Essays in Empirical Corporate Finance and Banking

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ABSTRACT

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This dissertation studies topics in the areas of empirical corporate finance, banking, and financial intermediation. In the first chapter, entitled *Personal Relationships in Loan Renegotiation: Evidence from Corporate Loans*, I estimate the effect of personal relationships between a loan officer and a firm on the probability to renegotiate a loan and the outcomes of the renegotiation. To identify this effect, I exploit a bank reorganization in Greece in the mid-2010s, which allows me to identify two types of firms: one, those whose personal relationships with loan officers were discontinued and those whose relationships were not. This paper's main conclusion is that personal relationships mitigate the cost of distress for the firm in a loan renegotiation. The firm is worse off following the interruption of its loan officer relationship, as it is less able to renegotiate, and the firm also receives tougher loan terms on renegotiated loans.

The insights from the second chapter, entitled *Lending Relationships and Moral Hazard in Loan Renegotiation*, can have important policy implications related to the rise of nonperforming loans (NPLs). Many banks operating in countries that were hit by the 2010 European debt crisis, faced a significant rise in NPLs. This rise became one of the main challenges that banks face, as high levels of NPLs tie up bank capital and thus reduce profitability and increase funding costs. In the second chapter, I provide empirical evidence that banks, through efficient renegotiation and strong relationships with firms, can prevent loan defaults. This analysis suggests that firms with more distant lending relatioships are

more likely to strategically delay a loan payment in order to efficiently trigger a loan renegotiation. This strategic behavior gives rise to the moral hazard phenomenon.

In the third chapter, entitled *Securing the Unsecured: Do stronger creditor rights affect firms' access to credit?*, I seek to understand whether stronger creditor rights influence firms' capital structure and access to finance. To answer this question, I use the passage of an enforcement on cash assets reform in Croatia that aimed to increase the collection of the unsecured debt. To identify exogenous variation across firms affected more by the reform versus those that were not, I use a novel dataset on courts' efficiency in dealing with the specific type of cases affected by the reform. The conclusion of the paper is that firms maintain higher leverage and have easier access to credit when creditor rights are stronger. The firms that benefit the most are medium size and have limited access to tangible assets. When firms are able to borrow more, they invest more in fixed assets.

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To my inspirational parents, Lia and Christos.

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Chapter 1

Personal Relations in Loan Renegotiation: Evidence from Corporate Loans [1]

1.1 Introduction

Are personal relationships with bank loan officers consequential for corporate borrowers? Could stronger relationships help a firm secure better loan terms in a renegotiation? Do stronger lending relationships impact the firm's capital structure? These are the key questions that I address in this paper. Financial intermediation theory suggests that relationships can be beneficial because they reduce information asymmetries and alleviate moral hazard (through monitoring, screening, repeated interaction etc.).[2] At the same time, strong relationships between borrowers and lenders may give rise to other problems, e.g., soft-budget-constraint problems, hold-up problems, or nepotistic behavior.[3] Whether the benefits of these relationships offset the costs is a challenging empirical question.

[1] I am extremely grateful to my advisors Charles Calomiris, Olivier Darmouni, Wei Jiang, and Daniel Wolfenzon for their support. I would like also to thank Simona Abis, Geert Bekaert, Patrick Bolton, Emily Breza, Xavier Giroud, Matthieu Gomez, Kinda Hachem, Andrew Hertzberg, Gur Huberman, Elias Papaioannou, Giorgia Piacentino, Tano Santos, Suresh Sundaresan, and Kairong Xiao for their helpful comments and discussions. Additionally, I am grateful to participants at Columbia Business School, Columbia Economics Department, Washington University in St. Louis WFA-CFAR, and the Fordham PhD Student Colloquium for comments.

[2] Classic references: Leland and Pyle 1977 Diamond 1984 Ramakrishnan and Thakor 1984 Fama 1985 Allen 1990

[3] Boot 2000;Bolton and Scharfstein 1996, Dewatripont and Maskin 1995;Sharpe 1990, Rajan 1992

Classic contract theory states that debt contracts are not complete contingent contracts, and for that reason in unanticipated states of the world, renegotiation is Pareto improving.[4] The renegotiation optimality arises due to the debt-overhang problem or due to a covenant violation. Thus, a hypothesis that arises from contract theory is that in a recession period both the bank and the firm will benefit from a successful loan renegotiation. By offering more flexible loan terms, a firm is expected to be less financially constrained, and a bank will prevent defaults on loan repayments. In this paper, I study the impact of personal relationships between loan officers and firms on loan renegotiations and I investigate whether personal relationships have significant effects on both the probability of successful renegotiation and on the terms of the renewed loans. Moreover, I provide evidence that lending relationships impact the capital structure and investment decisions of a firm.

There are two main challenges for accurately estimating the impact of personal relationships on loan renegotiation. The first is the difficulty of quantifying the value of a personal relationship. No direct measure of relationship intensity exists. The length of a given relationship may seem like a straightforward measure, but the endogeneity of the decision to sever an existing relationship will complicate the interpretation of the time span measure. The endogeneity factor creates the second challenge. A bank's decision to break an existing relationship may reflect its perception of the declining creditworthiness of the borrower. Under some circumstances a successful firm may seek to broaden its access to external finance by weakening its relationship with the particular bank. Such decisions are endogenous and would bias any results estimated by treating relationships as exogenous in

[4]Hart and Moore 1988, Rajan 1992, Aghion, Dewatripont, and Rey 1994, Bolton and Scharfstein 1996, Hart and Moore 1998, Maskin and Moore 1999, Gorton and Kahn 2000, Garleanu and Zwiebel 2008

a simple OLS framework.

To overcome these challenges, I use a 2013 bank reorganization in Greece. This experiment provides exogenous variation in the length of the relationships between loan officers and firms. Moreover, detailed confidential data on corporate loans allow me to quantify accurately the effect of interrupted personal relationships, by controlling for region, bank unit, and firm-specific effects. My central finding is that personal relationships between loan officers and firms have a significant positive impact on loan renegotiation. Firms with interrupted relationships are less likely to renegotiate a loan compared to firms experiencing continuous relationships. In addition, firms with interrupted relationships receive tougher loan terms on the loans that are renegotiated.

The empirical setting is based on the consolidation of a major commercial bank in Greece with business activity throughout the country. Bank network consolidation is a common response of banks to financial distress, as consolidation reduces operating costs and centralizes lending decisions.[5] During consolidation, some bank units are closed and the loan accounts from those units are merged with accounts in other surviving units. A bank unit closure interrupts personal relationships between loan officers and firms because merged accounts obtain new loan officers. Thus, after consolidation, two types of firms are identified: one, those whose loans were transferred to another unit and whose personal relationships were consequently discontinued, and two, those that remained at the same unit for the entire period.

The criterion for bank units' consolidation was geographic location. My identifying

[5]Several banks in Italy, Spain, Portugal have consolidated their network during the recent crisis as well as banks in the United States.

assumption is that the decision to close a bank unit is orthogonal to firms' characteristics. I find support for that assumption in the data by testing for differences in observable characteristics. First, I compare the loan terms and performance, and firm financial variables of borrowers in closed and open units in the pre-unit closure period and I find no statistically significant differences. Second, I provide out-of-sample evidence that the identifying assumption holds by using the Amadeus Bureau van Dijk dataset to test for differences based on the zip codes of bank units.

I apply a difference-in-difference methodology to estimate the effect of relationship interruption. I define as *treated* the firms with exogenously discontinued personal relationships and as *control* those with continuing relationships. Hard information passed from one loan officer to another because the transfer happened within the same bank. Observed differences between the two groups in the post-consolidation period should be driven by the consequences of interrupted personal relationships.

I find strong evidence for the significant effect of personal relationships on loan renegotiations at both extensive and intensive margins. Firms with interrupted relationships have a 26% lower probability of renegotiating at least one of their old loans upon their transfer to another unit, compared to firms that remained at the same unit. The unconditional probability of renegotiating a loan is 59% and the estimated effect corresponds to a 15% lower probability of renegotiation. Moreover, conditional on renegotiating a loan, affected firms received tougher loan terms on their renegotiated loans. The affected loan terms for firms whose accounts were transferred to another unit include higher interest rates, approximately 50% shorter maturities, and requirements that these firms pledge 70% more collateral per loan amount compared to firms that remained with their original bank unit for the entire period. The economic magnitudes of the impact on loan maturity and collateral are significant as they correspond, on average, to approximately a year's shorter maturity and an additional €0.88 of collateral for each euro of loan amount.

I also observe that firms alter their capital structure after their relationship with the bank is interrupted. The change in capital structure indicates that firms cannot substitute lending from other banks without cost when the relationship with one bank is exogenously interrupted. In particular, we observe that, when the relationship with the bank is interrupted, firms raise more equity and decrease their leverage. Firms only partially substitute loans from other banks to make up for the borrowing reduction from the bank whose relationship was severed, resulting in lower total debt. This change in a firm's sources of financing is likely to have important implications for the firm's business model and investment decisions.

A possible selection bias on renegotiation could change the interpretation of the results for loan term and performance differences between interrupted- and continuing-relationship loans. If the loan officer chooses to renegotiate with firms based on their performance, the intensive margin results will be biased. To address the concern of potential selection bias on renegotiation, I conduct two tests. First, I compare the pre-unit-closure period characteristics of the treated and control firms that renegotiated a loan in the post-unit-closure period and I find no statistically significant difference in pre-period measures of firm performance. Second, I consider all the hard information available to both the loan officer and the econometrician to identify the variables that can trigger a renegotiation. This test demonstrates that the loan officer who remained at the same unit for the entire period chose to renegotiate with treated firms with higher profitability prospects. Loan officer behavior, therefore, biases estimated coefficients towards zero, implying that my intensive margin results are conservative.

To further investigate the explanatory mechanism for the value of a relationship between a loan officer and a firm, I examine closer and more distant relationships separately. In most cases, a closer relationship entails fewer outside financing options, and when this closer relationship is interrupted, the expected effect is a significant informational loss. In contrast, firms with more distant relationships and, hence, greater outside financing options are expected to have stronger bargaining power. To test for the value of a stronger relationship, I construct two measures of relationship strength. The results from this analysis confirm the value of personal relationships, as a firm with a stronger previous loan-officer relationship displays a significant negative effect on its renegotiated loan terms when this relationship ends.

In the next section, I describe the paper's contributions to the literature. In section 1.3, I provide an overview of the institutional background and the dataset structure. In section 1.4, I present the empirical specification and a detailed comparison of the treated and control groups. The regression results are presented in section 1.5. In section 1.6, I conclude.

1.2 Related Literature

This paper contributes to the literature by being the first to combine two classic research streams—relationship banking and contract renegotiation—and by estimating the consequences of personal relationships between loan officers and firms for loan renegotiations.

The broader literature to which this study contributes addresses the role of relationships in credit markets. A rich theoretical literature on bank debt highlights the importance of informational asymmetry and moral hazard for financial intermediation.[6] Several empirical papers have examined the relationship between banks and borrowers for evidence to determine whether asymmetric information affects lending. Petersen and Rajan 1994 and Berger and Udell 1995 used the repeated interaction between a borrower and a financial institution as a measure of relationship. Mian 2006, Agarwal and Hauswald 2010, Canales and Nanda 2012, and Bolton et al. 2016 define relationship lending as a function of geographic distance. Ongena and Smith 2001 analyze the duration of a bank relationship with a firm. Sufi 2007 and Ivashina 2009 explore informational asymmetries in a lending syndicate. [7]

The empirical literature to which this paper is most closely related identifies the effects of personal relationships between bank employees and borrowers. These studies focus on how loan approvals or performance can be influenced by different factors such as cultural proximity (Fisman, Paravisini, and Vig 2017), social connections (Haselmann, Schoenherr, and Vig 2016), hierarchical and geographical distance (Liberti and Mian 2009), or the loan officer being on leave (Drexler and Schoar 2014). Several papers examine the effect on interest rates of strong interpersonal connections between a banker and a firm (Engelberg, Gao, and Parsons 2012), or the effect of a strong relationship as measured by the number of interactions (Herpfer 2017), or the effect of an interruption of a relationship caused by an

[6]Jaffee and Russell 1976; Stiglitz and Weiss 1981; Myers and Majluf 1984; Diamond 1984; Ramakrishnan and Thakor 1984;Sharpe 1990;Diamond 1991; Besanko and Kanatas 1993; Rajan and Winton 1995;Bolton and Freixas 2000

[7]Extensive surveys of this literature are provided by Ongenah and Smith 2000, Boot 2000, Srinivasan 2014, Kysucky and Norden 2015

executive's death or retirement (Karolyi 2017). Lastly, Hertzberg, Liberti, and Paravisini 2010 and Cole, Kanz, and Klapper 2015 provide insights on the effects of bank-specific governance policies on the moral hazard behavior of a loan officer.

Within the field of literature on relationship banking, this paper is the first to demonstrate how exogenous interruptions in bank-borrower relationships affect loan renegotiation. One important difference between new and renegotiated loans is the bank's prior exposure to the risk of default on pre-existing loans, which gives the borrower a stronger negotiation position. The risk of a higher probability of default is magnified in a crisis period, such as in Greece in 2010-2015, when banks faced very high delinquency ratios that drove their overall risk assessment. In such periods, the value of a successful renegotiation surges. This paper not only shows the effect of relationships on the probability of renegotiation, but also on the attributes of the loans that are renegotiated. Further, whereas existing literature focuses on estimating relationship effects on lending either at the extensive margin or on the interest rate only, this paper examines the impact on the three main variables characterizing the loan structure (i.e., interest rate, maturity, and collateral).

Given that the main focus of this paper is loan renegotiations, the analysis also contributes to the literature on contract renegotiation. Several influential papers have examined renegotiation in incomplete contracts as an out-of-equilibrium phenomenon.[8] Although the existing theory of contact renegotiation has evolved significantly, the empirical evidence on this topic is limited due to data restrictions. This paper investigates renegotiation independent of previous defaults on a loan payment, and for that reason is also related to the work

[8]Hart and Moore 1988, Rajan 1992, Aghion, Dewatripont, and Rey 1994, Von Thadden 1995, Bolton and Scharfstein 1996, Hart and Moore 1998, Maskin and Moore 1999, Gorton and Kahn 2000, Garleanu and Zwiebel 2008, Tirole 2010

of Roberts and Sufi 2009, who analyze in detail the factors triggering a renegotiation, and those determining its outcomes. Roberts 2015 is the closest to my paper, as he shows that a corporate loan renegotiation happens frequently, modifies significantly the initial loan terms, and is affected significantly by the duration of the lending relationship. Although similar conclusions to those found in these two papers arise in my analysis regarding the frequency and the outcomes of a renegotiation, my paper advances beyond these to estimate the effect of the relationship between a loan officer and a borrower on the probability of renegotiation and the nature of its outcomes.

A few empirical papers have considered other factors affecting loan renegotiation. The importance of the liquidation value of collateral (Benmelech and Bergman 2008) and of the mortgage securitization (Piskorski, Seru, and Vig 2010) on loan renegotiation are highlighted. James 1995 focuses on debt restructurings and shows that the financial condition of the firm determines the bank's incentives to make concessions. Lastly, Chodorow-Reich and Falato 2017 show the importance of the financial institution's health on contracting credit using covenant violations.

This paper also complements literature examining the impact of bank branch consolidation. In this area, the most relevant paper is by Nguyen 2016, as she examines how branch closures in the United States affect local access to credit. Lastly, this paper is linked to the strain of European debt crisis literature that employing micro–level data to identify the impact on bank lending.[9]

[9] Acharya et al. 2016, Bentolila, Jansen, and Jiménez 2016, Cingano, Manaresi, and Sette 2016, Popov and Van Horen 2015, De Marco 2016

1.3 Institutional Background & Data

Institutional Background

The identification setting and the data that I use in this paper come from a Greek bank and cover the period 2012–2015.[10] Several important facts characterize the economy and the banking sector of that period. From 2008 and until the end of 2016, the Greek GDP contracted by approximately 25%; unemployment rose to approximately 26% of the labor force, the highest level in the European Union; and investment declined by 75%. The collapse in investment was partially caused by a decreased access to credit. Access to finance was the most pressing concern for small- and medium-size enterprises (SMEs) operating in Greece, as 33% of SME owners consider this their most important problem.[11] This issue is critical given that SMEs account for more than 90% of private companies and 87% of total employment.

The Greek banking sector suffered during this period from a lack of access to international capital markets, deposit flight, and losses from the sovereign debt restructuring. Several banks were resolved and their deposits as well as a number of their loan portfolios were transferred to the four largest banks, thus causing a significant centralization of the sector. The four largest banks went through three large-scale recapitalizations (July 2013, May 2014, and December 2015) during this period. However, availability of long-term finance remained limited, and cost of credit was very high compared to EU standards. One of the main reasons for the limited credit supply was the deterioration of banking asset

[10] Gourinchas, Philippon, and Vayanos 2017 provide a detailed empirical and theoretical analysis of the Greek crisis.

[11] OECD, 2016 Financing SMEs Report

quality. In 2016, the nonperforming loans (NPLs) reached 45% of the loan portfolio, and provisions stood at 50% of total NPLs. In particular, in the corporate sector, about 60% of loans to small- and medium-size businesses were nonperforming. The rise of the NPLs ties up bank capital, thereby reducing profitability and increasing funding costs.

In this paper, I exploit a major internal reorganization that was implemented in one of Greece's largest banks, and led to the closure of bank units. It is important to clarify what a bank unit is and distinguish bank units from branch closures discussed in prior literature. In this case, a bank unit entails a center that manages corporate loans, and more centralized centers managing specific loan types. For the analysis that follows, a bank unit can be considered a type of branch, because, as in distinct branches, personal relationships develop between loan officers and the firm whose loans they manage.

The main goal of this internal reorganization was increased efficiency regarding NPL management. In response to the significant rise in the NPL ratio for corporate loans, the bank established specialized NPL workout units at the end of 2013. The new units were separate from the units responsible for loan origination and were responsible for monitoring, managing, or liquidating the nonperforming exposures. Approximately half of total loans to small- and medium-size corporations were transferred to these specialized NPL units.

This transfer of loans to the NPL units caused a significantly reduced workload for the original units assigned to manage corporate loans. Consequently, it became cost effective for the bank to consolidate the original units, by closing several and relocating the management of corporate loans to the closest unit that remained open. Originally there were 112 units that managed corporate loans, and after the mergers, there were 37. The bank intended to retain its network across the country, thereby ensuring clients would remain. For that reason, the main criterion for mergers was geographic location, and, further, mergers would only take place in areas already served by two or more units. The unit remaining open in a given area, would be the unit managing the largest volume of loans. An important feature of the consolidation was the relocation of the loan officers. Loan officers who had worked in units that closed were transferred to the newly established NPL units. Loan officers who worked in units that remained open continued to manage their old loans and became additionally responsible for the loans transferred from closed units. Because firms whose loans were transferred to the closest unit lost the relation with the loan officer who had managed their loans, the feature of unit mergers, provides a good setting for testing the effect of personal relationships on loan renegotiation.

Dataset structure

The main data used in this study come from one of the largest commercial banks in Greece. The dataset contains detailed annual information on corporate loans for approximately 8,000 small and medium nonretail enterprises (SME) covering four years (2012–2015). The construction of the sample is based on the ECB supervision guidelines for the Asset Quality Review (AQR). Under these guidelines, an SME is defined as a corporation that has annual turnover up to €50 million and employs fewer than 250 persons.

For several reasons, a restricted sample of nonretail SMEs is the most appropriate sample for the proposed analysis. First, it is necessary to exclude large corporations as they have access to other sources of financing, such as international banks and the stock market.

Moreover, credit for large corporations is approved at a centralized, and higher level at the bank, and for that reason, personal relations with loan officers are expected to be irrelevant. Second, the retail sector is also excluded from the dataset, as a different department at the bank manages this sector, and further approvals were required to obtain that dataset. By focusing on nonretail SMEs, I gained access to credible firm financial information, since the majority of the firms in the sample have audited financial statements, which they are required to submit to the bank. Moreover, by excluding very small firms, we can expect sample firms to be unaffected by the narrow local economic environment. Firms in the sample operate either regionally or nationally and their performance is expected to be affected by the economic conditions at the region and industry level.

The dataset includes detailed information on the loan terms and performance as well as basic firm financial information. Each firm has multiple loans at this bank, and the loan types vary from the more secure, such as leasing, to the less secure, such as factoring, letters of credit, revolving credit, and corporate bond loans. Moreover, an indicator of the bank unit responsible for each loan is included, which allows for tracking transfers across units. Personal relationships develop between loan officers and firms at the bank unit and changes in the bank unit indicator reflect interruptions of such relations.

Descriptive Statistics

As I focus on the merger of the original bank units, I exclude the loans that were transferred to the specialized NPL workout units. For that reason, the sample included is not representative of the Greek economy during this period, but rather represent the set of firms that performed relatively well during the crisis.

The final sample consists of loans to 3,984 firms located across the 13 geographic regions of the country. Following the merger, a single bank unit managed on average 340 loans to 107 firms. The original dataset includes loan-level information, but the main unit of analysis in this paper is at the firm level. The reason for this aggregation is to avoid duplication within firms, which could bias the results. To aggregate the dataset at the firm level, a simple summation is used for the exposure and the collateral value. For the interest rate, the maturity, and all performance-related variables, a weighted average is estimated, using as weights the ratio of the specific loan exposure over the total exposure of the firm.

Table 1.1 presents the summary statistics for the main variables in the pre-period (2012, 2013). A median firm in the sample has two loans with a total balance of approximately €536,000 and total collateral cover of €187,630. The average interest rate is 5.97%, and the average remaining maturity of the loans is approximately one and a half years (528.34 days). The median firm performs well, with no nonperforming exposures, and has not delayed a payment. Regarding the firm's financial information, the median firm was medium-size with approximately €6 million in total assets and €4.25 million in total debt. It has a positive EBITDA of approximately €270,000, and a high leverage equal to 0.69. The summary statistics confirm the fact that the sample is comprised of firms that performed relatively well during the crisis.

1.4 Empirical Methodology

Identification Strategy

The identification of relationship interruption is based on a bank's internal reorganization and the closure of bank units. I employ this exogenous variation, define appropriate treatment and control groups, and apply the difference-in-difference methodology to accurately estimate the effect of interrupted personal relationships between loan officers and firms on loan repayments.

The first step is to exclude loans were transferred to the specialized NPL units. The sample is thus constrained to relatively "good" firms. The next step is to identify the firms whose accounts were transferred to another unit because their original unit closed. By looking at the transfers across bank units, we observe a set of firms whose accounts were transferred to other bank units without their original unit closing. (This set of observations accounts for approximately 11% of the original sample.) It is necessary to exclude these firms from the analysis, due to the endogeneity of their transfer.

By using the closure of the original bank unit as the source of exogenous variation, I overcome the selection bias that may arise at the firm level. In particular, treated firms are defined as those whose loan accounts were transferred to another bank unit because their original unit closed. Control firms are defined as those whose loan accounts remained at the same unit during the whole period of the sample. This specification of treatment and control groups ensures that the variation comes only from the bank unit level and not from the firm level. Loans transferred from one unit to another as a result of a cause other than unit closure are excluded from the sample.

The feature that allows me to identify an interruption in personal relationships between loan officers and firms is the relocation of loan officers. Loan officers who worked in units that closed were transferred to the new NPL units. Loan officers who worked in the units that remained open continued to manage their old loans and became responsible for the loans that were transferred from the closed units. For that reason, firms that remained at the same unit continued to interact with the same loan officers, while firms whose accounts were transferred had to establish a new relationship with a different loan officer.

The baseline specification is a difference-in-difference, which allows me to compare the difference on the outcome variables between the firms in the treated group and those in the control group in the post-unit closure period (2014, 2015) relative to the difference that the two groups had in the pre-unit closure period (2012, 2013). The baseline regression is:

$$y_{iurt} = \alpha_i + \alpha_u + \alpha_{rt} + \delta(Post_t * Treat_{iur}) + \varepsilon_{iurt} \tag{1.1}$$

where y_{iurt} stands for the outcome variable for firm i obtaining a loan from bank unit u and located in region r in year t. $Treat_{iur}$ is a dummy variable equal to one for treated firms and zero for control firms. $Post_t$ is a dummy variable equal to zero for the period before the bank units' closure (from 2012 to 2013) and one after the closures (from 2014 to 2015). The baseline specification includes firm fixed effects (α_i) to capture any time-invariant characteristics of the firm. Bank unit fixed effects (α_u) as well as region $*$ year fixed effects (α_{rt}) are included to capture any time-invariant characteristics of the bank unit (e.g., a specific bank unit may have a higher lending limit) and any region and time-varying shocks respectively. For the treated firms, the bank-unit fixed effect is based on the unit to which their loans were transferred. The coefficient of interest is δ, which measures the difference in the outcome variable for the firms that experienced an interruption in their personal relationship with the loan officers, relative to the firms that did not, controlling for the pre-period difference.

The outcome variables of interest belong to four groups: (i) the probability of renegotiating a loan (extensive margin); (ii) renegotiated loan terms (intensive margin); and (iii), firm level effects. Regarding the loan terms of the renegotiated loans, I use as outcome variables the interest rate, the remaining maturity of the loan, and the collateral value relative to the loan balance. I also construct two additional variables to help capture the effect of collateral. The first is an unsecured loan dummy and the second is a type-of-collateral dummy. To capture firm outcomes from an interrupted relationship, I examine the firm's total assets, total debt, total equity, the ratio of equity over assets, and the firm's leverage estimated by the ratio of total debt over total assets.

Comparison of treated and control groups in the pre-unit closure period

The most important threat to identification is a possible selection bias that arises from the decision to close a bank unit. It is necessary to assume that the bank did not close units where debtors performed, or were expected to perform, worse. The main criterion for unit closure is geographic location: in areas where there were two or more units, the bank kept only one, while in areas with only one unit, it was optimal for the bank to keep it open to serve the main priority of client retention.

A comparison between the two groups in the pre-unit-closure period (2012–2013) provides evidence that there are no statistically significant differences across the two groups. Table 1.2 presents this comparison and includes variables related to loan terms, performance, and firms' financials. Column 1 shows the mean value and the standard deviation for the control firms, Column 2 for the treated firms, and Column 3 shows the *p*-value for the difference, with regional fixed effects included. The only variable significantly different across the two groups is the ratio of loans transferred to the NPL units from the originals: this variable is higher for the bank units that remained open. This difference indicates that, to the extent that units were selected for closure based on loan performance, the bank units that closed had better performing loans. For that reason, if a selection bias exists on which units closed, it will bias the results downwards. The fact that we do not observe any statistically significant difference for the loan terms, performance, and firms' financial information supports the assumption that the two groups shared similar characteristics. The probability is therefore high that the two groups would have continued to look similar if the personal relationships with loan officers had not been interrupted.

Out-of-sample comparison of firms located in exposed and control geographic areas

In this section, I perform an out-of-sample comparison of firms located in geographic areas where a bank unit closed (exposed areas), and firms located in areas where a unit remained open (control areas). This test provides further evidence that the local economic conditions, and the firms operating in exposed and control areas, are similar. The data for this comparison come from the Amadeus Bureau van Dijk dataset and cover the same period of the experiment (2012–2015). The Amadeus dataset provides information on firms' balance sheets and income statements. In addition, I use the bank units' zip codes to identify exposed and control areas. I match the firms' zip code with the bank units' zip code, and construct a subsample of the Amadeus dataset appropriate for the out-of-sample comparison.

Table 1.3 presents the results comparing firms located in exposed and control areas for the same period of the experiment (2012–2015). The first column shows the mean value and the standard deviation of firms located in the zip code areas where a bank unit remained open, and the second column for firms located in areas where a unit closed. The third column shows the *p*-value of the difference. The two groups are similar across most of the variables. There is a small and significant in total assets and the number of employees between the two groups, which suggests that firms in control areas are larger. To account for this difference, I include the firm's total assets as a control variable in the baseline results in section 1.5. Moreover, in subsection 1.5, I restrict the sample to areas in which firms have similar characteristics to provide further evidence that local economic conditions do not explain differences between control and treated groups.

The main variables of interest in Table 1.3 are those related to firms' financial performance. Any difference in these variables would suggest that the economic conditions differ between exposed and control areas. No statistically significant differences in these variables are observable (EBIT, EBITDA, net income, sales, gross profit, etc.).

Lastly, to capture the potential differences associated with unobserved economic indicators, I use the firms' financial characteristics as dependent variables and apply the baseline regression 1.1 to examine whether there is an out-of-sample effect of a bank unit closure. Column 4 of Table 1.3 shows the estimated coefficients from the difference-in-difference regressions on firms' variables. No coefficient is statistically significant. The fact that there is no statistically significant difference across any specification for the observable characteristics of firms' financial characteristics supports the assumption that economic conditions in the exposed and control areas are similar.

1.5 Results

First, I report the baseline results on loan renegotiation. I examine the impact of personal relationships between loan officers and firms on the probability of renegotiating a loan and, conditional on renegotiating a loan, I estimate the impact on the loan terms. Firm-level effects are included. Lastly, I examine how the strength of the relationship affects the results by using three different measures of relationship strength.

Baseline Results

Effect on Probability of Renegotiation

First, I analyze the extensive margin and the probability of renegotiating a loan after a firm's loans are transferred to another bank unit. I compare firms that experienced an exogenous interruption in the personal relationship with their loan officer with those that did not. The outcome variable of interest is the probability that a firm renegotiates at least one of its pre-existing loans. The data in this section are constructed at the firm level. More formally, the dependent variable is a dummy variable equal to one if the firm renegotiated at least one of its old loans and zero if the firm did not renegotiate any loan. Table 1.4 presents the results with different specifications regarding fixed effects and control variables. It can be seen that firms whose relationship with a loan officer is interrupted have a 26% lower probability of renegotiating at least one of their pre-existing loans, compared to firms with uninterrupted relationships, when firm, bank unit, and region-year fixed effects are included.

A loan renegotiation can be initiated either by the bank or by the firm and does not require a delayed loan payment. A renegotiation is expected to be mutually advantageous, as otherwise one of the parties would not agree to the new terms. The firm benefits by renegotiating a loan, because one or more of the initial loan terms is relaxed. At the same time, the bank prevents a default or improves its covenants. Since renegotiation benefits the firms, firms with interrupted relationships receive worse treatment as the result of the transfer of their loans to another bank unit.

The results from four different specifications are presented in Table 1.4, and the results are robust. In all four specifications, bank unit fixed effects are included to capture any unit-level time-invariant variation, and the standard errors are clustered at the unit level. Even though bank lending policies in general are similar across units, a larger unit or a unit that is higher in the organizational hierarchy may have different limits on loan amounts and the terms it is allowed to approve.

Moreover, I control for the regional differences by adding region or region-year fixed effects. In the first column, region and year fixed effects are included, while in the second column region-year fixed effects are included. When constructing the region-year fixed effects, I define the region more broadly than strict geography to ensure that at least two bank units are open per region in the post-unit closure period and I combine only neighboring regions that have similar industry composition. No adjustment is done on regions that have two or more bank units in the post-period. The specification defined in Column 2 is the baseline specification of the paper.

To control for firm level differences, in Columns 1 and 2 firm fixed effects are included to capture any time-invariant characteristics of the firm such as size, industry, etc. In Columns 3 and 4, I add different types of firm-level time-varying control variables. In Column 3, as control variables, the firm's lagged log of EBITDA and the lagged log of total loans at this bank are included. In Column 4, the lagged log of total assets is included as a control variable to control for changes in the size of the firm. The results remain robust when we control for time-varying firm characteristics, and this confirms that variations at the firm level during the years of the experiment are not driving the estimated coefficients.

Effect on Possible Renegotiation Outcomes

After a renegotiation, both the loan terms and loan amount can be altered, depending on the firm's needs and the bank's constraints. A renegotiation can have one or more of the following outcomes: an increase in the loan amount (in particular, in the case of a credit line), a decrease in the interest rate, an extension of the loan maturity, and either an increase or a decrease in collateral value. An increase in collateral is in most cases necessary if the loan amount is increased, but can be a requirement under other renegotiation outcomes as well. A decrease in collateral is possible, as it may be optimal for the bank to free up part of the assets previously pledged as collateral to let the firm use them for new loans.

Table 1.5 shows the possible renegotiation outcomes and the unconditional probability for a firm receiving each of these. By renegotiating a loan, more than one of the loan terms can change, and for that reason, the unconditional probabilities do not sum to 100. The most common outcome is an increase in collateral pledged, as this often is a requirement for other changes, and the least common change is an extension in loan maturity.

I estimate the effect of an interruption in the personal relationship with a loan officer on the probability of receiving each of the possible outcomes. Table 1.5 presents these results using the baseline regression specification. Firms with interrupted relationships have a lower probability compared to the control firms to receive any renegotiation outcome, except for a decrease in the interest rate. In particular, they have approximately 20% lower probability of increasing the loan amount, of increasing the loan's collateral, and of decreasing the collateral. Treated firms have also approximately 5% lower probability of extending the loan's maturity.

Effect on Loan Terms Conditional on a Renegotiation

Tables 1.6–1.9 present the results for the newly agreed loan terms after renegotiating a loan under different specifications. Overall, we observe that firms with interrupted loan officer relationships receive worse terms. Specifically, they receive 1% higher interest rates, 50% shorter maturities, and they have to pledge a 70% higher value of collateral relative to the total loan balance. Even though the estimated effect on the interest rate is statistically significant, the economic magnitude is small. In contrast, the estimated effects on maturity and collateral are both statistically and economically significant. This difference may be related to the fact that a range for the interest rate is generated by an automated program, while the maturity and the collateral are determined by the negotiation with the loan officer.

Interest Rate: Table 1.6 presents the results on the log of the interest rate for different empirical specifications. From the baseline regression, we observe that firms with interrupted loan officer relationships receive a 1% higher interest rate on renegotiated loans, compared to firms that remained at the same bank unit. The economic magnitude of this difference is small as it corresponds to only 0.07 basis points (likely due to the automated program-generated interest rate).

Maturity: Firms whose relationships with their loan officers are interrupted appear to receive significantly shorter maturities on their renegotiated loans. Table 1.7 includes the results for the remaining maturity, as measured by the log of remaining days. Based on the baseline specification, treated firms receive approximately 50% fewer days remaining on their renegotiated loans, which corresponds to approximately one-year-shorter maturities.

Collateral: The outcome variable that is used to estimate the effect on the collateral is the ratio of collateral value relative to the total loan balance. An absolute effect on collateral value would not be informative, as it could be driven by an increase in the loan amount. Table 1.8 shows the results on the log of collateral value relative to the total balance for renegotiated loans. Firms with interrupted loan officer relationships pledge a significantly higher value of collateral. The baseline specification shows that treated firms have to pledge 71.6% more collateral relative to the loan balance after their loans are transferred, which corresponds to and additional €0.88 of collateral for each euro of loan amount.

Loan amount: There is mixed evidence regarding the loan amount. Table 1.9 presents the results on the log of the total balance of the firm's renegotiated loans that performed well. This is a subset of the renegotiated loans, as nonperforming loans are excluded. This restriction is necessary because the loan balance can increase or remain the same, due to either the loan amount increased or the debtor delayed the loan payments. For that reason, the results presented in Table 1.9 should be seen as suggestive. The estimated result varies under different specifications. In particular, under the baseline specification, firms with interrupted relationships do not receive any statistically different loan amounts in the renegotiated loans relative to firms in the control group. When we control for lagged firm characteristics in Columns 3 and 4, however, firms with an interrupted relationships present higher loan balances on their renegotiated loans.

Effect on Renegotiated Loans' Collateral Type

In Table 1.8, the effect of sustained personal relationships on relaxing collateral requirements is presented, and the economic magnitude of this effect is large. In this section, I focus on the qualitative information regarding collateral types to further investigate the effect of continuing personal relationships on collateral. The value and the type of collateral are determined by the loan officer, not by an automated program (as is the case with interest rates), and for that reason, a measurable impact of personal relationships is expected on the collateral-related variables.

I construct two dummy variables that explore the impact of renegotiation on collateral pledged. The first is an *Unsecured Loan* dummy, which is one if the loan does not have any collateral pledged and is considered unsecured, and zero otherwise. Approximately 50% of the loans included in the analysis were unsecured in the pre unit closure period, and no statistically significant difference occurs between the treated and the control groups.

In addition, I construct a second dummy variable that quantifies the effect on the type of collateral. This is potentially important because the enforceability of collateral depends on its type. I construct a *Secure Type of Collateral* dummy that is equal to one if the collateral is considered highly secure and zero otherwise. I define real estate (commercial and residential), ships, deposits, and debt securities as highly secure. As less secure collateral types, I define accounts receivable, equities or convertible bonds, and other types of guarantees. Both the treated and control groups have pledged highly secure types of collateral in approximately 50% of their collateralized loans. In 2012 and 2013, approximately 36% of the secured loans employ receivables as collateral, 30% use real estate, and 20% use deposits.

Table 1.10 presents the results for the two collateral variables. Column 1, shows that firms in the treated group have an 18% lower probability of having an unsecured loan after a renegotiation. Column 2, shows that an interruption in the personal loan officer relationship induces firms to pledge more highly secure collateral on their renegotiated loans. In particular, treated firms are 31.3% more likely to pledge highly secure collateral compared to control firms.

Addressing possible threats to identification

Identifying Possible Selection Bias on Renegotiation

One concern regarding the validity of the estimated effects of an interrupted relationship is a possible selection bias concerning whether a loan is renegotiated. In subsections 1.4 and 1.4, I demonstrated that firms in the treated and control groups share similar pre-unit closure characteristics. Two of my main findings are that firms with interrupted personal relationships have a lower probability of renegotiating a loan (subsection 1.5), and conditional on a renegotiation, these firms receive worse loan terms (subsection 1.5). If loan officers choose to renegotiate with interrupted-relationship firms based on different criteria than those used for the continuous-relationship firms, then the intensive margin results would be biased. If loan officers for interrupted-relationship loans granted renegotiation more frequently for firms with relatively inferior performance, while loan officers for continuing-relationship firms did not do so, then the estimated effect of continuing relationships on the probability of renegotiation would exaggerate the true effect of the continuing relationship. In fact however, I find the opposite, implying that my estimated effect of continuing relationships on renegotiation terms is conservative.

I conduct two tests, which are presented in Tables 1.11 and 1.12. The first test compares the two types of firms that renegotiated a loan in the post-period. In Table 1.11, a similar analysis as in Table 1.2 is conducted, but for this test, I include only firms that renegotiated a loan in the post-unit closure period, and compare the pre-unit closure period characteristics between the treated and control groups. We observe no statistically significant difference in any of the variables between firms with interrupted and continuing relationships that renegotiated a loan in the post-unit closure period. Firms in the two groups that renegotiated loans in the post-unit closure period shared similar characteristics in the pre-period, which suggests no selection bias exists regarding whether a loan is renegotiated.

Table 1.12 presents a second test, which considers observable variables that can prompt a renegotiation. I rank firms within each bank unit based on the year-before-the-transfer value for each variable. The decision to renegotiate a loan is made by the loan officer at the bank unit, and, for that reason, any correlation outside the unit would not be informative for the criteria used to renegotiate a loan at the bank unit. The rank within the bank unit allows me to control for the variation at the bank unit and any criterion based on hard information used by the loan officer. I regress a dummy variable for renegotiating a loan on the firm's within-bank-unit rank separately for each variable and include bank-unit and region-year fixed effects. This test provides us with correlations that show which firms have a higher probability of renegotiating a loan, based on the previous year's hard information available to the loan officer and to the econometrician. Table 1.12, Column 1 presents the result for the whole sample (pre- and post-unit closure period for treated and control firms), Column 2 for control firms in the post-period, and Column 3 for treated firms in the post-period.

The most interesting observation comes from the estimations of the year-before-the-transfer EBITDA growth rate. We observe that, for the whole sample and for the control group in the post-period, there is no statistically significant effect, while for the treated group in the post-period there is a positive statistically significant correlation. As noted above, this finding indicates my estimates are conservative.

Table 1.12 implies a common rule with respect to the firm's leverage, total loans at the subject bank, and total loan-loss provision is applied for all firms, independent of whether they belong to the treated or control group. Moreover, firms in the whole sample and in the control group appear to be selected similarly for renegotiation based on their total assets, total debt, debt over EBITDA, and payments' days past due. In contrast, we do not observe a correlation between these variables and renegotiation for the treated group. Again, this finding indicates conservative estimates.

Controlling for firms' differences in zip codes locations

Subsection 1.4 discussed the observed differences between firms located in areas where a bank unit closed (exposed) and in areas where a bank unit remained open (control). Based on the zip code comparison using the Amadeus dataset presented in Table 1.3, firms located in the exposed areas are smaller in size. Even though I control for the log of total assets in the estimated results, in this section, I provide further evidence that firms' differences across geographic locations are not driving the results.

To accurately control for potential differences in the geographic location of the exposed and the control areas, I restrict the sample to areas in which firms across the two areas have no statistically significant difference in financial variables. In particular, I exclude firms located in Attica, the region where Athens is located, and I repeat the analysis. Table 1.13 presents the results of the subsample on the comparison of exposed and control areas using the Amadeus dataset. This finding confirms that no significant difference in financial variables obtains between the two groups in the subsample.

The next step is to show that, when I restrict the sample to areas where firms across the two groups are similar, the estimated results on the outcome variables hold. Table 1.14 present these results. The estimated coefficients for both the probability to renegotiate a loan and the intensive margin results on loan terms are similar in economic significance to the baseline results. This confirms that the baseline results are not driven by the differences on the firm's characteristics nor by the economic conditions at the zip-code level.

Controlling for bank unit size

If more than one unit operated in the same geographic area, the bank decided to keep the larger units open and close the smaller ones. For that reason, a possible concern regarding the identification strategy would be whether the difference in the size of the bank unit where the accounts were transferred can explain the results. To address this concern, I perform an analysis controlling for the size of the bank unit to which the loans were transferred.

I rank the units based on the number of corporate borrowers they manage in 2013. Figure 1.1, I plot an increasing series of the number of borrowers per bank unit versus the rank of the bank unit. From this plot, it is obvious that the larger units remained open and the smaller ones closed.

To test for an effect from the relative size of the bank unit, I construct first a measure of the unit's size based on the number of accounts managed per unit in 2013, the year before the reorganization. The relative rank of the units' size, for those that remained open, did not change after the reorganization. Second, I filter the observations based on the size of the unit where the treated firms were transferred. In particular, I restrict the sample of the treated firms to those transferred to a bank unit that was at most 20% larger than their original unit. Moreover, I restrict the control group and include only these firms managed at the unit receiving firms from units that were at most 20% smaller (from the "filtered" treated group).

After I construct the subsample that allows me to control for unit size, I repeat the analysis of the outcome variables of interest. The results are presented in Table 1.15. The results on the probability of renegotiating a loan and on the intensive margins of loan terms bear similar magnitude and significance to the baseline results. These findings show that the baseline results in all three groups of outcome variables are not driven by differences in unit size.

Other Firm-Level Effects

This section considers other effects on firms of a bank unit closure. Table 1.3 Column 4 shows no average real effect and, in particular, no effect following a bank unit closure on the firms located at the same zip-code area. In this section, I examine whether the bank unit closure and the interruption in the loan officer relationship is associated with any effects on the capital structure of the firms that borrowed from the closed units.

To test for firm-level effects, I apply the baseline specification 1.1 to the firms' financial variables provided by the bank. By using the bank data, I restrict the sample only to the bank clients. Table 1.16 presents these results. The main conclusion is that firms with interrupted loan officer relationships change their capital structure and their main sources of financing. In particular, relative to control firms, treated firms raise three times more equity, decrease their total debt by 78%, and decrease their total assets by 61%. These changes in the firms' capital structure cause a 6.4% decrease in the treated firms' leverage (measured by the ratio of total debt over total assets) and a 9% increase in their level of equity over assets. Moreover, I examine the effect of an interrupted relationship on a substitution of lending from other banks. I measure substitution by constructing a new variable, the *dependence ratio*, that is equal to the ratio of the amount of debt that a firm has at this bank relative to the total amount of its debt. The dependence ratio decreased 10%

for treated firms relative to control firms, suggesting that treated firms partially substituted loans from the subject bank with new bank loans.

The results show that the main sources of external financing are altered in the case of firms with interrupted relationships. The negative effect on both total debt and the dependence ratio shows that firms increase their relative lending from other banks, but they substitute only partially their total debt from other banks when their relationship with the subject bank is interrupted. These results, combined with a significant increase in equity, suggest that, when firms experience an interrupted relationship, they cover their financing needs with new sources of funds, including funds from other banks. These effects may explain observed changes in investment decisions, as implied by the decrease in total assets.

Results by Relationship Strength

In this section, I further investigate the value of a relationship between a loan officer and a firm. I construct two different measures of relationship strength and compare the impact of an interrupted relationship on firms with stronger relationships and those with weaker relationships. The main result of this section is that the interruption of a strong relationship with a loan officer has a more significant negative effect on the renegotiated loan terms. This conclusion is consistent across the three measures of relationship strength.

Relationship Strength Measured by the Dependence Ratio

The first measure of relationship strength is the ratio of a firm's total amount of loans at this bank over its total debt to other banks. This measure shows whether a firm had an established relationship with other banks or whether it borrowed predominantly from the bank featured in this study. One of the main assumptions for a mutually advantageous renegotiation is that the firm has outside options for financing, as those options would increase the firm's bargaining power. For small-and medium-size corporations,, the outside options for financing are either other local banks, or raising inside or outside private equity, as these relatively smaller firms do not have access to the stock market or from foreign banks.

It follows that if a firm borrows from other banks in the pre-period, and has an established relationship with them, it is easier for the firm to seek financing from other banks once the relationship with the subject bank is interrupted. On the other hand, if the firm depends mostly on the subject bank to satisfy its financing needs, then its negotiation power is limited. This section provides a comparison of these two groups, i.e., firms with closer relationships and fewer outside options versus firms with weaker relationships and more outside options.

As a measure of how close the relationship is, I estimate an indicator variable which I label the *Dependence Ratio*. The *Dependence Ratio* is defined as the ratio of the total balance at this bank over the total debt that a firm had to other banks in 2013, the year before the bank units' closure. First, I estimate the correlation between the *Dependence Ratio* and the probability of renegotiating a loan. Table 1.17 presents the correlation results. The first two columns show the results for the whole sample in the pre- and the post-unit closure period, where Columns 3 and 4 report results for the control firms, and Columns 5 and 6 for the treated firms. There is a positive and significant correlation between the dependence ratio and the probability of renegotiating a loan, suggesting that firms with closer relationships have a higher probability of renegotiating. Note that this correlation is not significant for the treated group in the post-period, although it was significant in the pre-period. This suggests a loss in the value of close relationships between the firm and the bank once the relationship with the loan officer is interrupted. Treated firms that borrowed predominantly from the subject bank in the pre-unit-closure period do not have a higher probability of renegotiating in the post-unit-closure period.

Figure 1.2 presents the density distribution of the *Dependence Ratio* in 2013. The higher the value of the ratio, the more dependent the firm is on this bank. The lower the value of the ratio, the less important this bank is to the firm, since it borrowed from multiple sources. As shown in Figure 1.2, the majority of firms borrow from multiple banks, while a smaller number borrows mostly from the subject bank. In the following analysis, I test separately the effect of interrupted relationships for firms with closer relations, and for those with more distant ones.

Tables 1.18–1.19 present the results for these two subsamples. Firms with a *closer relationship* are defined as those with a dependence ratio above 50%. Firms with a *less close relationship* are defined as those with a dependence ratio below 20%.[12] The results are estimated using the baseline empirical specification 1.1. Table 1.18 shows that, independent of whether the firm had a close relationship with the subject bank in the pre-unit closure period, the firm has a 28% lower probability of renegotiating a loan after the relationship is interrupted. Table 1.19 includes the results of the loan terms. Firms with a closer relationship pay a higher cost from interruption by receiving tougher loan terms on the renegotiated loans. In particular, firms with a close relationship that was interrupted receive higher in-

[12] The cutoffs were chosen to secure enough statistical power to the tests. The results remain robust under different cutoff thresholds, and tables are available upon request.

terest rates and shorter maturities, and have to pledge more collateral per loan balance, compared to firms with closer and uninterrupted relationships. On the other hand, firms with a weak relationship and more outside options for financing from other banks receive better loan terms on their renegotiated loans if the relationship with the subject bank is interrupted. In particular, they receive a lower interest rate and longer maturities, and have to pledge less collateral per total balance compared to firms with weak and uninterrupted relations.

The effect on firms with weaker relationships can be driven by two different channels. First, concerning firms for which only a small part of their total debt originated at this bank, the value of a personal relationship with a loan officer is very small. In that case, an interrupted relationship has little effect. A second channel explaining the results is that firms with established relationships with other banks have a stronger bargaining position with the subject bank. These firms with outside options can walk away more easily from the subject bank and negotiate better loan terms elsewhere. In these cases, we should see the continuing relationship with a loan officer at the subject bank as an incentive to retain the clients at the subject bank. Better bargaining position and a willingness to exercise it would also translate into better loan terms, as we observe in Table 1.19.

Relationship Strength Measured by the Relative Size of the Firm

In this section, I use as a third measure of relationship strength the relative size of the firm as a corporate client within a bank unit. I estimate this measure as the ratio of the total balance of the firm over the total amount of loans that each bank unit manages in 2014, which captures the exposure of that bank unit to each corporate borrower: The larger the ratio, the larger the exposure of the bank unit to this client. The relative size of the firm is potentially correlated with two effects. Although, for larger firms, the potential profit margin for the bank is higher, the bank is also exposed to a higher risk. As both the profit margin and the risk are higher for larger firms, it is expected that loan officers will have a stronger incentive to establish closer relationships with larger clients.

Figure 1.3 shows the density distribution of the relative size of the firm within the bank unit. As expected, the distribution is highly skewed, and the majority of the firms are small clients for the bank unit. The indicator of relative firm size is estimated based on 2014 data, the first year after the transfer occurred. I use the relative size after the transfer to control for different behaviors towards the large clients in the post-period independent of whether a firm was in the treated or control group. In the following tests, I look separately at larger and smaller firms. I define as larger firms those whose balance at the specific bank unit accounts for at least 1% of the total amount of corporate loans the unit manages. The rest of the sample is defined as relatively small firms.[13]

Tables 1.20–1.21 present the results for larger and smaller firms separately. From the results on the extensive margin, we observe that firms with smaller balances and an interrupted relationship have a lower probability of renegotiating a loan following the transfer to another bank unit. On the other hand, for firms with larger balances, no statistically significant difference obtains between the treated and control groups. Regarding the results on the intensive margin, firms with larger balances are affected the most when their relationship with a loan officer is interrupted. In particular, treated firms with a large balance

[13]The cutoffs were chosen to secure enough statistical power to the tests. The results remain robust under different cutoff thresholds, and tables are available upon request.

receive a higher interest rate and shorter maturities, and they have to pledge a higher value of collateral. For treated firms with smaller balances, there is no statistically significant difference from the control group on the renegotiated loan terms.

From the analysis based on the relative size of the firm, I identify important heterogeneity. Firms with smaller balances, once their relationship with the loan officer is interrupted, have a lower probability to renegotiate a loan; however, conditional on a renegotiation, they receive loan terms similar to those of the firms with continuous relationships. This finding suggests that the value of a relationship for small firms affects the extensive margin to renegotiate a loan. Larger firms with interrupted relationships have the same probability of renegotiating a loan as larger firms with uninterrupted relationships; however, conditional on a renegotiation, they receive tougher loan terms. This effect suggests that, when a loan officer manages a client with whom he or she does not have an established relationship, the officer compensates for the higher risk by requiring tougher loan terms.

Results by Firms Pre-Unit-Closure Period Performance

In this section, I analyze whether the firm's pre-unit-closure period loan performance influences the estimated results. I separate the sample between firms that delayed a loan payment in 2013, the year before the transfer, and firms that paid their loans on time. Tables 1.22–1.23 present the results of the main outcomes of interest separately for the two groups. A common conclusion is that the results are consistent independent of the previous behavior of the firm. We observe that both firms with good repayment behavior and those with delays in loan payments have a lower probability of renegotiating a loan when their loan officer relationships are interrupted. The results on the intensive margin are similar: Independent of the previous repayment behavior of the firm, treated firms receive tougher terms on their renegotiated loans.

Overall, the results indicate that the impact of an interrupted relationship on the probability of renegotiating a loan, and on the renegotiated loan's terms, is the same for firms with good repayment histories and for those with delays in their loan payments. These findings therefore support the hypothesis that firm characteristics are not driving the estimated results.

Results on New Loans

Although this paper's central goal is the analysis of the impact of personal relationships on corporate loan renegotiations, this section presents the results for new loans to ensure that a firm is not substituting a loan renegotiation with a loan origination. I use the same variation of a bank unit closure to define the treatment indicator, but in this case, I look only at the post-period new lending (years 2014, 2015). The regression that is used is the following:

$$y_{iurt} = \alpha + \alpha_u + \alpha_{rt} + \delta * Treat_{iur} + \varepsilon_{iurt} \tag{1.2}$$

where y_{iurt} stands for the outcome variable of interest for the firm i located in region r in year t getting a loan from the bank unit u. $Treat_{iur}$ is a dummy variable equal to one if the firm belongs in the treatment group (if the firm's loan accounts were transferred to another pre-existing bank unit because the original unit closed) and is equal to zero if the firm belongs to the control group (if the firm's loan accounts remained at the same unit for the whole period of the sample). The specification includes bank-unit fixed effects (α_u) as well as region-year fixed effects (α_{rt}). The coefficient of interest is δ, which measures the difference in the outcome variable for the firms that experienced an interruption in the personal relationship with the loan officer responsible for managing their loan accounts, relative to the firms that did not.

Table 1.24 presents the results on the new lending both at the extensive margin and at the intensive margin. Firms with interrupted relationships received 30% fewer new loans in the post-period than firms with continuous relations. A significant difference between the two groups is observed also on the new loans' terms. Treated firms received almost 90% lower volume of lending, with higher interest rates and shorter maturities.

The results on the new loans' extensive and intensive margins confirm further the value of personal relationships in corporate banking. Firms that lost their personal relationship with the responsible loan officer conclusively receive less lending and tougher loan terms both on new and renegotiated loans. Moreover, Table 1.16 illustrates that a firm with an interrupted relationship partially substitutes its total debt through other banks, as both the dependence ratio and the total debt decreases at the same time.

1.6 Conclusion

This paper's main conclusion is that personal relationships between a loan officer and firm mitigate the cost of distress for the firm in a loan renegotiation. Relationships help eliminate frictions that arise in loan renegotiation and when these relationships are interrupted, the renegotiation outcome is less likely to be the optimal contract. Using the consolidation of bank units as a source of exogenous variation, I analyze a proprietary dataset on corporate loans. The main finding is that the firm is worse off following the interruption of its loan officer relationship, as it is less able to renegotiate, and the firm also receives tougher loan terms on renegotiated loans. Moreover, I observe that firms alter their capital structure after their relationship with the bank is interrupted. The change in the capital structure suggests that firms cannot substitute lending from other banks without cost when the relationship with one bank is exogenously interrupted. Lastly, I examine closer and more distant lending relationships separately and I find that firms with stronger previous loan-officer relationships display a significant negative effect on their renegotiated loan terms when the relationship ends.

1.7 Figures and Tables

Table 1.1: Summary statistics based on the 2012–2013 values

Variable	Mean	Std. Dev.	Median
Loan-Related Variables:			
Total Balance	2,611,645	10,057,676.6	535,934.5
Interest Rate	.0597	.034	.0621
Total Collateral Value	3,234,279	20,128,240	187,630
Days Remaining (Maturity)	528.34	995.16	52.35
Unsecured loan (Dummy)	.35	.45	0
Secure type of collateral (Dummy)	.50	.45	.51
Number of loans per year	4.72	29.31	2
Performance-Related Variables :			
Nonperforming (Dummy)	.11	.31	0
Days Past Due	29.8	82.32	0
Days Past Due over Remaining Days	10.1	52.03	0
Total Provision	1,438,472	30,789,946	0
Debtor Renegotiated a Loan (Dummy)	.59	.49	0
At Least One Forborne (Dummy)	.05	.21	0
Firm's Financial Information :			
Total Assets	35,710,880	204,072,593	6,124,094
Total Debt	24,219,150	140,099,180	4,253,020
Total Equity	12,862,210	90,149,735	1,644,740
EBITDA	1,384,210	26,161,717	271,487
Total Debt over EBITDA ratio	-26.7	3,325.4	7.5
Leverage (Debt over Assets)	.72	.72	.69

This table displays summary statistics of the main variables. The variables are constructed at the firm level. A simple sum of all loans a firm holds each year is used for the total balance, total collateral value, number of loans, and total provision. A weighted average with weights equal to the ratio of the specific loan exposure over the total balance of the firm is used for the interest rate, days remaining, nonperforming (dummy), and days past due. All variables are based on the 2012–2013 values.

Table 1.2: Comparison of pre-period mean values for treated and control groups

Variable	(1) Control	(2) Treated	(3) p - value on Difference
Loan-Related Variables:			
Total Balance	934,255.3	791,929.6	(0.571)
	(1,497,483.4)	(1,232,984.1)	
Interest Rate	0.0618	0.0657	(0.323)
	(0.0325)	(0.0334)	
Days Remaining (Maturity)	516.9	723.4	(0.198)
	(985.4)	(1,207.4)	
Total Collateral Value	737,344.5	811,042.0	(0.831)
	(1,342,033.5)	(1,362,189.0)	
Unsecured Loan (Dummy)	0.548	0.478	(0.348)
	(0.498)	(0.501)	
Secure Type of Collateral	0.473	0.539	(0.483)
	(0.450)	(0.460)	
Performance-Related Variables:			
Nonperforming (Dummy)	0.106	0.124	(0.735)
	(0.308)	(0.330)	
Days Delayed over Remaining Days	11.20	17.98	(0.201)
	(63.98)	(65.67)	
Total Provision per Total Exposure	0.114	0.143	(0.467)
	(1.828)	(0.655)	
Ratio of Loans Transferred	0.462	0.434	(0.000)
to NPL specialized unit	(0.00107)	(0.00126)	
Debtor's Financial Information:			
Total Assets	10,712,500.0	7,983,826.1	(0.632)
	(15,995,732.5)	(12,173,003.3)	
Total Debt	7,473,765.0	6,565,431.2	(0.884)
	(11,426,411.1)	(10,640,303.1)	
Total Equity	3,543,320.3	2,207,924.3	(0.273)
	(6,546,923.7)	(3,996,019.6)	
EBITDA	1,413,431.1	596,890.4	(0.208)
	(26,589,773.0)	(2,541,047.6)	
Leverage (Debt over Assets)	0.704	0.717	(0.879)
	(0.316)	(0.240)	

This table displays the mean values and standard deviations separately for the treated and control groups. Column 3 reports the p-value for the difference between Columns 1 and 2. p-values are obtained from a regression of the main variable on a treatment indicator and region fixed effects. All variables and estimations are based on the 2012–2013 values.

Table 1.3: Summary statistics and Difference-in-Difference coefficients for firms in exposed and control areas - Match on the zip code

	(1) Mean values for Control areas	(2) Mean values for Exposed areas	(3) p - value on Difference	(4) Coefficient from DiD
Total Assets	2,046,750.4	1,825,612.9	(0.053)	-1,198.2
	(2,213,766.2)	(2,042,689.0)		(42,739.4)
Total Debt	498,087.1	458,946.9	(0.273)	12,978.2
	(1,031,146.5)	(974,348.2)		(13,685.6)
Shareholders Funds	906,964.1	780,377.7	(0.015)	-18,101.5
	(1,336,612.6)	(1,086,455.0)		(21,782.0)
Number of Employees	13.33	11.86	(0.034)	-0.131
	(15.84)	(12.78)		(0.317)
EBIT	95,948.5	70,551.1	(0.138)	-2,360.2
	(215,807.3)	(186,164.2)		(6,868.1)
EBIT (Growth Rate)	-0.450	-0.0356	(0.368)	2.070
	(29.85)	(8.002)		(3.758)
Net Income	35,171.8	20,397.8	(0.396)	-2,071.4
	(162,093.6)	(146,359.9)		(5,756.2)
Net Income	-0.482	-1.015	(0.252)	-0.632
(Growth Rate)	(11.70)	(25.73)		(0.976)
Operating Revenue	1,640,175.0	1,430,693.6	(0.205)	27,648.1
	(2,496,198.6)	(2,291,917.3)		(30,733.6)
Sales	1,609,808.0	1,401,415.3	(0.205)	25,852.7
	(2,476,738.2)	(2,267,099.8)		(30,432.5)
Gross Profit	440,155.2	374,192.1	(0.102)	-1,232.8
	(584,430.4)	(528,494.3)		(16,640.0)
Cash Flow	95,235.7	79,114.4	(0.094)	-1,455.9
	(194,803.9)	(171,806.5)		(7,490.2)
Return on Shareholders	12.50	5.947	(0.259)	-1.996
Funds (%)	(78.81)	(72.12)		(4.009)
Return on Total	3.153	2.784	(0.298)	-0.161
Assets (%)	(13.62)	(13.74)		(0.652)
Profit Margin (%)	2.185	1.091	(0.527)	0.0524
	(20.73)	(21.09)		(0.652)

The data source for this table is the Amadeus Bureau van Dijk database. This table in columns 1 and 2 displays the mean values and standard deviations for firms located in zip-code areas where a bank unit closed (exposed areas) and firms located in zip-code areas where a unit remained open (control areas). Column 3 reports the p-value for the difference between Columns 1 and 2. p-values are obtained from a regression of the main variable on an indicator of whether the firm is located at the zip-code area where a bank unit closed, and region fixed effects. Column 4 presents the estimate coefficients from difference-in-difference regressions on the firms' variables. All variables and estimations are based on 2012–2015 values.

Table 1.4: Effect on renegotiating a loan (extensive margin)

	(1)	(2)	(3)	(4)
Post ∗ Treat	-0.231**	-0.262***	-0.236**	-0.308***
	(0.0879)	(0.0783)	(0.114)	(0.0744)
Observations	12,540	12,540	7,220	11,023
R^2	0.284	0.287	0.042	0.302
Firm F.E.	✓	✓		
Bank Unit F.E.	✓	✓	✓	✓
Region F.E.	✓			
Year F.E.	✓			
Region ∗ Year F.E.		✓	✓	✓
Lagged log of EBITDA			✓	
Lagged log of Total Balance			✓	
Lagged log of Total Assets				✓
Cluster Level		Bank Unit		

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1. The dependent variable is a dummy variable that is equal to one if the firm *i* renegotiated a loan at time *t* and zero otherwise. The dummy variable *Treat* is equal to one if the firm experienced an interruption in its relationship with the loan officer due to the bank unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013). The bottom of the table depicts information on the fixed effects and the control variables included. Standard errors are corrected for clustering at the bank-unit level.

Table 1.5: Effect on different renegotiation outcomes (extensive margin)

	(1)	(2)	(3)	(4)	(5)
	Increased Maturity	*Increased Loan amount*	*Increased Collateral*	*Decreased Collateral*	*Decreased Interest Rate*
Post $*$ *Treat*	-0.0489**	-0.207***	-0.204***	-0.208***	-0.0592
	(0.0244)	(0.0659)	(0.0760)	(0.0521)	(0.0903)
Observations	12,540	12,540	12,540	12,540	12,540
R^2	0.025	0.068	0.125	0.115	0.128
Firm F.E.	✓	✓	✓	✓	✓
Bank Unit F.E.	✓	✓	✓	✓	✓
Region $*$ Year F.E.	✓	✓	✓	✓	✓
Cluster Level			Bank Unit		

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1 for different renegotiation outcomes. The dependent variables are dummy variables that are equal to one if firm i received that particular renegotiation outcome at time t and zero otherwise. The dummy variable *Treat* is equal to one if the firm experienced an interruption on its relationship with the loan officer due to the bank unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013). In the regressions, firm, bank-unit and region $*$ year fixed effects are included. Standard errors are corrected for clustering at the bank-unit level.

Table 1.6: Effect on log of Interest Rate - Only renegotiated loans

	(1)	(2)	(3)	(4)
Post ∗ *Treat*	0.00468	0.00982**	0.0162*	0.00946**
	(0.00375)	(0.00380)	(0.00856)	(0.00374)
Observations	5,368	5,368	4,191	4,875
R^2	0.022	0.028	0.021	0.027
Firm F.E.	✓	✓		
Bank Unit F.E.	✓	✓	✓	✓
Region F.E.	✓			
Year F.E.	✓			
Region ∗ Year F.E.		✓	✓	✓
Lagged log of EBITDA			✓	
Lagged log of Total Balance			✓	
Lagged log of Total Assets				✓
Cluster Level		Bank Unit		

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1 only for firms that renegotiated a loan over the sample period. The observations included in the analysis are the loans that were renegotiated at least once during the sample period. The dependent variable is the log of interest rate of the renegotiated loans that firm *i* had. The dummy variable *Treat* is equal to one if the firm experienced an interruption on its relationship with the loan officer due to the bank-unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013). The bottom of the table depicts information on the fixed effects and the control variables included. Standard errors are corrected for clustering at the bank-unit level.

Table 1.7: Effect on log of Remaining Days (Maturity) - Only renegotiated loans

	(1)	(2)	(3)	(4)
Post ∗ Treat	-0.775**	-0.489**	-0.874**	-0.429
	(0.363)	(0.210)	(0.337)	(0.282)
Observations	5,368	5,368	4,191	4,875
R^2	0.032	0.016	0.042	0.020
Firm F.E.	✓	✓		
Bank Unit F.E.	✓	✓	✓	✓
Region F.E.	✓			
Year F.E.	✓			
Region ∗ Year F.E.		✓	✓	✓
Lagged log of EBITDA			✓	
Lagged log of Total Balance			✓	
Lagged log of Total Assets				✓
Cluster Level	Bank Unit			

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1 only for firms that renegotiated a loan over the sample period. The observations included in the analysis are the loans that were renegotiated at least once during the sample period. The dependent variable is the log of days remaining of the renegotiated loans that firm *i* had. This variable is a measure of loan maturity. The dummy variable *Treat* is equal to one if the firm experienced an interruption on its relationship with the loan officer due to the bank-unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013). The bottom of the table depicts information on the fixed effects and the control variables included. Standard errors are corrected for clustering at the bank-unit level.

Table 1.8: Effect on log of Collateral per Total Balance - Only renegotiated loans

	(1)	(2)	(3)	(4)
Post ∗ *Treat*	1.268***	0.716***	0.815***	0.790***
	(0.355)	(0.249)	(0.184)	(0.270)
Observations	5,367	5,367	4,191	4,874
R^2	0.028	0.016	0.035	0.025
Firm F.E.	✓	✓		
Bank Unit F.E.	✓	✓	✓	✓
Region F.E.	✓			
Year F.E.	✓			
Region ∗ Year F.E.		✓	✓	✓
Lagged log of EBITDA			✓	
Lagged log of Total Balance			✓	
Lagged log of Total Assets				✓
Cluster Level		Bank Unit		

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1 only for firms that renegotiated a loan over the sample period. The observations included in the analysis are the loans that were renegotiated at least once during the sample period. The dependent variable is the log of collateral value per total balance of the renegotiated loans that firm *i* had. The dummy variable *Treat* is equal to one if the firm experienced an interruption on its relationship with the loan officer due to the bank-unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013). The bottom of the table depicts information on the fixed effects and the control variables included. Standard errors are corrected for clustering at the bank-unit level.

Table 1.9: Effect on log of Total Balance (exclude nonperforming loans) - Only renegotiated loans

	(1)	(2)	(3)	(4)
Post $*$ *Treat*	0.136	0.342	1.013***	0.516**
	(0.205)	(0.225)	(0.330)	(0.247)
Observations	4,068	4,068	3,274	3,766
R^2	0.031	0.043	0.040	0.045
Firm F.E.	✓	✓		
Bank Unit F.E.	✓	✓	✓	✓
Region F.E.	✓			
Year F.E.	✓			
Region $*$ Year F.E.		✓	✓	✓
Lagged log of EBITDA			✓	
Lagged log of Total Balance			✓	
Lagged log of Total Assets				✓
Cluster Level		Bank Unit		

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1 only for firms that renegotiated a loan over the sample period. The observations included in the analysis are the loans that were renegotiated at least once during the sample period and that were not in default. The dependent variable is the log of total balance of the renegotiated loans that firm i had. The nonperforming loans are excluded from this analysis as an increase in the total balance for these loans could be driven by the delayed payment. The dummy variable *Treat* is equal to one if the firm experienced an interruption on its relationship with the loan officer due to the bank-unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013). The bottom of the table depicts information on the fixed effects and the control variables included. Standard errors are corrected for clustering at the bank-unit level.

Table 1.10: Effect on variables related to collateral type - Only renegotiated loans

	(1) *Unsecured Loan*	(2) *Secure Type of Collateral*
Post ∗ *Treat*	-0.180**	0.313***
	(0.0773)	(0.103)
Observations	5,368	4,779
R^2	0.015	0.011
Firm F.E.	✓	✓
Bank Unit F.E.	✓	✓
Region ∗ Year F.E.	✓	✓
Cluster Level	Bank Unit	

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1 only for firms that renegotiated a loan over the sample period for two variables related to loan collateral. The observations included in the analysis are the loans that were renegotiated at least once during the sample period. Each column shows the estimated results for a different dependent variable. Column 1 presents the result for an unsecured loan dummy variable that is equal to one if firm's *i* loan had no collateral at time *t* and equal to zero otherwise. Column 2 presents the result for a secure type of collateral dummy variable that is equal to one if firm's *i* loan had more secure collateral at time *t* and zero otherwise. As more secure collateral, real estate (commercial or residential), ships, deposits, and debt securities are characterized. As less secure collateral, accounts receivable, equities or convertible bonds, and other types of guarantees are characterized. The dummy variable *Treat* is equal to one if the firm experienced an interruption on its relationship with the loan officer due to the bank-unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013). In the baseline regressions, firm, bank-unit, and region∗year fixed effects are included. Standard errors are corrected for clustering at the bank-unit level.

Table 1.11: Comparison of pre-period mean values for treated and control groups - Only firms that renegotiated a loan

Variable	(1) Control	(2) Treated	(3) p - value on Difference
Loan-Related Variables:			
Total Balance	950,778.6	812,330.8	(0.656)
	(1,521,659.9)	(1,286,015.7)	
Interest Rate	0.0632	0.0665	(0.328)
	(0.0339)	(0.0335)	
Days Remaining (Maturity)	402.8	565.2	(0.561)
	(839.2)	(1062.5)	
Total Collateral Value	790,269.9	797,576.3	(0.749)
	(1,387,600.2)	(1,356,898.3)	
Unsecured Loan (Dummy)	0.340	0.339	(0.942)
	(0.443)	(0.462)	
Secure Type of Collateral	0.467	0.510	(0.917)
	(0.445)	(0.455)	
Performance-Related Variables:			
Nonperforming (Dummy)	0.122	0.140	(0.254)
	(0.327)	(0.347)	
Days Delayed over Remaining Days	13.10	19.76	(0.266)
	(69.75)	(68.74)	
Total Provision per Total Exposure	0.136	0.156	(0.618)
	(2.007)	(0.700)	
Ratio of Loans Transferred	0.462	0.434	(0.000)
to NPL specialized unit	(0.00107)	(0.00126)	
Debtor's Financial Information:			
Total Assets	10,413,859.5	6,882,262.0	(0.187)
	(15,273,643.4)	(7,440,389.4)	
Total Debt	7,333,025.7	5,330,367.5	(0.134)
	(11,067,786.7)	(4,948,417.3)	
Total Equity	3,402,715.3	2,163,741.2	(0.459)
	(6,372,251.9)	(3,981,664.0)	
EBITDA	1,044,784.6	601,476.2	(0.479)
	(26,063,090.9)	(2,663,656.7)	
Leverage (Debt over Assets)	0.714	0.719	(0.521)
	(0.321)	(0.241)	

This table displays the mean values and standard deviations for the treated and control groups only for firms that renegotiated at least one loan. Column 3 reports the p-value for the difference between Columns 1 and 2. p-values are obtained from a regression of the main variable on a treatment indicator and region fixed effects. All variables and estimations are based on the 2012–2013 values.

Table 1.12: Identify possible selection on renegotiation

	(1) Whole Sample pre- & post-period	(2) Only control group post-period	(3) Only treated group post-period
	Renegotiation	*Renegotiation*	*Renegotiation*
All variables are in lagged logs:			
Firm Variables:			
EBITDA growth rate	0.000147	-0.0000467	0.00102***
	(0.000145)	(0.000300)	(0.000328)
Total Assets	0.000613**	0.000570**	0.000574
	(0.000229)	(0.000228)	(0.000685)
Total Debt	0.000853***	0.000802***	0.00141
	(0.000191)	(0.000192)	(0.000976)
Leverage	0.000635***	0.000602**	0.00132*
	(0.000205)	(0.000221)	(0.000763)
Debt over EBITDA	0.000653***	0.000706***	0.000849
	(0.000134)	(0.000119)	(0.000715)
Loan Variables:			
Total Balance	0.00136***	0.00169***	0.00229***
	(0.000186)	(0.000190)	(0.000638)
Total Provision	0.00166***	0.00186***	0.00172***
	(0.000148)	(0.000190)	(0.000572)
Days Past Due	0.00118***	0.00121***	0.000122
	(0.000120)	(0.000135)	(0.000337)
Bank Unit F.E.	✓	✓	✓
Region ∗ Year F.E.	✓	✓	✓
Cluster Level		Bank Unit	

This table displays the estimated coefficients from separate regressions of the renegotiation dummy variable on firm and loan variables. The independent variables are constructed as the firm's rank within bank unit based on each variable's value at the year before the transfer. Bank-unit and region∗year fixed effects are included, and standard errors are clustered at the bank-unit level. Column 1 reports the estimated coefficients for the whole sample (treated and control groups over the whole period). Column 2 reports the coefficients for the control group in the post-period. Column 3 reports the coefficients for the treated group in the post-period.

Table 1.13: Summary statistics and Difference-in-Difference coefficients for firms in exposed and control areas - Exclude Attica and match on the zip code

	(1) Mean values for Control areas	(2) Mean values for Exposed areas	(3) p - value on Difference	(4) Coefficient from DiD
Total Assets	1,626,610.8	2,014,598.6	(0.399)	87,059.6
	(1,743,132.7)	(2,138,976.2)		(71,485.0)
Total Debt	355,360.0	549,881.4	(0.110)	-19,373.3
	(790,474.9)	(1,038,056.8)		(17,135.4)
Shareholders funds	785,544.4	907,094.8	(0.814)	33,374.6
	(1,189,319.0)	(1,129,909.4)		(25,500.5)
Number of Employees	11.37	12.33	(0.459)	0.0734
	(12.07)	(13.80)		(0.588)
EBIT	83,964.2	60,042.6	(0.114)	14,415.8
	(172,477.7)	(169,693.1)		(12,099.1)
EBIT	-3.783	-0.167	(0.161)	26.74
(Growth Rate)	(79.72)	(9.234)		(26.61)
Net Income	32,158.8	9,409.9	(0.558)	9,655.2
	(137,600.4)	(138,692.2)		(8,291.5)
Net Income	-1.283	-0.942	(0.782)	1.042
(Growth Rate)	(16.41)	(20.60)		(0.975)
Operating revenue	1,219,683.1	1,503,067.8	(0.597)	70,398.0
	(1,796,479.7)	(2,435,916.8)		(49,835.7)
Sales	1,189,756.5	1,474,594.3	(0.576)	63,702.5
	(1,794,288.1)	(2,414,726.8)		(50,256.1)
Gross Profit	374,294.3	334,154.7	(0.436)	-4,988.0
	(454,295.2)	(475,334.0)		(37,769.4)
Cash Flow	88,236.9	81,123.2	(0.192)	5,758.8
	(175,649.1)	(168,534.7)		(14,592.4)
Return on Shareholders	6.544	-0.434	(0.146)	0.869
Funds (%)	(74.48)	(61.08)		(13.30)
Return on Total	3.821	1.522	(0.180)	0.744
Assets (%)	(14.44)	(11.76)		(1.113)
Profit Margin (%)	3.447	-0.876	(0.143)	0.906
	(21.71)	(20.89)		(0.814)

The data source for this table is the Amadeus Bureau van Dijk database, and it excludes the Attica region. The sample is restricted to areas where firms are similar. Columns 1 and 2 show the mean values and standard deviations for firms located in the zip code areas where a bank-unit closed (exposed areas) and firms located in the zip code areas where a unit remained open (control areas). Column 3 reports the p-value for the difference between Columns 1 and 2. p-values are obtained from a regression of the main variable on an indicator of whether the firm is located at the zip code area where a bank-unit closed and region fixed effects. Column 4 presents the estimate coefficients from difference-in-difference regressions on the firms' variables. All variables and estimations are based on 2012–2015 values.

Table 1.14: Effect on the probability of renegotiating a loan and on renegotiated loans' terms - Only areas with similar characteristics

	Extensive Margin	*Intensive Margin*		
	(1)	(1)	(2)	(3)
	Renegotiation	*Log of Interest Rate*	*Log of Remaining Maturity*	*Log of Collateral per Balance*
Post $*$ *Treat*	-0.200*	0.00617**	-0.930*	0.950***
	(0.108)	(0.00263)	(0.523)	(0.122)
Observations	6,039	2,226	2,226	2,225
R^2	0.272	0.027	0.052	0.039
Firm F.E.	✓	✓	✓	✓
Bank Unit F.E.	✓	✓	✓	✓
Region $*$ Year F.E.	✓	✓	✓	✓
Cluster Level		Bank Unit		

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1 only for firms located in areas where firms have no statistically different characteristics, as it is shown at table 2.9. In Column 1 of the results on the extensive margin, the dependent variable is a dummy that is equal to one if firm i renegotiated a loan at time t and zero otherwise. For the estimations on the intensive margin, the observations included in the analysis are the loans that were renegotiated at least once during the sample period. Results are estimated for three loan terms. Column 1 presents the result for the log of interest rate of the renegotiated loans firm i had. Column 2 is the estimated result for the log of days remaining of the renegotiated loans. Column 3 presents the result on the log of collateral value per total balance of the renegotiated loans. The dummy variable *Treat* is equal to one if the firm experienced an interruption in its relationship with the loan officer due to the bank-unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013).

Figure 1.1: Rank bank units based on the number of corporate borrowers per unit in 2013

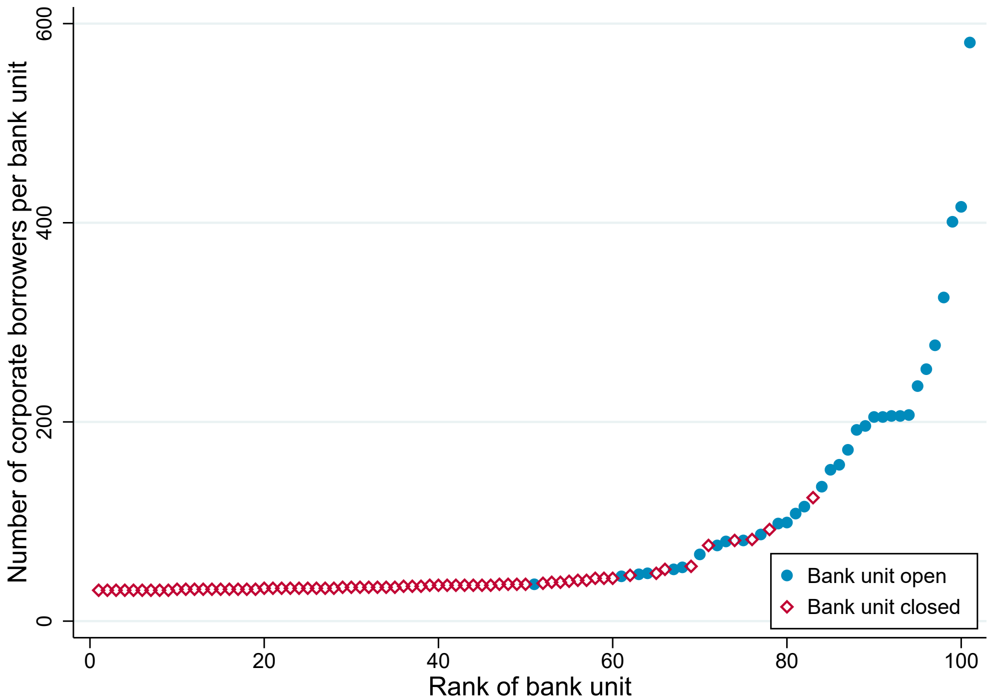

Figure 1.1 plots the number of corporate borrowers per bank unit in 2013 as a measure of relative size of the units. The red diamonds represent bank units that closed and the blue dots units that remained open.

Table 1.15: Effect on the probability of renegotiating a loan and on renegotiated loans' terms - Control for bank unit size

	Extensive Margin	*Intensive Margin*		
	(1)	(1)	(2)	(3)
	Renegotiation	*Log of Interest Rate*	*Log of Remaining Maturity*	*Log of Collateral per Balance*
Post $*$ *Treat*	-0.247***	0.00724***	-0.242***	0.966***
	(0.0450)	(0.000643)	(0.000000244)	(5.70e-14)
Observations	804	356	356	356
R^2	0.436	0.121	0.109	0.057
Firm F.E.	✓	✓	✓	✓
Bank Unit F.E.	✓	✓	✓	✓
Region $*$ Year F.E.	✓	✓	✓	✓
Cluster Level		Bank Unit		

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1 only for firms that were transferred to a bank unit that was at most 20% larger than their original unit. Observations on the control group are also restricted to those that were managed at the unit that received firms from units that were at most 20% smaller (from the "filtered" treated group).

In Column 1 of the results on the extensive margin, the dependent variable is a dummy that is equal to one if firm i renegotiated a loan at time t and zero otherwise. For the estimations on the intensive margin, the observations included in the analysis are the loans that were renegotiated at least once during the sample period. Results are estimated for three loan terms. Column 1 presents the result for the log of interest rate of the renegotiated loans that firm i had. Column 2 is the estimated result for the log of days remaining of the renegotiated loans. Column 3 presents the result on the log of collateral value per total balance of the renegotiated loans. The dummy variable *Treat* is equal to one if the firm experienced an interruption in its relationship with the loan officer due to the bank-unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013).

Table 1.16: Firm Level Effects

	(1) *Log of Total Assets*	(2) *Log of Total Debt*	(3) *Log of Total Equity*	(4) *Log of Equity over Assets*	(5) *Log of Leverage*	(6) *Total Balance over Total Debt*
$Post * Treat$	-0.611***	-0.783***	3.058**	0.0903***	-0.0637***	-0.106**
	(0.192)	(0.218)	(1.255)	(0.0306)	(0.0217)	(0.0406)
Observations	9,060	9,060	9,060	9,060	9,060	9,060
R^2	0.012	0.010	0.014	0.012	0.010	0.022
Firm F.E.	✓	✓	✓	✓	✓	✓
Bank unit F.E.	✓	✓	✓	✓	✓	✓
Region ∗ Year F.E.	✓	✓	✓	✓	✓	✓
Cluster Level	Bank unit					

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results of an interruption in the relationship between a loan officer and a firm from estimating equation 1.1 on firm financial variables. In Column 1, the dependent variable is the log of total assets of firm i at year t. In Column 2, the dependent variable is the log of total debt. In Column 3, the dependent variable is the log of total equity. In Column 4, the dependent variable is the log of the ratio of total equity over total assets. In Column 5, the dependent variable is the log of the firm's leverage. Leverage is defined as the ratio of total debt over total assets. In Column 6, the dependent variable is the ratio of the total balance at this bank over the total debt to all banks that a firm i has at year t. This is a measure of how much a firm borrows from this bank relative to its total debt from all banks. The dummy variable *Treat* is equal to one if the firm experienced an interruption on its relationship with the loan officer due to the bank unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013). Firm, bank unit, and region∗year fixed effects are included. The bottom of the table depicts information on the fixed effects and the control variables included. Standard errors are corrected for clustering at the bank unit level.

Table 1.17: Correlation of relationship strength with probability to renegotiate

	Whole sample		*Only control*		*Only treated*	
	(1)	(2)	(1)	(2)	(1)	(2)
	Pre-period	*Post-period*	*Pre-period*	*Post-period*	*Pre-period*	*Post-period*
Dependence Ratio	0.0440^{***}	0.147^{***}	0.0321^{**}	0.140^{***}	0.162^{***}	0.0621
	(0.0120)	(0.0272)	(0.0123)	(0.0267)	(0.0444)	(0.0811)
R^2	0.422	0.013	0.431	0.014	0.378	0.103
Bank Unit F.E.	✓	✓	✓	✓	✓	✓
Region $*$ Year F.E.	✓	✓	✓	✓	✓	✓
Cluster Level	Bank Unit					

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the estimated coefficients from separate regressions of the renegotiation dummy variable on the *dependence ratio*. The *dependence ratio* is the ratio of the total balance that a firm has at this bank over the total debt that the firm has at all banks. The *dependence ratio* is used as an approximate measure of relationship strength. Bank-unit and region∗year fixed effects are included, and standard errors are clustered at the bank unit level. The first two columns present the coefficients for the whole sample for the pre- and post-period separately. The second two columns present the coefficients only for the control group for the pre- and post-period separately. The last two columns present the coefficients only for the treated group for the pre- and post-period separately.

Figure 1.2: Density distribution of the *dependence ratio*

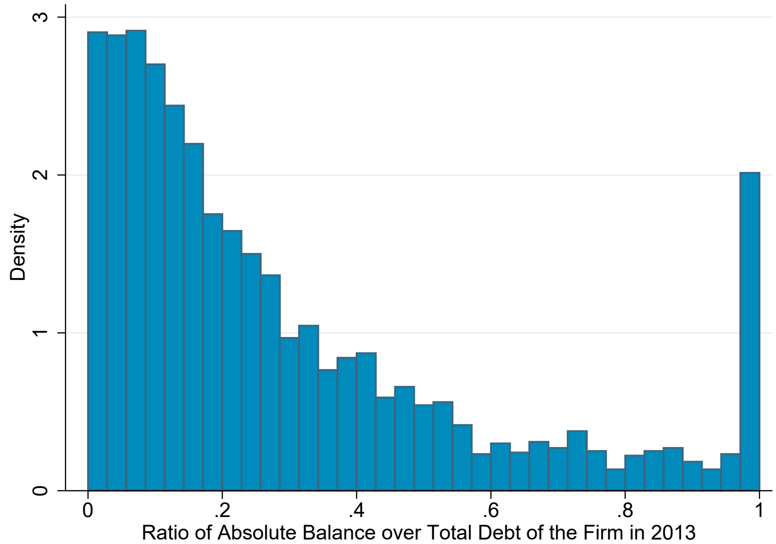

Figure 2.10 plots the density distribution of the *dependence ratio* as measured in 2013.
The *dependence ratio* is defined as the ratio of the total balance that a firm has at this bank over the total bank debt of the firm.

Table 1.18: Effect on renegotiating a loan (extensive margin) - Based on relationship strength

	Closer Relationship	*Less Close Relationship*
	(1)	(2)
	Renegotiation	*Renegotiation*
$Post * Treat$	-0.280**	-0.289**
	(0.105)	(0.132)
Observations	2,106	6,991
R^2	0.296	0.259
Firm F.E.	✓	✓
Bank Unit F.E.	✓	✓
Region ∗ Year F.E.	✓	✓
Cluster Level	Bank Unit	

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1 separately for firms with a closer relationship with the bank and for firms with a less close relationship. Firms with a *closer relationship* are defined those with dependence ratio above 50% in 2013. As firms with a *less close relationship*, are defined those with dependence ratio below 20% in 2013. The dependent variable is a dummy that is equal to one if firm i renegotiated a loan at time t and zero otherwise. The dummy variable *Treat* is equal to one if the firm experienced an interruption in its relationship with the loan officer due to the bank-unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013). The bottom of the table depicts information on the fixed effects and the control variables included. Standard errors are corrected for clustering at the bank-unit level.

Table 1.19: Effect on renegotiated loans' terms - Based on relationship strength

	Closer Relationship			*Less Close Relationship*		
	(1)	(2)	(3)	(1)	(2)	(3)
All in logs	*Interest Rate*	*Days Remaining*	*Collateral Value over Balance*	*Interest Rate*	*Days Remaining*	*Collateral Value over Balance*
Post ∗ Treat	0.0212***	-3.360***	1.686***	-0.0129***	2.349***	-0.587**
	(0.00477)	(0.996)	(0.261)	(0.00412)	(0.566)	(0.263)
Observations	985	985	985	3,107	3,107	3,106
R^2	0.011	0.031	0.037	0.007	0.031	0.019
Firm F.E.	✓	✓	✓	✓	✓	✓
Bank Unit F.E.	✓	✓	✓	✓	✓	✓
Region ∗ Year F.E.	✓	✓	✓	✓	✓	✓
Cluster Level	Bank Unit					

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1 separately for firms with a closer relationship with the bank and for firms with a less close relationship. Firms with a *closer relationship* are defined as those with dependence ratio above 50% in 2013. Firms with a *less close relationship* are defined those with dependence ratio below 20% in 2013. For these estimations, the observations included in the analysis are the loans that were renegotiated at least once during the sample period. Results are estimated for three loan terms. Column 1 presents the result for the log of interest rate of the renegotiated loans that firm *i* had. Column 2 is the estimated result for the log of days remaining of the renegotiated loans. Column 3 presents the result on the log of collateral value per total balance of the renegotiated loans. The dummy variable *Treat* is equal to one if the firm experienced an interruption in its relationship with the loan officer due to the bank-unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013). In the baseline regressions, firm, bank-unit, and region∗year fixed effects are included. Standard errors are corrected for clustering at the bank-unit level.

Figure 1.3: Density distribution of the firm's relative size

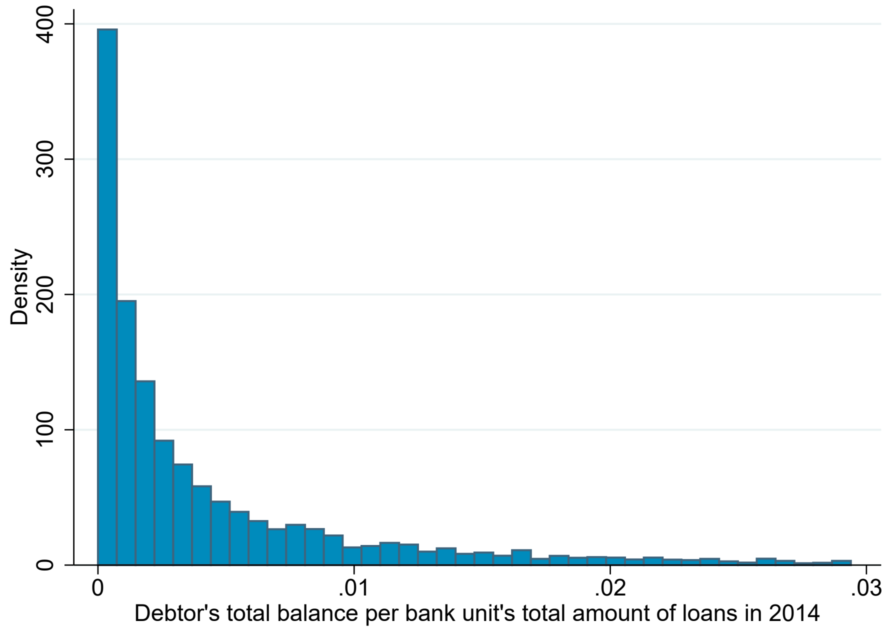

Figure 2.12 plots the density distribution of the firm's relative size as a corporate client within bank unit. The measure of firm's relative size is the ratio of the firm's total balance at the bank unit over the total amount of loans that each bank unit manages in 2014.

Table 1.20: Effect on renegotiating a loan (extensive margin) - By firm's relative size as bank's corporate client within unit in 2014

	Smaller balance	*Larger balance*
	(1)	(2)
	Renegotiation	*Renegotiation*
$Post * Treat$	-0.385***	-0.102
	(0.0540)	(0.116)
Observations	10,296	2,937
R^2	0.264	0.389
Firm F.E.	✓	✓
Bank Unit F.E.	✓	✓
Region * Year F.E.	✓	✓
Cluster Level	Bank Unit	

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1 separately for firms that had a smaller balance at the bank unit in 2014 and for firms that had a larger balance. Larger firms are defined as those whose balance at the specific bank unit accounts for at least 1% of the total amount of corporate loans that the unit manages. The rest of the sample is defined as relatively smaller firms. The dependent variable is a dummy variable that is equal to one if firm *i* renegotiated a loan at time *t* and zero otherwise. The dummy variable *Treat* is equal to one if the firm experienced an interruption in its relationship with the loan officer due to the bank-unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013). The bottom of the table depicts information on the fixed effects and the control variables included. Standard errors are corrected for clustering at the bank-unit level.

Table 1.21: Effect on renegotiated loans' terms - By firm's relative size as bank's corporate client within unit in 2014

	Smaller balance			*Larger balance*		
	(1) *Interest Rate*	(2) *Days Remaining*	(3) *Collateral Value over Balance*	(1) *Interest Rate*	(2) *Days Remaining*	(3) *Collateral Value over Balance*
All in logs						
*Post * Treat*	0.00631	-0.0467	0.256^{*}	0.0186^{***}	-4.194^{***}	1.330^{***}
	(0.00444)	(0.477)	(0.149)	(0.00679)	(0.652)	(0.430)
Observations	3,366	3,366	3,365	2,002	2,002	2,002
R^2	0.010	0.031	0.021	0.008	0.021	0.019
Firm F.E.	✓	✓	✓	✓	✓	✓
Bank Unit F.E.	✓	✓	✓	✓	✓	✓
Region * Year F.E.	✓	✓	✓	✓	✓	✓
Cluster Level	Bank Unit					

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1 separately for firms that had a smaller balance at the bank unit in 2014 and for firms that had a larger balance. Larger firms are defined as those whose balance at the specific bank unit accounts for at least 1% of the total amount of corporate loans that the unit manages. The rest of the sample is defined as relatively smaller firms. For these estimations, the observations included in the analysis are the loans that were renegotiated at least once during the sample period. Results are estimated for three loan terms. Column 1 presents the result for the log of interest rate of the renegotiated loans that firm *i* had. Column 2 is the estimated result for the log of days remaining of the renegotiated loans. Column 3 presents the result on the log of collateral value per total balance of the renegotiated loans. The dummy variable *Treat* is equal to one if the firm experienced an interruption in its relationship with the loan officer due to the bank unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013). In the baseline regressions, firm, bank-unit, and region*year fixed effects are included. Standard errors are corrected for clustering at the bank-unit level.

Table 1.22: Effect on renegotiating a loan (extensive margin) - By firm's pre-period performance

	Delayed loan payment	*On time loan payment*
	(1)	(2)
	Renegotiation	*Renegotiation*
Post ∗ *Treat*	-0.301***	-0.189*
	(0.0883)	(0.107)
Observations	3,764	9,469
R^2	0.420	0.245
Firm F.E.	✓	✓
Bank Unit F.E.	✓	✓
Region ∗ Year F.E.	✓	✓
Cluster Level	Bank Unit	

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1 separately for firms that delayed a loan payment in 2013 and for firms that paid their loans on time in 2013. The dependent variable is a dummy variable that is equal to one if firm *i* renegotiated a loan at time *t* and zero otherwise. The dummy variable *Treat* is equal to one if the firm experienced an interruption in its relationship with the loan officer due to the bank-unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013). The bottom of the table depicts information on the fixed effects and the control variables included. Standard errors are corrected for clustering at the bank-unit level.

Table 1.23: Effect on renegotiated loans' terms - By firm's pre-period performance

	Delayed loan payment			*On time loan payment*		
	(1)	(2)	(3)	(1)	(2)	(3)
All in logs	*Interest Rate*	*Days Remaining*	*Collateral Value over Balance*	*Interest Rate*	*Days Remaining*	*Collateral Value over Balance*
Post ∗ Treat	0.0136***	-1.127***	0.461**	0.00955***	-0.698*	1.034***
	(0.00280)	(0.311)	(0.170)	(0.00345)	(0.370)	(0.348)
Observations	1,866	1,866	1,866	3,502	3,502	3,501
R^2	0.014	0.033	0.017	0.006	0.012	0.019
Firm F.E.	✓	✓	✓	✓	✓	✓
Bank Unit F.E.	✓	✓	✓	✓	✓	✓
Region ∗ Year F.E.	✓	✓	✓	✓	✓	✓
Cluster Level	Bank Unit					

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1 separately for firms that delayed a loan payment in 2013 and for firms that paid their loans on time in 2013. For these estimations, the observations included in the analysis are the loans that were renegotiated at least once during the sample period. Results are estimated for three loan terms. Column 1 presents the result for the log of interest rate of the renegotiated loans that firm *i* had. Column 2 is the estimated result for the log of days remaining of the renegotiated loans. Column 3 presents the result on the log of collateral value per total balance of the renegotiated loans. The dummy variable *Treat* is equal to one if the firm experienced an interruption in its relationship with the loan officer due to the bank-unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013). In the baseline regressions, firm, bank-unit, and region∗year fixed effects are included. Standard errors are corrected for clustering at the bank-unit level.

Table 1.24: Effect on new loans

	Extensive margin	Intensive margin			
	(1)	(1)	(2)	(3)	(4)
	Log of Number of New Loans	*Log of Total Loan Amount*	*Log of Interest rate*	*Log of Days remaining*	*Log of Collateral per Loan Amount*
Treat	-0.301***	-0.896***	0.00806**	-0.791*	-0.128
	(0.0603)	(0.164)	(0.00341)	(0.453)	(0.0987)
Observations	4,254	4,254	4,254	4,254	4,217
R^2	0.053	0.011	0.008	0.020	0.012
Bank Unit F.E.	✓	✓	✓	✓	✓
Region ∗ Year F.E.	✓	✓	✓	✓	✓
Cluster Level			Bank Unit		

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.2 for the new loans that firms received in the post-period (2014–2015). The dependent variable for the extensive margin is the log of number of new loans that firm *i* received at year *t*. On the intensive margin, the dependent variables are the loan terms of the new loans. In particular, these are the log of the total amount, log of interest rate, log of remaining days, log of collateral value per loan amount. The dummy variable *Treat* is equal to one if the firm experienced an interruption on its relationship with the loan officer due to the bank-unit closure, and zero otherwise. Bank-unit, and region∗year fixed effects are included. Standard errors are corrected for clustering at the bank-unit level.

Table 1.25: Effect on new loans' performance

	(1) *Log of Days Past Due per Remaining Days*	(2) *Nonperforming (Dummy)*	(3) *Log of Total Provision per Total Balance*
Treat	0.192	0.0627	-0.0469
	(0.138)	(0.0510)	(0.0379)
Observations	4,254	4,254	4,217
R^2	0.002	0.004	0.013
Bank Unit F.E.	✓	✓	✓
Region $*$ Year F.E.	✓	✓	✓
Cluster Level		Bank Unit	

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.2 for the new loans performance that a firm *i* received in the post-period (2014–2015). The results are estimated for three variables related to loans' performance. The dependent variables are the log of a loan's payment days past due over remaining days, a nonperforming dummy variable, and the log of total loan-loss provision over total balance. The dummy variable *Treat* is equal to one if the firm experienced an interruption in its relationship with the loan officer due to the bank-unit closure, and zero otherwise. Bank-unit, and region$*$year fixed effects are included. Standard errors are corrected for clustering at the bank-unit level.

Chapter 2

Lending Relationships and Moral Hazard in Loan Renegotiation

2.1 Introduction

Most corporate credit is mediated by a personal relationship between a firm and a loan officer, as this connection helps to mitigate agency problems. Financial intermediation theory suggests that relationships can be beneficial because they reduce information asymmetries and alleviate moral hazard (through monitoring, screening, repeated interaction etc.).[1] In this paper, I study the impact of personal relationships between loan officers and firms on loan renegotiations and I investigate whether personal relationships could prevent a moral hazard behavior to trigger a loan renegotiation. A renegotiation can be initiated by either the creditor or the borrower prior to or coincident with default on a loan. Classic contract theory suggests that in unanticipated states of the world, renegotiation is Pareto improving.[2] Especially when financial distress results from a macroeconomic shock, it is probable that both the bank and the borrower will benefit from a successful renegotiation. Renegotia-

[1] Classic references: Leland and Pyle 1977 Diamond 1984 Ramakrishnan and Thakor 1984 Fama 1985 Allen 1990

[2] Hart and Moore 1988, Rajan 1992, Aghion, Dewatripont, and Rey 1994, Bolton and Scharfstein 1996, Hart and Moore 1998, Maskin and Moore 1999, Gorton and Kahn 2000, Garleanu and Zwiebel 2008

tion relaxes inefficient constraints on the borrowers as well as the bank and can increase expected loan repayments.

There are two main challenges for accurately estimating the impact of personal relationships on loan renegotiation. The first is the difficulty of quantifying the value of a personal relationship. No direct measure of relationship intensity exists. The length of a given relationship may seem like a straightforward measure, but the endogeneity of the decision to sever an existing relationship will complicate the interpretation of the time span measure. The endogeneity factor creates the second challenge. A bank's decision to break an existing relationship may reflect its perception of the declining creditworthiness of the borrower. Under some circumstances a successful firm may seek to broaden its access to external finance by weakening its relationship with the particular bank. Such decisions are endogenous and would bias any results estimated by treating relationships as exogenous in a simple OLS framework.

To overcome these challenges, I use a 2013 bank reorganization in Greece. This experiment provides exogenous variation in the length of the relationships between loan officers and firms. Moreover, detailed confidential data on corporate loans allow me to quantify accurately the effect of interrupted personal relationships, by controlling for region, bank unit, and firm-specific effects. My central finding is that personal relationships between loan officers and firms have a significant positive impact on loan renegotiation. Firms with interrupted relationships are less likely to renegotiate a loan compared to firms experiencing continuous relationships. In addition, firms with interrupted relationships default more frequently on the loans that they want to trigger a renegotiation.

The empirical setting is based on the consolidation of a major commercial bank in

Greece with business activity throughout the country. Bank network consolidation is a common response of banks to financial distress, as consolidation reduces operating costs and centralizes lending decisions.[3] During consolidation, some bank units are closed and the loan accounts from those units are merged with accounts in other surviving units. A bank unit closure interrupts personal relationships between loan officers and firms because merged accounts obtain new loan officers. Thus, after consolidation, two types of firms are identified: one, those whose loans were transferred to another unit and whose personal relationships were consequently discontinued, and two, those that remained at the same unit for the entire period.

The criterion for bank units' consolidation was geographic location. My identifying assumption is that the decision to close a bank unit is orthogonal to firms' characteristics. I find support for that assumption in the data by testing for differences in observable characteristics. First, I compare the loan terms and performance, and firm financial variables of borrowers in closed and open units in the pre-unit closure period and I find no statistically significant differences. Second, I provide out-of-sample evidence that the identifying assumption holds by using the Amadeus Bureau van Dijk dataset to test for differences based on the zip codes of bank units.

I apply a difference-in-difference methodology to estimate the effect of relationship interruption. I define as *treated* the firms with exogenously discontinued personal relationships and as *control* those with continuing relationships. Hard information passed from one loan officer to another because the transfer happened within the same bank. Observed

[3] Several banks in Italy, Spain, Portugal have consolidated their network during the recent crisis as well as banks in the United States.

differences between the two groups in the post-consolidation period should be driven by the consequences of interrupted personal relationships.

I find strong evidence for the significant effect of personal relationships on loan renegotiations at both extensive and intensive margins. Firms with interrupted relationships have a 26% lower probability of renegotiating at least one of their old loans upon their transfer to another unit, compared to firms that remained at the same unit. The unconditional probability of renegotiating a loan is 59% and the estimated effect corresponds to a 15% lower probability of renegotiation.

Moreover, contingent on successful renegotiation in the post-unit-closure period, I identify a positive effect of continuing personal relationships on the delinquency of renegotiated loans. Greek banks, like most of the banks operating in countries hit by the 2010 European debt crisis and the preceding financial crisis, faced a significant increase in their nonperforming loan (NPL) ratios. In contrast to firms with continuous relationships, those whose relationships were interrupted are found to delay payments on their renegotiated loans by additional days, and their renegotiated loans have a 15.4% higher probability of becoming nonperforming. In response to the worse average performance of the interrupted-relationship loans, the bank increases the level of loan-loss provisions, and we observe an approximately 70% increase, which corresponds to additional €0.10 of loan-loss provisions for each euro of loan balance.

A possible selection bias on renegotiation could change the interpretation of the results for loan performance differences between interrupted- and continuing-relationship loans. If the loan officer chooses to renegotiate with firms based on their performance, the intensive margin results will be biased. To address the concern of potential selection bias on renegotiation, I conduct two tests. First, I compare the pre-unit-closure period characteristics of the treated and control firms that renegotiated a loan in the post-unit-closure period and I find no statistically significant difference in pre-period measures of firm performance. Second, I consider all the hard information available to both the loan officer and the econometrician to identify the variables that can trigger a renegotiation. This test demonstrates that the loan officer who remained at the same unit for the entire period chose to renegotiate with treated firms with higher profitability prospects. Loan officer behavior, therefore, biases estimated coefficients towards zero, implying that my intensive margin results are conservative.

To further investigate the explanatory mechanism for the value of a relationship between a loan officer and a firm, I examine closer and more distant relationships separately. In most cases, a closer relationship entails fewer outside financing options, and when this closer relationship is interrupted, the expected effect is a significant informational loss. In contrast, firms with more distant relationships and, hence, greater outside financing options are expected to have stronger bargaining power. To test for the value of a stronger relationship, I construct three different measures of relationship strength. The results from this analysis allow us to identify two different mechanisms that could drive the repayment behavior of the firm; a moral hazard behavior when the firm has access to outside sources of financing and the expectation of repeated interaction with the subject bank in the future when the firm has no outside option to seek financing and lower bargaining power.

This paper's main conclusion is that personal relationships mitigate the cost of distress for both the firm and the bank in a loan renegotiation. The firm is worse off following the interruption of its loan officer relationship, as it is less able to renegotiate. The bank experiences higher default ratios and makes higher loan-loss provisions for the renegotiated loans of interrupted relationship firms, as firms in order to trigger a renegotiation delay the loan repayments.

In the next section, I describe the paper's contributions to the literature. In section 2.3, I provide an overview of the institutional background and the dataset structure. In section 2.4, I present the empirical specification and a detailed comparison of the treated and control groups. The regression results are presented in section 2.5. In section 2.6, I conclude.

2.2 Related Literature

This paper contributes to the literature by being the first to combine two classic research streams—relationship banking and contract renegotiation—and by providing evidence that a moral hazard behavior is more likely to arise when the relationship between the borrower and the bank is interrupted.

The broader literature to which this study contributes addresses the role of relationships in credit markets. A rich theoretical literature on bank debt highlights the importance of informational asymmetry and moral hazard for financial intermediation.[4] Several empirical papers have examined the relationship between banks and borrowers for evidence to determine whether asymmetric information affects lending. Petersen and Rajan 1994 and Berger and Udell 1995 used the repeated interaction between a borrower and a financial

[4] Jaffee and Russell 1976; Stiglitz and Weiss 1981; Myers and Majluf 1984; Diamond 1984; Ramakrishnan and Thakor 1984;Sharpe 1990;Diamond 1991; Besanko and Kanatas 1993; Rajan and Winton 1995;Bolton and Freixas 2000

institution as a measure of relationship. Mian 2006, Agarwal and Hauswald 2010, Canales and Nanda 2012, and Bolton et al. 2016 define relationship lending as a function of geographic distance. Ongena and Smith 2001 analyze the duration of a bank relationship with a firm. Sufi 2007 and Ivashina 2009 explore informational asymmetries in a lending syndicate. [5]

The empirical literature to which this paper is most closely related identifies the effects of personal relationships between bank employees and borrowers. These studies focus on how loan approvals or performance can be influenced by different factors such as cultural proximity (Fisman, Paravisini, and Vig 2017), social connections (Haselmann, Schoenherr, and Vig 2016), hierarchical and geographical distance (Liberti and Mian 2009), or the loan officer being on leave (Drexler and Schoar 2014). Several papers examine the effect on interest rates of strong interpersonal connections between a banker and a firm (Engelberg, Gao, and Parsons 2012), or the effect of a strong relationship as measured by the number of interactions (Herpfer 2017), or the effect of an interruption of a relationship caused by an executive's death or retirement (Karolyi 2017). Lastly, Hertzberg, Liberti, and Paravisini 2010 and Cole, Kanz, and Klapper 2015 provide insights on the effects of bank-specific governance policies on the moral hazard behavior of a loan officer.

Within the field of literature on relationship banking, this paper is the first to demonstrate how exogenous interruptions in bank-borrower relationships affect the likelihood to renegotiate a loan and the factors that will trigger the renegotiation. One important difference between new and renegotiated loans is the bank's prior exposure to the risk of

[5] Extensive surveys of this literature are provided by Ongenah and Smith 2000, Boot 2000, Srinivasan 2014, Kysucky and Norden 2015

default on pre-existing loans, which gives the borrower a stronger negotiation position. The risk of a higher probability of default is magnified in a crisis period, such as in Greece in 2010-2015, when banks faced very high delinquency ratios that drove their overall risk assessment. In such periods, the value of a successful renegotiation surges. This paper not only shows the effect of relationships on the probability of renegotiation, but also on the variables related to loan performance. Further, the overall effect on the loans performance, the duration of the estimated effects, and an analysis based on measures of relationship strength allow me to provide suggestive evidence of a moral hazard behavior to trigger a loan renegotiation.

Given that the main focus of this paper is loan renegotiations, the analysis also contributes to the literature on contract renegotiation. Several influential papers have examined renegotiation in incomplete contracts as an out-of-equilibrium phenomenon.[6] Although the existing theory of contact renegotiation has evolved significantly, the empirical evidence on this topic is limited due to data restrictions. This paper investigates renegotiation independent of previous defaults on a loan payment, and for that reason is also related to the work of Roberts and Sufi 2009, who analyze in detail the factors triggering a renegotiation, and those determining its outcomes. Roberts 2015 is the closest to my paper, as he shows that a corporate loan renegotiation happens frequently, modifies significantly the initial loan terms, and is affected significantly by the duration of the lending relationship. Although similar conclusions to those found in these two papers arise in my analysis regarding the frequency and the outcomes of a renegotiation, my paper advances beyond these to esti-

[6]Hart and Moore 1988, Rajan 1992, Aghion, Dewatripont, and Rey 1994, Von Thadden 1995, Bolton and Scharfstein 1996, Hart and Moore 1998, Maskin and Moore 1999, Gorton and Kahn 2000, Garleanu and Zwiebel 2008, Tirole 2010

mate the effect of the relationship between a loan officer and a borrower on the probability of renegotiation and the performance of the renegotiated loans.

A few empirical papers have considered other factors affecting loan renegotiation. The importance of the liquidation value of collateral (Benmelech and Bergman 2008) and of the mortgage securitization (Piskorski, Seru, and Vig 2010) on loan renegotiation are highlighted. James 1995 focuses on debt restructurings and shows that the financial condition of the firm determines the bank's incentives to make concessions. Lastly, Chodorow-Reich and Falato 2017 show the importance of the financial institution's health on contracting credit using covenant violations.

This paper also complements literature examining the impact of bank branch consolidation. In this area, the most relevant paper is by Nguyen 2016, as she examines how branch closures in the United States affect local access to credit. Lastly, this paper is linked to the strain of European debt crisis literature that employing micro–level data to identify the impact on bank lending.[7]

2.3 Institutional Background & Data

The institutional setting and the data used in this paper are the same as in Chapter 1. For more details refer to the corresponding section of the first chapter.

The identification setting and the data that I use in this paper come from a Greek bank and cover the period 2012–2015. I exploit a major internal reorganization that was implemented in one of Greece's largest banks, and led to the closure of bank units. In response

[7] Acharya et al. 2016, Bentolila, Jansen, and Jiménez 2016, Cingano, Manaresi, and Sette 2016, Popov and Van Horen 2015, De Marco 2016

to the significant rise in the NPL ratio for corporate loans, the bank established specialized NPL workout units at the end of 2013. This transfer of loans to the NPL units caused a significantly reduced workload for the original units assigned to manage corporate loans. Consequently, it became cost effective for the bank to consolidate the original units, by closing several and relocating the management of corporate loans to the closest unit that remained open. An important feature of the consolidation was the relocation of the loan officers. Loan officers who had worked in units that closed were transferred to the newly established NPL units. Loan officers who worked in units that remained open continued to manage their old loans and became additionally responsible for the loans transferred from closed units. Because firms whose loans were transferred to the closest unit lost the relation with the loan officer who had managed their loans, the feature of unit mergers, provides a good setting for testing the effect of personal relationships on loan renegotiation.

The dataset contains detailed annual information on corporate loans for approximately 8,000 small and medium nonretail enterprises (SME) covering four years (2012–2015). It includes detailed information on the loan terms and performance as well as basic firm financial information. Moreover, an indicator of the bank unit responsible for each loan is included, which allows for tracking transfers across units. Personal relationships develop between loan officers and firms at the bank unit and changes in the bank unit indicator reflect interruptions of such relations.

As I focus on the merger of the original bank units, I exclude the loans that were transferred to the specialized NPL workout units. The final sample consists of loans to 3,984 firms located across the 13 geographic regions of the country. Table 2.1 presents the summary statistics for the main variables in the pre-period (2012, 2013). A median firm in the sample has two loans with a total balance of approximately €536,000 and total collateral cover of €187,630. The average interest rate is 5.97%, and the average remaining maturity of the loans is approximately one and a half years (528.34 days). The median firm performs well, with no nonperforming exposures, and has not delayed a payment. Regarding the firm's financial information, the median firm was medium-size with approximately €6 million in total assets and €4.25 million in total debt. It has a positive EBITDA of approximately €270,000, and a high leverage equal to 0.69. The summary statistics confirm the fact that the sample is comprised of firms that performed relatively well during the crisis.

2.4 Empirical Methodology

Identification Strategy

The identification of relationship interruption is based on a bank's internal reorganization and the closure of bank units. I employ this exogenous variation, define appropriate treatment and control groups, and apply the difference-in-difference methodology to accurately estimate the effect of interrupted personal relationships between loan officers and firms on loan repayments.

The first step is to exclude loans were transferred to the specialized NPL units. The sample is thus constrained to relatively "good" firms. The next step is to identify the firms whose accounts were transferred to another unit because their original unit closed. By looking at the transfers across bank units, we observe a set of firms whose accounts were transferred to other bank units without their original unit closing. (This set of observations accounts for approximately 11% of the original sample.) It is necessary to exclude these firms from the analysis, due to the endogeneity of their transfer.

By using the closure of the original bank unit as the source of exogenous variation, I overcome the selection bias that may arise at the firm level. In particular, treated firms are defined as those whose loan accounts were transferred to another bank unit because their original unit closed. Control firms are defined as those whose loan accounts remained at the same unit during the whole period of the sample. This specification of treatment and control groups ensures that the variation comes only from the bank unit level and not from the firm level. Loans transferred from one unit to another as a result of a cause other than unit closure are excluded from the sample.

The feature that allows me to identify an interruption in personal relationships between loan officers and firms is the relocation of loan officers. Loan officers who worked in units that closed were transferred to the new NPL units. Loan officers who worked in the units that remained open continued to manage their old loans and became responsible for the loans that were transferred from the closed units. For that reason, firms that remained at the same unit continued to interact with the same loan officers, while firms whose accounts were transferred had to establish a new relationship with a different loan officer.

The baseline specification is a difference-in-difference, which allows me to compare the difference on the outcome variables between the firms in the treated group and those in the control group in the post-unit closure period (2014, 2015) relative to the difference that the two groups had in the pre-unit closure period (2012, 2013). The baseline regression is:

$$y_{iurt} = \alpha_i + \alpha_u + \alpha_{rt} + \delta(Post_t * Treat_{iur}) + \varepsilon_{iurt} \tag{2.1}$$

where y_{iurt} stands for the outcome variable for firm i obtaining a loan from bank unit u and located in region r in year t. $Treat_{iur}$ is a dummy variable equal to one for treated firms and zero for control firms. $Post_t$ is a dummy variable equal to zero for the period before the bank units' closure (from 2012 to 2013) and one after the closures (from 2014 to 2015). The baseline specification includes firm fixed effects (α_i) to capture any time-invariant characteristics of the firm. Bank unit fixed effects (α_u) as well as region $*$ year fixed effects (α_{rt}) are included to capture any time-invariant characteristics of the bank unit (e.g., a specific bank unit may have a higher lending limit) and any region and time-varying shocks respectively. For the treated firms, the bank-unit fixed effect is based on the unit to which their loans were transferred. The coefficient of interest is δ, which measures the difference in the outcome variable for the firms that experienced an interruption in their personal relationship with the loan officers, relative to the firms that did not, controlling for the pre-period difference.

The outcome variables of interest belong to three groups: (i) the performance of all loans that a firm has; (ii) the probability of renegotiating a loan (extensive margin); and (iii), loan performance of the renegotiated loans. Regarding the effect on loan performance, I examine three different outcome variables: (i) the ratio of a loan's payment days past due over the number of remaining days; (ii) a nonperforming dummy variable; and (iii), the ratio of total provisions over total loan balance.

Comparison of treated and control groups in the pre-unit closure period

The most important threat to identification is a possible selection bias that arises from the decision to close a bank unit. It is necessary to assume that the bank did not close units where debtors performed, or were expected to perform, worse. The main criterion for unit closure is geographic location: in areas where there were two or more units, the bank kept only one, while in areas with only one unit, it was optimal for the bank to keep it open to serve the main priority of client retention.

A comparison between the two groups in the pre-unit-closure period (2012–2013) provides evidence that there are no statistically significant differences across the two groups. Table 2.2 presents this comparison and includes variables related to loan terms, performance, and firms' financials. Column 1 shows the mean value and the standard deviation for the control firms, Column 2 for the treated firms, and Column 3 shows the p-value for the difference, with regional fixed effects included. The only variable significantly different across the two groups is the ratio of loans transferred to the NPL units from the originals: this variable is higher for the bank units that remained open. This difference indicates that, to the extent that units were selected for closure based on loan performance, the bank units that closed had better performing loans. For that reason, if a selection bias exists on which units closed, it will bias the results downwards. The fact that we do not observe any statistically significant difference for the loan terms, performance, and firms' financial information supports the assumption that the two groups shared similar characteristics. The probability is therefore high that the two groups would have continued to look similar if the personal relationships with loan officers had not been interrupted.

I report also parallel trend graphs in Figures 2.1 and 2.2, which show the results for the total balance. Figure 2.1a plots the mean values for the treated and the control groups, and Figure 2.1b plots the mean value of the residuals from a regression of total balance on region fixed effects. Figure 2.2 plots the regression coefficients and the confidence intervals from a regression of the log of total balance on region and bank unit fixed effects. Another important variable for which the parallel trends hypothesis should be tested is the nonperforming dummy, as higher default ratios could predict the bank unit closure. A similar analysis providing evidence against this hypothesis is conducted in Figures 2.3 and 2.4. These graphs support the hypothesis that no pre-trend difference regarding the loan amount or performance existed to predict bank unit closure.

Out-of-sample comparison of firms located in exposed and control geographic areas

In this section, I perform an out-of-sample comparison of firms located in geographic areas where a bank unit closed (exposed areas), and firms located in areas where a unit remained open (control areas). This test provides further evidence that the local economic conditions, and the firms operating in exposed and control areas, are similar. The data for this comparison come from the Amadeus Bureau van Dijk dataset and cover the same period of the experiment (2012–2015). The Amadeus dataset provides information on firms' balance sheets and income statements. In addition, I use the bank units' zip codes to identify exposed and control areas. I match the firms' zip code with the bank units' zip code, and construct a subsample of the Amadeus dataset appropriate for the out-of-sample comparison.

Table 2.3 presents the results comparing firms located in exposed and control areas for the same period of the experiment (2012–2015). The first column shows the mean value and the standard deviation of firms located in the zip code areas where a bank unit remained open, and the second column for firms located in areas where a unit closed. The third column shows the p-value of the difference. The two groups are similar across most of the variables. There is a small and significant in total assets and the number of employees between the two groups, which suggests that firms in control areas are larger. In subsection 2.5, I restrict the sample to areas in which firms have similar characteristics to provide further evidence that local economic conditions do not explain differences between control and treated groups.

The main variables of interest in Table 2.3 are those related to firms' financial performance. Any difference in these variables would suggest that the economic conditions differ between exposed and control areas. No statistically significant differences in these variables are observable (EBIT, EBITDA, net income, sales, gross profit, etc.).

Lastly, to capture the potential differences associated with unobserved economic indicators, I use the firms' financial characteristics as dependent variables and apply the baseline regression 2.1 to examine whether there is an out-of-sample effect of a bank unit closure. Column 4 of Table 2.3 shows the estimated coefficients from the difference-in-difference regressions on firms' variables. No coefficient is statistically significant. The fact that there is no statistically significant difference across any specification for the observable characteristics of firms' financial characteristics supports the assumption that economic conditions in the exposed and control areas are similar.

2.5 Results

First, I report the baseline results on on loan renegotiation and on loan performance. I examine the impact of personal relationships between loan officers and firms on the probability of renegotiating a loan and on the loan repayment. In the second part of the paper, I examine how the strength of the relationship affects the results by using three different measures of relationship strength.

Baseline Results

Effect on Probability of Renegotiation

First, I analyze the extensive margin and the probability of renegotiating a loan after a firm's loans are transferred to another bank unit. I compare firms that experienced an exogenous interruption in the personal relationship with their loan officer with those that did not. The outcome variable of interest is the probability that a firm renegotiates at least one of its pre-existing loans. The data in this section are constructed at the firm level. More formally, the dependent variable is a dummy variable equal to one if the firm renegotiated at least one of its old loans and zero if the firm did not renegotiate any loan. Table 2.4 presents the results with different specifications regarding fixed effects and control variables. It can be seen that firms whose relationship with a loan officer is interrupted have a 26% lower probability of renegotiating at least one of their pre-existing loans, compared to firms with uninterrupted relationships, when firm, bank unit, and region-year fixed effects are included.

A loan renegotiation can be initiated either by the bank or by the firm and does not require a delayed loan payment. A renegotiation is expected to be mutually advantageous, as otherwise one of the parties would not agree to the new terms. The firm benefits by renegotiating a loan, because one or more of the initial loan terms is relaxed. At the same time, the bank prevents a default or improves its covenants. Since renegotiation benefits the firms, firms with interrupted relationships receive worse treatment as the result of the transfer of their loans to another bank unit.

The results from four different specifications are presented in Table 2.4, and the results are robust. In all four specifications, bank unit fixed effects are included to capture any unit-level time-invariant variation, and the standard errors are clustered at the unit level. Even though bank lending policies in general are similar across units, a larger unit or a unit that is higher in the organizational hierarchy may have different limits on loan amounts and the terms it is allowed to approve.

Moreover, I control for the regional differences by adding region or region-year fixed effects. In the first column, region and year fixed effects are included, while in the second column region-year fixed effects are included. When constructing the region-year fixed effects, I define the region more broadly than strict geography to ensure that at least two bank units are open per region in the post-unit closure period and I combine only neighboring regions that have similar industry composition. No adjustment is done on regions that have two or more bank units in the post-period. The specification defined in Column 2 is the baseline specification of the paper.

To control for firm level differences, in Columns 1 and 2 firm fixed effects are included to capture any time-invariant characteristics of the firm such as size, industry, etc.

In Columns 3 and 4, I add different types of firm-level time-varying control variables. In Column 3, as control variables, the firm's lagged log of EBITDA and the lagged log of total loans at this bank are included. In Column 4, the lagged log of total assets is included as a control variable to control for changes in the size of the firm. The results remain robust when we control for time-varying firm characteristics, and this confirms that variations at the firm level during the years of the experiment are not driving the estimated coefficients.

Effect on Loan Performance Conditional on a Renegotiation

In this subsection, I estimate the effect of an interruption in the relationship between a firm and a loan officer on the renegotiated loan's performance. Identifying such an effect on delinquency is of crucial importance for gauging the effect of renegotiation for the bank. A crucial challenge that banks face during a recession is a steep increase in nonperforming loans (NPLs). Even though the "bad" firms that drive the high rise in the NPLs are excluded from this analysis, identifying the impact of personal relationships in the "good" firms is also significant. The "good" firms compose the most relevant group of firms for which the bank can prevent delays on loan repayments. For that reason, the value of personal relationships surges in a loan renegotiation during a recession period because they help restrict delinquencies.

Table 2.5 presents the results for the baseline empirical specification (2.1) for three variables related to loan performance ((i) log of a loan's payment days past due over the remaining days of the loan; (ii) nonperforming dummy variable; and (iii), log of total provisions over total balance). The firm outcome variables in this section are analyzed only for the loans renegotiated in the post-unit-closure period. We observe that firms with an interrupted relationship perform worse on the renegotiated loans compared to firms with an uninterrupted relationship. Treated firms have a 15.4% higher probability of defaulting on a renegotiated loan, and the bank makes a 70% higher level of provisions on renegotiated loans with interrupted relationships.

Days Past Due a Payment: Firms whose accounts were exogenously transferred to another bank unit delay their payments by more than double the number of days than those that remained at the same unit. The outcome variable is the log of a loan payment's days past due over the remaining days of the loan. In the first column of Table 2.5 we see that the number of days delayed per remaining days is 1.5 times higher for the treated group, which corresponds to approximately 27 additional days of delay.

Nonperforming: The second column reports the result for a nonperforming dummy equal to one if the loan is characterized as nonperforming and zero otherwise. An exposure is characterized as nonperforming if it is 90 days or more past due, or if it is impaired. A firm with an interrupted relationship has a 15.4% higher probability of its loan becoming nonperforming after renegotiation, compared to a firm with an uninterrupted relationship, which corresponds to an approximately 2% higher probability to default.

Loan Provisions: The last outcome variable examined here is the log of the ratio of total loan-loss provisions that the bank needs to make for the renegotiated loans relative to the total balance of the renegotiated loan. For the renegotiated loans of firms with interrupted relations, the level of provisions per total loan balance is approximately 70% higher, which corresponds to an additional €0.10 of loan-loss provisions for each euro of loan balance.

Unconditional Results on All Loans

The results on loan performance presented in the previous subsection were estimated conditional on the firm renegotiating a loan. In this subsection, I estimate the effect of an interruption in the loan officer relationship on all loans that a firm has at the subject bank. The results on all loans reveal whether significant differences between the two groups on the loan performance exist. I include in the set of all loans old loans that were not renegotiated, old loans that were renegotiated, and new loans. For this analysis, I use the baseline specification with firm, bank unit, and region-year fixed effects, and Table 2.6 presents the results.

In Table 2.6, the unconditional effect on the loan performance is presented. We do not observe a statistically significant difference in the number of days that a loan is delayed, or in the level of provisions between the firms whose relationship was interrupted and those with continuous relationships. However, we observe a higher probability of a loan becoming nonperforming if the firm experiences an interruption in the relationship with the loan officer. In particular, firms with interrupted relationships have a 9% higher probability of their loans' becoming nonperforming than do firms with continuous relationships.

By comparing the unconditional results on all loans with the results on the renegotiated loans from the previous subsection, an interesting insight arises. Based on the previous subsection, firms with interrupted relationships default more on the renegotiated loans compared to firms with continuous relationships. At the same time, firms with interrupted relationships have, on average, the same performance on all the loans they have been granted. The fact that the treated firms delay their payments by more days than the control firms only on the renegotiated loans is an indication of a moral hazard behavior. Treated firms have a stronger incentive to strategically default on a loan in order to trigger a renegotiation compared to the control firms. When the relationship with the loan officer is interrupted and a firm continues to repay its loan on time, the loan officer may be less willing to renegotiate a loan agreement as there is no credible threat that the firm will default on its loan. At the same time, when the relationship with the loan officer has been interrupted, the firm faces a significantly lower goodwill and reputation loss by strategically defaulting on a loan. These two dynamics - the unwillingness of the loan officer to renegotiate a loan unless a credible threat to default exists and the low cost if it strategically default on a payment - cause an increase in the likelihood that the firm with interrupted loan officer relationships will behave under moral hazard in order to trigger a loan renegotiation.

Sequence of Events

This subsection provides an analysis of the sequence of the events and the duration of the effect using the figures 2.5 - 2.8. Figures 2.5 - 2.8 plot the effect of an interruption in the relationship between a loan officer and a firm on the outcome variables of interest. The coefficients are obtained from a regression of the outcome variable on the treatment indicator and on region and bank-unit fixed effects.

Figure 2.5 shows that the negative effect on renegotiation appears in 2014, the first year after the interruption in the relationship between the loan officer and the firm, while in 2015 there is no statistically significant difference on the extensive margin for renegotiation between the treated and the control group. Figure 2.6 plots the effect on the log of days past due over remaining days for the loans that were renegotiated in the post-period, figure

2.7 for the nonperforming dummy variable, and figure 2.8 for the log of the total provision per total balance. The statistically significant increase at the days past due and at the total provisions lasts for two years in the post-period, while the increase in the nonperforming dummy lasts only for one year after the interruption in the relationship.

The duration of the effects indicates that treated firms delay a loan repayment in order to trigger a renegotiation that occurs in 2015, the year after the interruption in the loan officer relationship. The observation that the level of provisions increases for two years at the post-period suggests that the bank will carry the cost of the strategic delay in a loan repayment for a longer period, as firms with nonperforming loans and delays in loan repayments are characterized as riskier clients for the bank.

Addressing possible threats to identification

Identifying Possible Selection Bias on Renegotiation

One concern regarding the validity of the estimated effects of an interrupted relationship is a possible selection bias concerning whether a loan is renegotiated. In subsections 2.4 and 2.4, I demonstrated that firms in the treated and control groups share similar pre-unit closure characteristics. Two of my main findings are that firms with interrupted personal relationships have a lower probability of renegotiating a loan (subsection 2.5), and conditional on a renegotiation, these firms perform worse (subsection 2.5). If loan officers choose to renegotiate with interrupted-relationship firms based on different criteria than those used for the continuous-relationship firms, then the intensive margin results would be biased. If loan officers for interrupted-relationship loans granted renegotiation more frequently for firms with relatively inferior performance, while loan officers for continuing-relationship firms did not do so, then the estimated effect of continuing relationships on the probability of renegotiation would exaggerate the true effect of the continuing relationship. In fact however, I find the opposite, implying that my estimated effect of continuing relationships on renegotiated loans performance is conservative.

I conduct two tests, which are presented in Tables 2.7 and 2.8. The first test compares the two types of firms that renegotiated a loan in the post-period. In Table 2.7, a similar analysis as in Table 2.2 is conducted, but for this test, I include only firms that renegotiated a loan in the post-unit closure period, and compare the pre-unit closure period characteristics between the treated and control groups. We observe no statistically significant difference in any of the variables between firms with interrupted and continuing relationships that renegotiated a loan in the post-unit closure period. Firms in the two groups that renegotiated loans in the post-unit closure period shared similar characteristics in the pre-period, which suggests no selection bias exists regarding whether a loan is renegotiated.

Table 2.8 presents a second test, which considers observable variables that can prompt a renegotiation. I rank firms within each bank unit based on the year-before-the-transfer value for each variable. The decision to renegotiate a loan is made by the loan officer at the bank unit, and, for that reason, any correlation outside the unit would not be informative for the criteria used to renegotiate a loan at the bank unit. The rank within the bank unit allows me to control for the variation at the bank unit and any criterion based on hard information used by the loan officer. I regress a dummy variable for renegotiating a loan on the firm's within-bank-unit rank separately for each variable and include bank-unit and region-year fixed effects. This test provides us with correlations that show which firms have a higher probability of renegotiating a loan, based on the previous year's hard information available to the loan officer and to the econometrician. Table 2.8, Column 1 presents the result for the whole sample (pre- and post-unit closure period for treated and control firms), Column 2 for control firms in the post-period, and Column 3 for treated firms in the post-period.

The most interesting observation comes from the estimations of the year-before-the-transfer EBITDA growth rate. We observe that, for the whole sample and for the control group in the post-period, there is no statistically significant effect, while for the treated group in the post-period there is a positive statistically significant correlation. As noted above, this finding indicates my estimates are conservative.

The same observation rises also from the density distribution graphs. In figure 2.9 I plot the density distributions of the firm's rank within bank-unit based on the year-before-the-transfer log of EBITDA separately for the treated and the control group. The blue line presents the density of all firms in the group and the red line presents the density of the firms that renegotiated a loan in the post-period. It becomes apparent from the plots that there is no selection on renegotiation based on the lag EBITDA for the control group. On the other hand, for the treated group the density distribution of those that renegotiated a loan in the post-period shifts to the right, representing that there is a positive correlation between last year's EBITDA and renegotiation. This finding is consistent with the result from table 2.8 and indicates that the estimated results are conservative.

Table 2.8 implies a common rule with respect to the firm's leverage, total loans at the subject bank, and total loan-loss provision is applied for all firms, independent of whether they belong to the treated or control group. Moreover, firms in the whole sample and in the control group appear to be selected similarly for renegotiation based on their total assets, total debt, debt over EBITDA, and payments' days past due. In contrast, we do not observe a correlation between these variables and renegotiation for the treated group. Again, this finding indicates conservative estimates.

Controlling for firms' differences in zip codes locations

Subsection 2.4 discussed the observed differences between firms located in areas where a bank unit closed (exposed) and in areas where a bank unit remained open (control). Based on the zip code comparison using the Amadeus dataset presented in Table 2.3, firms located in the exposed areas are smaller in size. Even though I control for the log of total assets in the estimated results, in this section, I provide further evidence that firms' differences across geographic locations are not driving the results.

To accurately control for potential differences in the geographic location of the exposed and the control areas, I restrict the sample to areas in which firms across the two areas have no statistically significant difference in financial variables. In particular, I exclude firms located in Attica, the region where Athens is located, and I repeat the analysis. Table 2.9 presents the results of the subsample on the comparison of exposed and control areas using the Amadeus dataset. This finding confirms that no significant difference in financial variables obtains between the two groups in the subsample.

The next step is to show that, when I restrict the sample to areas where firms across the two groups are similar, the estimated results on the outcome variables hold. Table 2.10 presents these results. The estimated coefficients for both the probability to renegotiate a loan and the intensive margin results on loan performance are similar in economic significance to the baseline results. This confirms that the baseline results are not driven by the differences on the firm's characteristics nor by the economic conditions at the zip-code level.

Results by Relationship Strength

In this section, I further investigate the value of a relationship between a loan officer and a firm. I construct three different measures of relationship strength and compare the impact of an interrupted relationship on firms with stronger relationships and those with weaker relationships. This section sheds light on two different mechanisms that could drive the repayment behavior of a firm; a moral hazard behavior when the firm has access to outside sources of financing and the expectation of repeated interaction with the subject bank in the future when the firm has no outside option to seek financing and lower bargaining power.

Relationship Strength Measured by the Dependence Ratio

The first measure of relationship strength is the ratio of a firm's total amount of loans at this bank over its total debt to other banks. This measure shows whether a firm had an established relationship with other banks or whether it borrowed predominantly from the bank featured in this study. One of the main assumptions for a mutually advantageous renegotiation is that the firm has outside options for financing, as those options would increase the firm's bargaining power. For small-and medium-size corporations, the outside options for financing are either other local banks, or raising inside or outside private equity, as these relatively smaller firms do not have access to the stock market or from foreign banks.

It follows that if a firm borrows from other banks in the pre-period, and has an established relationship with them, it is easier for the firm to seek financing from other banks once the relationship with the subject bank is interrupted. On the other hand, if the firm depends mostly on the subject bank to satisfy its financing needs, then its negotiation power is limited. This section provides a comparison of these two groups, i.e., firms with closer relationships and fewer outside options versus firms with weaker relationships and more outside options.

As a measure of how close the relationship is, I estimate an indicator variable which I label the *Dependence Ratio*. The *Dependence Ratio* is defined as the ratio of the total balance at this bank over the total debt that a firm had to other banks in 2013, the year before the bank units' closure. First, I estimate the correlation between the *Dependence Ratio* and the probability of renegotiating a loan. Table 2.11 presents the correlation results. The first two columns show the results for the whole sample in the pre- and the post-unit closure period, where Columns 3 and 4 report results for the control firms, and Columns 5 and 6 for the treated firms. There is a positive and significant correlation between the dependence ratio and the probability of renegotiating a loan, suggesting that firms with closer relationships have a higher probability of renegotiating. Note that this correlation is not significant for the treated group in the post-period, although it was significant in the pre-period. This suggests a loss in the value of close relationships between the firm and the bank once the relationship with the loan officer is interrupted. Treated firms that borrowed predominantly from the subject bank in the pre-unit-closure period do not have a higher probability of renegotiating in the post-unit-closure period.

Figure 2.10 presents the density distribution of the *Dependence Ratio* in 2013. The higher the value of the ratio, the more dependent the firm is on this bank. The lower the value of the ratio, the less important this bank is to the firm, since it borrowed from multiple sources. As shown in Figure 2.10, the majority of firms borrow from multiple banks, while a smaller number borrows mostly from the subject bank. In the following analysis, I test separately the effect of interrupted relationships for firms with closer relationships, and for those with more distant ones.

Tables 2.12–2.13 present the results for these two subsamples. Firms with a *closer relationship* are defined as those with a dependence ratio above 50%. Firms with a *less close relationship* are defined as those with a dependence ratio below 20%.[8] The results are estimated using the baseline empirical specification 2.1. Table 2.12 shows that, independent of whether the firm had a close relationship with the subject bank in the pre-unit closure period, the firm has a 28% lower probability of renegotiating a loan after the relationship is interrupted.

Based on the estimations in Table 2.13, treated firms whose relationship with the subject bank is close perform better, and their loans have a lower probability of becoming nonperforming, compared to the control firms. Despite receiving worse loan terms on their renegotiated loans, firms with close and interrupted relationships perform better, compared to firms with close and continuing relationships. These results suggest that firms with limited or no outside options may have lower bargaining power and may try to gain the trust of the new loan officer by performing better.

On the other hand, firms that have outside financing options and whose relationships

[8]The cutoffs were chosen to secure enough statistical power to the tests. The results remain robust under different cutoff thresholds, and tables are available upon request.

with the subject bank were interrupted, ceteris paribus, default more on renegotiated loans. These firms have less to lose if they negotiate harder by threatening default to gain better terms. That creates a higher incentive for them than for the control firms to default on their loans at the subject bank. Overall, the fact that the bank unit closure causes an improvement in loan terms and an increase in defaults for firms with outside financing options, posits *moral hazard* behavior as a possibility for gaining better terms on renegotiated loans.

Relationship Strength Measured by the Number of Interactions

The second measure of relationship strength I use is the number of interactions each firm had with the bank in 2013. I identify interaction as either the issuance of a new loan or a renegotiation of an old loan. The number of interactions in 2013 shows the minimum number of contact events that a firm had with the loan officer the year before the bank units' closure, and this number indicates their relationship strength.

Table 2.14 shows the summary statistics of the number of loans, new loans, and renegotiations per year. The mean number of interactions per year is 3.6, and the median is 2, which is the same as the number of loans. Figure 2.11 presents the density distributions of the number of loans, interactions, new loans, and renegotiations. We observe that the distributions are highly skewed, as is indicated by the summary statistics.

In the following analysis, I separate the sample between those firms with more than two interactions in 2013 and those with two or fewer. The number two is selected as this is the median of the total number of interactions in 2013.[9] Firms with more than two interactions are expected to have a closer relationship with the loan officers who negotiated two or more

[9]The results remain robust under different cutoff thresholds and tables are available upon request.

loan contracts in the previous year. On the other hand, firms with two or fewer interactions are expected to have a weaker relationship. More frequent interactions with a loan officer in the pre-period suggests that frictions arising in loan contracts were overcome in the past, and therefore, the parties would be more likely to reach an optimal contract in the future. Once the relationship is interrupted, the benefit of the previous history is lost.

The results of this analysis are presented in tables 2.15–2.16. Table 2.15 presents the results for the probability of renegotiating a loan for the two groups. Firms with more than two interactions in the pre-unit closure period and an interrupted relationship have a higher probability of renegotiating a loan than firms with more than two interactions and uninterrupted relations. On the other hand, among the firms with two or fewer interactions, those with interrupted relationships have a 37% lower probability of renegotiating than those with uninterrupted relationships. With respect to the effect on renegotiated loans' performance, an increase occurs both in defaults and in provisions among firms with two or fewer interactions. Table 2.16 presents the results on renegotiated loans' performances. On the other hand, among firms with more than two interactions, we do not observe any statistically significant effect for delays on loan repayments or defaults, and we observe a decrease in the level of provisions.

The results discussed in this subsection confirm the hypothesis that personal relationships have an additional value for loan renegotiation. This added value is more important for firms with closer relationships with the bank (i.e., firms with more than two interactions), as these firms, despite receiving tougher loan terms, continue to perform well. This behavior could be explained as a signal for future expected interactions. On the other hand, firms with two or fewer interactions in the pre-period appear to default more on their renegotiated loans, and this result suggests a less valuable relationship for them. Overall, the results from this subsection suggest that the expectation of future regular interactions with the bank affects the behavior of the firm, as we do not observe a significant effect in defaults among the firms with frequent interaction.

Relationship Strength Measured by the Relative Size of the Firm

In this section, I use as a third measure of relationship strength the relative size of the firm as a corporate client within a bank unit. I estimate this measure as the ratio of the total balance of the firm over the total amount of loans that each bank unit manages in 2014, which captures the exposure of that bank unit to each corporate borrower: The larger the ratio, the larger the exposure of the bank unit to this client. The relative size of the firm is potentially correlated with two effects. Although, for larger firms, the potential profit margin for the bank is higher, the bank is also exposed to a higher risk. As both the profit margin and the risk are higher for larger firms, it is expected that loan officers will have a stronger incentive to establish closer relationships with larger clients.

Figure 2.12 shows the density distribution of the relative size of the firm within the bank unit. As expected, the distribution is highly skewed, and the majority of the firms are small clients for the bank unit. The indicator of relative firm size is estimated based on 2014 data, the first year after the transfer occurred. I use the relative size after the transfer to control for different behaviors towards the large clients in the post-period independent of whether a firm was in the treated or control group. In the following tests, I look separately at larger and smaller firms. I define as larger firms those whose balance at the specific bank unit accounts for at least 1% of the total amount of corporate loans the unit manages. The rest of the sample is defined as relatively small firms.[10]

Tables 2.17–2.18 present the results for larger and smaller firms separately. From the results on the extensive margin, we observe that firms with smaller balances and an interrupted relationship have a lower probability of renegotiating a loan following the transfer to another bank unit. On the other hand, for firms with larger balances, no statistically significant difference obtains between the treated and control groups. Regarding renegotiated loans' performance, firms with smaller balances and interrupted relations evidently delay longer in their loan repayments, default more on their loans, and have an increased level of provision for these loans. In particular, among firms with smaller balances, treated firms have a 50% higher probability of becoming nonperforming. On the other hand, among firms with larger balances, even though the treated firms delay their payments by additional days, these firms have an 87% lower probability of becoming nonperforming.

Results by Firms Pre-Unit-Closure Period Performance

In this section, I analyze whether the firm's pre-unit-closure period loan performance influences the estimated results. I separate the sample between firms that delayed a loan payment in 2013, the year before the transfer, and firms that paid their loans on time. Tables 2.19–2.20 present the results of the main outcomes of interest separately for the two groups. We observe that both firms with good repayment behavior and those with delays in loan payments have a lower probability of renegotiating a loan when their loan officer relationships are interrupted.

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[10]The cutoffs were chosen to secure enough statistical power to the tests. The results remain robust under different cutoff thresholds, and tables are available upon request.

With respect to the renegotiated loans' performance, we observe a difference between the two groups. In particular, among the firms that delayed a loan payment in the past, treated firms have a lower probability of becoming nonperforming (or a higher probability to perform well in the post-period if the loan was delinquent in the pre-period) and a lower level of provisions is recorded after the relationship with the loan officer is interrupted. In contrast, treated firms with on time loan payments in the past have a higher probability of becoming nonperforming in the post-period, and a higher level of provisions is offered.

Overall, the results indicate that the impact of an interrupted relationship on the probability of renegotiating a loan is the same for firms with good repayment histories and for those with delays in their loan payments. The results on the renegotiated loans' performance suggest a moral hazard behavior from firms with previously on-time loan payments, as these firms appear to default more on their loans in the post-unit-closure period.

Moral Hazard or Anticipated Repeated Interaction

Arguably, the superior outcomes of continuing-relationship firms reflect, in part, endogenous decisions made by those firms, and not merely soft information about their condition. The loan repayment behavior of the treated firms may be driven by two distinct underlying mechanisms. In the first mechanism, the repayment behavior is driven by moral hazard, and in the second, it is driven by the expectation of repeated interaction with the subject bank. These two behaviors have opposite predictions for firms' performance. According to the moral hazard hypothesis, a treated firm has an incentive to strategically default on its loan payments, to exercise its negotiation power, and to gain better loan terms. Under this hypothesis, we would observe an increase in loan defaults. However, under the repeated interaction hypothesis, a firm would highly value the relationship with the bank and, in anticipation of repeated interactions, would perform well in the present, despite possibly unfavorable loan terms.

We observe that firms whose personal relationships with a loan officer have been interrupted perform worse on their renegotiated loans. The results from two tests suggest that this difference in renegotiated loan performance may reflect moral hazard. First, in subsection 2.5, we discussed that there is a positive statistically significant correlation between renegotiating a loan and the EBITDA growth rate only for the treated group in the post-period. Given this positive correlation, we cannot conclude that the estimated increase on loan defaults is driven by the firms' profitability. Moreover, the unconditional results discussed in section 2.5 reveal no significant effect on payment delays, and on the level of provisions for all loans that firms had. This result indicates that firms with interrupted relationships continued to repay on time the loans that were not renegotiated.

The analysis based on relationship strength in section 2.5 allows us to examine separately different groups of firms whose behavior may be driven by the anticipated repeated interaction with the bank. Specifically, firms with a higher dependence ratio (subsection 2.5), and firms with a greater number of interactions with the bank in the pre-unit consolidation period (subsection 2.5), perform better on the renegotiated loans, despite receiving tougher loan terms in the post-period, compared to the control group. This result points to behavior driven by an anticipated repeated interaction with the bank. Namely, firms with a close pre-period relationship with the bank, despite the interrupted relationship with the loan officer, perform well on their loans in anticipation of future interactions with the new loan officer. On the other hand, firms that do not place a high value on the relationship with this bank (either firms with low dependence ratios and outside options to seek financing or firms with a low number of interactions) have a stronger incentive to exercise their bargaining position, and default strategically on their loans to gain better loan terms. Overall, the analysis on the two groups of firms based on relationship strength allows us to identify two opposite plausible mechanisms driving the behavior of a firm: repeated interaction and moral hazard.

2.6 Conclusion

Personal relationships have a significant positive effect in corporate loan renegotiation, mitigating the costs of distress for both firms and banks. A relationship between a loan officer and a firm helps eliminate frictions that arise in loan renegotiation. When a relationship is interrupted, the renegotiation outcome is less likely to be beneficial and the efficient contract is less likely to be achieved. Using the consolidation of bank units as a source of exogenous variation, I analyze a proprietary dataset on corporate loans. I find strong evidence that a personal relationship with a loan officer significantly affects loan renegotiation outcomes both at the extensive and intensive margins. Notably, I observe that firms with interrupted relationships have a lower probability of renegotiating a loan upon their transfer to another bank unit. The value of maintaining relationships is also visible in measures of loan delinquency. Strong personal relationships between the bank officer and the firm prevent defaults. Firms with interrupted relationships perform worse on their renegotiated loans than those with stable relationships. This paper also analyzes the repayments of the firms with interrupted relationships: Their behavior could be driven, in part, either by moral hazard or by the expectation of future regular interaction with the bank. A detailed analysis based on relationship strength allows us to examine separately different firms with closer and weaker relationships and find suggestive evidence of these two opposite mechanisms.

2.7 Figures and Tables

Table 2.1: Summary statistics based on the 2012–2013 values

Variable	Mean	Std. Dev.	Median
Loan-Related Variables:			
Total Balance	2,611,645	10,057,676.6	535,934.5
Interest Rate	.0597	.034	.0621
Total Collateral Value	3,234,279	20,128,240	187,630
Days Remaining (Maturity)	528.34	995.16	52.35
Unsecured loan (Dummy)	.35	.45	0
Secure type of collateral (Dummy)	.50	.45	.51
Number of loans per year	4.72	29.31	2
Performance-Related Variables :			
Nonperforming (Dummy)	.11	.31	0
Days Past Due	29.8	82.32	0
Days Past Due over Remaining Days	10.1	52.03	0
Total Provision	1,438,472	30,789,946	0
Debtor Renegotiated a Loan (Dummy)	.59	.49	0
At Least One Forborne (Dummy)	.05	.21	0
Firm's Financial Information :			
Total Assets	35,710,880	204,072,593	6,124,094
Total Debt	24,219,150	140,099,180	4,253,020
Total Equity	12,862,210	90,149,735	1,644,740
EBITDA	1,384,210	26,161,717	271,487
Total Debt over EBITDA ratio	-26.7	3,325.4	7.5
Leverage (Debt over Assets)	.72	.72	.69

This table displays summary statistics of the main variables. The variables are constructed at the firm level. A simple sum of all loans a firm holds each year is used for the total balance, total collateral value, number of loans, and total provision. A weighted average with weights equal to the ratio of the specific loan exposure over the total balance of the firm is used for the interest rate, days remaining, nonperforming (dummy), and days past due. All variables are based on the 2012–2013 values.

Table 2.2: Comparison of pre-period mean values for treated and control groups

Variable	(1) Control	(2) Treated	(3) *p* - value on Difference
Loan-Related Variables:			
Total Balance	934,255.3	791,929.6	(0.571)
	(1,497,483.4)	(1,232,984.1)	
Interest Rate	0.0618	0.0657	(0.323)
	(0.0325)	(0.0334)	
Days Remaining (Maturity)	516.9	723.4	(0.198)
	(985.4)	(1,207.4)	
Total Collateral Value	737,344.5	811,042.0	(0.831)
	(1,342,033.5)	(1,362,189.0)	
Unsecured Loan (Dummy)	0.548	0.478	(0.348)
	(0.498)	(0.501)	
Secure Type of Collateral	0.473	0.539	(0.483)
	(0.450)	(0.460)	
Performance-Related Variables:			
Nonperforming (Dummy)	0.106	0.124	(0.735)
	(0.308)	(0.330)	
Days Delayed over Remaining Days	11.20	17.98	(0.201)
	(63.98)	(65.67)	
Total Provision per Total Exposure	0.114	0.143	(0.467)
	(1.828)	(0.655)	
Ratio of Loans Transferred to NPL specialized unit	0.462	0.434	(0.000)
	(0.00107)	(0.00126)	
Debtor's Financial Information:			
Total Assets	10,712,500.0	7,983,826.1	(0.632)
	(15,995,732.5)	(12,173,003.3)	
Total Debt	7,473,765.0	6,565,431.2	(0.884)
	(11,426,411.1)	(10,640,303.1)	
Total Equity	3,543,320.3	2,207,924.3	(0.273)
	(6,546,923.7)	(3,996,019.6)	
EBITDA	1,413,431.1	596,890.4	(0.208)
	(26,589,773.0)	(2,541,047.6)	
Leverage (Debt over Assets)	0.704	0.717	(0.879)
	(0.316)	(0.240)	

This table displays the mean values and standard deviations separately for the treated and control groups. Column 3 reports the *p*-value for the difference between Columns 1 and 2. *p*-values are obtained from a regression of the main variable on a treatment indicator and region fixed effects. All variables and estimations are based on the 2012–2013 values.

Figure 2.1: Trends of the total exposure for the treatment and control groups (mean values and residuals)

(a) mean values

(b) residuals from region f.e. and a constant

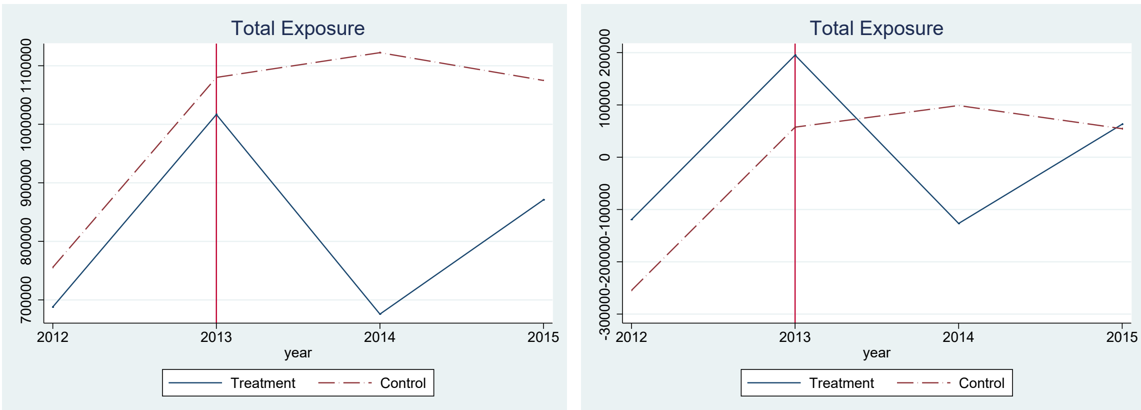


Figure 2.1a plots the mean values of the total exposure for the treated and control groups over the period from 2012 until 2015. Figure 2.1b plots the residuals from a regression of the total exposure on region fixed effects for the treated and control groups for the same period.

Figure 2.2: Regression coefficients and confidence intervals after controlling for region and bank unit fixed effects

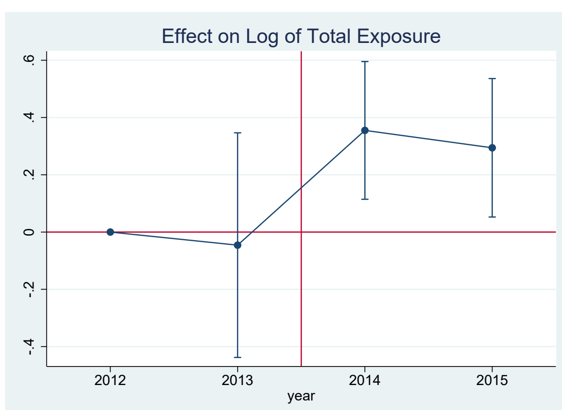


Figure 2.2 plots the effect of an interruption in the relationship between a loan officer and a firm on the log of total exposure. The coefficients are obtained from a regression of the log on total exposure on the treatment indicator and on region and bank-unit fixed effects. Bars show 95% confidence intervals.

Figure 2.3: Trends for the nonperforming dummy for the treatment and control groups (mean values and residuals)

(a) mean values (b) residuals from region f.e. and a constant

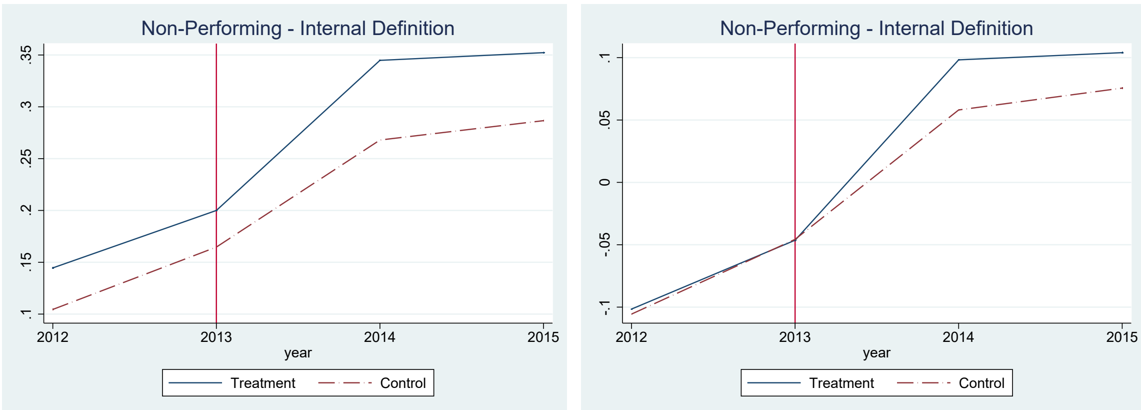


Figure 2.3a plots the mean values of the nonperforming dummy variable for the treated and the control groups from 2012 until 2015. Figure 2.3b plots the residuals from a regression of the nonperforming dummy variable on region fixed effects for the treated and control groups over the same period.

Figure 2.4: Regression coefficients and confidence intervals after controlling for region and bank-unit fixed effects

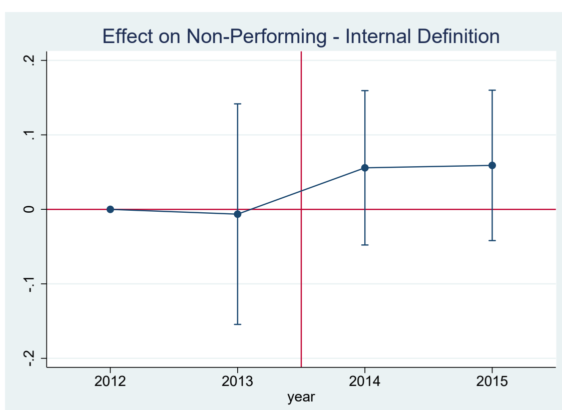


Figure 2.4 plots the effect of an interruption in the relationship between a loan officer and a firm on the nonperforming dummy variable. The coefficients are obtained from a regression of the nonperforming dummy on the treatment indicator and on region and bank-unit fixed effects. Bars show 95% confidence intervals.

Table 2.3: Summary statistics and Difference-in-Difference coefficients for firms in exposed and control areas - Match on the zip code

	(1) Mean values for Control areas	(2) Mean values for Exposed areas	(3) p - value on Difference	(4) Coefficient from DiD
Total Assets	2,046,750.4	1,825,612.9	(0.053)	-1,198.2
	(2,213,766.2)	(2,042,689.0)		(42,739.4)
Total Debt	498,087.1	458,946.9	(0.273)	12,978.2
	(1,031,146.5)	(974,348.2)		(13,685.6)
Shareholders Funds	906,964.1	780,377.7	(0.015)	-18,101.5
	(1,336,612.6)	(1,086,455.0)		(21,782.0)
Number of Employees	13.33	11.86	(0.034)	-0.131
	(15.84)	(12.78)		(0.317)
EBIT	95,948.5	70,551.1	(0.138)	-2,360.2
	(215,807.3)	(186,164.2)		(6,868.1)
EBIT (Growth Rate)	-0.450	-0.0356	(0.368)	2.070
	(29.85)	(8.002)		(3.758)
Net Income	35,171.8	20,397.8	(0.396)	-2,071.4
	(162,093.6)	(146,359.9)		(5,756.2)
Net Income	-0.482	-1.015	(0.252)	-0.632
(Growth Rate)	(11.70)	(25.73)		(0.976)
Operating Revenue	1,640,175.0	1,430,693.6	(0.205)	27,648.1
	(2,496,198.6)	(2,291,917.3)		(30,733.6)
Sales	1,609,808.0	1,401,415.3	(0.205)	25,852.7
	(2,476,738.2)	(2,267,099.8)		(30,432.5)
Gross Profit	440,155.2	374,192.1	(0.102)	-1,232.8
	(584,430.4)	(528,494.3)		(16,640.0)
Cash Flow	95,235.7	79,114.4	(0.094)	-1,455.9
	(194,803.9)	(171,806.5)		(7,490.2)
Return on Shareholders	12.50	5.947	(0.259)	-1.996
Funds (%)	(78.81)	(72.12)		(4.009)
Return on Total	3.153	2.784	(0.298)	-0.161
Assets (%)	(13.62)	(13.74)		(0.652)
Profit Margin (%)	2.185	1.091	(0.527)	0.0524
	(20.73)	(21.09)		(0.652)

The data source for this table is the Amadeus Bureau van Dijk database. This table in columns 1 and 2 displays the mean values and standard deviations for firms located in zip-code areas where a bank unit closed (exposed areas) and firms located in zip-code areas where a unit remained open (control areas). Column 3 reports the p-value for the difference between Columns 1 and 2. p-values are obtained from a regression of the main variable on an indicator of whether the firm is located at the zip-code area where a bank unit closed, and region fixed effects. Column 4 presents the estimate coefficients from difference-in-difference regressions on the firms' variables. All variables and estimations are based on 2012–2015 values.

Table 2.4: Effect on renegotiating a loan (extensive margin)

	(1)	(2)	(3)	(4)
Post ∗ Treat	-0.231**	-0.262***	-0.236**	-0.308***
	(0.0879)	(0.0783)	(0.114)	(0.0744)
Observations	12,540	12,540	7,220	11,023
R^2	0.284	0.287	0.042	0.302
Firm F.E.	✓	✓		
Bank Unit F.E.	✓	✓	✓	✓
Region F.E.	✓			
Year F.E.	✓			
Region ∗ Year F.E.		✓	✓	✓
Lagged log of EBITDA			✓	
Lagged log of Total Balance			✓	
Lagged log of Total Assets				✓
Cluster Level		Bank Unit		

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1. The dependent variable is a dummy variable that is equal to one if the firm *i* renegotiated a loan at time *t* and zero otherwise. The dummy variable *Treat* is equal to one if the firm experienced an interruption in its relationship with the loan officer due to the bank unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013). The bottom of the table depicts information on the fixed effects and the control variables included. Standard errors are corrected for clustering at the bank-unit level.

Table 2.5: Effect on variables related to loan performance - Only renegotiated loans

	(1) *Log of Days Past Due over Remaining Days*	(2) *Nonperforming (Dummy)*	(3) *Log of Total Provision per Total Balance*
Post ∗ *Treat*	1.576***	0.154***	0.696***
	(0.176)	(0.0395)	(0.216)
Observations	5,368	5,368	5,368
R^2	0.013	0.043	0.123
Firm F.E.	✓	✓	✓
Bank Unit F.E.	✓	✓	✓
Region ∗ Year F.E.	✓	✓	✓
Cluster Level		Bank Unit	

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1 only for firms that renegotiated a loan over the sample period for three variables related to loan performance. The observations included in the analysis are the loans that were renegotiated at least once during the sample period. Each column shows the estimated results for a different dependent variable. Column 1 presents the estimated result for the log of days past due a loan payment over the number of remaining days. Column 2 is the estimated result for a nonperforming dummy variable that is equal to one if the loan is characterized as nonperforming and zero otherwise. Column 3 presents the result on the log of total value of loan-loss provision over total loan balance. The dummy variable *Treat* is equal to one if the firm experienced an interruption on its relationship with the loan officer due to the bank-unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013). In the baseline regressions, firm, bank-unit, and region∗year fixed effects are included. Standard errors are corrected for clustering at the bank-unit level.

Table 2.6: Unconditional effect on loans' performance

	(1)	(2)	(3)
	Log of Days Past Due over Remaining Days	*Nonperforming (Dummy)*	*Log of Total Provision per Total Balance*
Post $*$ *Treat*	-0.140	0.0951***	0.0198
	(0.107)	(0.0308)	(0.0247)
Observations	14,540	14,540	14,538
R^2	0.048	0.164	0.209
Firm F.E.	✓	✓	✓
Bank unit F.E.	✓	✓	✓
Region $*$ Year F.E.	✓	✓	✓
Cluster Level		Bank unit	

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1 for all loans that firms hold over the sample period. The observations included in the analysis are the new loans, loans that were renegotiated, and old loans that were not renegotiated. The dependent variables are the log of days past due over the number of remaining days, a nonperforming dummy variable, and the log of total provision over total balance for the loans that firm i had at year t. The dummy variable *Treat* is equal to one if the firm experienced an interruption in its relationship with the loan officer due to the bank-unit closure ,and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013). Firm, bank-unit, and region$*$year fixed effects are included. Standard errors are corrected for clustering at the bank-unit level.

Figure 2.5: Renegotiation Dummy - Regression coefficients and confidence intervals after controlling for region and bank-unit fixed effects

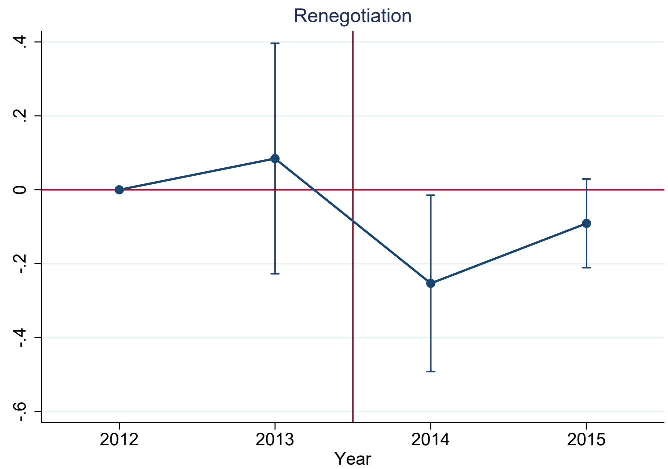


Figure 2.5 plots the effect of an interruption in the relationship between a loan officer and a firm on the renegotiation dummy variable. The coefficients are obtained from a regression of the renegotiation dummy on the treatment indicator and on region and bank-unit fixed effects. Bars show 95% confidence intervals.

Figure 2.6: Days Past Due over Remaining Days - Regression coefficients and confidence intervals after controlling for region and bank-unit fixed effects

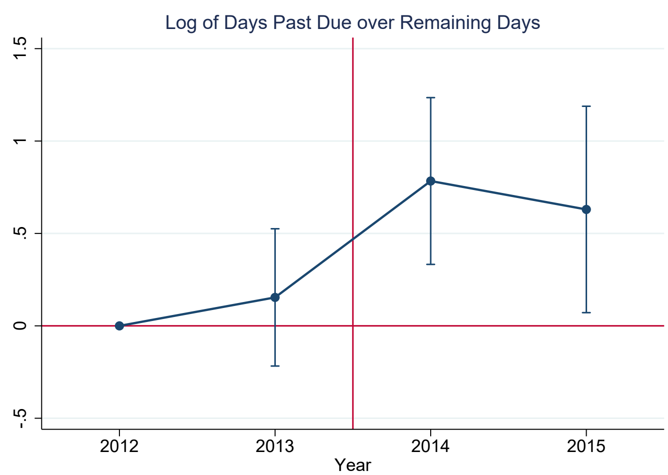


Figure 2.6 plots the effect of an interruption in the relationship between a loan officer and a firm on the log of days past due over remaining days for the loans that were renegotiated in the post-period. The coefficients are obtained from a regression of the ratio on the treatment indicator and on region and bank-unit fixed effects. Bars show 95% confidence intervals.

Figure 2.7: Nonperforming Dummy - Regression coefficients and confidence intervals after controlling for region and bank-unit fixed effects

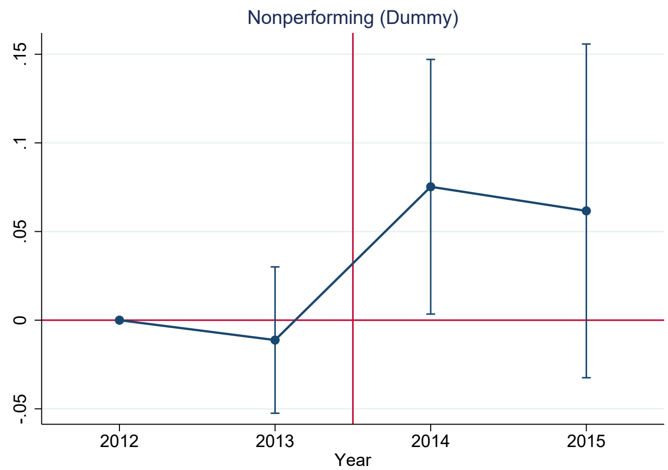


Figure 2.7 plots the effect of an interruption in the relationship between a loan officer and a firm on the nonperforming dummy variable for the loans that were renegotiated in the post-period. The coefficients are obtained from a regression of the nonperforming dummy on the treatment indicator and on region and bank-unit fixed effects. Bars show 95% confidence intervals.

Figure 2.8: Total Provision per Total Balance - Regression coefficients and confidence intervals after controlling for region and bank-unit fixed effects

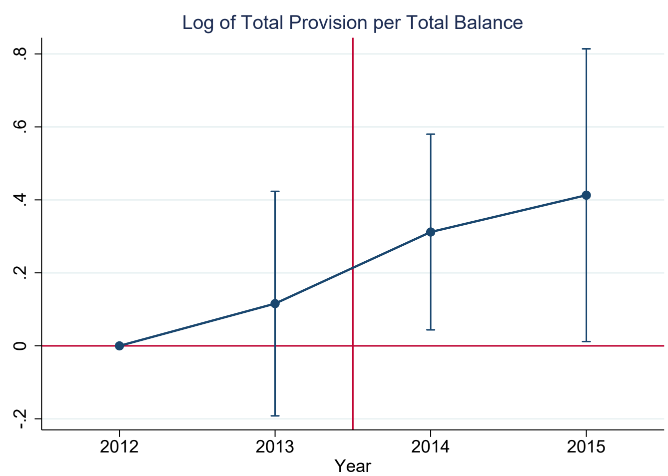


Figure 2.8 plots the effect of an interruption in the relationship between a loan officer and a firm on the log of the total provision per total balance for the loans that were renegotiated in the post-period. The coefficients are obtained from a regression of ratio on the treatment indicator and on region and bank-unit fixed effects. Bars show 95% confidence intervals.

Table 2.7: Comparison of pre-period mean values for treated and control groups - Only firms that renegotiated a loan

Variable	(1) Control	(2) Treated	(3) p - value on Difference
Loan-Related Variables:			
Total Balance	950,778.6	812,330.8	(0.656)
	(1,521,659.9)	(1,286,015.7)	
Interest Rate	0.0632	0.0665	(0.328)
	(0.0339)	(0.0335)	
Days Remaining (Maturity)	402.8	565.2	(0.561)
	(839.2)	(1062.5)	
Total Collateral Value	790,269.9	797,576.3	(0.749)
	(1,387,600.2)	(1,356,898.3)	
Unsecured Loan (Dummy)	0.340	0.339	(0.942)
	(0.443)	(0.462)	
Secure Type of Collateral	0.467	0.510	(0.917)
	(0.445)	(0.455)	
Performance-Related Variables:			
Nonperforming (Dummy)	0.122	0.140	(0.254)
	(0.327)	(0.347)	
Days Delayed over Remaining Days	13.10	19.76	(0.266)
	(69.75)	(68.74)	
Total Provision per Total Exposure	0.136	0.156	(0.618)
	(2.007)	(0.700)	
Ratio of Loans Transferred	0.462	0.434	(0.000)
to NPL specialized unit	(0.00107)	(0.00126)	
Debtor's Financial Information:			
Total Assets	10,413,859.5	6,882,262.0	(0.187)
	(15,273,643.4)	(7,440,389.4)	
Total Debt	7,333,025.7	5,330,367.5	(0.134)
	(11,067,786.7)	(4,948,417.3)	
Total Equity	3,402,715.3	2,163,741.2	(0.459)
	(6,372,251.9)	(3,981,664.0)	
EBITDA	1,044,784.6	601,476.2	(0.479)
	(26,063,090.9)	(2,663,656.7)	
Leverage (Debt over Assets)	0.714	0.719	(0.521)
	(0.321)	(0.241)	

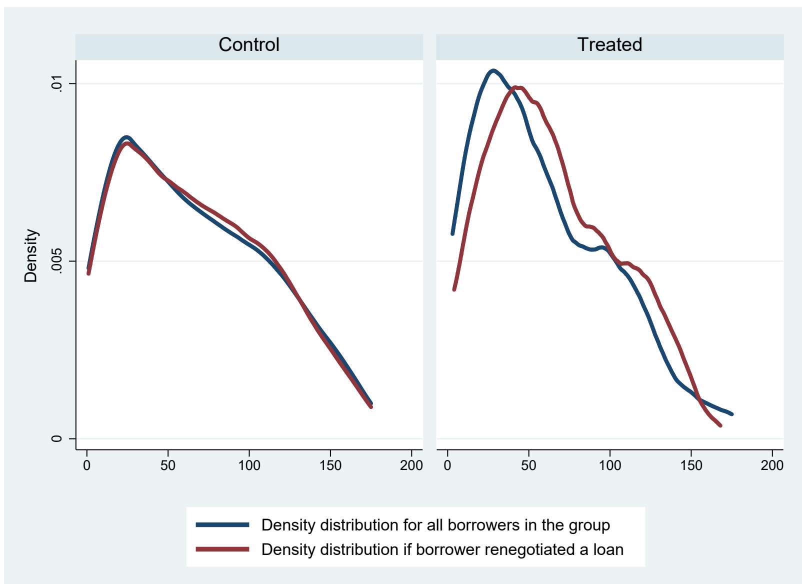
This table displays the mean values and standard deviations for the treated and control groups only for firms that renegotiated at least one loan. Column 3 reports the p-value for the difference between Columns 1 and 2. p-values are obtained from a regression of the main variable on a treatment indicator and region fixed effects. All variables and estimations are based on the 2012–2013 values.

Table 2.8: Identify possible selection on renegotiation

	(1) Whole Sample pre- & post-period	(2) Only control group post-period	(3) Only treated group post-period
	Renegotiation	*Renegotiation*	*Renegotiation*
All variables are in lagged logs:			
Firm Variables:			
EBITDA growth rate	0.000147	-0.0000467	0.00102***
	(0.000145)	(0.000300)	(0.000328)
Total Assets	0.000613**	0.000570**	0.000574
	(0.000229)	(0.000228)	(0.000685)
Total Debt	0.000853***	0.000802***	0.00141
	(0.000191)	(0.000192)	(0.000976)
Leverage	0.000635***	0.000602**	0.00132*
	(0.000205)	(0.000221)	(0.000763)
Debt over EBITDA	0.000653***	0.000706***	0.000849
	(0.000134)	(0.000119)	(0.000715)
Loan Variables:			
Total Balance	0.00136***	0.00169***	0.00229***
	(0.000186)	(0.000190)	(0.000638)
Total Provision	0.00166***	0.00186***	0.00172***
	(0.000148)	(0.000190)	(0.000572)
Days Past Due	0.00118***	0.00121***	0.000122
	(0.000120)	(0.000135)	(0.000337)
Bank Unit F.E.	✓	✓	✓
Region $*$ Year F.E.	✓	✓	✓
Cluster Level		Bank Unit	

This table displays the estimated coefficients from separate regressions of the renegotiation dummy variable on firm and loan variables. The independent variables are constructed as the firm's rank within bank unit based on each variable's value at the year before the transfer. Bank-unit and region$*$year fixed effects are included, and standard errors are clustered at the bank-unit level. Column 1 reports the estimated coefficients for the whole sample (treated and control groups over the whole period). Column 2 reports the coefficients for the control group in the post-period. Column 3 reports the coefficients for the treated group in the post-period.

Figure 2.9: Density of the firm's rank within bank unit based on profitability



Density of the firm's rank within bank unit based on the year-before-the-transfer log of EBITDA separately for treated and control firms as well as for those that renegotiated a loan in the post-period and those that didn't.

Table 2.9: Summary statistics and Difference-in-Difference coefficients for firms in exposed and control areas - Exclude Attica and match on the zip code

	(1) Mean values for Control areas	(2) Mean values for Exposed areas	(3) p - value on Difference	(4) Coefficient from DiD
Total Assets	1,626,610.8	2,014,598.6	(0.399)	87,059.6
	(1,743,132.7)	(2,138,976.2)		(71,485.0)
Total Debt	355,360.0	549,881.4	(0.110)	-19,373.3
	(790,474.9)	(1,038,056.8)		(17,135.4)
Shareholders funds	785,544.4	907,094.8	(0.814)	33,374.6
	(1,189,319.0)	(1,129,909.4)		(25,500.5)
Number of Employees	11.37	12.33	(0.459)	0.0734
	(12.07)	(13.80)		(0.588)
EBIT	83,964.2	60,042.6	(0.114)	14,415.8
	(172,477.7)	(169,693.1)		(12,099.1)
EBIT	-3.783	-0.167	(0.161)	26.74
(Growth Rate)	(79.72)	(9.234)		(26.61)
Net Income	32,158.8	9,409.9	(0.558)	9,655.2
	(137,600.4)	(138,692.2)		(8,291.5)
Net Income	-1.283	-0.942	(0.782)	1.042
(Growth Rate)	(16.41)	(20.60)		(0.975)
Operating revenue	1,219,683.1	1,503,067.8	(0.597)	70,398.0
	(1,796,479.7)	(2,435,916.8)		(49,835.7)
Sales	1,189,756.5	1,474,594.3	(0.576)	63,702.5
	(1,794,288.1)	(2,414,726.8)		(50,256.1)
Gross Profit	374,294.3	334,154.7	(0.436)	-4,988.0
	(454,295.2)	(475,334.0)		(37,769.4)
Cash Flow	88,236.9	81,123.2	(0.192)	5,758.8
	(175,649.1)	(168,534.7)		(14,592.4)
Return on Shareholders	6.544	-0.434	(0.146)	0.869
Funds (%)	(74.48)	(61.08)		(13.30)
Return on Total	3.821	1.522	(0.180)	0.744
Assets (%)	(14.44)	(11.76)		(1.113)
Profit Margin (%)	3.447	-0.876	(0.143)	0.906
	(21.71)	(20.89)		(0.814)

The data source for this table is the Amadeus Bureau van Dijk database, and it excludes the Attica region. The sample is restricted to areas where firms are similar. Columns 1 and 2 show the mean values and standard deviations for firms located in the zip code areas where a bank-unit closed (exposed areas) and firms located in the zip code areas where a unit remained open (control areas). Column 3 reports the p-value for the difference between Columns 1 and 2. p-values are obtained from a regression of the main variable on an indicator of whether the firm is located at the zip code area where a bank-unit closed and region fixed effects. Column 4 presents the estimate coefficients from difference-in-difference regressions on the firms' variables. All variables and estimations are based on 2012–2015 values.

Table 2.10: Effect on the probability of renegotiating a loan and on loans' performance - Only areas with similar characteristics

	Extensive Margin	*Intensive Margin*		
	(1)	(1)	(2)	(3)
	Renegotiation	*Log of Days Past Due over Remaining Days*	*Nonperforming (Dummy)*	*Log of Total Provision per Total Balance*
Post ∗ *Treat*	-0.200*	0.420***	0.285***	0.727***
	(0.108)	(0.151)	(0.0568)	(0.200)
Observations	6,039	2,225	2,225	2,225
R^2	0.272	0.016	0.047	0.140
Firm F.E.	✓	✓	✓	✓
Bank Unit F.E.	✓	✓	✓	✓
Region ∗ Year F.E.	✓	✓	✓	✓
Cluster Level	Bank Unit			

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1 only for firms located in areas where firms have no statistically different characteristics, as it is shown at table 2.9. In Column 1 of the results on the extensive margin, the dependent variable is a dummy that is equal to one if firm i renegotiated a loan at time t and zero otherwise. For the estimations on the intensive margin, the observations included in the analysis are the loans that were renegotiated at least once during the sample period. Results are estimated for three variables related to loan performance. Each column shows the estimated results for a different dependent variable. Column 1 presents the result for the log of days past due a loan payment over the number of remaining days. Column 2 is the estimated result for a nonperforming dummy variable that is equal to one if the loan is characterized as nonperforming and equal to zero otherwise. Column 3 presents the result on the log of total value of loan-loss provision over total loan balance. The dummy variable *Treat* is equal to one if the firm experienced an interruption in its relationship with the loan officer due to the bank-unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013).

Table 2.11: Correlation of relationship strength with probability to renegotiate

	Whole sample		*Only control*		*Only treated*	
	(1)	(2)	(1)	(2)	(1)	(2)
	Pre-period	*Post-period*	*Pre-period*	*Post-period*	*Pre-period*	*Post-period*
Dependence Ratio	0.0440***	0.147***	0.0321**	0.140***	0.162***	0.0621
	(0.0120)	(0.0272)	(0.0123)	(0.0267)	(0.0444)	(0.0811)
R^2	0.422	0.013	0.431	0.014	0.378	0.103
Bank Unit F.E.	✓	✓	✓	✓	✓	✓
Region ∗ Year F.E.	✓	✓	✓	✓	✓	✓
Cluster Level			Bank Unit			

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the estimated coefficients from separate regressions of the renegotiation dummy variable on the *dependence ratio*. The *dependence ratio* is the ratio of the total balance that a firm has at this bank over the total debt that the firm has at all banks. The *dependence ratio* is used as an approximate measure of relationship strength. Bank-unit and region∗year fixed effects are included, and standard errors are clustered at the bank unit level. The first two columns present the coefficients for the whole sample for the pre- and post-period separately. The second two columns present the coefficients only for the control group for the pre- and post-period separately. The last two columns present the coefficients only for the treated group for the pre- and post-period separately.

Figure 2.10: Density distribution of the *dependence ratio*

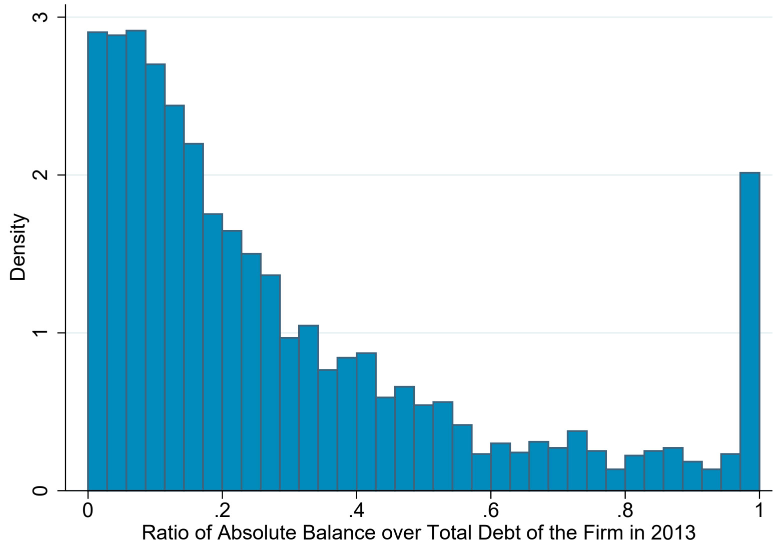


Figure 2.10 plots the density distribution of the *dependence ratio* as measured in 2013.
The *dependence ratio* is defined as the ratio of the total balance that a firm has at this bank over the total bank debt of the firm.

Table 2.12: Effect on renegotiating a loan (extensive margin) - Based on relationship strength

	Closer Relationship	*Less Close Relationship*
	(1)	(2)
	Renegotiation	*Renegotiation*
Post ∗ *Treat*	-0.280**	-0.289**
	(0.105)	(0.132)
Observations	2,106	6,991
R^2	0.296	0.259
Firm F.E.	✓	✓
Bank Unit F.E.	✓	✓
Region ∗ Year F.E.	✓	✓
Cluster Level	Bank Unit	

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1 separately for firms with a closer relationship with the bank and for firms with a less close relationship. Firms with a *closer relationship* are defined those with dependence ratio above 50% in 2013. As firms with a *less close relationship*, are defined those with dependence ratio below 20% in 2013. The dependent variable is a dummy that is equal to one if firm *i* renegotiated a loan at time *t* and zero otherwise. The dummy variable *Treat* is equal to one if the firm experienced an interruption in its relationship with the loan officer due to the bank-unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013). The bottom of the table depicts information on the fixed effects and the control variables included. Standard errors are corrected for clustering at the bank-unit level.

Table 2.13: Effect on renegotiated loans' performance - Based on relationship strength

	Closer Relationship			*Less Close Relationship*		
	(1)	(2)	(3)	(1)	(2)	(3)
	Log of DPD over Remaining	*Nonperforming (Dummy)*	*Log of Provision per Balance*	*Log of DPD over Remaining*	*Nonperforming (Dummy)*	*Log of Provision per Balance*
Post∗Treat	-0.0896	-1.015***	-0.0235	1.641***	0.353***	-0.146
	(0.346)	(0.0627)	(0.237)	(0.320)	(0.0530)	(0.133)
Observations	985	985	985	3,107	3,107	3,106
R^2	0.032	0.067	0.072	0.013	0.043	0.026
Firm F.E.	✓	✓	✓	✓	✓	✓
Bank Unit F.E.	✓	✓	✓	✓	✓	✓
Region∗Year F.E.	✓	✓	✓	✓	✓	✓
Cluster Level	Bank Unit					

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1 separately for firms with a closer relationship with the bank and for firms with a less close relationship. Firms with a *closer relationship* are defined those with dependence ratio above 50% in 2013. Firms with a *less close relationship* are defined those with dependence ratio below 20% in 2013. For these estimations, the observations included in the analysis are the loans that were renegotiated at least once during the sample period. Results are estimated for three variables related to loan performance. Column 1 presents the result for the log of a loan payment's days past due over the number of remaining days. Column 2 is the estimated result for a nonperforming dummy variable that is equal to one if the loan is characterized as nonperforming and equal to zero otherwise. Column 3 presents the result on the log of total value of loan-loss provision over total loan balance. The dummy variable *Treat* is equal to one if the firm experienced an interruption on its relationship with the loan officer due to the bank unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013). In the baseline regressions, firm, bank-unit, and region∗year fixed effects are included. Standard errors are corrected for clustering at the bank-unit level.

Table 2.14: Summary statistics for interactions in 2013

	mean	sd	min	p5	p25	p50	p75	p95	max
Number of loans	3.624004	3.505157	1	1	1	2	5	11	31
Number of interactions	3.630204	3.524094	0	1	1	2	5	11	29
Number of new loans	1.821081	2.249178	0	0	0	1	3	6	14
Number of renegotiations	1.809123	2.236438	0	0	1	1	2	6	23

This table displays the summary statistics of the number of loans, interactions, new loans, and renegotiations that a firm has per year. An interaction is defined as either the issuance of a new loan or a renegotiation of a loan.

Figure 2.11: Density of number of interactions

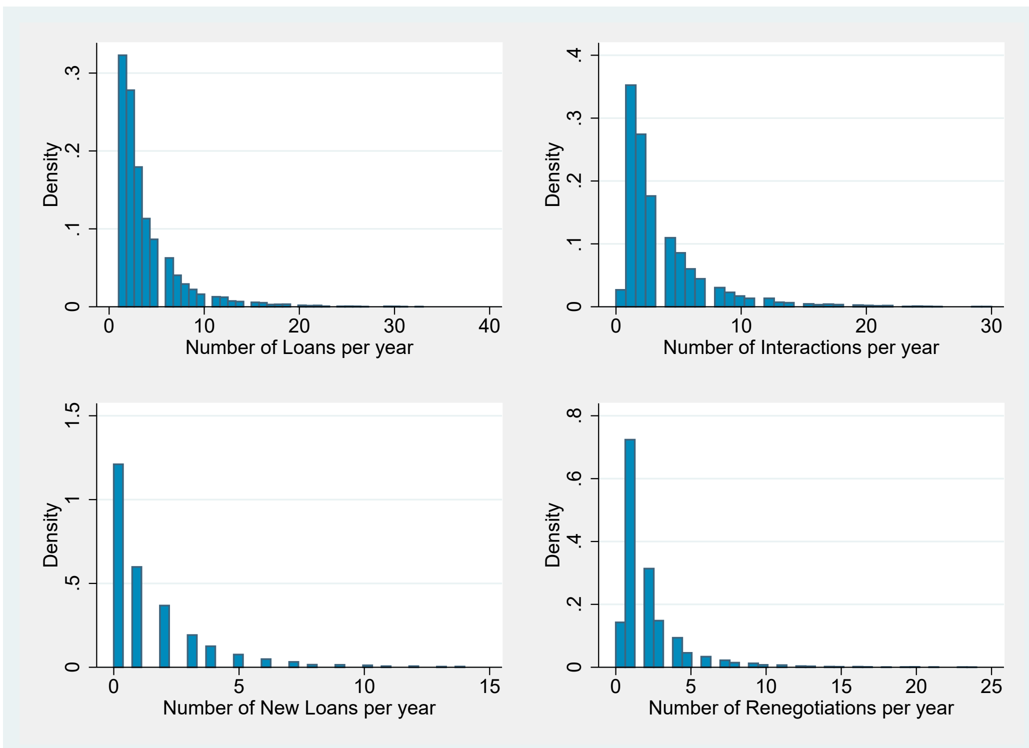


Figure 2.11 plots the density distributions of the number of loans, interactions, new loans, and renegotiations that a firm has per year. An interaction is defined as either the issuance of a new loan or a renegotiation of a loan.

Table 2.15: Effect on renegotiating a loan (extensive margin) - By number of interactions in 2013

	More than two interactions	*Two or less interactions*
	(1)	(2)
	Renegotiation	*Renegotiation*
Post ∗ Treat	0.270***	-0.373***
	(0.0946)	(0.0517)
Observations	5,141	7,626
R^2	0.384	0.164
Firm F.E.	✓	✓
Bank Unit F.E.	✓	✓
Region ∗ Year F.E.	✓	✓
Cluster Level	Bank Unit	

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1 separately for firms that had more than two interactions with the bank in 2013 and for firms that had two or fewer interactions in 2013. An interaction is defined as either the issuance of a new loan or a renegotiation of an existing loan. The dependent variable is a dummy variable that is equal to one if firm *i* renegotiated a loan at time *t* and zero otherwise. The dummy variable *Treat* is equal to one if the firm experienced an interruption in its relationship with the loan officer due to the bank unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013). The bottom of the table depicts information on the fixed effects and the control variables included. Standard errors are corrected for clustering at the bank-unit level.

Table 2.16: Effect on renegotiated loans' performance - By number of interactions in 2013

	More than two interactions			*Two or less interactions*		
	(1) *Log of DPD over Remaining*	(2) *Nonperforming (Dummy)*	(3) *Log of Provision per Balance*	(1) *Log of DPD over Remaining*	(2) *Nonperforming (Dummy)*	(3) *Log of Provision per Balance*
Post ∗ Treat	0.540	0.0238	-0.460***	2.308***	0.257***	0.740***
	(0.543)	(0.0392)	(0.109)	(0.648)	(0.0723)	(0.185)
Observations	2,845	2,845	2,844	2,302	2,302	2,302
R^2	0.014	0.043	0.040	0.030	0.051	0.049
Firm F.E.	✓	✓	✓	✓	✓	✓
Bank Unit F.E.	✓	✓	✓	✓	✓	✓
Region∗Year F.E.	✓	✓	✓	✓	✓	✓
Cluster Level	Bank Unit					

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1 separately for firms that had more than two interactions with the bank in 2013 and for firms that had two or fewer interactions in 2013. An interaction is defined as either the issuance of a new loan or a renegotiation of a loan. For these estimations, the observations included in the analysis are the loans that were renegotiated at least once during the sample period. Results are estimated for three variables related to loan performance. Column 1 presents the result for the log of days past due a loan payment over the number of remaining days. Column 2 is the estimated result for a nonperforming dummy variable that is equal to one if the loan is characterized as nonperforming and equal to zero otherwise. Column 3 presents the result on the log of total value of loan-loss provision over total loan balance. The dummy variable *Treat* is equal to one if the firm experienced an interruption in its relationship with the loan officer due to the bank-unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013). In the baseline regressions, firm, bank-unit, and region∗year fixed effects are included. Standard errors are corrected for clustering at the bank-unit level.

Figure 2.12: Density distribution of the firm's relative size

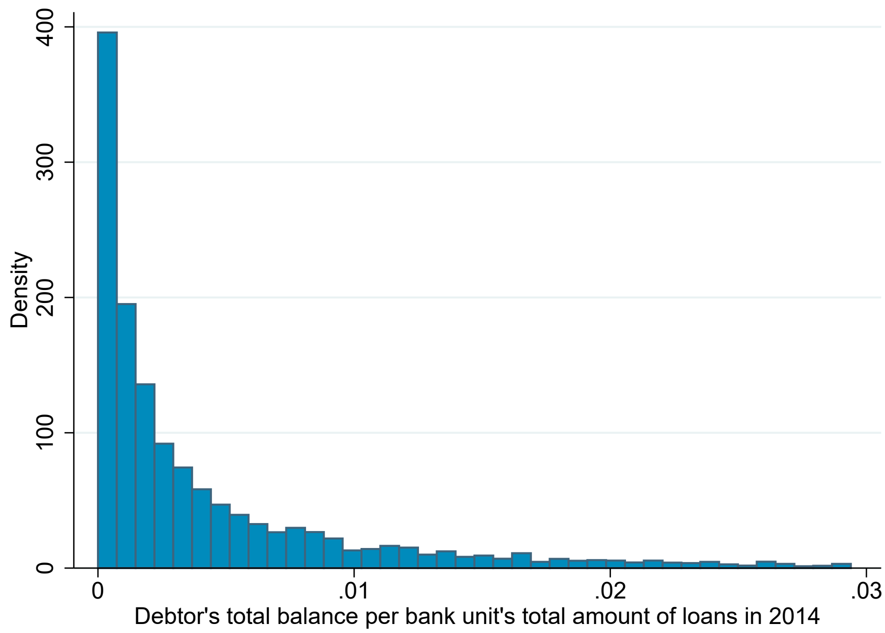


Figure 2.12 plots the density distribution of the firm's relative size as a corporate client within bank unit. The measure of firm's relative size is the ratio of the firm's total balance at the bank unit over the total amount of loans that each bank unit manages in 2014.

Table 2.17: Effect on renegotiating a loan (extensive margin) - By firm's relative size as bank's corporate client within unit in 2014

	Smaller balance	*Larger balance*
	(1)	(2)
	Renegotiation	*Renegotiation*
$Post * Treat$	-0.385***	-0.102
	(0.0540)	(0.116)
Observations	10,296	2,937
R^2	0.264	0.389
Firm F.E.	✓	✓
Bank Unit F.E.	✓	✓
Region * Year F.E.	✓	✓
Cluster Level	Bank Unit	

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1 separately for firms that had a smaller balance at the bank unit in 2014 and for firms that had a larger balance. Larger firms are defined as those whose balance at the specific bank unit accounts for at least 1% of the total amount of corporate loans that the unit manages. The rest of the sample is defined as relatively smaller firms. The dependent variable is a dummy variable that is equal to one if firm *i* renegotiated a loan at time *t* and zero otherwise. The dummy variable *Treat* is equal to one if the firm experienced an interruption in its relationship with the loan officer due to the bank-unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013). The bottom of the table depicts information on the fixed effects and the control variables included. Standard errors are corrected for clustering at the bank-unit level.

Table 2.18: Effect on renegotiated loans' performance - By firm's relative size as bank's corporate client within unit in 2014

	Smaller balance			*Larger balance*		
	(1) *Log of DPD over Remaining*	(2) *Nonperforming (Dummy)*	(3) *Log of Provision per Balance*	(1) *Log of DPD over Remaining*	(2) *Nonperforming (Dummy)*	(3) *Log of Provision per Balance*
Post∗Treat	1.876***	0.502***	0.833***	1.825***	-0.877***	0.538
	(0.204)	(0.0204)	(0.141)	(0.378)	(0.165)	(0.493)
Observations	3,366	3,366	3365	2,002	2,002	2,002
R^2	0.026	0.134	0.113	0.007	0.129	0.154
Firm F.E.	✓	✓	✓	✓	✓	✓
Bank Unit F.E.	✓	✓	✓	✓	✓	✓
Region∗Year F.E.	✓	✓	✓	✓	✓	✓
Cluster Level	Bank Unit					

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1 separately for firms that had a smaller balance at the bank unit in 2014 and for firms that had a larger balance. Larger firms are defined as those whose balance at the specific bank unit accounts for at least 1% of the total amount of corporate loans that the unit manages. The rest of the sample is defined as relatively smaller firms. For these estimations, the observations included in the analysis are the loans that were renegotiated at least once during the sample period. Results are estimated for three variables related to loan performance. Column 1 presents the result for the log of days past due a loan payment over the number of remaining days. Column 2 is the estimated result for a nonperforming dummy variable that is equal to one if the loan is characterized as nonperforming and zero otherwise. Column 3 presents the result on the log of total value of loan-loss provision over total loan balance. The dummy variable *Treat* is equal to one if the firm experienced an interruption in its relationship with the loan officer due to the bank-unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013). In the baseline regressions, firm, bank-unit, and region∗year fixed effects are included. Standard errors are corrected for clustering at the bank-unit level.

Table 2.19: Effect on renegotiating a loan (extensive margin) - By firm's pre-period performance

	Delayed loan payment	*On time loan payment*
	(1)	(2)
	Renegotiation	*Renegotiation*
Post ∗ *Treat*	-0.301***	-0.189*
	(0.0883)	(0.107)
Observations	3,764	9,469
R^2	0.420	0.245
Firm F.E.	✓	✓
Bank Unit F.E.	✓	✓
Region ∗ Year F.E.	✓	✓
Cluster Level	Bank Unit	

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1 separately for firms that delayed a loan payment in 2013 and for firms that paid their loans on time in 2013. The dependent variable is a dummy variable that is equal to one if firm *i* renegotiated a loan at time *t* and zero otherwise. The dummy variable *Treat* is equal to one if the firm experienced an interruption in its relationship with the loan officer due to the bank-unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013). The bottom of the table depicts information on the fixed effects and the control variables included. Standard errors are corrected for clustering at the bank-unit level.

Table 2.20: Effect on renegotiated loans' performance - By firm's pre-period performance

	Delayed loan payment			*On time loan payment*		
	(1)	(2)	(3)	(1)	(2)	(3)
	Log of DPD over Remaining	*Nonperforming (Dummy)*	*Log of Provision per Balance*	*Log of DPD over Remaining*	*Nonperforming (Dummy)*	*Log of Provision per Balance*
Post∗Treat	1.263***	-0.129**	-0.411***	1.706***	0.202***	0.637***
	(0.370)	(0.0503)	(0.126)	(0.256)	(0.0514)	(0.134)
Observations	1,866	1,866	1,866	3,502	3,502	3,501
R^2	0.027	0.078	0.040	0.051	0.050	0.040
Firm F.E.	✓	✓	✓	✓	✓	✓
Bank Unit F.E.	✓	✓	✓	✓	✓	✓
Region∗Year F.E.	✓	✓	✓	✓	✓	✓
Cluster Level	Bank Unit					

Standard errors in parentheses

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

This table displays the results from estimating equation 1.1 separately for firms that delayed a loan payment in 2013 and for firms that paid their loans on time in 2013. For these estimations, the observations included in the analysis are the loans that were renegotiated at least once during the sample period. Results are estimated for three variables related to loan performance. Column 1 presents the result for the log of a loan payment's days past due over the number of remaining days. Column 2 is the estimated result for a nonperforming dummy variable that is equal to one if the loan is characterized as nonperforming and equal to zero otherwise. Column 3 presents the result on the log of total value of loan-loss provision over total loan balance. The dummy variable *Treat* is equal to one if the firm experienced an interruption in its relationship with the loan officer due to the bank-unit closure, and zero otherwise. The dummy variable *Post* is equal to one if the year of the observation is after the reorganization (either 2014 or 2015) and zero if it is before the reorganization (either 2012 or 2013). In the baseline regressions, firm, bank-unit, and region∗year fixed effects are included. Standard errors are corrected for clustering at the bank-unit level.

Chapter 3

Securing the Unsecured:

Do stronger creditor rights affect firms' access to credit?

3.1 Introduction

In the recent years, policymakers around the world have debated about the importance of institutions on economic growth and on fostering entrepreneurship. Several academic papers have also pointed out the significance of institutions, such as efficient and transparent justice sector, legal investor protection and credit bureaus, in supporting the business environment and credit supply both from a theoretical and from an empirical point of view. Porta et al. 1998, with their seminal paper "Law and Finance" and the subsequent literature examined the link between creditor rights and financial development. Weak protection of creditors' rights has been seen by many economists as a significant source of financial frictions and several papers have focused on bankruptcy law and its effects on financial markets with controversial findings. In this context, the broad research question that this paper contributes is what is the impact of a financial reform on the businesses' capital structure.

In an attempt to answer this broad research question, I employ the passage of a reform on "Execution of Cash Assets" in Croatia in 2011 as a quasi-natural experiment to examine how a more efficient enforcement mechanism determines a firm's leverage level, the access to credit, the total volume of loans per firm, and the investment level. Starting from the mid-2000s the Croatian authorities embarked on a major reform effort in response to EU concerns about a weak justice system and enforcement mechanism. Among other adopted reforms, Croatian policymakers changed the way that judicial decisions on cash assets were enforced. After the enforcement reform in 2011, the Financial Agency (FINA) under the Ministry of Finance is responsible for enforcing the collection of monetary judgments once the creditor obtains a writ of execution through courts. An improved system of cash collection was expected to improve the business environment and increase credit supply. FINA, in collaboration with the Croatian National Bank and other banks, uses a database of banks and other financial institutions (i.e. credit unions) that covers deposits and savings accounts linked with the personal identification number of the account holders. It has also the power to electronically enforce payment orders against debtors' bank accounts and in favor of creditors once the writ of execution is forwarded to it. Accounts are seized if holdings are insufficient to pay off the writ of execution. However, certain portions of the accounts may be protected, such as portions of salaries that are considered essential for basic living expenses. The importance of this reform is that it affected only unsecured creditors as it didn't apply to any collateralized obligation.

Croatia is an appropriate setting to test these research questions since the variation of the courts' efficiency is very wide. I employ a unique dataset on courts' efficiency in 2010, a year prior the reform, on the specific type of cases that were affected mostly by the reform. Based on courts efficiency, I define as treated the firms that are located in towns with more efficient courts as in these courts a possible case will be heard within a year and the change on the enforcement mechanism will have a greater impact on them. As control group, I define the firms that are located in towns with less efficient courts as in these courts it will take two to three years for a case to be heard and therefore the enforcement reform won't have a significant impact. Based on this characterization, I apply a simple Difference-in-Differences method to estimate the effect of the reform on leverage. Since both groups are treated by the reform, the estimated results could be seen as a downward biased estimation of the true results.

The baseline results show that firms operating in towns with more efficient courts have an increase in their leverage ratios after the reform compared to firms operating in towns with less efficient courts. Moreover, I estimate the impact of the reform on access to credit. In order to do that I construct a dummy variable, *Non Zero Leverage*, that is equal to one when a firm has positive leverage and zero otherwise. This variable can be seen as a measure of the extensive margin of the reform and captures the impact of the reform on firms that prior to the reform had zero leverage. I find that firms that operate in towns with efficient courts have higher probability to have positive leverage after the reform compared to firms that operate in towns with inefficient courts. Another variable that is examined is the log of total debt and I find similar results. Lastly, in an attempt to answer the question of where do the firms spend the extra funds they raise, I test the impact of the reform on other firm outcome variables and I find that the reform has a significant impact on the investment levels. In particular, firms in towns with efficient courts seem to have higher capital expenditures after the reform compared to firms in towns with inefficient courts. Overall, the main conclusion is that firms affected more by the reform, borrow more, have easier access to credit, and invest more.

The reform is expected to benefit more firms with low intensity on tangible assets. To test this, I create an industries index based on the ratio of tangible fixed assets over total assets and I define as treated the firms that belong in industries in the lower end of the rank and as control the firms that belong in industries in the higher end. Then, a Difference-in-Difference-in-Differences analysis is applied and similar patterns are found. Moreover, I assess the potential impact of any regional macroeconomic conditions using two different approaches; the first is to add lagged unemployment variables as control variables and the second is to match the regions based on their unemployment level. The results remain robust on these specifications. Lastly, to identify the size of the firms that are affected more by the reform, the analysis is repeated for three different categories of sizes. Based on this, it appears that the medium sized firms are predominantly driving the estimated results.

3.2 Related Literature & Project's Contribution

This paper contributes to the legal and financial development literature that was pioneered by Porta et al. 1998. Since then several papers have examined the relationship between creditor rights protection and credit market development [1]. Haselmann, Pistor, and Vig 2009 find that after a legal change, banks increase the supply of credit and that foreign-owned banks respond more strongly than domestic. The effect of legal institutions on financial contracts has also been examined in several papers using a cross-country setting. Demirgüç-Kunt and Maksimovic 1998 show that in countries with more efficient legal systems more firms use long-term external financing while they have lower profit

[1] Levine 1998, Levine 1999, Djankov et al. 2003, Djankov et al. 2008, Demirgüç-Kunt and Maksimovic 1998

rates. Giannetti 2003 and Qian and Strahan 2007 find that stronger creditor rights promote leverage and in particular long-term debt. Davydenko and Franks 2008 find that changes in bankruptcy codes and laws affect contracts by analyzing recovery rates in the U.K., France, and Germany. Most of these papers focus on cross-country comparisons and use country level aggregate data. Even though the results and the contribution of these papers is significant, the results may be biased as they could be correlated with other unobserved country variables that also affect financial development.

This paper is more closely related to recent research work that has used within-country firm specific data to analyze the effect of a change in the enforcement quality and in the contracting environment. Visaria 2009 exploits the introduction of specialized debt recovery tribunals in India that speed up the resolution of debt recovery. Using loan level data and a cross-state variation, as the program was introduced in different states at different times, he finds that the introduction of debt recovery tribunals increases the probability of loan repayment and lowers the cost of bank credit for firms. Overall, Visaria argues that the speedier the processing of debt recovery, the lower the interest rates charged per unit of loan.

Lilienfeld-Toal, Mookherjee, and Visaria 2012 argue that when credit supply is inelastic, the existence of specialized courts has an adverse distributive impact by increasing access to credit for big firms and reducing access to credit for small firms. They documented this result both theoretically and empirically using Indian firm level data and the same judicial reform that was studied before by Visaria 2009. On the empirical side, they show that post the reform new long-term borrowing and fixed assets expanded for large firms but for small firms it decreased causing a redistribution effect on credit. Moreover, in a paper by Gopalan, Mukherjee, and Singh 2016 that used the same setting with Lilienfeld-Toal, Mookherjee, and Visaria 2012 and Visaria 2009 it is shown that when the debt contract enforcement costs are lower, long-term debt increases while short-term debt and trade credit increases. The firms that are affected the most are those that borrowed from multiple lenders, had abnormally short-term debt maturity structures, and are smaller in size.

Vig 2013 examines the effect of law on debt structure. More specifically, he uses a securitization reform in India that strengthened the rights of secured creditors by allowing them to bypass the judicial process to liquidate the assets of the debtors. In particular, by examining a cross-sectional variation based on firms' asset tangibility and period when the reform was implemented, he found that the reform caused a decrease in secured debt, total debt, debt maturity, and asset growth and an increase in liquidity in firms. The observed changes are larger for firms with a higher proportion of tangible assets, as these firms are more affected by the secured transactions law. The fact that firms reduced secured debt relative to other forms of financing suggests that firms may have been worse off. Both Lilienfeld-Toal, Mookherjee, and Visaria 2012 and Vig 2013 are part of a literature that argues that strengthening creditors rights makes it more difficult for smaller firms to access credit.

Ponticelli and Alencar 2016 use micro level data to show the importance of court enforcement on the financial reform effectiveness. They examine the effect of a bankruptcy reform in Brazil that increased secured creditors chances to recover their claims when a firm is liquidated. Using an instrument to measure potential extra-jurisdiction and variation in court congestion across Brazilian districts, they find that the access to credit, investment and productivity are increased in districts with lower court congestion.

A small but rising literature has analyzed the effect of collateral reforms. Haselmann, Pistor, and Vig 2009 find that lending increases after bankruptcy and collateral reforms are implemented in Eastern European countries. They also identify the importance of collateral and differentiate an effect of a collateral law from a bankruptcy law arguing that collateral is a very important variable especially for emerging economies.

Campello and Larrain 2015 contribute both to the cross-country and the within-country literature by examining a recent wave of reforms related to collateral across Eastern European countries. They examine in detail a legal reform in Romania that allowed movable assets to be pledged as collateral. They document that the reform improved more the business environment for firms with more intensive use of movable assets and increased access to credit. Calomiris et al. 2017 analyze the problem from the creditors' side using loan-level data with specific information on the assets that were pledged as collateral. They find that in countries with weak collateral laws the loan-to-values of loans collateralized with movable assets are lower and that production shifts towards the immovable-intensive sectors creating distortions in the allocation of resources. By looking into a collateral law reform in Slovakia, they confirm their main finding.

The main contribution of this paper is to examine the impact of a reform that affected mostly the unsecured creditors. This is an important contribution to the literature as the majority of small and medium enterprises around the world have limited access to tangible fixed assets that could be pledged as credible collateral. For that reason, it is worth examining the impact of a reform that made easier the collection of cash assets and by this way increased the probability to repay an unsecured obligation. In addition, this paper provides evidence from Croatia, a country that has not been examined in this literature before, and uses proprietary data to estimate courts efficiency.

3.3 Institutional Setting

In June 2004, the European Council granted the status of candidate country to Croatia and negotiations for accession were opened in October 2005. On July 1, 2013 Croatia became the 28th member of the European Union (EU) having undertaken significant reforms during its long EU accession process. In order to achieve this significant national objective was required for Croatia to fulfill 'closing benchmarks' agreed with the EU across all sectors of the state and the economy with justice reforms to be perhaps the most sensitive and contentious of others that were implemented. In June 2007, the European Commission issued a screening report covering Chapter 23 ("Judiciary and Fundamental Rights") which provided a snapshot of Croatia's legislation, judicial practices and institutional arrangements, and the reforms necessary to fulfill the closing benchmarks. As a result of the reforms, many new laws and institutions are now in place in Croatia, yielding a stronger justice system.

One set of these reforms aimed to improve the enforcement of judicial decisions. Even though it was not a requirement for the EU accession, the Ministry of Finance took advantage of the reforms momentum and passed an *Act on Execution of Cash Assets* which entered into force on January 1, 2011. Under this new Act the enforcement of judicial decisions over cash assets was accelerated as it was centralized under one institution, the Financial Agency (FINA). An improved system of cash collection was expected to improve the business environment and increase credit supply.

Before the passage of the Act the enforcement procedure was as follows: When a debtor defaulted on a liability, the creditor had to initiate a judicial procedure at the debtor's competent court. Once the trial was completed and a valid writ of execution was issued from the courts, the creditor had to request from a financial institution (bank) to transfer the specified cash amount from the debtor's specified account number to the creditors by providing the required documentation including the enforcement writ from the court. The main issue with the previous enforcement procedure was that the writ obtained from the courts referred to a unique account number and the debtor had the incentive, if he wanted to strategically default on an obligation, to open a new account, transfer the cash, and by this way avoid paying back the creditor. The banks will not agree to provide any information about the existence of any other account linked to same person or company as these are considered confidential information. At the end, the creditor will be left with no option to receive back the payment.

After the passage of the Act on Execution of Cash Assets, the creditor will still need to initiate a judicial procedure to get a valid writ of execution form the debtor's competent court but the main innovation of the reform is observed on the way the decision is enforced. First, the writ of execution has been generalized and applies to any account that the person or the company holds in any financial institution (bank or credit union) that operates in Croatia. The novel difference is that once the creditor has received a valid writ from the courts, he will have to request from the Financial Agency (FINA) to transfer the specified amount from any account that the debtor holds to the creditors account. The Act introduced also a single methodology to estimate the interest rate for the amount owed.

The significant part is that the creditor gets one single order of payment that applies to all the accounts that the debtor has. FINA, in collaboration with the Croatian National Bank and other banks, holds a unified electronic database of deposits and savings accounts linked with the personal identification number of the account holders, both individuals and corporations. The database includes the accounts on all commercial banks and credit unions operating in Croatia. FINA is also enabled to electronically enforce payment orders against debtors' bank accounts and in favor of creditors once the writ of execution is forwarded to it. The payment to the creditor is subsequently processed from any of the debtor's accounts that has enough cash to cover the debt otherwise the debtor's accounts will be seized until the overdue liabilities have been repaid. After the repayment has been fulfilled, FINA informs the banks to unblock the debtor's accounts.

However, certain portions of accounts may be protected and some transfers are allowed to be made while the accounts are seized. These exemptions, where the execution is not carried out, are payments to employees and former employees, operating costs (electricity, water, etc.), purchase of goods for completed regular operations, tax payments. The prohibited actions include rerouting cash and financial flows to another legal or natural person, transactions which would be voidable in bankruptcy proceedings, transactions in whichever form would result in a reduction of the debtor's assets. Payments of any advances on profit or dividends, payments of profits to the management or supervisory board or to employees are also prohibited. Lastly, a debtor who has not settled its due liabilities is not allowed to lend.

Overall, the implementation of the Act on Execution of Cash Assets had a significant contribution as it altered the way that judicial decisions were enforced, improved the system of debt collection and strengthened creditors' rights. It is worth mentioning that when the new enforcement mechanism was adopted approximately 66% of outstanding debts were over 360 days old and most likely not collectible, 630,021 writs were transferred to FINA at the initiation of the new enforcement mechanism, while at the end of the first year of implementation, on December 2011, 971,424 enforcement writs were processed. The main impact of the reform is to promote trust between creditors and debtors, increase trust in the judiciary, and harmonize the Croatian legislation with EU standards. By having one institution responsible for transferring the cash amount owed and one unified database, the reform aims to the faster collection of receivables and repayment of credit.

3.4 Data and Empirical Methodology

In the following sections, I use a Difference-in-Differences methodology on a firm-level dataset to examine the effect of the enforcement reform on the firms leverage. Fist, I describe the data used in the analysis. Next, I explain the institutional features used in the identification strategy. Then, I present the results. Lastly, some robustness tests are included.

Data

The firm-level dataset used in this paper comes from Amadeus, a dateset compiled by Bureau van Dijk. In this dataset the financial statements (balance sheet and income statement) of millions of firms operating in European countries are included. Amadeus data are collected from local information providers, which in most cases are local registries. Croatia has a very comprehensive and detailed dataset in Amadeus as all private companies registered under the Commercial Court, which is necessary for an official settlement, are required to submit their annual financial information to the Register of Business Entities and to Chamber of Commerce. The Register of Business Entities holds also a publicly available database with firm level information. In this paper the dataset used includes 368,430 observations, from 63,844 firms, covering the period from 2008 until 2013.

The data regarding court efficiency measures come from the Croatian Ministry of Justice. The unique feature of this dataset is that it includes a detailed decomposition based on the different types of cases as well as between incoming, pending and resolved. Moreover, the dataset is from 2010, the year prior to the enforcement reform. This fact is very important for the identification strategy as if the efficiency data would be from a more recent year, a selection bias problem may arose. For identification purposes in this paper I look into enforcement cases, as this is the type of cases that the enforcement reform is applied to, and I estimate as a measure of efficiency the ratio of resolved over incoming plus previous year backlog. Lastly, the unemployment data at regional level that are used as controls in the analysis are taken from the Croatian Bureau of Statistics.

In Croatia there are 65 municipal courts that hear enforcement cases located in different cities across 21 regions. In the analysis, Zagreb, the capital of Croatia, is excluded in order to provide more accurate results and eliminate any possible bias that the inclusion of the dominant city could cause. Table 3.1 shows the number of firms and the number of observations operating in each city. By using the ratio of resolved over incoming plus previous year backlog as measure of efficiency, I rank the cities where the municipal courts are located as “efficient” court towns and “inefficient” court towns. An interesting feature of this dataset is that the variation of the courts efficiency is relatively large. In particular, the “most inefficient” city has clearance ratio equal to 16% and the “most efficient” city has clearance ratio equal to 84%. The average of the court efficiency measure is equal to 0.62 and the standard deviation is 0.14. To proceed with the analysis I define as “efficient” court cities those with the top 40% clearance ratio (the measure varies from 84% to 69%) and as “inefficient” court cities those with the lowest 40% clearance ratio (the measure varies from 60% to 16%). In total 54 cities are included in the analysis.

The dataset contains a wide variety of industries that are similarly distributed in the “efficient” court towns and in the “inefficient” ones. In particular, 24 industries are included in the sample and Table 3.2 presents the number of firms per industry included in the two categories as well as the total number of observations per industry.

The basic outcome variables are *Leverage*, *Non Zero Leverage*, and *Log of Total Debt*. *Leverage* is defined as the ratio between total debt and the book value of total assets. *Non Zero Leverage* is a dummy that is equal to 1 when leverage is postive (not zero leverage) and equal to zero if the firm has no leverage. *Log of Total Debt* is the natural logarithm of Short term plus Long term debt plus one. Another outcome variable that is included in the analysis is the *Log CAPEX*, where the Capital Expenditures (CAPEX) is estimated as the difference in Fixed Assets plus depreciation. Moreover, as *Profitability* is defined the ratio of Earning Before Interest and Taxes(EBIT) to Total Assets. Table 3.3 presents the summary statistics of *Leverage*, *Non Zero Leverage*, *Total Debt*, *Log of Total Debt*, *Capital Expenditures*, *Log of Capital Expenditures*, *Total Assets*, *Log of Total Assets*, *EBIT*, *Profitability*, and *Sales* for separately the “efficient” court towns and the “inefficient” court towns. The average leverage overall in Croatia is 19.5%, while in the “efficient” court towns is 19.9% and in the “inefficient” court towns is 19.1%. The fraction of *Non Zero*

Leverage is also higher by 1.4% in the "efficient" court towns and it is equal to 50.2%, while in the "inefficient" court towns is equal to 48.8%. On the contrary, the *Log of Total Assets*, the *EBIT* and the *Log of Sales* are higher in the "inefficient" court towns. For example, the average EBIT of the firms in the "efficient" court towns is 2.3% lower than the average EBIT in the "inefficient" court towns.

Identification Strategy

In an attempt to identify the effect of an enforcement reform on leverage, I take advantage of an important characteristic of the institutional setting to define the treatment and control group and subsequently apply the Difference-in-Differences method.

The enforcement reform provided more security on the collection of cash assets after the debtor had defaulted on a liability. The creditor still has to go through the court system in order to receive the writ of execution and then request a money transfer from the debtor's bank accounts. Since the enforcement reform did not affect the way that courts operate, I am going to use the measure of courts efficiency as an indicator to define the treatment and control group. In particular, the treatment group consists of the firms that operate in "efficient" court towns while the control group consists of the firms that operate in "inefficient" court towns. The fact that supports the identification strategy here is that judicial proceedings in the "inefficient" court towns take a long period of time, which may be up to 3 years, while in the "efficient" court towns a case will be heard in less than a year from the submission date. Because in the "inefficient" court towns the proceedings are long and the creditors are aware of this, the enforcement reform has a much lower impact. The creditors may have a stronger belief that they will be able to collect the assets but they know that there will be a long waiting time until they get the court decision on hand. On the contrary, the creditors in the more “efficient” court towns are more affected by the enforcement reform as they know that the justice system works relatively well, the procedures are not that lengthy and so they have stronger trust that they are going to receive the cash amount owned back. It is significant here to mention that both groups are affected by the reform as the enforcement reform covered the whole country, but the treatment group is supposed to be affected more than the control. For that reason, the estimated results could be seen as a downward biased estimation of the true impact.

Another important feature of the institutional setting is that firms cannot choose neither the jurisdiction that they operate nor the creditor can choose the jurisdiction where the case is submitted. The jurisdiction is defined by the location that the debtor’s firm is registered. A potential identification flow is that it could be argued that firms choose ex-ante their location based on courts efficiency and that could be a selection bias problem that would drive the estimated results. Even though I don’t have a formal evidence to support the contrary, I believe that the main factors that affect the location decision when they establish a new firm are the market environment, the social network, and possibly other weather and location conditions. Since most of the firms in the sample are small firms that operate only at a local level and they don’t have an incentive to move their headquarters in a different city, I think that the above argument is valid but I acknowledge that further evidence should be provided.

After the assignment of treatment and control group based on the firm’s location, I estimate the effect of the enforcement reform on the firms leverage by comparing the difference between the treatment and control group prior the reform (2008-2010) and after the reform (2011-2013).

The baseline Difference-in-Differences model that is used to estimate the effect of stricter enforcement is the following:

$$y_{ijt} = \alpha_i + \alpha_t + \delta(Post_t * Treat_j) + \varepsilon_{ijt} \tag{3.1}$$

where y_{ijt} stands for the outcome variable of interest (e.g.*Leverage*, *NonZeroLeverage*, *LogTotalDebt*) for firm i that is located in town j in year t. $Treat_j$ is a dummy variable that is equal to one if the firm is located in a city with an "efficient" court (treatment group) and is equal to zero if the firm is located in a city with an "inefficient" court (control group). $Post_t$ is a dummy variable that is equal to zero before the reform is implemented (from 2008 to 2010) and equal to one after the reform is implemented (from 2011 to 2013). The baseline specification includes firm fixed effects (α_i) to capture any time-invariant firm characteristics as well as year fixed effects (α_t) to capture any aggregate time varying shocks. The term ε_{ijt} is the error term. The coefficient of interest is δ and it measures the pre-post difference in the outcome variable for the firms operating in "efficient" court towns relative to the pre-post difference for the firms operating in "inefficient" court towns.

For the Difference-in-Difference methodology to estimate correctly the effect of the enforcement reform on firms leverage, it is necessary to check whether the parallel trends assumption holds, in other words whether possible outcome variables followed parallel trends for both groups prior to the reform. The assumption that follows is that the difference between the trends of the two groups prior the reform would continue to be the same in the future if the reform was not implemented. In that case any possible change in the difference of the two trends will be caused by the reform. Figure 3.1 show the trends of the two groups for the Leverage Ratio, the Non-Zero Leverage Dummy, and the Log of Total Debt.

3.5 Results

Results on Leverage Ratio

Based on the identification strategy described in detail above, I compare the level of leverage held by firms located in towns with efficient courts (treatment group) with the level of leverage held by firms located in towns with inefficient courts (control group). Table 3.4 presents the results based on three different identification strategies. The first column reports the results based on the basic regression on the whole sample with firm and year fixed effects and robust standard errors. It seems that the reform increased the leverage level in firms operating in towns with efficient courts by 0.46 percentage points more than in firms operating in towns with inefficient courts. Given that the average leverage level is 19.5 percentage points an increase by 0.46 percentage points is relatively small but could be economically significant for the firm's performance especially when the firm is small.

It is important that the results remain robust to alternative specifications. The second column presents the results when firm and industry-year fixed effects are included. The industry-year fixed effects capture any common variation that could occur at the industry level across years. Even though all industries are similarly distributed in the two groups, the results could be biased if leverage was increasing in an industry that is dominant in towns included in the treatment group. As it is shown in Table 3.4, the impact of the reform remains almost at the same level and statistically significant when industry-year fixed effects are included. The third column presents the results with firm, industry and region-year fixed effects. The variation to identify the treatment and control group is at the town level and for that reason I include in the analysis region-year fixed effects to capture any variation across years that could affect the region level. The results increase in level and remain statistically significant. In particular, the firms that operate in towns with efficient courts increase their leverage level by 0.59 percentage points more than firms that operate in towns with inefficient courts and this effect is not driven by firm characteristics, by changes at the industry level, and by other reasons that may affect the aggregate region level.

The next three columns of table 3.4 present the results for the same three different specifications but for only the “small” firms. As small firms I use the characterization provided by the Amadeus dataset. They define as small the firms that are below the fifth decile based on three variables (turnover, total assets $*$ median value of turnover per employee of the industry, number of employees $*$ median value of turnover per employee of the industry). A smaller sub-samlpe (177,935 observations) consists of small firms and shows that this is the dominant category of firms. It is important to notice whether the reform affected mostly the small firms as this is the category that is assumed to have biggest difficulties to access external funds. As it is shown, the results remain positive and increase both in magnitude and in statistical significance. In particular, it seems that firms that operate in towns with efficient courts have 0.58 percentage points more leverage after the reform than firms operating in towns with less efficient courts when I control for firm characteristic. As in the analysis for the whole sample, the results remain positive and statistically significant when industry-year fixed effects are included. Moreover, the estimated impact of the reform for small firms increases further when firm, industry and region-year fixed effects are included. Under this specification, small firms in the treatment group have 0.69 percentage points more leverage after the reform than small firms in the control group.

Results on Access to Credit

The second variable that the impact of the reform is examined is the access to leverage. One of the most important aspects of the reform is that it strengthens the possibility to collect cash assets and for that reason it makes the repayment of an unsecured loan more possible. Due to this fact firms that didn't have access to loans, as they couldn't pledge fixed assets as collateral, face a higher probability to receive a loan approval after the reform. In order to evaluate the access to leverage, a dummy variable, named *Non Zero Leverage*, is constructed that is equal to one when leverage is positive and zero when leverage is equal to zero. To evaluate the impact of the enforcement on cash assets on access to leverage, the same baseline regression is used with dependent variable the *NonZeroLeverage* dummy variable.

The results are presented in Table 3.5. The first column includes the results for the whole sample with firm and year fixed effects and it shows that firms located in towns with efficient courts have 0.85 percentage points higher probability to have positive leverage after the reform than firms located in towns with inefficient courts. The result is statistically and economically significant as it shows that building trust in the economy that a loan is going to be repaid promotes leverage and gives access to bank financing to more firms.

It is important to notice that the results are robust when firm and industry-year fixed effects are included to capture any common variations across industries. On the other hand, the regression coefficient decreases in statistical significance when both firm, industry and region-year fixed effects are included. This could be driven by the fact that access to loans is promoted more in some regions contrary to others.

As before, the results are consistent when I control only for small firms. It is important to notice that the coefficient of interest increases both in magnitude and in statistical significance when I restrict the analysis only to small firms. It seems that small firms that are located in towns with efficient courts have 1.05 percentage points higher probability to have access to leverage than small firms located in towns with inefficient courts. The results are robust when firm and industry-year fixed effects are included but they loose the statistical significance when region-year fixed effects are included.

Results on Total Debt

Another important variable for the analysis is the level of the total debt that a firm uses. For that reason the analysis is repeated for the natural logarithm of the total debt (*LogTotalDebt*). As total debt is defined the sum of the short term debt and of the long term debt as presented in the firm's balance sheet.

The variable that is used in the analysis is the natural logarithm of the total debt. Table 3.6 shows the estimated results. The results for the log of total debt are in the same direction as before. Firms operating in towns with efficient courts have 0.102 basis points higher loans than firms operating in towns with inefficient courts. Given that the average value of the log of total debt is 6.367 the economic impact of the reform is relatively small. The results are statistically significant when we include firm and year fixed effects as well as when firm and industry year fixed effects are included. The coefficient of interest decreases in significance though when region year fixed effects are included.

When I repeat the analysis for small firms the results under all specifications are consistent and increase both in magnitude and in statistical significance. For small firms that operate in towns with efficient courts the logarithm of total debt increases by 0.127 basis points more after the reform relative to firms that operate in towns with inefficient courts. The results in this case are robust to industry-year fixed effects but they decrease in statistical significance with region-year fixed effects.

Results on Investment

The evidence provided above shows that the reform had a significant effect on promoting leverage and on increasing the amount of loans that firms had access to. Another important aspect that needs to be examined is where did the firms spent the extra funds they had access to through loans. For that reason the same analysis was repeated to identify where the funds were spend. As it was expected the main variable the showed up significant in the analysis was the investment level.

In particular, the analysis was repeated for the capital expenditures logarithmic level. As the level of CAPEX is not provided in the Amadeus dataset, it was estimated indirectly by the sum of the depreciation and the difference of the fixed assets. The main variable that was used in the analysis is the logarithm of the CAPEX. The results are presented in Table 3.7.

It appears that firms that borrowed more, invested more. Firms located in towns with efficient courts invested more and the level of log capital expenditures increased by 0.118 basis points more than firms located in towns with inefficient courts after the reform. The results are statistically significant when firm and year fixed effects are included as well as with firm and industry-year fixed effects. The coefficient of interest decreases in significance though with region-year fixed effects, as in the previous parts of the analysis.

In the columns four to six, the analysis is repeated but only for small firms. The results remain consistent with the previous part, they are positive and statistically significant when we add firm and year fixed effects as well as when we add firm and industry-year fixed effects. As before, the statistical significance weakens when we include region year fixed effects. An important observation that needs to be made is that when we eliminate the sample for small firms the magnitude of the coefficient weakens. This is in contrast with the results observed for the loans level and it raises a question that remains to be answered of where were the extra funds spent.

The intuition and the economic significance of the results on investment are relatively large. It is obvious on the analysis that firms who borrowed more and gained access to bank funds, invested the amount raised on fixed assets. Since investment is an important driver of the firm's performance and of the local economy in general, the fact that more firms had access to bank financing and that loans were spent on investment could have other general equilibrium effects that need to be estimated.

Analysis based on Industries Tangibility Index

The reform on enforcement on cash assets is supposed to benefit mostly the industries that didn't have any assets to pledge as collateral in order to get approval for a loan. These industries are usually characterized by a very low intensity of tangible assets. Firms that belong to these industries have in most cases low or zero tangible assets and for that reason they don't have any credible assets to pledge as collateral for a business loan. As the reform was supposed to increase the probability to collect an unsecured loan, banks are expected to offer a unsecured loan more easily and firms with less tangible assets are expected to benefit the most.

In order to test this hypothesis and identify the effect of the reform on the industries that were expected to benefit the most, I take advantage of the fact that some industries are more intensive than others in their use for tangible assets. To do so empirically, I construct an index of industries based on the ratio of tangible fixed assets over total assets. The data that are used for the index construction come from firms based in Croatia and cover the period 2008-2010 (pre-reform period). Then, I assign the industries at the lower end of the tangibility index as the ones that are affected the most by the reform ("treatment group") while those at the higher end of the tangibility index as those that are affected the least ("control group").

The empirical methodology that was selected to test this result was a Difference-in-Difference-in-Differences Methodology, with the first difference to be a simple pre-post the reform, the second to be based on the firm location, and the third to be based on the tangibility ranking. The firms that are assumed to be affected the most are the firms that are located in a town with efficient courts and are at the lower end of the tangibility index. The regression that was used to test this is the following:

$$y_{ijzt} = \alpha_i + \alpha_t + \delta_1(Post_t * Treat_j * Rank_z) + \delta_2(Post_t * Treat_j) + \delta_3(Post_t * Rank_z)$$
$$+\delta_4(Treat_j * Rank_z) + \delta_5 * Post_t + \delta_6 * Treat_j + \delta_7 * Rank_z + \varepsilon_{ijzt}$$

where y_{ijzt} stands for the outcome variable of interest (e.g.*Leverage*, *NonZeroLeverage*, *LogTotalDebt*, *LogCAPEX*) for firm i that is located in town j in year t and belongs in industry z. $Treat_j$ is a dummy variable that is equal to one if the firm is located in a town with an "efficient" court and is equal to zero if the firm is located in a town with an "inefficient" court. $Rank_z$ is a dummy variable that is equal to one if the firm belongs to an industry that is at the lower end of the tangibility index and is equal to zero id the firm belongs to an industry that is at the higher end of the tangibility index. $Post_t$ is a dummy variable that is equal to zero before the reform is implemented (from 2008 to 2010) and equal to one after the reform is implemented (from 2011 to 2013). The baseline specification includes firm fixed effects (α_i) to capture any time-invariant firm characteristics as well as year fixed effects (α_t) to capture any aggregate time varying shocks. The term ε_{ijzt} is the error term. The coefficient of interest is δ_1 and it measures the pre-post difference in the outcome variable for the firms operating in "efficient" court towns and with low tangible assets relative to the other firms that are affected less by the reform.

Table 3.8 presents the results from the above regression for variables of interest. It seems that the estimation for *Leverage* did not survive this robustness check and the impact of the reform is not statistically significant when I control for the tangibility index. On the other hand, the results for the other variables remain statistically significant. In particular, from the estimated effect on *NonZeroLeverage* it appears that the probability to have access to bank financing for firms operating in towns with more efficient courts and the lower end of the tangibility index increased more by 2.25 percentage points relative to other firms that were not affected that much by the reform. Moreover, the *LogTotalDebt* seems to be higher by 0.259 basis points for firms operating in towns with efficient courts and in industries with less tangible assets compared to the other. These effects are both statistically and economically significant. The access to credit especially for small firms that operate in towns outside the capital of the country is one of the most important issues that affects the firms' performance and growth opportunities. To that direction evidence is also provided that the funds raised from increase to credit were invested. The fourth column of Table 3.8 presents the results on *LogCAPEX* which is used as a measure to estimate the impact of the reform on investment. From this estimation, it appears that firms affected more by the reform based on the criteria discussed above increased investment relative to the others, in particular the *LogCAPEX* seems to be higher by 0.218 basis points for firms affected more relative to the others.

Control for Unemployment

One important concern that needs to be addressed in order to strengthen the validity of the results is a possible macroeconomic effect that may drive the results. Croatia was one of the countries that suffered a lot from the 2009 crisis and entered a prolonged period of recession until early 2015. Since the effect of the crisis was so significant, it is necessary to control for the case that areas with more inefficient courts are the ones that suffered mostly from the crisis and for that reason the leverage level was lower. Even though a possible effect from the financial crisis would be captured by the region-year fixed effects, an alternative way to control for this is by adding controls for the unemployment at region level.

Table 3.9 presents the results for the three different variables of interest with unemployment level as a control variable. In particular, as controls are used three years lagged unemployment prior the reform multiplied with a year dummy to capture the continuous time variation of unemployment during the years. The results are consistent with unemployment levels as controls and this shows that the general economic conditions at the region level do not drive the variation in the leverage level.

The impact of the reform on leverage weakens with unemployment controls both on magnitude and on statistical significance. In particular, firms operating in towns with efficient courts have higher leverage by 0.24 than firms operating in towns with inefficient courts after the reform. The estimated coefficient is smaller than what was estimated in the previous part of the analysis and this happens because a macro level characteristic as is unemployment prior the reform may be correlated with other factors that affect the firm's performance.

In contrast with the estimated effects on leverage, the impact of the reform on access to bank financing as it is captured by the *NonZeroLeverage* variable seems to strengthen with unemployment controls. More specifically, the estimated coefficient, with unemployment controls, shows that firms operating in towns with efficient courts have 0.75 percentage points higher probability to have positive leverage than firms operating in towns with inefficient courts. The result is slightly smaller in economic magnitude but stronger in statistical significance.

On the same direction as the impact on access to bank financing, the impact on total volume of total debt is robust to unemployment controls. In particular, the effect of the reform on the logarithm of the total debt is positive and statistically significant with regional unemployment levels as control variables. As it is shown on Table 3.9 in the third column, firms in towns with efficient courts have 8.84 basis points higher log of total debt than firms in towns with inefficient courts when unemployment controls are added. As before, the result weakens on economic level but its statistical significance strengthens.

Matching Regions on Unemployment

Another approach to control for the effect that the general economic conditions could have on the estimated results is to match the towns where the firms are located based on the unemployment level. For this part of the analysis it is important the fact that the variation on court efficiency is on town level while the variation on unemployment is on region level. For that reason, there are several regions where exact matching can be succeed with within region variation on court efficiency, in other words there are several regions where towns with both efficient and inefficient courts are located. Table 3.10 presents the results with matched court locations. In this part firm and industry-year fixed effects are included.

The results with matched court locations for all variables of interest strengthen both on economic and on statistical significance. In particular, the coefficient of interest for *Leverage* increases from 0.46 percent to 0.73 percent while the coefficient for *NonZeroLeverage* increases from 0.85 percent to 1.03 percent and for *LogTotalDebt* increases from 0.102 to

0.130. This fact is very important as it shows that it is not the general economic conditions or other location wise characteristics that bias upwards the estimated results and for that reason the validity of the estimated results is stronger.

Analysis based on Firms' Size

The analysis above shows that firms operating in tows with more efficient courts have more debt and easier access to credit after the reform. But a question that remains to be answered though is what type of firms are those that influence mostly the results. Unfortunately, the characterization of size provided by the Amadeus dataset doesn't distribute firms equally and the majority of the firms included in the sample are characterized as small. In an attempt to deal with this issue, I rank the firms based on their total assets and create an alternative measure of size. Based on a ranking on firms' pre-reform average of total assets, I define as small the firms that belong in the first three deciles of the distribution, as medium the firms in the fourth until the eighth decile, and as large the firms that belong in a decile higher than the eighth.

Table 3.11 presents the results for the three different categories of sizes for the variables of interest. The results in Table 3.11 are estimated with firm and industry$*$year fixed effects. It is a very interesting conclusion to observe that the main results of the analysis are driven from the medium size firms. For all the different dependent variables of interest that the analysis was repeated (*Leverage, NonZeroLeverage, LogTotalDebt*) the main coefficient of interest shows up as statistically significant only for medium sized firms. Moreover, the economic magnitude of the coefficient is almost double the size of the estimated coefficient for the whole sample. This is an expected result as the significance comes only from one of the three categories of sizes that the whole sample includes.

More specifically, it seems that a medium size firm that operates in a town with efficient courts has by 1.01 percentage points higher leverage after the reform compared to a medium size firm that operates in a town with inefficient courts. The results for small and for large firms are not even close to statistically significant. Regarding the access to credit, a medium sized firm that operates in a town with efficient courts has an 1.80 percentage points higher probability to get access to bank financing after the reform than a firm that operates in a town with inefficient courts.

3.6 Conclusion

Croatia in January 2011 passed an Act on Execution of Cash Assets that affected significantly the way that judicial decisions of unsecured loans were enforced. Before the passage of the reform, a creditor received a writ of execution on cash assets from the courts that was linked on a specific bank account of the debtor. After the reform, the creditor has to request from a Financial Agency (FINA) to electronically transfer the amount owed from the debtor's bank account. FINA holds a unified database of all the banks accounts that are under the debtors identification number and has the authority to transfer the necessary funds from any of the debtor's account that includes the necessary amount. The importance of the reform is that it applies to only unsecured debts, in other words this mechanism could not be used to to collect any debt that has a fixed asset as collateral.

To identify the impact of the reform, I use a novel dataset on courts performance in 2010 based on the specific type of cases that was related to the new enforcement mechanism. As treated I define the firms that are located in towns with more efficient courts as these are the ones that receive the writ of execution from the courts within a very short period of time and can take better advantage of the new mechanism. As control I define the firms that are located in towns with less efficient courts as they will have to wait a long period of time until they receive the writ of execution from the courts. The main result of the paper is that firms that operate in an environment that was affected more by the reform had higher leverage after the reform was passed. Additionally, these firms that were located in towns with efficient courts had easier access to credit after the reform compared to those located in towns with inefficient courts. Firms that borrowed more, ended up investing more. After examining the impact of the reform on the investment level, it appears that the funds raised as debt after the reform were invested on fixed assets. Lastly, the firms that are affected the most are the medium sized firms and the firms with less tangible fixed assets. This finding provides evidence that firms with bigger difficulties to gain access to credit are benefited the most by a reform that made more secure the collection of unsecured debt and strengthened creditors' rights.

3.7 Figures and Tables

Table 3.1: Court Locations

Municipal Court Town	Number of Firms	Number of Observations
Beli Manastir	110	634
Benkovac	177	1,006
Bjelovar	601	3,462
Buje	918	5,298
Cakovec	1,460	8,434
Crikvenica	252	1,453
Dakovo	294	1,702
Daruvar	265	1,533
Delnice	283	1,642
Dubrovnik	1,600	9,275
Glina	35	197
Gospic	372	2,136
Hrvatska Kostajnica	36	207
Imotski	248	1,433
Ivanic-grad	538	3,105
Jastrebarsko	215	1,246
Karlovac	1,014	5,865
Knin	133	760
Koprivnica	430	2,473
Korcula	269	1,554
Krapina	400	2,322
Krizevci	285	1,639
Krk	489	2,853
Kutina	361	2,098
Labin	428	2,483
Makarska	491	2,850
Mali Losinj	165	952
Metkovic	378	2,181
Nasice	144	835
Nova Gradiska	188	1,068
Ogulin	133	774
Opatija	1,116	6,437
Osijek	1,530	8,810

Court Locations (continue)

Municipal Court Town	Number of Firms	Number of Observations
Pag	51	297
Pazin	560	3,264
Porec	1,434	8,242
Pozega	371	2,114
Pula	2,225	12,812
Rab	143	835
Rijeka	3,452	20,006
Rovinj	639	3,667
Samobor	876	5,060
Sesvete	944	5,427
Sibenik	1,111	6,412
Sinj	239	1,382
Sisak	571	3,288
Slatina	223	1,289
Slavonski Brod	653	3,759
Split	5,199	30,049
Stari Grad	190	1,099
Supetar	399	2,316
Trogir	796	4,594
Valpovo	174	1,005
Varazdin	1,785	10,364
Velika Gorica	857	4,945
Vinkovci	401	2,277
Virovitica	238	1,356
Vrbovec	501	2,888
Vukovar	294	1,693
Zabok	451	2,608
Zadar	1,651	9,539
Zagreb	21,096	121,574
Zapresic	594	3,416
Zlatar	230	1,336
Zupanja	138	800
Total	63,844	368,430

Table 3.2: Industries

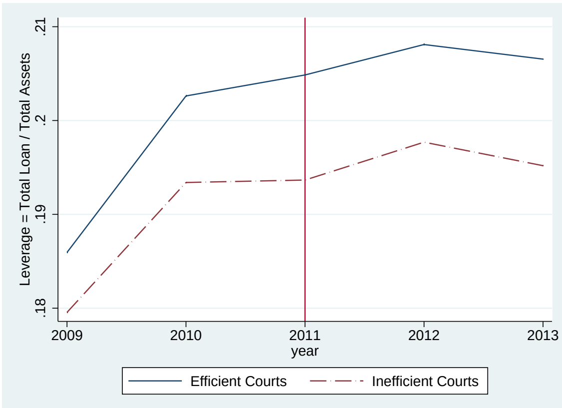
Industry Name	Number of Firms: Efficient Courts	Inefficient Courts	Total	Observations: Total
Agriculture,Forestry, Fishing and Hunting	517	526	1,043	5,968
Mining, Quarrying, Oil & Gas Extraction	51	76	127	733
Utilities	66	62	128	729
Construction	1,997	2,780	4,777	27,451
Manufacturing (Food & Beverage, Textiles & Apparel)	491	606	1,097	6,322
Manufacturing (Wood, Paper, Coal, Plastic Products)	659	747	1,406	8,158
Manufacturing (Metal, Machinery,Furniture)	910	1,173	2,083	12,112
Wholesale Trade	2,777	3,379	6,156	35,696
Retail Trade (Automobile, Furniture, Electronics, Food and Beverage, Health, Clothing)	845	1,212	2,057	11,896
Retail Trade (Sports, Book, Music, General Stores)	559	836	1,395	8,093
Transportation and Warehousing (Air, Rail, Water, Truck, Pipeline)	571	855	1,426	8,245
Transportation and Warehousing (Postal Service, Couriers, Warehouse)	21	15	36	204
Information	238	276	514	2,963
Finance and Insurance	52	91	143	826
Real Estate and Rental and Leasing	1,063	1,299	2,362	13,466
Professional, Scientific, Technical Services	2,223	2,845	5,068	29,362
Management of Companies and Enterprises	6	10	16	91
Administrative, Waste Management	555	700	1,255	7,248
Educational Services	137	193	330	1,935
Health Care and Social Assistance	110	167	277	1,587
Arts, Entertainment and Recreation	175	250	425	2,438
Accomodations and Food Services	847	1,347	2,194	12,602
Other Services	534	723	1,257	7,263
Public Administration	5	8	13	74
Total	15,409	20,176	35,585	205,462

Table 3.3: Descriptive Statistics

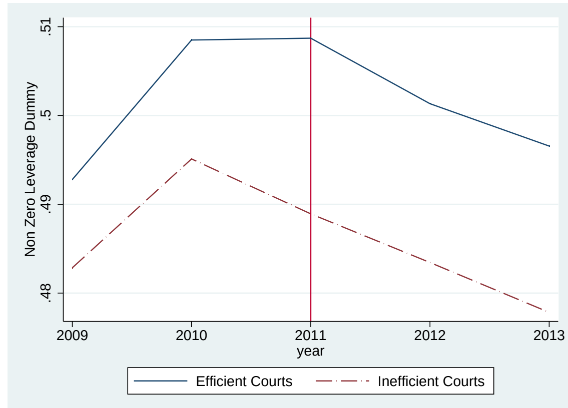
Variables	All firms	Efficient Courts	Inefficient Courts
Leverage	0.195	0.199	0.191
	(0.301)	(0.304)	(0.299)
Non Zero Leverage	0.494	0.502	0.488
	(0.500)	(0.500)	(0.500)
Total Debt	1,083,546.5	1,045,419.6	1,112,640.9
	(3,771,309.6)	(3,604,122.2)	(3,893,827.7)
Log(Total Debt)	6.367	6.454	6.302
	(6.613)	(6.599)	(6.623)
Capital Expenditures	419,471.8	413,377.5	424,213.6
	(1,904,415.2)	(1,850,830.9)	(1,945,085.7)
Log(Capital Expenditures)	8.551	8.548	8.554
	(4.763)	(4.759)	(4.766)
Total Assets	3,807,154.7	3,757,796.4	3,844,819.7
	(10,394,800.8)	(10,410,141.5)	(10,382,966.0)
Log(Total Assets)	13.42	13.40	13.44
	(1.957)	(1.959)	(1.955)
EBIT	113,163.2	110,899.8	114,890.4
	(1,097,113.9)	(976,180.6)	(1,181,097.9)
Profitability	-0.0688	-0.0631	-0.0731
	(3.339)	(2.845)	(3.672)
Sales	3,134,625.9	3,183,321.6	3,097,466.5
	(11,311,598.9)	(11,398,933.6)	(11,244,405.3)
Number of Firms	35,585	15,409	20,176
Number of Obs.	205,462	88,927	116,535

Figure 3.1: Access to Credit and Enforcement Reform - Average Effect

(a) Leverage Ratio



(b) Non-Zero Leverage



(c) Log of Total Debt

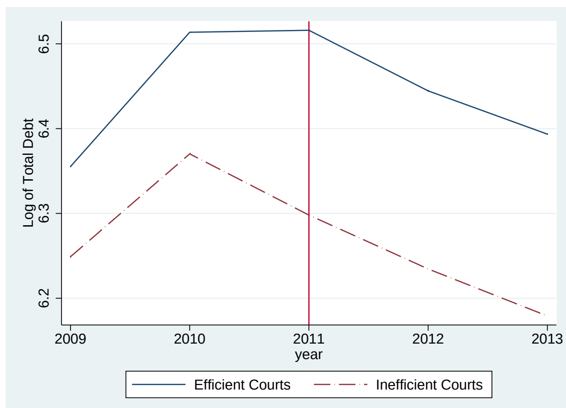


Table 3.4: Results on Leverage Ratio

	(1)	(2)	(3)	(4)	(5)	(6)
Post ∗ *Treat*	0.00465**	0.00460**	0.00591**	0.00583***	0.00571***	0.00694**
	(0.00188)	(0.00188)	(0.00293)	(0.00206)	(0.00206)	(0.00320)
Observations	205,462	205,462	205,462	177,935	177,935	177,935
R^2	0.002	0.005	0.004	0.002	0.005	0.004
Firm F.E.	✓	✓	✓	✓	✓	✓
Year F.E.	✓			✓		
Industry ∗ Year F.E.		✓			✓	
Industry F.E.			✓			✓
Region ∗ Year F.E.			✓			✓
Controls	Firm	Firm	Firm	Firm	Firm	Firm
Standard Errors	Robust	Robust	Robust	Robust	Robust	Robust
Sample Included	Whole sample	Whole sample	Whole sample	Small only	Small only	Small only

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.5: Results on Non Zero Leverage

	(1)	(2)	(3)	(4)	(5)	(6)
Post $*$ *Treat*	0.00836**	0.00850**	0.00634	0.0104***	0.0104***	0.00528
	(0.00358)	(0.00359)	(0.00530)	(0.00390)	(0.00390)	(0.00573)
Observations	205,462	205,462	205,462	177,935	177,935	177,935
R^2	0.001	0.002	0.002	0.001	0.002	0.002
Firm F.E.	✓	✓	✓	✓	✓	✓
Year F.E.	✓			✓		
Industry $*$ Year F.E.		✓			✓	
Industry F.E.			✓			✓
Region $*$ Year F.E.			✓			✓
Controls	Firm	Firm	Firm	Firm	Firm	Firm
Standard Errors	Robust	Robust	Robust	Robust	Robust	Robust
Sample Included	Whole sample	Whole sample	Whole sample	Small only	Small only	Small only

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.6: Results on Log of Total Debt

	(1)	(2)	(3)	(4)	(5)	(6)
Post $*$ *Treat*	0.102**	0.102**	0.0909	0.127***	0.126***	0.0760
	(0.0436)	(0.0437)	(0.0650)	(0.0463)	(0.0464)	(0.0688)
Observations	205,462	205,462	205,462	177,935	177,935	177,935
R^2	0.001	0.002	0.002	0.001	0.003	0.002
Firm F.E.	✓	✓	✓	✓	✓	✓
Year F.E.	✓			✓		
Industry $*$ Year F.E.		✓			✓	
Industry F.E.			✓			✓
Region $*$ Year F.E.			✓			✓
Controls	Firm	Firm	Firm	Firm	Firm	Firm
Standard Errors	Robust	Robust	Robust	Robust	Robust	Robust
Sample Included	Whole sample	Whole sample	Whole sample	Small only	Small only	Small only

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.7: Results on Log of Capital Expenditures

	(1)	(2)	(3)	(4)	(5)	(6)
Post ∗ *Treat*	0.118**	0.109**	0.0605	0.0925*	0.0888*	0.0475
	(0.0466)	(0.0465)	(0.0726)	(0.0541)	(0.0540)	(0.0833)
Observations	124,460	124,460	124,460	102,539	102,539	102,539
R^2	0.079	0.083	0.081	0.096	0.100	0.098
Firm F.E.	✓	✓	✓	✓	✓	✓
Year F.E.	✓			✓		
Industry ∗ Year F.E.		✓			✓	
Industry F.E.			✓			✓
Region ∗ Year F.E.			✓			✓
Controls	Firm	Firm	Firm	Firm	Firm	Firm
Standard Errors	Robust	Robust	Robust	Robust	Robust	Robust
Sample Included	Whole sample	Whole sample	Whole sample	Small only	Small only	Small only

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.8: Results based on Courts' Efficiency and on Tangibility Index

	(1)	(2)	(3)	(4)
	Leverage	*NonZeroLeverage*	*Log(TotalDebt)*	*Log(CAPEX)*
Post $*$ *Court efficiency* $*$ *Intangibility*	0.00209	0.0225**	0.259**	0.218*
	(0.00491)	(0.00907)	(0.112)	(0.122)
Observations	128,915	128,915	128,915	73,702
R^2	0.003	0.001	0.001	0.077
Firm F.E.	✓	✓	✓	✓
Year F.E.	✓	✓	✓	✓
Controls	Firm	Firm	Firm	Firm
Standard Errors	Robust	Robust	Robust	Robust
Sample Included	Whole sample	Whole sample	Whole sample	Whole sample

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.9: Results with Unemployment Controls

	(1)	(2)	(3)	(4)
	Leverage	*NonZeroLeverage*	*Log(Total Debt)*	*Log(CAPEX)*
$Post * Treat$	0.00245*	0.00755***	0.0884***	0.134***
	(0.00132)	(0.00261)	(0.0309)	(0.0437)
Observations	205,462	205,462	205,462	124,460
R^2	0.006	0.002	0.002	0.084
Firm F.E.	✓	✓	✓	✓
Year F.E.	✓	✓	✓	✓
Controls	Unemployment	Unemployment	Unemployment	Unemployment
Standard Errors	Robust	Robust	Robust	Robust
Sample Included	Whole sample	Whole sample	Whole sample	Whole sample

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.10: Results with Matching based on Unemployment Levels

	(1)	(2)	(3)
	Leverage	*NonZeroLeverage*	*Log(TotalDebt)*
$Post * Treat$	0.00731***	0.0103**	0.130**
	(0.00233)	(0.00440)	(0.0534)
Observations	136,925	136,925	136,925
R^2	0.007	0.003	0.003
Firm F.E.	✓	✓	✓
Industry∗Year F.E.	✓	✓	✓
Controls	Unemployment	Unemployment	Unemployment
Standard Errors	Robust	Robust	Robust
Sample Included	Whole sample	Whole sample	Whole sample

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Table 3.11: Results per Firms' Size

	Leverage			*NonZeroLeverage*			*Log(TotalDebt)*		
	(1)	(2)	(3)	(1)	(2)	(3)	(1)	(2)	(3)
Post ∗ Treat	-0.000237	0.0101***	0.000679	-0.00108	0.0180***	-0.000107	-0.0447	0.219***	0.0262
	(0.00420)	(0.00322)	(0.00337)	(0.00802)	(0.00650)	(0.00594)	(0.0852)	(0.0772)	(0.0819)
Observations	41,035	77,206	60,239	41,035	77,206	60,239	41,035	77,206	60,239
R^2	0.005	0.007	0.010	0.003	0.004	0.003	0.004	0.005	0.003
Firm F.E.	✓	✓	✓	✓	✓	✓	✓	✓	✓
Industry∗Year F.E.	✓	✓	✓	✓	✓	✓	✓	✓	✓
Controls	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm	Firm
Standard Errors	Robust	Robust	Robust	Robust	Robust	Robust	Robust	Robust	Robust
Sample Included	Small	Medium	Large	Small	Medium	Large	Small	Medium	Large

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

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