contracts are often designed to mitigate potential agency costs of the debt, including debt overhang (Myers 1977, Smith and Warner 1979, and Sufi 2007), and therefore the ability to renegotiate debt contracts *ex post* can be correlated with firm characteristics that also affect the potential agency cost of debt overhang *ex ante*, such as investment opportunities. As a result, examining contractual terms that may affect the easiness of debt renegotiation may not be able to identify the causal effect of debt renegotiation. On the other hand, actual events of debt renegotiations are often driven by firm characteristics and macroeconomic conditions, all of which can affect investment as well. As such, examining the impact of actual renegotiation events also may not identify the causal effect of debt renegotiation.

In this paper, I overcome the challenge by exploiting exogenous variation in the ability to renegotiate debt contracts generated by mergers between lenders of the same firms. Loan contract renegotiations, especially those involving loan amount, loan maturity, or loan interest rate, often require unanimous consent of all lenders participated in the loan (Wight, Cooke, and Gray 2009 and Nikolaev 2015). Because each single lender can hold up the renegotiation, increasing the number of lenders makes such renegotiations more difficult (Gertner and Scharfstein 1991, Berglöf and Von Thadden 1994, Bolton and Scharfstein 1996, and Bris and Welch 2005). When two lenders participated in the same loan merge, the total number of lenders decreases by one, and renegotiation becomes easier. On the other hand, bank mergers are unlikely to be motivated by factors related to the affected borrowers because each lender often lends to many borrowers but the number of affected borrowers is often much smaller. As such, mergers between lenders participated in the same syndicated loans are likely to satisfy both the relevance and the exclusion conditions. I therefore use the lender mergers as natural experiments to identify the causal effect of the ability to renegotiate loan contracts on debt overhang.

Specifically, I first identify all mergers between financial firms in SDC from 1987 to 2012; I then match the names of the acquirers and the targets of the mergers with lender names in DealScan. I identify all outstanding syndicated loans in which both the acquirer and the target participated and designate the borrowers of such loans as treated firms. To find control firms, I first require the control firms to also borrow from either the acquirer or the target of the merger (but not from both), and then require the control firms to be in the same two-digit industries and to be in the same terciles sorted on firm size, Tobin's Q, and cash flow.

I first show, using the difference-in-differences method, that the incidence of loan renegotiations increases for treated firms after the lender mergers. In particular, I show that lender mergers positively affect loan renegotiations involving changes in loan pricing, maturity, or amount, which often require unanimous lender consent, but not other types of renegotiations. The result is consistent with the argument that these lender mergers, by reducing the number of lenders, are more likely to affect renegotiations that require unanimity.

In the same difference-in-differences framework, I find that treated firms increase capital expenditure investment, relative to control firms, after the merger. The result is consistent with the argument that lender mergers decrease the number of lenders and make *ex post* loan renegotiation easier, which then facilitates the transfer of wealth from creditors to shareholders and mitigates debt overhang.

To ensure that easier loan renegotiation triggered by the lender mergers indeed causes increases in corporate investment, I further examine the impact of loan renegotiation on corporate investment using an instrumental variable approach, in which the lender mergers are used as the instrument for loan renegotiation. Consistent with the idea that lender mergers make loan renegotiations easier and hence increase corporate investment, I find that the exogenous component of loan renegotiations due to the lender mergers has a positive effect on corporate capital investment.

The positive effect of lender mergers on investment can also be driven by lenders' increased incentives to monitor, which may also lead to more efficient investment. To rule out this possibility, I exclude cases in which one of the merging lenders is the lead lender of the syndicated loan. In syndicated loans, monitoring responsibilities are often delegated to lead lenders, and therefore monitoring increases only when the lead lenders' shares in the loan increase as a result of the mergers. However, after excluding mergers involving lead lenders, I still find the same positive effect of participant lender mergers on corporate investment, suggesting that the baseline results are unlikely to be driven by increased lender monitoring.

To further examine whether the increases in investment indeed improves investment efficiency (as opposed to overinvestment), I investigate whether the positive effect of lender mergers on corporate investment is stronger for firms with higher Tobin's Q, a proxy for investment opportunities. Specifically, I partition the sample according to whether the firm's Q is above or below the sample median and find that the positive effect concentrates in firms with above-median Q's. The result suggests that lender mergers increase investment for firms who have good investment opportunities but are unable (or unwilling) to invest otherwise due to debt overhang. The result is therefore consistent with the argument that lender mergers, which make renegotiation easier, mitigate debt overhang and improve investment efficiency.

I provide two additional tests to ensure that the effect is indeed driven by the mitigation of debt overhang. In the first test, I examine whether the existence of performance pricing provisions alters the impact of lender mergers on corporate investment. Performance pricing provisions in loan contracts allow loan interest rates to change according to firm performance and credit risk. Specifically, loan interest rates decrease when firm performance improves and credit risk decreases. By design, performance pricing can alleviate the debt overhang problem because lenders receive lower returns from new investment even if such investment improves firm performance and reduces credit risk. It follows that loans with performance pricing provisions make debt overhang less of a problem, and therefore the lender mergers should have a smaller, if any, effect on corporate investment. To this end, I partition the sample into two subsamples according to whether the loans have performance pricing provisions. I indeed find that the lender mergers have no effect on investment for firms with loans containing performance pricing provisions and have a stronger effect for loans without performance pricing provisions, lending further support that the effect is driven by the mitigation of debt overhang when the loans do not have performance pricing provisions.

In the second test, I examine whether secured or unsecured loans make a difference. Values of secured loans depend largely on their collateral, which are arguably less sensitive to future investment the firms may make. As such, new investment may benefit the secured loan lenders less, which mitigates the debt overhang problem (Stulz and Johnson 1985). Because secured loans do not cause severe debt overhang *ex ante*, lender mergers should have a much smaller, if any, effect on corporate investment if the loans are secured. To test this conjecture, I partition the sample according to whether the loans are secured or not, and investigate the effect of lender mergers on corporate investment for these two subsamples. Consistent with the conjecture, I indeed find that the positive effect of lender mergers on investment only present in firms with unsecured loans but not for firms with secured loans, lending further support to the argument that lender mergers mitigate debt overhang and therefore increase corporate investment.

A common concern for the difference-in-differences specification is that the results may be driven by inherent differences between treated and control firms, that is, treated and control firms may have different dynamics of corporate investment in the absence of lender mergers. To mitigate such a concern, I first plot the trend of corporate investment of the treated and control firms separately over the six-year window and show that the treated and control firms have similar trends before the lender mergers. I then conduct a standard falsification test in which I examine the effect of fictional mergers, occurring four years before the actual mergers, on corporate investment while maintaining the assignment of treated and control firms. If the baseline results are driven by the inherent differences between treated and control firms, the effect is also likely to show up in the falsification test. In contrast, I find no statistically significant effect of these fictional mergers in the falsification test, suggesting that the baseline results are unlikely to be driven by non-parallel trends between treated and control firms.

A second threat to the identification comes from the possibility that lender mergers may be correlated with unobservable investment opportunities, especially those not captured by Tobin's Q. For example, lenders of borrowers with good investment opportunities may proactively pursue mergers, which then introduces the reverse causality problem. A direct implication of the concern is that lender mergers should affect not only capital expenditure investment but also other types of investments as well. On the other hand, however, if lender mergers increase capital investment via their mitigation of debt overhang, lender mergers may not affect R&D investment or acquisition expenses. R&D investment is risky and therefore may not benefit creditors as much as capital expenditure investment. Acquisitions, as shown in Billett, King, and Mauer (2004), in fact destroy creditor value. Therefore, R&D investment and acquisition expenses may not be subject to debt overhang.[1] To this end, I investigate whether lender mergers have a similar impact on R&D and acquisition expenses. Inconsistent with the idea that lender mergers may be correlated with investment opportunities, the lender mergers have no effect on R&D investment and have a negative effect on acquisition expenses. The results therefore suggest that the baseline results of the positive effect of lender mergers on capital expenditure investment are unlikely to be driven by the correlation between lender mergers and unobservable investment opportunities.

This paper is the first to test the impact of *ex post* debt renegotiation on debt overhang.

[1]This argument does not rule out the possibility that leverage may negatively affect R&D investment or acquisition expenses. It only states that leverage does not affect R&D investment and acquisition expenses via the debt overhang channel.

While previous studies show that firms more subject to debt overhang *ex ante* may design debt contracts that can be more easily renegotiated (Myers 1977, Aivazian and Callen 1980, Sufi 2007, and Christensen and Nikolaev 2012), they provide no direct evidence of the effect of renegotiation on debt overhang. The analysis of debt renegotiation in Roberts and Sufi (2009) and Roberts (2015) suggests that debt renegotiation may be used to resolve the debt overhang problem, but they also provide no direct evidence, especially no direct causal evidence, that debt renegotiation mitigates debt overhang.

This paper contributes more generally to the literature on the effects of debt enforcement. For example, Anderson and Sundaresan (1996), Mella-Barral and Perraudin (1997), Fan and Sundaresan (2000), and Davydenko and Strebulaev (2007) investigate the impact of debt enforcement on either *ex ante* strategic default incentives or *ex post* liquidation efficiency for firms in financial distress. Other papers have also looked at the implications of debt enforcement or creditor rights on asset pricing (Garlappi, Shu, and Yan 2008, Garlappi and Yan 2011, Hackbarth, Haselmann, and Schoenherr 2015 and Favara, Schroth, and Valta 2012). While these studies all take the asset side of the firms as given, I show in this paper that debt renegotiation can affect corporate investment, and hence the asset side of the balance sheet.

Several recent studies investigate the impact of debt enforcement on investment. For example, Becker and Strömberg (2012) show that managerial fiduciary duties to creditors for firms in distress mitigate both underinvestment and risk-shifting incentives. Alanis, Chava, and Kumar (2015) find that shareholder bargaining power (against bondholders) in default can dampen underinvestment induced by debt overhang. Favara et al. (2016) show that an imperfect enforcement of debt contracts in default induces leveraged firms to invest more, suggesting that allocating more control rights to shareholders in bankruptcy mitigates debt overhang. Different from the existing literature, which mostly focuses on debt enforcement in default, this paper focuses on debt renegotiation that may happen outside of distress or default. This paper also differs from these papers by using lender mergers to generate exogenous variation in loan renegotiability and therefore is able to identify the causal effect of debt renegotiation on debt overhang.

The rest of the paper is organized as follows. Section 2 develops the hypotheses; section 3 describes the natural experiment and sample construction; section 4 presents the main empirical results, section 5 presents some robustness test results; and section 6 concludes.

2 Hypotheses

Lender mergers can affect corporate investment via three different channels, which I call the renegotiation efficiency hypothesis, the renegotiation opportunistic underinvestment hypothesis, and the monitoring hypothesis.

The renegotiation efficiency hypothesis stems from the well-known debt overhang problem of Myers (1977), that is, firms underinvest in low-risk projects because shareholders do not capture all the returns of such investment and part of the returns goes to the creditors, even when such investment benefits the firm as a whole. However, as Myers (1977) himself notes that *ex post* renegotiation may lead to "an arrangement in which creditors accept less than the face amount of their securities in exchange for the owner's commitment to put up funds for future investment", that is, *ex post* renegotiation may mitigate debt overhang and restore investment efficiency. This argument is formally modeled in Aivazian and Callen (1980) and Gertner and Scharfstein (1991), who show that easier renegotiation increases investment efficiency for firms with good investment opportunities.

Empirically, Roberts and Sufi (2009) and Roberts (2015) show that debt contract renegotiations, especially those outside of distress or default, often result in lower interest rates and additional credit when firms' assets increase and credit risk decreases. With renegotiation, Shareholders will therefore be able to capture a larger fraction of the returns of investments that increase asset base and reduce credit risk of the firm, and hence will have more incentives to invest. It follows that lender mergers, which reduce the total number of lenders and make renegotiation easier, should mitigate debt overhang and increase corporate investment, especially for those with more investment opportunities. Based on this argument, I develop the following renegotiation efficiency hypothesis:

Hypothesis 1 (Renegotiation Efficiency Hypothesis)**.** *Due to increased ability to renegotiate loan contracts, lender mergers should increase investment of affected borrowers, and the effect should be stronger for firms with more investment opportunities.*

The increased ability to renegotiate loan contracts can also have a negative effect on corporate investment. Bergman and Callen (1991) argue that the possibility of renegotiation creates the incentive of shareholders to opportunistically underinvest because underinvestment results in fewer assets that can accessed by creditors. Consequently, the firm can strategically default to force concessions from the creditors in renegotiation. Based on this argument, I development the following renegotiation opportunistic underinvestment hypothesis:

Hypothesis 2 (Renegotiation Opportunistic Underinvestment Hypothesis)**.** *Due to increased ability to renegotiate loan contracts, lender mergers should decrease investment of affected borrowers.*

Lender mergers, as those explored in this paper, not only decrease the number of lenders but also increase the concentration of lenders, which can enhance the monitoring incentives of the merging lenders (Sufi 2007 and Ivashina 2009). Increased monitoring, on the other hand, may either increase investment if the firm was underinvesting or decrease investment if the firm was overinvesting, that is, the effect of increased lender monitoring due to lender mergers on investment can go either ways. In syndicated loans, monitoring responsibilities are often delegated to the lead lenders, and therefore the monitoring effect should only matter if one of the merging lenders is a lead lender. I therefore develop the following monitoring hypothesis:

Hypothesis 3 (Monitoring Hypothesis)**.** *Due to increased lender monitoring, lender mergers can either increase or decrease investment of affected borrowers, and the effect should mainly come from mergers involving lead lenders.*

3 Sample Construction and Identification Strategy

3.1 Sample Construction

The sample construction starts with all mergers between financial firms from 1987-2012 in the SDC mergers and acquisitions database. I begin the merger sample from 1987 because only since then the DealScan database starts to have a comprehensive coverage of loans. I stop the merger sample in 2012 because I need three years of data after the merger in the analysis. In the second step, I obtain lenders' information from the LPC DealScan database, and match the lender names with the names of the acquirers and the targets of the financial mergers. In matching acquirer names, I not only match the names of the lenders directly involved in the merger, but also match the names of the parent companies of the lenders and acquirers. Wherever possible, I use the addresses of the companies in both databases to facilitate the match. I then retain all mergers for which both the acquirer and the target can be matched with lenders in the DealScan database. All matches are manually checked to ensure accuracy. This procedure produces a sample of 877 mergers between lenders in the

Dealscan database.

The next step is to identify firms affected by these mergers, that is, to find the treated firms. I require that the treated firm to have an outstanding loan of which both the acquirer and the target of the merger are lenders. I exclude firms in financial and utility industries and firms with missing key variables. This procedure produces a sample of 1,326 treated firms involved in 45 mergers. On average, each merger affects about thirty firms. However, the median number of firms affected by a merger is only seven. The distribution of the mergers across time is presented in Table 1. The mergers are fairly evenly distributed across time, with the maximum number of seven mergers occurred in years 1999 and 2000.

Next, I use the following procedure to find control firms. To ensure control firms are not affected by the mergers as well, I exclude firm-year observations (seven years) of the treated firms surrounding the merger events from the potential control firm list. I then require that control firms also have a loan outstanding borrowed from either the acquiring lender or the target lender (but not from both) at the time of the merger. Restricting control firms to those who also borrow from the merging lenders ensures that unobservable characteristics of the merging lenders do not drive the results. To make treated and control firms more comparable, I then follow a similar procedure as in Hong and Kacperczyk (2010) and Derrien and Kecskés (2013) to refine the set of control firms. Specifically, I require control firms to be in the same two-digit SIC industries and to be in the same terciles sorted based on total assets, Tobin's Q, and cash flow as their treated counterparts. This procedure produces a sample of 3,226 control firms.

The empirical methodology requires specifying a time window around the merger dates. In choosing the appropriate time window, the trade-off is always between a long window that may incorporate information unrelated to the merger and a short window that may contain too few observations. In the baseline specification, I choose a six-year window, which contains three years before the merger and three years after the merger. To ensure clean identification, I discard firm fiscal years during which the merger occurred. In robustness checks, I also try two-year and four-year windows and find similar results.

The final step of sample construction involves matching both treated and control firms in the sample with their financial information in Compustat and detailed loan information from DealScan. In particular, I extract loan renegotiation information from the facility amendment file in DealScan.

3.2 Identification Strategy

I use the mergers between lenders participated in the same syndicated loans as exogenous shocks to the numbers of lenders the firms have and hence to the ability to renegotiate loan contracts. Loan contract renegotiations, especially those involving loan amount, loan maturity, or loan interest rate, often require unanimous consent of all lenders participated in the loan (Wight, Cooke, and Gray 2009 and Nikolaev 2015). Because each single lender can hold up the renegotiation, increasing the number of lenders makes such renegotiations more difficult (Gertner and Scharfstein 1991, Berglöf and Von Thadden 1994, Bolton and Scharfstein 1996, and Bris and Welch 2005). When two lenders of the same firm merge, the total number of lenders decreases by one and therefore the ability to renegotiate loan contracts increases. On the other hand, lenders often lend to hundreds of firms at each point in time and are therefore unlikely to make merger decisions based on factors related to one particular borrower. As such, the mergers between lenders are likely to satisfy both the relevance and the exclusion conditions. In this paper, I therefore treat the mergers as natural experiments and examine their impact on corporate investment.

To identify the causal effect of lender mergers on investment, I adopt the difference-in-differences specification as follows:

$$Y_{it} = \alpha_{ij} + \alpha_t + \beta Treat_{ij} \times Post_{ijt} + \gamma X_{it-1} + \epsilon_{ijt}, \quad (1)$$

where Y_{it} is capital investment of firm i in year t; $Treat_{ij}$ equals one if firm i is a treated firm in merger j, and zero otherwise; $Post_{ijt}$ equals one if the firm year observation is after the announcement of merger j; α_{ij} is the merger-firm fixed effects; α_t is the year fixed effects; and X_{it-1} is a vector of control variables. In this specification, $Treat_{ij}$ and $Post_{ijt}$ are subsumed by the merger-firm fixed effects and the year fixed effects, respectively. I use merger-firm fixed effects instead of just firm fixed effects because a firm can be a treated firm in one merger and a control firm in another. The difference-in-differences coefficient estimate β captures the marginal effect of the merger in affecting corporate investment. To account for the potential correlation between firms affected by the same merger, I cluster standard errors by merger in all estimation results reported below. However, the results are robust if I instead cluster standard errors by firm or merger-firm pair.

3.3 Variables and Summary Statistics

I use the facility amendment file in DealScan to construct variables of renegotiation. I first define *Renegotiation* as the total number of loan renegotiations within the fiscal year; I define *Material Change* as the number of loan renegotiations that affect the amount of credit, pricing, or maturity; I finally define *Pricing Change*, *Maturity Change*, and *Credit Change* as the number of loan renegotiations that affect pricing, maturity, and the amount of credit, respectively.

I use *Capex*, defined as capital expenditure (CAPX) scaled by lagged total assets (AT), to measure corporate investment. I focus on capital expenditure because capital expenditure investment is more likely to be subject to debt overhang. Capital expenditure investment is likely to benefit both shareholders and creditor as it increases the tangible assets that can be taken over by creditors in the event of bankruptcy. R&D investment, on the other hand, is riskier and may only benefit shareholders often at the expense of creditors. As shown in Billett, King, and Mauer (2004), mergers and acquisitions destroy both shareholder value and bondholder value. I therefore expect the debt overhang effect to be more pronounced on capital expenditure than R&D investment and mergers and acquisitions.

The control variables include: *Tobin's Q* — market value of total assets (PRCC_F×CSHO-CEQ+AT) divided by book value of total assets (AT), *Cash Flow* — operating cash flow (IB+DP) scaled by total assets (AT), *Leverage* – total debt (DLTT + DLC) scaled by total assets (AT), and *Sale Growth* — the growth rate of sales (SALE). Except for *Cash Flow*, which is contemporaneous, other control variables are all lagged by one year.

Table 2 reports the summary statistics of variables used in the empirical analysis. An average firm in the sample has a 10% chance of renegotiating its loan contract each year. Similar to the findings in Roberts (2015), about half of these renegotiations involving loan pricing, maturity, or amount changes. The table shows that the average capital expenditure investment is about 9.7%. The average Tobin's Q is 1.74, which is similar to the average Tobin's Q of the Compustat universe. The average leverage ratio is about 30.1%, which is slightly higher than an average Compustat firm.

4 Main Results

4.1 Lender Merger and Loan Renegotiation

In this subsection, I first examine the impact of the lender mergers on loan renegotiations. The renegotiation efficiency hypothesis argues that lender mergers, by reducing the number of lenders, make renegotiation easier, and hence positively affect corporate investment. The hypothesis holds only if the lender mergers truly affect loan renegotiations, that is, the relevance condition is satisfied.

To test, I replace the dependent variable in Equation (1) with measures of loan renegotiation. The results are presented in Table 3. In columns (1) and (2), I present the results for *Renegotiation*, the total number of loan renegotiations during the fiscal year, both with and without the control variables. In both columns, the difference-in-differences estimates, that is, the coefficients on $Treat \times Post$, are positive and statistically significant, suggesting that loan renegotiations are more likely to happen for treated firms after the mergers.

In columns (3) and (4), I then focus on loan renegotiations that often require unanimous lender consent, that is, renegotiations that lead to changes in pricing, maturity, or the amount of credit (Wight, Cooke, and Gray 2009 and Nikolaev 2015), which I call *Material Change*. The decrease of the number of lenders due to the mergers is likely to have a stronger effect on renegotiations that require unanimous consent. Consistent with this argument, the difference-in-differences estimates are again positive and statistically significant, suggesting that lender mergers have a positive effect on loan renegotiations that likely require unanimous lender consent. In contrast, loan renegotiations not involving loan pricing, maturity, or amount often do not require unanimous consent and can be decided either by the lead lender or a simple majority. In these cases, the mergers, which decrease the number of lenders by one, should have a smaller effect. I therefore replace the dependent variable in Equation (1) with *Non-Material Change*, the number of loan renegotiations not involving loan pricing, maturity, or amount. The results are presented in columns (5) and (6). In contrast to the results in columns (1)-(4), the difference-in-differences estimates are much smaller and statistically insignificant, suggesting that the lender mergers has almost no effect on loan renegotiations that do not require unanimous lender consent.

I then examine the impact of the lender mergers on loan renegotiations involving loan pricing, maturity, and amount separately, and the results are presented in columns (7)-(12). Consistent with the argument that the lender mergers should make these loan renegotiations easier, the difference-in-differences estimates are all positive and statistically significant.

Overall, the results in Table 3 suggest that the mergers between the lenders of the same firm do make loan renegotiations, especially those require unanimous consent, easier.

4.2 Lender Merger and Capital Investment

I present the baseline results of estimating Equation (1) in Table 4. In column (1), I present the results without any control. The difference-in-differences estimate, that is, the coefficient on $Treat \times Post$, is positive and statistically significant at the 1% level. In column (2), I then include *Tobin's Q* as the only control variable in the regression because *Tobin's Q* is often considered the sufficient statistic for investment. The difference-in-differences estimate remains positive and statistically significant. Finally in columns (3), I further include *Cash Flow*, *Leverage* and *Sale Growth* as additional controls. The literature (for example, Fazzari, Hubbard, and Petersen 1988) often argues that corporate investment responds positively to internally generated cash flow if the firm is financially constrained. I include *Leverage* because it may capture the level effect of debt overhang (Lang, Ofek, and Stulz 1996). I include *Sale Growth* to capture investment opportunities that may not be captured by *Tobin's Q*. Nonetheless, the difference-in-differences estimate remains positive and statistically significant.

The effect is also economically significant. The mergers increase capital expenditure expenses by about 1.5 percentage points, which is more than 16% of the sample mean. Overall, the results in Table 4 suggest that lender mergers increase capital expenditure investment by treated firms relative to control firms.

4.3 Lender Merger, Renegotiation, and Capital Investment: An Instrumental Variable Approach

This subsection investigates whether easier loan renegotiations triggered by the lender mergers *cause* the response of capital investment as documented above. To achieve this goal, I combine the intuitions from the above two subsections in an instrumental variable estimation framework, that is, I estimate the following equation:

$$Y_{it} = \alpha_{ij} + \alpha_t + \beta Renegotiation + \gamma X_{it-1} + \epsilon_{ijt}, \tag{2}$$

using $Treat \times Post$ as the instrument for measures of renegotiation. In this framework, the results presented in Table 3 are the first-stage estimation results, and the results presented in Table 4 are the reduced form estimation results. Equation (2) is the structural equation of interest and β captures the marginal effect of the exogenous component of loan renegotiation driven by the lender mergers on capital investment.

Because the first-stage regressions are essential those presented in Table 3, I only present the second-stage regression results in Table 5, with columns (1)-(3) using *Renegotiation* as the renegotiation measure and columns (4)-(6) using *Material Change* as the renegotiation measure. In all columns, the coefficient estimates on the renegotiation measures are all positive and statistically significant,[2] suggesting that the exogenous component of loan renegotiations triggered by the lender mergers has a positive effect on capital investment. The result therefore further supports the renegotiation efficiency hypothesis that the lender mergers make loan renegotiations easier, which then increases corporate investment.

[2] In fact, the estimates are equal to the reduced form estimates (Table 4) divided by the first-stage estimates (Table 3). The slightly smaller sample size is due to the drop of singleton group observations (i.e., single observations within fixed effects).

4.4 Monitoring vs Renegotiation

The results above are consistent with both the renegotiation efficiency hypothesis and the monitoring hypothesis. The monitoring hypothesis argues that increased lender monitoring due to concentrated lender shares may also alleviate the debt overhang problem and increase investment efficiency. The monitoring hypothesis, however, also suggests that the effect, if it exists at all, should concentrate in lender mergers involving the lead lenders because monitoring responsibilities are often delegated to lead lenders in syndicated loans. To distinguish between these two different hypotheses, I therefore examine whether the same effect emerges from merges between participant lenders.

To this end, I follow the same procedure as in Ivashina (2009) to identify lead lenders of syndicated loans. I then exclude all treated firms for which one of the merging lenders is a lead lender. I then re-estimate Equation (1) on this subsample.[3] If the baseline results are driven by the monitoring hypothesis, the effect should at least be much weaker on this subsample than on the full sample. The results are presented in Table 6. All difference-in-differences estimates are positive and statistically significant with magnitudes similar to those reported in Table 4, suggesting that the baseline results are not driven by mergers involving lead lenders. The result is therefore inconsistent with the monitoring hypothesis and favors the renegotiation efficiency hypothesis.

4.5 Renegotiation and Investment Efficiency

To provide further support to the renegotiation efficiency hypothesis and to show that the increase in investment due to lender mergers improves efficiency, I examine whether the effects of lender mergers vary with investment opportunities *ex ante*. According to the

[3] I do not report separately the results of lender mergers involving lead lenders only because the sample size is too small to achieve statistical power.

renegotiation efficiency hypothesis, the increased ability to renegotiate loan contracts due to the mergers should mitigate debt overhang and improve investment efficiency, that is, it should increase investment of firms with more investment opportunities, but not necessarily of firms with fewer investment opportunities. To capture the cross-sectional heterogeneity of investment opportunities, I partition the sample into two subsamples, a low-Q subsample and a high-Q subsample, according to whether the firm's *Tobin's Q* is below or above the sample median. Firms in the low-Q subsample are likely to have fewer investment opportunities than firms in the high-Q subsample.

The results of estimating Equation (1) on the low-Q and high-Q subsamples separately are presented in Table 7, with columns (1)-(3) for the high-Q subsample and columns (4)-(6) for the low-Q subsample. The difference-in-differences estimates are all positive and statistically significant at the 5% level in columns (1)-(3), and the estimates are much smaller in magnitude and statistically insignificant in columns (4)-(6). The differences between the coefficients, that is the differences between columns (1) and (4), columns (2) and (5), and columns (3) and (6) are all statistically significant at the 10% level. The results suggest that lender mergers increase investment mostly for high-Q firms, that is, firms have more investment opportunities, which is consistent with the renegotiation efficiency hypothesis that the increased ability to renegotiate loan contracts mitigates debt overhang and improves investment efficiency.

4.6 Performance Pricing and Debt Overhang

Recognizing the potential agency cost of debt overhang *ex post*, many loan contracts put in place covenants that may mitigate the debt overhang problem. Performance pricing provisions contained in many loan contracts achieve exactly this goal. Performance pricing allows loan spreads to be adjusted according to borrower performance and credit risk, that is, loan spreads decrease when credit risk decreases and loan spreads increase when credit risk increases (Asquith, Beatty, and Weber 2005). As such, when firms make investment that reduces credit risk, loan cost decreases, that is, shareholders will be capturing more benefits from such investment. As a result, shareholders' incentives to make such investment increase. It follows that the effect of renegotiation on mitigating debt overhang will be diminished if the loan contracts already contain the performance pricing provision.

To test this conjecture, I split the sample according to whether the loan contract contains the performance pricing provision and re-estimate Equation (1) on the two subsamples. The results are presented in Table 8, with columns (1)-(3) for loans without the performance pricing provision and columns (4)-(6) for loans with the provision. The difference-in-differences estimates are all positive and statistically significant at the 1% level in columns (1)-(3) for loans without the performance pricing provision. In contrast, the estimates are all much smaller and statistically insignificant in columns (4)-(6) for loans with the performance pricing provision. Furthermore, the differences of the estimates between those in columns (1)-(3) and columns (4)-(6) are all statistically significant at the 5% level. The results suggest that the performance pricing provision itself can mitigate debt overhang and hence renegotiation adds no additional value in improving investment efficiency. The results lend further support to the renegotiation efficiency hypothesis that the positive effect of lender mergers on corporate investment is driven by its mitigation of the debt overhang problem.

4.7 Secured Loans and Debt Overhang

The debt overhang problem stems from the fact that part of the returns of investment goes to the creditors *ex post*, which happens if the value of the debt is sensitive to performance or credit risk of the firm. Secured debt is arguably less sensitive to changes in firm performance and credit risk because secured debt holders have access to the collateral, whose value is often not sensitive to future investment. In fact, Stulz and Johnson (1985) show that issuing secured debt can resolve the debt overhang problem. It follows that the effect of renegotiation in mitigating debt overhang will be limited if the loan is secured.

To this end, I split the sample according to whether the loan is secured and re-estimate Equation (1) on the non-secured and secured loan subsamples to test this conjecture. The results are presented in Table 9, with columns (1)-(3) for the unsecured loan subsample and columns (4)-(6) for the secured loan subsample. The difference-in-differences estimates in columns (1)-(3) are all positive and statistically significant at the 1% level. In contrast, the estimates in columns (4)-(6) are much smaller in magnitude and are statistically insignificant. Furthermore, the differences of the estimates between those in columns (1)-(3) and columns (4)-(6) are all statistically significant at the 5% level. The results are consistent with the argument that borrowers of secured loans are less subject to the debt overhang problem because values of secured loans mostly depend on collateral values and are relatively less sensitive to future investment. The results therefore provide further support to the renegotiation efficiency hypothesis that the increased ability to renegotiate loan contracts due to lender mergers mitigate debt overhang mainly for firms borrowing unsecured debt.

5 Placebo and Robustness Tests

5.1 The Parallel Trend Condition

The identification of the difference-in-differences method relies on the parallel trend condition, that is, outcome variables move in parallel trends in the absence of the treatment. While the parallel trend condition is non-testable, I follow the advice in Roberts and Whited (2012) to conduct a visual examination of the condition by plotting the outcome variable, *Capex*, of the treated and control firms over the six-year window. The result is presented in

Figure 1. First, capital investment of both the treated and control firms decreases over time, which is a common trend among all Compustat firms (Fu, Huang, and Wang 2015). Second, while the control firms continue their trend before the mergers, the treated firms' capital investment decreases at a much slower rate after the mergers, which is consistent with the hypothesis that the lender mergers have a positive effect on capital investment.

5.2 A Falsification Test

To further ensure that the baseline results are not driven by pre-existing trend differences between treated and control firms, I conduct a diagnostic falsification test. For each merger in the sample, I create a fictional merger that occurs four years before the actual merger. At the same time, I maintain the assignment of the treated and control firms, that is, the treated and control firms in the placebo test are the same treated and control firms as those in the baseline tests. I also focus on a six-year window around the fictional mergers, that is, three years before and three years after the fictional mergers. I then estimate the following difference-in-differences specification using the fictional merger events as the treatment:

$$Y_{it} = \alpha_{ij} + \alpha_t + \beta Treat_{ij} \times Pseudo\ Post_{ijt} + \gamma X_{it-1} + \epsilon_{ijt}, \tag{3}$$

where all variables are defined exactly the same as those in Equation (1), except for $Pseudo\ Post$, which equals one if the firm-year observation is after the fictional merger, and zero otherwise.

Under this specification, β captures the effect of the fictional lender mergers. If the baseline results are driven by pre-existing trend differences between treated and control firms, the effect is also likely to show up in the placebo test.

The results of the placebo test are presented in Table 10. In all columns, the difference-in-differences estimates are negative and statistically insignificant, suggesting that the baseline results are not driven by treated firms having an increasing trend of capital expenditure investment relative to control firms in the absence of the lender mergers. The results indicate (but do not prove) that the parallel trend condition is likely to be satisfied because a similar positive effect of the fictional mergers on capital expenditure should be observed otherwise.

5.3 R&D Investment and Acquisition Expenses

An alternative explanation of the positive effect of lender mergers on capital investment is that the lenders foresee the growth potentials of their borrowers and proactively pursue mergers, or more generally, lender mergers are correlated with unobservable borrower growth opportunities. The average Q measure (as opposed to the marginal Q) used as the control in the regressions is at best a proxy for investment opportunities, and as shown in Erickson and Whited (2000), the average Q is measured with substantial measurement error. Furthermore, lenders often get access to private information, which may include information on growth opportunities that are not captured by Q, which is only based on public information. To mitigate such concerns, I examine, in this subsection, whether lender mergers also affect corporate R&D expenses and acquisition expenses. To the extent that lender mergers are correlated with unobservable growth opportunities, it should also affect R&D expenses and acquisition expenses. On the other hand, however, if the positive effect of lender mergers on capital investment is only driven by reduced debt overhang, the lender mergers may not have a similar impact on R&D investment and acquisition expenses. On the one hand, R&D investment is often risky and does not always result in assets that can be accessed by the lenders, that is, R&D investment may not benefit creditors and is therefore less likely to be subject to debt overhang. On the other hand, as shown in Billett, King, and Mauer (2004), acquisitions often hurt both the shareholders and creditors of the acquirers, and are therefore also less likely to be subject to debt overhang. As such, lender mergers, if they affect capital investment only via the debt overhang channel, they should not significantly affect R&D investment and acquisition expenses the same way as they affect capital expenditure investment.

I therefore replace the dependent variable in Equation (1) with *R&D*, defined as R&D expense (XRD) divided by lagged total assets (AT), and *Acquisition*, defined as acquisition expense (AQC) divided by lagged total assets. The results are presented in Table 11, with columns (1)-(3) for *R&D* and columns (4)-(6) for *Acquisition*. Focusing on the results for R&D expenses first, the difference-in-differences estimates are all statistically insignificant, suggesting that lender mergers do not have large impact on R&D expense. For acquisition expenses, all estimates are negative and statistically significant at the 1% level. The results are not consistent lender mergers being correlated with unobservable investment opportunities because, if so, lender mergers should also positively affect R&D expenses and acquisition expenses. The negative and statistically significant effect on *Acquisition* may come from increased lender monitoring due to more concentrated lender shares, which prevents managers from pursuing acquisitions that destroy both shareholder and creditor values. Overall, the results in Table 11 suggest that the baseline results are unlikely to be driven by the correlation between lender mergers and unobservable investment opportunities, and are therefore consistent with the renegotiation efficiency hypothesis.

5.4 Different Test Windows

A final concern is that the six-year window over which I conduct the empirical analysis may be too long. A longer window may include confounded factors that also affect corporate investment and therefore may introduce bias into the difference-in-differences estimates. To mitigate this concern, I try two shorter windows, a four-year window, that is, two years before and two years after the mergers, and a two-year window, that is, one year before and one year after the mergers, to assess the robustness of the baseline results. The results are presented in Table 12, with columns (1)-(3) for the four-year window and columns (4)-(6) for the two-year window. In all columns, the difference-in-differences estimates are all positive and statistically significant at least at the 10% level and the coefficient estimates are no smaller, if not larger, than those reported in Table 4. The drop in statistical significance for the two-year window is probably due to the decrease in the number of observations and hence the decrease in statistical power. Overall, the results in Table 12 alleviate the concern that the baseline results are biased by confounding factors included in a too long window.

6 Conclusion

This paper examines the effect of the ability to renegotiate debt contract *ex post* on debt overhang using mergers between lenders of the same borrower as natural experiments. I find that after lender mergers, effected firms increase their capital expenditure investment relative to control firms, which I attribute to the increased ability to renegotiate loan contracts due to the reduction of the number of lenders. I find that the effect is unlikely to be driven by increased lender monitoring because the effect persists for mergers involving only participant lenders. Further evidence shows that the effect is stronger for firms with high Q, for unsecured loans, and for loans without performance pricing provisions, all of which support the argument that lender mergers mitigate debt overhang and improve investment efficiency.

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Table 1: Distribution of Lender Mergers

This table presents the annual distribution of the mergers used in this paper. The mergers are merger and acquisition deals between lenders in the DealScan database from 1987 to 2012.

Year	Number of Mergers	Percent
1989	1	2.22
1990	1	2.22
1992	1	2.22
1994	1	2.22
1995	5	11.11
1996	1	2.22
1997	2	4.44
1998	2	4.44
1999	7	15.56
2000	7	15.56
2001	3	6.67
2003	1	2.22
2004	1	2.22
2005	1	2.22
2007	2	4.44
2008	4	8.89
2010	1	2.22
2011	1	2.22
2013	2	4.44
2014	1	2.22
Total	45	100

Table 2: Summary Statistics

This table reports the summary statistics of the variables used in this paper. The variables are: *Renegotiation*– the total number of loan renegotiations within the fiscal year, *Material Change*– the number of loan renegotiations affecting the amount of credit, pricing, or maturity; *Pricing Change* – the number of loan renegotiations affecting pricing, *Maturity Change* – the number of loan renegotiations affecting maturity, and *Credit Change* – the number of loan renegotiations affecting the amount of credit, respectively. *Capex* – capital expenditure (CAPX) scaled by lagged total assets (AT), *R&D* – R&D expense (XRD) scaled by lagged total assets (AT), *Acquisition* – Acquisition expense (AQC) scaled by lagged total assets (AT), *Tobin's Q* – market value of total assets (PRCC_F × CSHO+AT-CEQ) divided by total assets (AT), *Cash Flow* – cash flow (IB+DP) scaled by total assets (AT), *Leverage* – total liability (DLC+DLTT) scaled by total assets (AT)*Sale Growth* – change in sales (SALE) divided by lagged sales.

	count	mean	sd	p25	p50	p75
Renegotiation	23,711	0.112	0.566	0.000	0.000	0.000
Material Change	23,711	0.058	0.397	0.000	0.000	0.000
Non-Material Change	23,711	0.054	0.339	0.000	0.000	0.000
Pricing Change	23,711	0.021	0.226	0.000	0.000	0.000
Maturity Change	23,711	0.030	0.280	0.000	0.000	0.000
Credit Change	23,711	0.023	0.199	0.000	0.000	0.000
Capex	23,711	9.677	15.881	3.026	5.214	9.011
R&D	14,466	4.759	7.032	0.469	2.280	5.828
Acquisition	22,136	5.174	14.724	0.000	0.036	3.287
Tobin's Q	23,711	1.744	1.068	1.135	1.424	1.926
Cash Flow	23,711	0.078	0.088	0.049	0.086	0.123
Leverage	23,711	0.301	0.202	0.157	0.284	0.406
Sale Growth	23,711	0.321	1.984	-0.009	0.077	0.206

Table 3: Lender Mergers and Loan Renegotiations

This table reports the baseline difference-in-differences estimation results of $Y_{it} = \alpha_{ij} + \alpha_t + \beta Treat_{ij} \times Post_{jt} + \gamma X_{it-1} + \epsilon_{ijt}$. The dependent variables are *Renegotiation*– the total number of loan renegotiations within the fiscal year, *Material Change*– the number of loan renegotiations affecting the amount of credit, pricing, or maturity; *Pricing Change* – the number of loan renegotiations affecting pricing, *Maturity Change* – the number of loan renegotiations affecting maturity, and *Credit Change* – the number of loan renegotiations affecting the amount of credit, respectively. *Treat* equals one if the firm is a treated firm of the merger, and zero otherwise. *Post* equals one if the firm-year observation is after the merger. All regressions include year fixed effects and merger-firm fixed effects. Standard errors are clustered by merger. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

	Renegotiation		*Material Change*		*Non-Material Change*		*Pricing Change*		*Credit Change*		*Maturity Change*	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Post × *Treat*	0.081**	0.087**	0.084***	0.088***	-0.002	-0.001	0.038**	0.040**	0.048***	0.050***	0.022*	0.024*
	(0.034)	(0.035)	(0.026)	(0.027)	(0.013)	(0.013)	(0.016)	(0.016)	(0.014)	(0.015)	(0.012)	(0.012)
Tobin's Q		-0.002		-0.006		0.004		-0.004		-0.003		-0.002
		(0.009)		(0.007)		(0.006)		(0.003)		(0.002)		(0.005)
Cash Flow		-0.580***		-0.376***		-0.203**		-0.141**		-0.154**		-0.155**
		(0.167)		(0.139)		(0.078)		(0.064)		(0.067)		(0.073)
Leverage		0.146		0.111**		0.035		0.074**		0.037*		0.010
		(0.088)		(0.053)		(0.049)		(0.028)		(0.021)		(0.044)
Sale Growth		-0.005*		-0.003**		-0.002		-0.001		-0.001		-0.001
		(0.003)		(0.001)		(0.002)		(0.001)		(0.001)		(0.001)
Constant	0.060***	0.037	0.036*	0.028	0.024	0.009	0.017	0.017	0.021**	0.020**	0.003	0.000
	(0.019)	(0.022)	(0.020)	(0.020)	(0.014)	(0.021)	(0.012)	(0.013)	(0.010)	(0.010)	(0.017)	(0.018)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Merger-Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23,711	23,711	23,711	23,711	23,711	23,711	23,711	23,711	23,711	23,711	23,711	23,711
Adjusted R-squared	0.139	0.146	0.103	0.109	0.091	0.093	0.075	0.080	0.072	0.076	0.098	0.099

Table 4: Lender Mergers and Capital Investment

This table reports the baseline difference-in-differences estimation results of $Y_{it} = \alpha_{ij} + \alpha_t + \beta Treat_{ij} \times Post_{jt} + \gamma X_{it-1} + \epsilon_{ijt}$. The dependent variable is *Capex*. *Treat* equals one if the firm is a treated firm of the merger, and zero otherwise. *Post* equals one if the firm-year observation is after the merger. All regressions include year fixed effects and merger-firm fixed effects. Standard errors are clustered by merger. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

	(1)	(2)	(3)
Post × *Treat*	1.957***	1.721***	1.671**
	(0.642)	(0.633)	(0.655)
Tobin's Q		3.358***	3.254***
		(0.505)	(0.488)
Cash Flow			2.618
			(2.297)
Leverage			-7.150*
			(4.047)
Sale Growth			-0.449***
			(0.110)
Constant	46.188***	40.453***	41.470***
	(1.757)	(1.876)	(1.791)
Year Fixed Effects	Yes	Yes	Yes
Merger-Firm Fixed Effects	Yes	Yes	Yes
Observations	23,711	23,711	23,711
Adjusted R-squared	0.462	0.482	0.488

Table 5: Lender Merger, Loan Renegotiation, and Capital Investment: Instrumental Variable Estimation Results

This table reports the instrumental variable estimation results of $Y_{it} = \alpha_{ij} + \alpha_t + \beta Renegotiation + \gamma X_{it-1} + \epsilon_{ijt}$ using $Treat \times Post$ as the instrument for measures of renegotiation. In columns (1)-(3), the renegotiation measure is *Renegotiation*, the total number of loan renegotiations during the fiscal year; in columns (4)-(6), the renegotiation measure is *Material Change*, the number of loan renegotiations affecting loan pricing, maturity, or amount. All regressions include year fixed effects and merger-firm fixed effects. Standard errors are clustered by merger. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Renegotiation	24.101**	20.865**	19.308**			
	(11.446)	(10.486)	(9.648)			
Material Change				23.434**	20.328**	19.141**
				(9.663)	(9.205)	(8.782)
Tobin's Q		3.721***	3.243***		3.675***	3.325***
		(0.500)	(0.446)		(0.461)	(0.438)
Cash Flow			13.949***			9.958***
			(5.290)			(3.424)
Leverage			-9.878**			-9.185**
			(4.120)			(3.900)
Sale Growth			-0.384***			-0.430***
			(0.125)			(0.116)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Merger-Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	23,654	23,654	23,654	23,654	23,654	23,654
Adjusted R-squared	0.395	0.467	0.569	0.368	0.434	0.559

Table 6: The Effects of Participant Lender Mergers

This table reports the difference-in-differences estimation results of $Y_{it} = \alpha_{ij} + \alpha_t + \beta Treat_{ij} \times Post_{jt} + \gamma X_{it-1} + \epsilon_{ijt}$ with participant lender mergers as the treatment. The dependent variable is *Capex*. *Treat* equals one if the firm is a treated firm of the merger, and zero otherwise. *Post* equals one if the firm-year observation is after the merger. All regressions include year fixed effects and merger-firm fixed effects. Standard errors are clustered by merger. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

	(1)	(2)	(3)
Post × *Treat*	1.846**	1.554**	1.491*
	(0.737)	(0.722)	(0.761)
Tobin's Q		3.495***	3.443***
		(0.551)	(0.496)
Cash Flow			1.656
			(2.118)
Leverage			-6.752
			(4.435)
Sale Growth			-0.542***
			(0.126)
Constant	46.000***	39.875***	40.835***
	(1.793)	(1.930)	(1.756)
Year Fixed Effects	Yes	Yes	Yes
Merger-Firm Fixed Effects	Yes	Yes	Yes
Observations	19,192	19,192	19,192
Adjusted R-squared	0.458	0.480	0.486

Table 7: Lender Mergers and Investment Efficiency

This table reports the difference-in-differences estimation results of $Y_{it} = \alpha_{ij} + \alpha_t + \beta Treat_{ij} \times Post_{jt} + \gamma X_{it-1} + \epsilon_{ijt}$ on subsamples partitioned on *Tobin's Q*. Columns (1)-(3) present results on observations whose *Tobin's Q* is above the sample median, and columns (4)-(6) present results on observations whose *Tobin's Q* is below the sample median. The dependent variable is *Capex*. *Treat* equals one if the firm is a treated firm of the merger, and zero otherwise. *Post* equals one if the firm-year observation is after the merger. All regressions include year fixed effects and merger-firm fixed effects. Standard errors are clustered by merger. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

	High Q			Low Q		
	(1)	(2)	(3)	(4)	(5)	(6)
Post × *Treat*	2.609**	2.176**	2.039**	0.414	0.390	0.427
	(1.143)	(1.039)	(1.006)	(1.064)	(1.007)	(0.922)
Tobin's Q		2.032***	2.113***		6.120***	5.218***
		(0.559)	(0.536)		(1.874)	(1.705)
Cash Flow			0.404			4.826
			(5.507)			(3.207)
Leverage			-0.167			-15.454***
			(7.634)			(3.057)
Sale Growth			-0.601***			-0.409***
			(0.202)			(0.128)
Constant	54.255***	48.719***	48.924***	1.696	-4.360	2.550
	(1.071)	(1.802)	(1.664)	(2.145)	(2.770)	(3.094)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Merger-Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	11,848	11,848	11,848	11,863	11,863	11,863
Adjusted R-squared	0.545	0.554	0.559	0.521	0.524	0.536

Table 8: Performance Pricing and the Effect of Lender Mergers

This table reports the baseline difference-in-differences estimation results of $Y_{it} = \alpha_{ij} + \alpha_t + \beta Treat_{ij} \times Post_{jt} + \gamma X_{it-1} + \epsilon_{ijt}$ on subsamples partitioned on whether the loan contracts contain the performance pricing provision. Columns (1)-(3) present results on observations whose loans contain the performance pricing provision, and columns (4)-(6) present results on observations whose loans do not contain the performance pricing provision. The dependent variable is *Capex*. *Treat* equals one if the firm is a treated firm of the merger, and zero otherwise. *Post* equals one if the firm-year observation is after the merger. All regressions include year fixed effects and merger-firm fixed effects. Standard errors are clustered by merger. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

	No Performance Pricing			Performance Pricing		
	(1)	(2)	(3)	(4)	(5)	(6)
Post × *Treat*	2.735***	2.443***	2.395***	0.995	0.829	0.768
	(0.697)	(0.616)	(0.651)	(0.980)	(0.961)	(0.950)
Tobin's Q		3.512***	3.351***		3.172***	3.108***
		(0.721)	(0.770)		(0.629)	(0.550)
Cash Flow			2.256			3.205
			(2.864)			(2.182)
Leverage			-8.744*			-4.689
			(4.709)			(4.936)
Sale Growth			-0.500***			-0.362*
			(0.086)			(0.187)
Constant	44.621***	38.957***	40.152***	17.625***	11.393***	12.878***
	(1.711)	(2.057)	(2.133)	(1.544)	(1.897)	(1.542)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Merger-Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,013	14,013	14,013	9,698	9,698	9,698
Adjusted R-squared	0.469	0.488	0.494	0.454	0.477	0.480

Table 9: Secured Loans and the Effect of Lender Mergers

This table reports the baseline difference-in-differences estimation results of $Y_{it} = \alpha_{ij} + \alpha_t + \beta Treat_{ij} \times Post_{jt} + \gamma X_{it-1} + \epsilon_{ijt}$ on subsamples partitioned on whether the loans are secured or not. Columns (1)-(3) present results on observations for unsecured loans, and columns (4)-(6) present results for secured loans. The dependent variable is *Capex*. *Treat* equals one if the firm is a treated firm of the merger, and zero otherwise. *Post* equals one if the firm-year observation is after the merger. All regressions include year fixed effects and merger-firm fixed effects. Standard errors are clustered by merger. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

	Unsecured			Secured		
	(1)	(2)	(3)	(4)	(5)	(6)
Post × *Treat*	2.811***	2.468***	2.464***	0.477	0.543	0.331
	(0.687)	(0.670)	(0.682)	(0.746)	(0.774)	(0.827)
Tobin's Q		2.985***	2.933***		4.079***	3.887***
		(0.536)	(0.522)		(0.667)	(0.648)
Cash Flow			3.179			1.769
			(3.372)			(1.664)
Leverage			-3.988			-11.847***
			(5.057)			(4.165)
Sale Growth			-0.455***			-0.442***
			(0.153)			(0.136)
Constant	21.710***	15.950***	17.162***	47.936***	40.877***	42.271***
	(0.868)	(1.381)	(1.442)	(3.443)	(3.368)	(3.487)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Merger-Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	15,266	15,266	15,266	8,445	8,445	8,445
Adjusted R-squared	0.449	0.466	0.470	0.485	0.513	0.523

Table 10: A Falsification Test

This table reports the falsification estimation results of $Y_{it} = \alpha_{ij} + \alpha_t + \beta Treat_{ij} \times PseudPost_{jt} + \gamma X_{it-1} + \epsilon_{ijt}$. The dependent variable is *Capex*. *Treat* equals one if the firm is a treated firm of the merger, and zero otherwise. *Pseudo Post* equals one if the firm-year observation is after the fictional merger, which occurs four years before the actual merger. All regressions include year fixed effects and merger-firm fixed effects. Standard errors are clustered by merger. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

	(1)	(2)	(3)
Post × *Treat*	-0.925	-0.539	-0.706
	(1.289)	(0.997)	(1.014)
Tobin's Q		5.354***	5.295***
		(0.531)	(0.497)
Cash Flow			0.340
			(3.871)
Leverage			-2.008
			(4.060)
Sale Growth			-0.793***
			(0.137)
Constant	19.473***	10.941***	12.028***
	(1.509)	(0.933)	(1.119)
Year Fixed Effects	Yes	Yes	Yes
Merger-Firm Fixed Effects	Yes	Yes	Yes
Observations	23,300	23,300	23,300
Adjusted R-squared	0.513	0.558	0.568

Table 11: The Impact of Lender Mergers on R&D and Acquisition Expenses

This table reports the difference-in-differences estimation results of $Y_{it} = \alpha_{ij} + \alpha_t + \beta Treat_{ij} \times Post_{jt} + \gamma X_{it-1} + \epsilon_{ijt}$. The dependent variables are *R&D* in columns (1)-(3) and *Acquisition* in columns (4)-(6). *Treat* equals one if the firm is a treated firm of the merger, and zero otherwise. *Post* equals one if the firm-year observation is after the merger. All regressions include year fixed effects and merger-firm fixed effects. Standard errors are clustered by merger. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

	R&D Expense			Acquisition Expense		
	(1)	(2)	(3)	(4)	(5)	(6)
Post × *Treat*	0.387	0.262	0.282	-2.643***	-2.763***	-2.628***
	(0.274)	(0.256)	(0.271)	(0.880)	(0.877)	(0.803)
Tobin's Q		1.123***	1.191***		2.371***	2.312***
		(0.162)	(0.205)		(0.673)	(0.626)
Cash Flow			-2.270			-12.689***
			(1.428)			(2.926)
Leverage			-1.644			-19.256***
			(2.102)			(2.447)
Sale Growth			-0.198***			0.099
			(0.068)			(0.148)
Constant	6.966***	4.984***	6.246***	9.250***	4.970***	10.646***
	(0.423)	(0.477)	(0.688)	(1.892)	(1.787)	(1.605)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Merger-Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	14,466	14,466	14,466	22,136	22,136	22,136
Adjusted R-squared	0.647	0.659	0.663	0.150	0.161	0.185

Table 12: Different Test Windows

This table reports the difference-in-differences estimation results of $Y_{it} = \alpha_{ij} + \alpha_t + \beta Treat_{ij} \times Post_{jt} + \gamma X_{it-1} + \epsilon_{ijt}$ over two-year and four-year windows. The dependent variable is *Capex*. *Treat* equals one if the firm is a treated firm of the merger, and zero otherwise. *Post* equals one if the firm-year observation is after the merger. All regressions include year fixed effects and merger-firm fixed effects. Standard errors are clustered by merger. Significance at 1%, 5%, and 10% levels are indicated by ***, **, and *, respectively.

	Four-Year Window			Two-Year Window		
	(1)	(2)	(3)	(4)	(5)	(6)
Post × *Treat*	2.077***	1.721**	1.743**	2.177*	2.118*	2.057*
	(0.708)	(0.705)	(0.705)	(1.184)	-1.162	(1.147)
Tobin's Q		3.531***	3.590***		2.114	2.317
		(0.858)	(0.825)		(2.981)	(2.826)
Cash Flow			-1.598			4.482
			(3.758)			(8.781)
Leverage			-2.766			7.083
			(8.070)			(15.634)
Sale Growth			-0.350***			-0.065
			(0.112)			(0.150)
Constant	7.760***	1.807	3.055	1.790	-0.664	-5.887
	(2.851)	(2.863)	(3.809)	(5.874)	(6.113)	(13.074)
Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Merger-Firm Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,291	16,291	16,291	8,365	8,365	8,365
Adjusted R-squared	0.479	0.500	0.502	0.490	0.498	0.500

Figure 1: Capital Investment Surrounding the Mergers

This figure shows the evolution of *Capex* of treated and control firms surrounding the lender mergers.