Essays in Empirical Corporate Finance

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ABSTRACT

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This dissertation studies empirical corporate finance problems related to financial intermediation. The dissertation is composed of three chapters. The first chapter exploits a natural experiment to quantify how much consumer credit borrowers are willing to pay for a good credit reputation. A department store in Chile offered its delinquent borrowers whose balance was higher than an arbitrary cutoff a renegotiation that reduced the monthly payment due required to appear in good standing to other lenders through a credit bureau. Using the variation induced by the cutoff in a fuzzy regression discontinuity design, I find that borrowers repay 10% of their monthly income to have a good credit reputation for 6 to 8 months. Even though previous work has documented the effect of reputational mechanisms on repayment behavior, this is the first paper that quantifies the value of a good credit reputation.

The second chapter focuses on the same group of borrowers as the first chapter and analyzes how the renegotiation campaign, which can be understood as a market for credit reputation, may introduce an inefficiency in consumer credit markets. I obtain an individual-level dataset of Chilean bank debt data, which I match to the department store data with the use of the unique national tax identifier. Because borrowers who renegotiate at The Store are indistinguishable from other borrowers in good standing in the credit bureau, other lenders may end up with a less creditworthy pool of borrowers. Thus, renegotiation may impose a negative externality that has not been previously studied in the literature. Indeed, I document that banks increase their lending to borrowers whose balance is above the cutoff, that is, who were affected by the renegotiation campaign, relatively more than

borrowers below the cutoff who were unaffected by it. Further, I find evidence consistent with a potential externality, as borrowers whose balance is above the cutoff default more and in higher amounts after the renegotiation campaign relative to borrowers whose balance is below the cutoff. I discuss some policy implications of this result.

Finally, the third chapter, co-authored with Emily Breza, studies the effect of the provision of trade credit on the contracting outcomes of a large client and its suppliers. We exploit a regulation change that reduced from 90 to 30 the number of days a large supermarket chain in Chile could wait to pay its suppliers. The regulation serves as a natural experiment as it was only binding for small suppliers, defined as those firms whose yearly revenues were less than an arbitrary cutoff. Focusing on a narrow window of firms with yearly revenues around the cutoff, we find that small suppliers sell their products at prices 5%-10% lower than large suppliers after the regulation change. We also provide suggestive evidence to isolate the mechanisms through which trade credit (or a lack thereof) affect market outcomes. These results help evaluate the tradeoffs small firms face when negotiating trade credit terms with large clients.

Table of Contents

List of Figures

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Dedication

This project is dedicated to the two loves of my life, Cotita and Tomas. Coti, you have made this possible and I will always thank you for sharing my dreams and allowing me to share yours.

Part I

The Value of a Good Credit Reputation: Evidence from Credit Card Renegotiations

1 Introduction

Public information on borrower repayment behavior, credit reputation, is an important determinant of the functioning of uncollateralized consumer credit markets. In these markets, lenders are more willing to supply credit to borrowers who have a reputation of timely repayment. As a result, borrowers have an incentive to repay their debt to have a good repayment record.[1] Indeed, previous studies have documented that reputational concerns affect repayment (Brown and Zehnder (2007); De Janvry, McIntosh, and Sadoulet (2010)). Anecdotal evidence also supports this view. For example, popular websites warn borrowers about the negative effect of bankruptcy or foreclosure on credit scores (see, for example, "Effect of Foreclosure on Credit Score" on CreditCardForum, viewed on March 27, 2013).

Beyond showing that credit reputation affects behavior, it is important to understand quantitatively how much borrowers are willing to pay for a good credit reputation. This willingness to pay is a central behavioral parameter that affects the allocation and price of consumer credit. It is also important to practitioners interested in credit contracts that maximize profitability, and to policy-makers interested in the design of efficient credit information-

[1] Eaton and Gersovitz (1981) and Diamond (1989), among others, provide models of debt repayment based on the value of a good credit reputation. Bulow and Rogoff (1989a) and Bulow and Rogoff (1989b) examine this motive for repayment in the context of sovereign lending. Pagano and Jappelli (1993) and Padilla and Pagano (2000) study the effects of information sharing on credit supply and on repayment behavior, respectively.

sharing mechanisms. Yet, to my knowledge, there are no estimates of this willingness to pay. The main empirical problem associated with this measurement is that borrowers may choose to repay for various reasons (e.g., the option of reducing the principal due and repaying it in full at a later date, or for moral and social benefits). The challenge is to isolate how much borrowers repay for a good credit reputation from how much they repay for all other reasons.

One way to address the empirical challenge is with the following ideal experiment. Consider two identical unsecured consumer credit borrowers, T and C (for "Treated" and "Control", respectively). Suppose T and C are in default and, as a result, have a bad credit reputation (i.e., both appear in default to other lenders). By revealed preferences, the value of a good credit reputation for both borrowers is less than the full payment due. Suppose the creditor offers both borrowers a renegotiation that reduces their respective payments due. The offers differ in that T's credit reputation is restored upon repayment of the lower amount but borrower C's is not. Therefore, by repaying the lower amount, both T and C obtain all the benefits of repayment except C does not obtain a good credit reputation. The difference in the ex post repayment of T and C measures how much they would pay to have a good credit reputation.

This paper exploits a natural experiment that resembles the ideal setting described above to quantify how much borrowers are willing to pay for a temporary good credit reputation. This measurement is the paper's contribution to the literature. Further, since the natural experiment can be replicated as a controlled field experiment and used to estimate this measure in other contexts and institutional settings, the contribution is also methodological.

I conduct my analysis on a proprietary dataset from a large department store in Chile (The Store). The Store issues unsecured installment credit cards used by borrowers to buy products or for cash advances. Repayment of this credit is structured into fixed monthly installments. A borrower who misses an installment becomes delinquent and receives a negative entry in the credit bureau (and, therefore, has a bad credit reputation). This entry is eliminated if the late installments are paid before 180 days, which gives the borrower a

good (or better) credit reputation.[2] After 180 days late the debt is written off, which implies that the negative entry remains in the credit bureau for 5 years. The information in the credit bureau is used by lenders to learn about a borrower's unobserved creditworthiness, but it is also used informally and many times illegally in other settings (e.g., when evaluating job candidates or for long-term cellular phone contracts). Note that credit bureaus are ubiquitous worldwide (Miller (2000)).

The natural experiment is based on an unexpected monthly campaign The Store started in February 2010. The campaign consists on calling borrowers who are late by more than 30 days and less than 180 days to offer a renegotiation. Borrowers who endogenously accept a renegotiation offer reduce their monthly payment due and increase the number of installments. Importantly, borrowers who renegotiate can have a good credit reputation as long as they remain in good standing with respect to the new payment terms. Note that, absent a renegotiation, delinquent borrowers can always pay less than their amount due. However, delinquent borrowers who do not renegotiate can only obtain a good repayment record by making their payment due in full. Thus, renegotiation gives delinquent borrowers who want to pay a lower installment (or who can only pay a lower installment) the chance to obtain a good credit reputation by doing so. Therefore, the causal effect of renegotiation on ex post repayment corresponds to the value borrowers assign to making a payment due in full and obtaining a good credit reputation.

The empirical strategy is to estimate the causal effect of renegotiation on ex post repayment. But because renegotiation is an endogenous outcome, a naive comparison of borrowers who renegotiate and those who do not may yield biased estimates. I address this concern by exploiting the fact that, during the campaign, renegotiations were only offered to borrowers whose outstanding balance was above an arbitrary cutoff of 50,000 Chilean pesos (roughly USD100). As a result, the fraction of borrowers who renegotiate is discontinuously higher

[2]Lenders have an incentive to form their own time series before information is deleted from the credit bureau. Thus, a more general interpretation of the setting is that credit reputation may lie in a continuum, where, all else equal, a borrower who has a negative entry in the credit bureau has a worse credit reputation than one who does not.

for borrowers whose balance as of January 2010, the month before the campaign started, is above the cutoff relative to borrowers below the cutoff. Using a fuzzy regression discontinuity (fuzzy RD) design that exploits the discontinuity in the probability of renegotiation at the cutoff (Imbens and Lemieux (2007); Lee and Lemieux (2010)), I find that renegotiation causally increases ex post repayment by 29% of the initial balance, or approximately 10% of the median monthly income of borrowers near the cutoff. I also find that renegotiation reduces the propensity to default only for a period of six to eight months. Because borrowers may have been willing to pay slightly higher installments, this estimate provides a lower bound on the borrowers' maximum willingness to pay for a temporary, for six to eight months, good credit reputation.

A potential concern is that borrowers may choose to repay their installment in full for reasons other than to have a good credit reputation. For example borrowers may pay in full for some intrinsic value associated with meeting a commitment. Thus, the causal effect of renegotiation would also measure this intrinsic value, confounding the value of a good credit reputation. The empirical setting allows me to address this concern in the following way. Upon write off (after 180 days late), all borrowers–including those above and below the cutoff–are contacted by phone or email to reduce their payment due.[3] Critically, however, borrowers who have been written off may *not* improve their credit reputation. To the extent that the two modifications (i.e., renegotiation and write off) are comparable, I link borrower T in the ideal setting to borrowers above the cutoff, who are exposed to the renegotiation campaign, and link borrower C in the ideal setting to borrowers below the cutoff, who may also reduce their payment due upon write off but not to obtain a good credit reputation.

The rest of the paper is organized as follows. Section 2 presents the empirical setting. Section 3 measures the willingness to pay for a temporary good credit reputation. Section 4 presents the concluding remarks.

[3]Upon write off, The Store contacts borrowers to offer a choice of repayment plan. This choice is open ended, and borrowers typically choose to pay lower amounts in fewer installments. On average, The Store is able to recover approximately 33% of the written off balance.

2 Empirical Setting

2.1 The Department Store Industry in Chile

The Store corresponds to one of three large department stores in Chile. These stores issue installment credit cards, which, taken together represent approximately 41% of the 11.6 million credit cards issued in Chile by number–the balance being bank issued revolving-debt credit cards.[4] Clients use these credit cards to buy products on credit (typically only for products sold by the issuer) and to take short-term cash advances. Repayment is structured into fixed monthly installments that include capital and interests, and stores usually add monthly charges for insurance payments and fees. The interest rate on this debt depends on the product bought and on the number of monthly installments (and, in particular, not on the borrower's ex ante credit risk), and, as of March 2010 it ranged from 20% to 50% yearly.[5]

When a borrower does not pay an installment in full on time, The Store adds fees to the outstanding balance and cancels the credit line. The restriction on new credit is relaxed if the late installment is paid within 180 days. After 180 days without payment, the debt is written off. The debt is unsecured, so The Store does not recover a product sold on credit. Debt collection may be judicially enforced, but typically an extra judicial settlement is achieved even if no balance is recovered.

2.2 Credit Information in Chile

Credit information in Chile is collected by a public and a private credit bureau. The public bureau (credit registry) concerns banks, which are forced by regulation to report each week both current outstanding balances (positive information) and defaults (negative information). The private bureau concerns all other unregulated credit-granting institutions,

[4]Figure taken from http://www.retailfinanciero.org as of 1Q 2012.

[5]Information obtained from the Chilean banking and financial institutions regulator, SBIF, at www.sbif.cl, as of March 2010.

including department stores. These institutions voluntarily exchange only negative information that is current by week. In particular, other lenders cannot observe the balance of a department store borrower who is in good standing.

The Store notifies the credit bureau that a borrower is "late" or "delinquent" after one installment is not paid in full on time. The Store updates the notification to "serious default" after the borrower is late by a pre-defined number of days (typically before 90 days). Finally, a borrower who is written off by The Store (180 days late) receives a serious default notification that lasts for up to 5 years. This long term notification is maintained even if the borrower agrees to pay a fraction of the balance after write off. On the other hand, a borrower who pays the delinquent fraction of her debt before write off has her entry removed from the credit bureau, and no record of this entry remains in the system. The credit bureau shares this information through financial information aggregators, the most widely used being DICOM (an Equifax subsidiary). These companies match the individual-level data through a unique national tax identifier. As of the time of this study, anyone can access a borrower's DICOM credit report by paying a small fee.

The information in the Chilean credit bureaus is used by credit-granting institutions to evaluate new credit applications (Cowan and De Gregorio (2003)). This information sharing arrangement is similar to those found in developed countries like the U.S., where evaluating an applicant's credit history is a prevalent practice (e.g., Barren and Staten (2003); Hunt (2006)). The law restricts the uses of information in the credit bureau to credit decisions. The extent to which non-financial institutions use this information is unclear, but anecdotal evidence suggests that this is a real concern for Chileans. For example, people with a bad credit reputation may be denied job opportunities or long-term phone contracts.[6] This externality may add to the borrower's concern for having a good credit reputation. Indeed,

[6]Barren and Staten (2003) argue that in the United States, consumer reporting agencies may release credit files only for permissible purposes, defined to be those "in conjunction with a variety of voluntary, consumer initiated transactions." In practice, employers may ask for the job applicant's permission to access her credit report, in effect allowing discrimination based on this measure. More generally, these external costs may be related to the "threat of direct sanctions" that sustain repayment in Bulow and Rogoff (1989a)'s model.

a July 23, 2011 article in the *The New York Times* indicates that: "A bad DICOM credit score usually means being blacklisted for jobs, mortgages or political office. If you are in DICOM (i.e., having a negative entry in DICOM), if you are not in hell, you are on the way there."

2.3 The renegotiation campaign

Formally, delinquent borrowers at The Store have always been able to renegotiate their terms of repayment. Renegotiations can be initiated by the borrower, but, according to management, in practice almost all are initiated by The Store. This suggests that most borrowers are not aware that they may, in fact, initiate a renegotiation. At the time of this study, renegotiations were only allowed for borrowers whose outstanding balance, computed as the sum of all remaining installments including capital and interests, was above 50,000 pesos (approximately USD100).[7]

In February 2010, The Store started a monthly renegotiation campaign targeting borrowers who, as of the last day of the previous month, met three selection criteria:

1. Borrowers late by more than 30 days and less than 180 days,
2. Borrowers who had first used The Store credit card at least six months earlier,
3. Borrowers with at most one renegotiation in the last year and two renegotiations in the last three years.

I refer to the subsample of borrowers with a positive balance as of January 2010 who satisfy these three selection criteria as the "renegotiation offer sample." The renegotiation offer sample represents 13.6% of the total number of borrowers with a positive balance as of January 2010 (the "full sample").[8] Cross-sectional means and standard deviations of selected

[7]The Store had not specifically approached borrowers with loan balances relatively close to the cutoff before February 2010, as the fraction of renegotiations before this date is trivially low for these balances. This suggests that the balance cutoff was unknown to borrowers.

[8]Because of a confidentiality agreement with The Store, I cannot disclose the number of borrowers for any of the samples.

variables as of January 2010 for the full sample (Columns 1 and 2) and for the renegotiation offer sample (Columns 3 and 4) are shown in Panel A of Table 1. Relative to the full sample, borrowers in the renegotiation offer sample have on average a higher balance (588,000 pesos vs. 397,000 pesos – roughly US$1200 vs. US$800), are slightly younger (42 years old vs. 45 years old), started using The Store credit card 17 months later (tenure of 79 months vs. 96 months), and have a higher default rate (45% vs. 6%).

At the time of application for a Store credit card, borrowers in the renegotiation offer sample report an average (median) monthly income of approximately 285,000 pesos (201,000 pesos), lower than the full sample average (median) of 342,000 pesos (250,000 pesos) and the Chilean median of 360,000 pesos.[9] Finally, by construction, all borrowers in the renegotiation offer sample have missed at least one installment, and, because of these late installments, owe immediately 47% (35%) of their outstanding balance on average (median).

During February 2010, The Store called borrowers in the renegotiation offer sample who owed more than 50,000 pesos (the "cutoff") to offer a renegotiation of the terms of repayment. The cutoff was not revealed to borrowers. The campaign was repeated on subsequent months, each month selecting borrowers who were eligible as of the previous month. Thus, borrowers in the renegotiation offer sample could receive a renegotiation offer in subsequent months if they met the three selection criteria and had an outstanding balance above the cutoff. In particular, approximately 10% of all borrowers in the renegotiation offer sample whose balance as of January 2010 was lower than the cutoff–who were initially ineligible for the offer–become eligible in future months.[10] I do not observe which borrowers were called, but according to The Store's management, approximately 30% of all borrowers in the renegotiation offer sample whose balance as of January 2010 was above the cutoff were contacted.

[9]Figure taken from the Chilean statistics bureau, www.ine.cl. United States 2010 median annual household income was USD49,277, roughly 1.0 MM pesos per person per month (figure taken from Census Bureau).

[10]As a placebo test, I also estimate at 10% the fraction of initially ineligible borrowers who become eligible for an offer in later months if the campaign cutoff were instead 60,000 pesos (USD120). This suggests that borrowers do not manipulate their balance and their default status simultaneously to be eligible for an offer, at least over and above what would be expected for borrowers with balances within 10,000 pesos of the cutoff. This further supports the notion that borrowers are unaware of the cutoff.

The remaining 70% of all borrowers in the renegotiation offer sample with balances higher than the cutoff were either (i) not called, (ii) not available to answer the phone, or (iii) had provided a wrong or non-existent phone number. This fact does not affect the empirical strategy, but rather implies that my estimates may apply to a selected group of borrowers and not to the entire sample. I explore how this selection may affect the interpretation below.

2.3.1 The discontinuity in the fraction of renegotiated borrowers

Figure 1 shows the fraction of borrowers in the renegotiation offer sample who renegotiate their debt within six months of the start of the campaign in bins of outstanding balances of 10,000 pesos (USD20). Visual inspection reveals this fraction is discontinuously higher for borrowers whose balance as of January 2010 is higher than the cutoff than for borrowers whose balance is lower than it. The figure also shows the ordinary least squares (OLS) fitted values and 90% confidence intervals of the regression:

$$renegotiation = \omega + \pi treated + f\left(amount - 50,000\right) + \eta, \tag{1}$$

where *renegotiation* is a dummy that equals 1 if a borrower renegotiates within six months, *treated* is a dummy that equals 1 if a borrower's initial balance is higher than the cutoff, and $f\left(\right)$ is a 4th degree polynomial.[11] The polynomial f, depicted graphically as the smooth line on both sides of the cutoff, controls for any underlying relationship between the fraction of borrowers who renegotiate and the initial balance (*amount*). The coefficient π, which in the plot corresponds to the difference in the vertical axis between the points where the left and right polynomials intersect the cutoff, is a measure of the size of the discontinuity. As

[11] The polynomial is evaluated on $amount - 50000$ pesos so that the coefficient on *treated* corresponds to the effect of the renegotiation at the cutoff. I run all the main regressions on a subsample of borrowers in the renegotiation offer sample whose balance as of January 2010 is between 10,000 and 200,000 pesos. Standard errors are clustered at the "comuna" level, a Chilean geographical division comparable to a U.S. county (332 comunas in this sample).

per visual inspection, the discontinuity is large and significant six months after selection.[12]

To formalize these results, Table 2 reports the OLS estimates of coefficient π and the constant ω of regression (1) for renegotiations that occur within 1, 3, 6, 12, and 20 months after the campaign starts. The point estimates imply that the phone campaign increases the fraction of renegotiated borrowers above the cutoff by 12.0% six months after selection relative to borrowers below the cutoff. The coefficient π is significant at the 1% level and is roughly constant for at least 20 months after selection. Thus, the impact of the phone campaign is highest in the first months after selection.

2.3.2 Terms of the renegotiation offer

Phone callers were instructed to read from a script that detailed the renegotiation offer. The offer had the following terms:

1. Number of monthly installments chosen by the borrower between 6 and 48, with no grace periods,

2. Interest rate on outstanding balance is a function of the number of days late,

3. Borrower is immediately zero days late with The Store and in good standing with the credit bureau, and

4. Borrower is banned from taking on new credit at The Store until the full renegotiated balance is paid.

The first two terms imply that the renegotiation offer is equivalent to a debt rescheduling, with a higher outstanding balance spread out into more monthly installments, each with a lower required payment. Note that The Store does not offer to forgive any principal owed.

[12]The positive and convex relationship for balances below the cutoff shows that borrowers who were initially ineligible for the renegotiation campaign may become eligible in later months. The positive and concave relationship for balances above the cutoff can be explained by the fact that phone callers have minimum renegotiation targets to meet, in effect giving them an incentive to first target borrowers with larger balances. These two facts make it harder to find a discontinuity in the fraction of renegotiated borrowers at the cutoff.

The third term brings the borrower in good standing with The Store and with the credit bureau. The final term fully restricts access to new credit from The Store until the entire balance is paid off. This restriction is more stringent than the one faced by someone who is still in default, who can regain access to new credit at The Store by paying the late installments.

Table 3 shows summary statistics of the terms of repayment for renegotiations that occur within six months after the start of the campaign, for borrowers whose initial balance is between 50,000 and 100,000 pesos (i.e., for borrowers whose balance is at most 50,000 pesos from the cutoff and are eligible for a renegotiation). Relative to before the renegotiation, monthly installments are reduced on average (median) by 33.5% (41.7%). Note that the first payment due is reduced by more because delinquent borrowers already owe three accumulated installments. The average number of installments (including those accumulated) increases from 6.3 before the renegotiation to 11.7 after the renegotiation. This increase overcomes the reduction in monthly payments, as outstanding balances, calculated as the sum of all remaining installments, increase on average (median) by 31.5% (34.3%). Clients pay on average 5.9% of the balance up front, but 64.1% of all borrowers pay zero upfront. By valuing the discounted debt cash flow structure at the current outstanding balance, I estimate an implied monthly interest rate of 4.0% (3.1%) for these contracts. This is my best estimate for the rate borrowers use to discount future cash flows, which I use in the next section to measure ex post repayment.

3 Measuring the willingness to pay for a good credit reputation

3.1 Empirical implementation

The outcome of interest, the ex post debt repayment as a fraction of initial balance, is not directly observable. I construct this variable using the available data on monthly balances and transactions in the following manner. Each monthly net payment, $payments_t$, is calculated as the sum of outstanding balance reductions (payments in good standing including capital and interest), plus recoveries for loans written off, plus up-front payments in renegotiations, minus the value of new credit (products and cash advances),

$$\begin{aligned} payments_t &= 1\left(\Delta balance_t < 0\right)\left(-\Delta balance_t\right) + 1\left(writeoff_t = 1\right) recoveries_t + \\ &\quad 1\left(renegotiation_t = 1\right) upfront_t - 1\left(newcredit_t > 0\right) newcredit_t. \end{aligned}$$

The main outcome variable, $npv\left(r\right)$, is the sum of the present value as of January 2010 of the monthly payments divided by the borrower's initial outstanding balance,

$$npv\left(r\right) = \frac{1}{balance_{Jan2010}} \sum_{t=1}^{20} \frac{payments_t}{\left(1+r\right)^t},$$

and is a function of the discount rate r.[13] Note that, by construction, the outcome variable includes payments borrowers make after write off.

I argue above that the excess repayment induced by renegotiation can be attributed to the willingness to pay for a good credit reputation. Therefore, the empirical challenge is to determine the causal effect of renegotiation on $npv\left(r\right)$. As a first step, this measurement

[13] I use one discount rate for all borrowers, and winsorize the outcome variable at the 1st and 99th percentiles.

may be implemented through the following regression model:

$$npv\left(r\right)=\alpha\left(r\right)+\beta\left(r\right)renegotiation+\epsilon, \tag{2}$$

where *renegotiation* is a dummy variable that equals 1 if the borrower renegotiates. Table 4 reports the results of the OLS regression of (2) for a range of monthly discount rates $r\in\{0\%,1\%,\ldots,9\%\}$. The sample corresponds to borrowers in the renegotiation offer sample whose balance as of January 2010 is between 50,000 pesos and 200,000 pesos (i.e., who are initially eligible for a renegotiation). The coefficient $\beta^{OLS}\left(r\right)$ is always positive: borrowers who renegotiate repay a relatively larger fraction of their debt. The coefficients range from a minimum of 19% for a 9% monthly discount rate, to 45% with no discounting, all significantly different from zero at the 99% confidence level.

However, the estimated $\beta^{OLS}\left(r\right)$ coefficient does not necessarily measure the causal effect of renegotiation. The problem is that borrowers who renegotiate may be different in some unobserved manner from borrowers who don't renegotiate, violating strict exogeneity. Thus, OLS may result in biased estimates of the true coefficients.

3.2 The fuzzy regression discontinuity

I exploit the discontinuity in the probability of renegotiation induced by the phone campaign at the 50,000 pesos cutoff to estimate the causal effect of renegotiation on repayment. Intuitively, the idea is to compare the repayment of borrowers whose balance as of January 2010 is slightly higher than the cutoff with borrowers whose balance is slightly lower than the cutoff.[14] I implement this comparison through a fuzzy RD design. In order to conclude that the difference in the $npv\left(r\right)$ of borrowers above and below the cutoff is caused by the renegotiation, I need to assume that these two groups are statistically indistinguishable before the

[14]In the Appendix I examine the campaign's three other selection criteria that could be used as a source of exogenous variation in the probability of renegotiation: the 30 days late margin, the 6 month tenure margin, and the maximum number of renegotiations in the past years margin.

campaign. In the fuzzy RD setting, this is equivalent to assuming that the cross-sectional distribution of the unobserved residual is continuous at the 50,000 pesos cutoff.

Following Lee and Lemieux (2010), I test whether the data rejects the identification assumption by examining the distribution of borrowers and the cross-sectional distribution of predetermined observable variables at the cutoff. First, the distribution of borrowers is relatively smooth around the cutoff, as is verified by visual inspection of the histogram shown in Figure 2. More formally, McCrary (2008) provides a statistical test of continuity in the density of borrowers at the cutoff: the estimated test statistic is not statistically different from zero and would be found in the interval $[-0.108, 0.180]$ with a 95% probability.[15]

Second, Figure 3 shows plots of the averages for age, a dummy for female borrowers, fraction of balance that is late, and tenure, all as of January 2010, grouped in bins of 10,000 pesos (USD20) of the initial balance. The plots also show the fitted values from the borrower-level regression of each of these variables on the polynomial f and the $treated$ variable. Visual inspection suggests there are no statistical discontinuities in the cross-sectional distributions of any of these variables around the cutoff. Table 5 formalizes these results. Column (1) shows the partial effect of each predetermined variable on the outcome variable $npv\,(r)$ using $r = 4\%$. Notably, $npv\,(4\%)$ is lower for borrowers who are late by a higher fraction of their balance, who have been late for a longer period, who earn less and who are female. Column (2) shows the coefficient on $treated$ of a modified version of regression (1) where the outcome is each predetermined variable:

$$variable = \alpha + \pi treated + f\,(amount - 50,000) + \epsilon.$$

Column (3) shows the p-value of the test that π equals zero in each individual regression. The results confirm that none of the predetermined covariates jump at the threshold. These two results therefore fail to reject the identification assumption.

[15] Generated with the code developed and made public by Justin McCrary, see http://emlab.berkeley.edu/~jmccrary/DCdensity/.

By substituting equation (1) into the regression model (2) and relabeling coefficients, I obtain the fuzzy RD reduced form:

$$npv\left(r\right)=\alpha_{0}\left(r\right)+\beta\left(r\right)\times\pi\times treated+\tilde{f}\left(amount-50,000\right)+\chi. \tag{3}$$

Note that the fuzzy RD setting implements a Wald estimator for $\beta\left(r\right)$. This estimator is simply the ratio of the coefficient of *treated* on regression (3), $\beta\left(r\right)\times\pi$, to the coefficient on *treated* on regression (1), π. The fuzzy RD procedure is akin to a setting where, conditional on $\widetilde{f}\left(amount-50,000\right)$, *treated* is an instrumental variable for *renegotiation* (Hahn, Todd, and Van der Klaauw (2001)). I estimate $\beta\left(r\right)$ in the cross-section of borrowers in the renegotiation offer sample using a two-stage least squares (2SLS) procedure where (1) is the first stage and (2) is the second stage.

An outstanding balance that is higher than the cutoff means a borrower is more likely to be offered a renegotiation, but borrowers still endogenously choose whether to accept the offer. Following Lee and Lemieux (2010), I assume that crossing the 50,000 pesos cutoff does not affect repayment other than through the change in the probability of receiving an offer to renegotiate (excludability), and that no borrower that would have accepted a renegotiation with a debt balance just below the cutoff would not have renegotiated with a debt balance just above the cutoff (monotonicity). Then, $\beta\left(r\right)$ estimates the causal effect of renegotiation on "compliers" of this instrument–a local average treatment effects (LATE). Compliers of this instrument are borrowers who renegotiate because they owe more than the cutoff.[16]

3.3 Results: the willingness to pay for a good credit reputation

Figure 4 shows the average $npv\left(r\right)$ in bins of 10,000 pesos and the fitted values of the reduced form regression (6) for $r=4\%$. Visual inspection of the plot reveals the discontinuity in

[16]The excludability assumption is supported by the fact that no other Store policy changes discontinuosly at a 50,000 pesos balance. The estimated fuzzy RD coefficients are also local in the sense that the LATE interpretation applies only to the sub-population of compliers with outstanding balances close to the cutoff (see Angrist and Pischke (2008), chapter 6 for details).

repayment induced by the renegotiation campaign: $npv\,(4\%)$ is significantly higher for borrowers whose balance as of January 2010 is right above the cutoff relative to borrowers right below it. Given the identification assumption, this discontinuity in the average repayment at the cutoff for all borrowers in the renegotiation offer sample (i.e., the reduced form estimate) is attributed to the renegotiation campaign, which causally increases the ex post repayment of the *treated* instrument's compliers.

Table 6 reports the fuzzy RD regression (3) for the same range of monthly discount rates as in the OLS regression results shown above. The estimated coefficient $\beta\,(r)$ varies from a low of 13% when the monthly discount rate is 9%, to 44% with no discounting, and is significantly different from zero (for at least a 10% significance level) for monthly discount rates that are lower than 7%. Note that results are similar to the OLS estimates. This is consistent with a setting in which, conditional on borrower eligibility, the renegotiation offer is randomly assigned and always accepted.[17]

The lower payment due obtained by renegotiation reduces the price of having a good repayment record for a certain period of time. This induces borrowers to start making payments in full in order to obtain a good credit reputation. Borrowers below the cutoff may always choose to make payments below their amount due but, by doing so, cannot obtain a good credit reputation. Further, borrowers can restructure their terms of repayment upon write off, thus lowering the price of any other behavioral benefit of making the payment due in full. By comparing borrowers above and below the cutoff through the fuzzy RD, I disentangle what fraction of these repayments is made to have a good credit reputation.

At my preferred discount rate of 4% per month, renegotiation increases repayment by 29% of the initial balance. This is a lower bound on the willingness to pay for a good credit reputation because, presumably, some borrowers would have been willing to pay higher installments for the same period of time. This estimate corresponds to 7.3% (9.7%) of the

[17]The Appendix includes four additional fuzzy RD regressions assuming "placebo" discontinuities at other cutoffs close to 50,000 pesos, using the same specification and interval width as is presented here. In all cases, the results show no discontinuity at these cutoffs.

average (median) monthly reported income of borrowers in the renegotiation offer sample whose balance is between 40,000 pesos and 60,000 pesos.

Even though the results are valid for a relatively large range of discount rates, it is not surprising that statistical significance is lost for relatively high discount rates. Renegotiation reduces the initial monthly installment but delays initial payments to the future. A sufficiently high discount rate that puts a lot of weight on the relatively lower initial payments will compensate for the fact that borrowers who renegotiate pay more over time. Nonetheless, the results hold by making the implicit assumption that discount rates are below a maximum level.

I assess the importance of a good credit reputation relative to other incentives borrowers may have to repay their debt. Visual inspection of figure 4 suggests that at the cutoff, borrowers whose balance is below the cutoff repay approximately 40% to 45% of their initial balance, higher than the estimated value of a good credit reputation. The exact figure of 43% is obtained from the constant coefficient α on the reduced form regression (3) (results not shown). Note, however, that this 43% is the average repayment for all borrowers in the renegotiation offer sample whose balance as of January 2010 is slightly less than the cutoff. Hence, it does not necessarily represent what the group of compliers would have repaid absent the renegotiation. Indeed, the compliers' counterfactual repayment is not identified with a fuzzy RD, which only allows identification of the change in repayment given by $\beta(r)$.

I test whether compliers are observably different from borrowers in the renegotiation offer sample by using the methodology found in section 4.4.4 of Angrist and Pischke (2008). The idea is to test whether compliers are more or less likely than an average borrower from the renegotiation offer sample to have a value of a pre-determined variable that is above the sample median. Column (3) of Table 7 shows that as of January 2010 compliers are almost as likely as borrowers in the renegotiation offer sample whose balance is within 10,000 pesos of the campaign cutoff to be late by more than 97% of their balance, to be in arrears by more than 87 days, to have a credit score that is above 472, to be female, and to be older

than 37 years old. Compliers are relatively less likely to have tenures at The Store that are longer than 49 months, and to have a monthly income that is above 150,000 pesos (U$ 500), but this relative likelihood is in both cases no lower than 0.75.[18]

Based on this analysis, compliers seem to be slightly poorer and to have had a shorter tenure at The Store, but look very much like other borrowers in the renegotiation offer sample in all the other dimensions. Note that, as per Table 5, monthly income and tenure at The Store are (weakly) positively correlated with ex post repayment. This suggests that compliers would have repaid relatively less than 43% of their balance on average absent the renegotiation campaign. Nevertheless, based on the similarity in the other variables, I use the 43% figure as a baseline estimate for what the average repayment of compliers would have been absent the renegotiation campaign.

Note that, as per Figure 1, there are very few renegotiations for borrowers whose initial balance is below the cutoff. How, then, do we rationalize this relatively high level of ex post repayment? I estimate that roughly 42% of all borrowers whose initial balance is 40,000 to 50,000 pesos (i.e., below the cutoff) repay their initial debt in full, 25% repay a fraction of their debt, and 33% repay zero or less. The first group corresponds to borrowers who receive a positive shock and are able and willing to make their payments in full. Presumably, the value of a good credit reputation for these borrowers becomes higher than the required payment due to obtain it after the campaign starts (which also suggests that my estimates are an even lower bound of the value of a good credit reputation). The second group corresponds to borrowers who obtain some value from repaying less than their full balance (e.g., for social or moral concerns, or from reducing the future payment due). In particular, some of these borrowers repay a fraction of their balance after write off when they cannot obtain a good credit reputation (on average, payments after write off represent 11.5% of the initial balance, or 14.2% for those borrowers who are written off). The third group corresponds to borrowers who either do not pay, or are able to pay and get in good standing only to fall in default

[18]The Appendix includes tests of whether the value of a good credit reputation varies based on predetermined characteristics like age, tenure and income.

again.

3.4 How long does the good credit reputation last for?

Borrowers who renegotiate acquire a good credit reputation for as long as they remain in good standing. To estimate this period of time, I run the fuzzy RD using $default90\,(m)$ as the outcome, defined as a dummy variable that equals 1 if the borrower is at least 90 days late (including write off) as of m months after January 2010.[19] The results are reported in Table 8. Three months after the campaign starts, renegotiation causally reduces the default rate by 44%. But after month eight the causal effect on default is statistically insignificant. This result holds for at least 20 months after selection.

Renegotiation mechanically reduces default in the first months of the campaign because delinquent borrowers are immediately transitioned to zero days late at The Store. Then, borrowers choose to pay some monthly installments and remain in good standing. But, the evidence suggests that they only buy a temporary good credit reputation which lasts, on average, for at most 8 months.

4 Conclusion

This paper quantifies how much consumer credit borrowers are willing to pay for a temporary good credit reputation. Borrowers value a temporary good credit reputation because it gives the option to access formal credit markets. Other margins through which a good reputation may be valuable, like future access to jobs, cannot be tested in this study due to lack of data.

Presumably, the estimated willingness to pay for a good credit reputation depends on the institutional setting. But the natural experiment I exploit may be replicated in a field

[19] Results using different default windows (30, 60, 120, and 180 days) share the same pattern but vary in the length of the period of good reputation. I use 90 days as a conservative upper bound on the period of time a delinquent borrower will remain unreported by The Store.

experiment. Thus, a more general interpretation of the paper's first contribution is to provide a methodology to estimate this measure in other settings. An interesting area for future research is to compare estimates obtained in different settings and time periods, and to correlate the variation in these estimates with the business cycle.

The results are relevant for policy makers interested in how reputation shapes consumer credit markets. The results are also relevant for practitiones interested in the design of debt contracts should consider that households value having a good repayment record. Indeed, my estimates suggest that borrowers are willing to pay a non-trivial portion of their monthly income for a good credit reputation. Failure to account for this may result in lower repayment or contract take-up. This fact may help explain the relatively unsuccessful history of the various loan modification programs implemented in the US mortgage markets following the financial crisis.

Part II

Credit Reputation, Future Lending and Information Externalities

1 Introduction

This chapter studies the first-order theoretical margin through which rational borrowers may benefit from a temporary good credit reputation: it gives an option to access formal credit markets. If this option is valuable, some borrowers would exercise it and take on more debt from other lenders. This linkage between lenders illustrates the premise behind the value of a credit bureau as a source of future lending: it generates reputational incentives for borrowers to repay their loans.

But this linkage may also induce an inefficiency. If lenders and borrowers are able to manipulate the information that is shared through credit bureaus, then other lenders who use this information may mistakenly categorize borrowers as more creditworthy than they really are. Since there is no observable market for credit reputation, these ideas are hard to verify empirically.

To examine these hypotheses I collect a dataset of individual-level bank debt data and match it to data used in Chapter 1 of this dissertation with the use of the unique Chilean tax identifier. In the first chapter I document how a large Chilean department store offers renegotiations to its delinquent borrowers that allow borrowers to reduce the price of a good credit reputation. Critically, the offer is only made to borrowers who as of January 2010 owe more than an arbitrary balance cutoff, 50,000 pesos. Hence, there is a large and significant discontinuity in the fraction of borrowers who renegotiate their debt for borrowers whose balance is slightly higher than the cutoff relative to borrowers whose balance is slighlty

lower.

In this chapter I study how access to bank debt and repayment of the bank debt varies for borrowers on both sides of the cutoff. I find that one year after the renegotiation campaign starts, borrowers above the cutoff increase their bank debt, specifically mortgage debt, significantly more than borrowers below the cutoff. This evidence is consistent with the claim that borrowers who renegotiate buy a temporary good credit reputation for the period of time during which they want to take on bank debt. It is also consistent with the fact that the supply of mortgage debt depends on the borrower's credit reputation, as in the United States where (FICO) credit scores are an important determinant of the availability and price of mortgage credit (e.g., Mian and Sufi (2009); Keys, Mukherjee, Seru, and Vig (2010)).

Further, because of the way credit information is shared, renegotiations are not observable by users of the credit bureau. Indeed, borrowers from The Store who have renegotiated are indistinguishable, in terms of their credit reputation, from borrowers who have been able to repay their original installments. Therefore credit reputation is a noisier signal of the borrower's true creditworthiness. Because of this, other lenders may end up with a less creditworthy pool of borrowers. I find evidence consistent with this claim as, ex post, bank default rates increase more for borrowers above the cutoff relative to borrowers below it. Thus, renegotiations can be understood as a form of collusion between The Store and borrower that imposes a negative informational externality on a third party, banks. Documenting this effect, which has been unexplored by the theoretical literature on renegotiations, is the main contribution of this chapter.

Aghion and Bolton (1987) present a theoretical setting where optimal bilateral contracts may be welfare decreasing. Musto (2004) documents a pattern similar to my results for borrowers whose credit reputation improves as they emerge from bankruptcy. A policy implication is that the design of credit bureaus should be robust to potential collusion among agents.

The results suggest that lenders may benefit from offering modifications that allow delin-

quent borrowers to improve their credit reputations upon repayment. This relates to previous literature that has found that, in general, renegotiation of delinquent mortgage debt has not been a successful policy tool following the 2008 financial crisis, as measured by low take-up and unchanged recovery rates.[20] However, the overall welfare effect is unclear, as these bilateral modifications may impose an informational externality on other lenders. Further, mortgage credit markets are significantly different to The Store's type of credit (e.g., among other differences, mortgages are collateralized and open to securitization).

2 Empirical implementation

I collect an individual-level dataset of bank debt aggregated across all banks in Chile for borrowers in the renegotiation offer sample. I use these data to test whether renegotiation allows borrowers to increase their bank debt. Due to privacy concerns, the bank debt data were provided with invented identification numbers. Therefore, the data are not matched to other borrower characteristics, and in particular, to outstanding balances at The Store as of January 2010. This renders a fuzzy RD implementation impossible.

Therefore, I base my identification on a differences-in-differences empirical strategy. This strategy rests on the assumption that borrowers who as of January 2010 owed more than the cutoff by a sufficiently small amount, for example, 10,000 pesos (USD20), would have had time trajectories of the outcome variables that are similar to borrowers who owed less than the cutoff, absent the renegotiation campaign. In this section, I refer to the group of borrowers in the renegotiation offer sample who as of January 2010 owe between 50,000 and 60,000 pesos as the Treated group, and borrowers who as of January 2010 owe between 40,000 and 50,000 pesos as the Control group.[21] I obtain, through SINACOFI, an aggregator and distributor

[20] Among others, Piskorski, Seru, and Vig (2010) and Agarwal, Amromin, Ben-David, Chomsisengphet, and Evanoff (2011) argue that securitization reduces incentives for lenders to renegotiate, and Adelino, Gerardi, and Willen (2012) shift the blame of this failure to features particular to mortgage markets. Mayer, Morrison, Piskorski, and Gupta (2011) argue that the strategic behavior of borrowers anticipating a renegotiation may be more costly than its potential benefits. More consistent with my results, Fuster and Willen (2012) find a strong effect of mortgage payment size on repayment behavior, but not related to ex post renegotiations.

[21] The results using a window of 5,000 pesos (USD10) around the cutoff are quantitatively and qualitatively

of financial information, two separate datasets, one for the Treated group and another for the Control group, with individual-level bank debt data for the months of December 2009, December 2010, March 2011, and April 2011. If the identification assumption is valid, the outcomes of the Control group provide a valid counterfactual for the Treated group.[22]

2.1 Cross-sectional and time-series evidence for the identification assumption

To motivate the identification assumption, Table 1 reports the cross-sectional means and standard deviations of some variables in The Store dataset as of January 2010 and in the bank dataset as of December 2009, for the Treated and Control groups and for both groups combined (denoted as the Full Sample). Both groups should, in theory, be quite similar before the renegotiation offer given that they differ on average by only USD20 in their balance at The Store. Indeed, as per the *t-stats* reported in the last column of Table 1, the means of almost all variables are not statistically different between the Treated and Control groups. Three variables are statistically different between both groups: internal credit score at The Store (difference equals 4% of that variable's standard deviation), mortgage debt balance (4.8% of standard deviation), and fraction of borrowers in default (2.4% of standard deviation). But these differences are not economically important relative to their standard deviations, suggesting they would hardly result by themselves in a differential change in access to bank debt in the future.

Nevertheless, the concern with a differences-in-differences identification strategy is not that borrowers above and below the cutoff are different ex-ante but that they have different time trends in outcomes. I study this concern using variables from the data provided by The Store for which the longer time series is available–no earlier time series data is available for

similar, although some statistical significance is lost.

[22]The diffs-in-diffs identification strategy is essentially a "discretization" of the regression discontinuity design, where the latter can be understood as the limiting case of the former when the interval width converges to zero. The fuzzy RD strategy is thus "better" than the diffs-in-diffs because the "Control" group, in this case borrowers below the cutoff, is identical to the "Treatment" group, borrowers above the cutoff.

the bank debt data. Figure 5 shows time series plots of the average balance at The Store , days late, internal credit score, and fraction of borrowers that are late by more than 90 days displayed separately for Treated and Control groups for six months before selection into the renegotiation offer sample. Visual inspection reveals that the graph lines of both groups are parallel before selection into the renegotiation offer sample for all four variables. This suggests that the Control group provides a valid counterfactual for the Treated group, notwithstanding the ex ante cross sectional differences.

2.2 Specification

The effect of the renegotiation offer on bank debt can be tested using a specification with fixed effects and time trends for each month where data is available,

$$Debt_{it} = \alpha_i + \xi_t + \omega post_t + \beta treated_i \times post_t + \epsilon_{i,t}, \tag{4}$$

where $treated_i$ equals 1 if borrower i is in the Treated group and $post_t$ equals 1 if t is month December 2010, March 2011, or April 2011. The coefficient of interest is β, the interaction of $post_t$ and $treated_i$, which measures the differential change in debt for Treated borrowers relative to Control borrowers after the renegotiation campaign. α_i and ξ_t are borrower and month fixed effects. Individual fixed effects are included to increase the precision of the estimates, as they are irrelevant if the identification assumption holds.

The same specification can be used to test the effect of the renegotiation on default, where default is measured both at the extensive margin with a dummy variable, and as the fraction of the balance that is in default.

3 Results

3.1 The effect of an improved reputation on the access to external credit

Columns (1) through (4) of Table 2 reports the regression results of the differences-in-differences specification (4) when the outcomes are total debt in good standing, consumer debt, commercial debt (i.e., debt used for commercial purposes), and mortgage debt. The effect on total bank debt in good standing (column 1) is positive and significant. Thus, bank debt in good standing is roughly 210,000 pesos (USD500) or 6% higher for the Treated group relative to the Control group after the renegotiation campaign. Note that the average trend after the campaign is negative for Control borrowers (a decrease of 81,000 pesos as per the coefficient on *post*), whereas Treated borrowers on average reverse the trend and increase their average debt in good standing (the sum of *post* and $post \times treated$ is 129,000 pesos).

Columns (2) through (4) of Table 2 show that the increase in debt is driven by mortgages: on average, Treated group borrowers increase their mortgage debt by approximately 206,000 pesos more than the Control group borrowers (approximately a 10% increase). Note that consumer debt (column 2) is insignificantly higher for the Treated group. This suggests that, on average, borrowers are not taking out a new consumer loan to pay back the renegotiated balance at The Store.

These results are consistent with the notion that borrowers value a good credit reputation because it allows them to access credit markets in the future. Note that borrowers who renegotiate cannot consumer on credit at The Store until their full renegotiated balance is paid off. This trade off is of first-order importance for a consumer planning to buy a house, who gives up short-term consumption at The Store in return for access to mortgage debt. Borrowers may be able to substitute consumption at The Store through other department stores, but this margin cannot be tested in this study. Moreover, as in the US mortgage market, credit availability is highly sensitive to the borrower's credit reputation.

The reported increase in bank debt is quantitatively higher than the Store campaign cutoff. But the time-zero value of a good credit reputation represents an option to obtain better terms of repayment. Thus, observed differences in equilibrium quantities do not necessarily correspond to the value that borrowers obtain from a good credit reputation. For example, a good credit reputation may allow borrowers to obtain a lower interest rate in their mortgage. Therefore, their willingness to pay would be (at least) the amount of the mortgage multiplied by the reduction in interest rates (which are not available in the data). Further, the signal of creditworthiness also depends on the repayment status of all the debts outstanding that individuals may have. I do not observe total debt with all lenders, but the assumption is that both Control and Treated groups have similar levels and equivalent trends of total debt outstanding before the renegotiation.

3.2 Externality effect on other lenders

Borrowers who renegotiate are indistinguishable to users of the credit bureau from other individuals in good standing. Thus, in the presence of renegotiation banks may end up lending to a pool of borrowers that is less creditworthy. I study this claim by looking at the default rates of borrowers in the Control and Treated groups. Column (5) of Table 2 reports the regression results when the outcome variable is a dummy that equals 1 if the borrower is in default, conditional on having bank debt. The interaction coefficient ($post \times treated$) is the average increase in default rates for the Treated group relative to before the campaign starts and relative to the Control group. The point estimate is positive and significant at 2.9%. Thus, individuals in the Treated group are ex post significantly worse payers than those in the Control group. This relative increase in the default rate is large when compared to the baseline default rate of 33% (as per Table 1).

Column (6) of Table 2 shows that the increase in defaults is not driven mechanically by the higher level of debt reported in column (1). Indeed, bank debt in default as a fraction of outstanding balance, a measure of default that controls for the level of debt, is 2.3%

higher for Treated borrowers relative to Control borrowers after the renegotiation campaign. Therefore, more Treated borrowers default, and the amount in default is a larger fraction of their balances.[23]

This result suggests that renegotiation imposes a negative informational externality on banks by pooling together less creditworthy borrowers with borrowers in good standing. One possible interpretation is that both The Store and the borrower understand this effect and collude to extract rents from banks. This externality effect has not been documented by the theoretical literature, which has assumed that renegotiations are an ex post efficient outcome of a mutually beneficial bilateral agreement. Rather, this effect is similar in spirit to Aghion and Bolton (1987), who show how optimal bilateral contracts may be socially inefficient. Further, this result illustrates the fundamental disconnect that exists between a borrower's true willingness to repay a loan and the same borrower's credit reputation, and also the potential problems that arise in the design of information-sharing mechanisms like credit bureaus if agents may manipulate the information they report.

4 Conclusion

This paper shows that credit reputation is valuable to borrowers because it affects future access to formal credit. More importantly, if agents have the ability to trade and manipulate their credit reputation then credit-information sharing mechanisms may be welfare decreasing. In particular,debt contract renegotiations may have negative welfare effects by imposing an information externality on other lenders. A reasonable conjecture is that this provides incentives for the other lenders to ration their lending (e.g., Stiglitz and Weiss (1981)). However, this is speculative, as a welfare analysis of the renegotiation mechanism is outside the scope of this paper.

The results are relevant for the design and implementation of credit bureaus as information-sharing mechanisms should consider the potential for strategic manipulation. These sharing

[23]The results also hold conditioning on the subsample of borrowers who have a mortgage.

mechanisms are a prevalent feature of credit markets worldwide (Miller (2000); Jappelli and Pagano (2002); Jappelli and Pagano (2006); Djankov, McLiesh, and Shleifer (2007)). A better understanding of the contracting implications of this manipulation could potentially help in the design of more robust credit information systems.

Part III

The Effect of Trade Credit on Product Markets: Evidence from the Suppliers of a Large Supermarket

1 Introduction

We examine the causal effect of differences in the terms of trade credit on the bilateral contracting relationships of a large client – a supermarket– and its suppliers. It is customary in the supermarket industry that suppliers extend trade credit to their clients. For example, Walmart uses four times more trade credit than short term external financing.[24] Better trade credit terms are more favorable to the large client and may positively affect the demand for the supplier's products. But these terms are costly to offer, as the supplier must allocate its scarce resources between trade credit and other investments. Alternatively, firms may use prices as a margin to offset the costs of providing trade credit.

Yet there is little empirical evidence to analyze these trade offs. The main challenge associated with providing this evidence is that firms that extend trade credit terms that are more advantageous to their clients are preciesly those that may benefit the most by doing so. Hence, the correlation between trade credit terms and contracting outcomes may render biased estimates of the effect of the former on the latter. An ideal experimental setting to address this concern would consist of randomizing the set of trade credit contract terms among identical suppliers of the same large client. Any differences in the ex-post contracting outcomes (prices, quantities) can then be causally attributed to the different trade credit

[24]Based on internal calculations from publicly available information as of January 2013.

terms.

This paper exploits a natural experiment that resembles an ideal experimental setting to identify how the terms of trade credit affect contracting outcomes between suppliers and their large client. In January 2007, one of the two large supermarket chains that operate in Chile (the "Supermarket") agreed with the Chilean government to reduce the number of days in which it paid its small suppliers, from approximately 90 to no more than 30 days (the "Agreement"). Small suppliers were defined in the Agreement as those that during the last 12 months had, (i) sold less than an arbitrary fixed amount to the Supermarket (60,000 UF, roughly $2.4 million) and (ii) sold less than another arbitrary fixed amount in total to all their clients (100,000 UF, roughly $4.0 million).[25] The Agreement was the result of the government's concern that the large difference in the size of the supermarket and its suppliers would negatively affect the terms at which the latter sold their products.

We obtain from the Supermarket a propietary dataset that contains all the purchases it made from Chilean suppliers, at the product-month level, between January 2006 and August 2011. We denote suppliers that sold less than 60,000 UF during 2006 as the Treated group and suppliers that sold more than 60,000 UF as the Control group. We do not directly observe the supplier's total revenues.[26] Thus, we focus on eligibility for shorter days payable as determined by the first criteria only (60,000 UF cutoff). Because of this, Control suppliers are never small and hence their available payment terms are not affected. Treated suppliers, on the other hand, are small as long as their total revenues during 2006 are less than 100,000 UF. We exploit the variation in the probability of receiving shorter payable days around the 60,000 UF cutoff as a source of identification: the probability is positive for Treated suppliers and zero for Control suppliers.

By looking at a sufficiently narrow window around the 60,000 UF cutoff, we may assume that Treated and Control suppliers would have evolved in a similar fashion absent the

[25]The UF, or Unidad de Fomento is Chile's inflation-indexed unit of account. Its value is published on a daily basis by the Central Bank based on the previous month's realized inflation.

[26]We only observe transactions with the Supermarket.

Agreement. Using this assumption, we estimate the causal effect of shorter days payable on the ex post contracting and market outcomes using a differences in differences strategy (diffs-in-diffs). This strategy compares the change in outcomes of Treated firms before and after the agreement with the same change for Control firms.

We first show that, on average, Treated firms sell their products at significantly lower prices than Control firms after the reform, relative to prices before the reform. Our different specifications suggest a 5%-10% price effect resulting from shortening the payables by at most 60 days. These magnitudes imply a range of yearly interest rates of 31% to 55%, significantly higher than contemporary bank rates of 7% to 11%.

The supplier firms we study are substantially smaller than the Supermarket itself. Therefore we do not believe that financing constraints on the part of the buyer are the key drivers of our results.[27] These results are, however, consistent with other models of trade credit such as inventory management and asymmetric bargaining power. Indeed, we find that the price effect is driven almost entirely by non-perishable products where the terms of credit may better align with the product inventory cycle. Further, we document that the price declines are much stronger for firms and products with lower market share with the Supermarket and for firms selling more different product types conditional on size.

We study other contracting margins that may be affected by the change in days payable. We document that, for each product, the probability of making a sale, units sold on each sale, and a normalized measure of revenues are lower for Treated firms relative to Control firms during the first year after the Agreement. However, we find that normalized product revenues become insignificantly different for both sets of firm after the first year. Thus, even though the Agreement may initially hurt Treated suppliers, the long run effect of the reduction in days payable is ambiguous.

Many empirical papers have studied theories for the provision of trade credit in the cross

[27]Klapper, Laeven, and Rajan (2011) and Murfin and Njoroge (2012) also study supplier-buyer relationships where the suppliers tend to be substantially smaller than the buyers. They rule out financing constraints as the main driver of the provision of trade credit, as well.

section (see Ng, Smith, and Smith (2002), Giannetti, Burkart, and Ellingsen (2011), Klapper, Laeven, and Rajan (2011), among others). In this study, we contribute to this literature by providing causal estimates of the effects of trade credit on market outcomes. Our paper is most related to Barrot (2013)[28], which also uses careful identification to understand the interaction between trade credits, profits, and competition. One key difference is that while Barrot (2013) studies all suppliers and purchasers of one specific product (trucking services) at the firm level, we focus on one large buyer but have information on thousands of distinct products and prices.

We begin with an overview of the existing theories of trade credit in Section 2. We then describe the data and the empirical setting in Section 3 and present our identification strategy in Section 4. Sections 5 presents our main results. Finally, Section 6 concludes.

2 Theories of Trade Credit

The finance and economics literature on the provision of trade credit is quite vast and offers several theories to explain trade credit's widespread use. We consider five channels through which trade credit might operate: financing constraints, risk sharing, inventory costs, asymmetric bargaining power, and incentives for quality.

The most standard theory of trade credit is as an alternative or substitute to formal, interest-bearing credit or debt. Under this theory, firms with deeper acces to capital or to the capital markets may be efficient providers of credit to otherwise constrained firms (see for example Petersen and Rajan (1997), Smith (1987) and Fisman and Love (2003)). These credit constraints may arise from information asymmetries, or asymmetric collateral values. Trade credit counterparties may have better monitoring technologies and may be able to provide stronger repayment incentives due to the repeated nature of supplier-buyer interactions relative to traditional financial intermediaries or debt holders. It also may be

[28]The author finds that trade credit may provide barriers to entry for financially constrained firms. He shows that a French law reducing trade credit prevalence in the trucking industry resulted in more entry into the sector. Financially stronger firms experienced the most negative consequences.

the case that the buyer's collateral is more valuable to the supplier than to a bank.

A second, related theory, described by Cunat (2007) suggests that firms may use trade credit to help share risk. For example, if the two contracting parties make relationship-specific investments, then the financially stronger party will have incentives to protect its investment and extend credit to help the weaker firm weather shocks.

While financing constraints and risk sharing are potentially very important in shaping a firm's decision to enter into trade credit agreements, it has also been documented that small firms often serve as creditors, with large, unconstrained firms (such as Walmart or Target) acting as borrowers (see Wilson and Summers (2003) and Murfin and Njoroge (2012)). In our setting, we also observe this pattern. As a result we focus on motivations for trade credit aside from financing constraints or risk sharing.

Third, purchasers may demand goods on credit to offset inventory costs. Daripa and Nilsen (2010) construct a model in which trade credit delivers a subsidy to buyers so that they are willing to hold costly inventory in the supplier's goods. The authors argue that trade credit may be a more efficient means of delivering the subsidy than an adjustment in price. This theory suggests that firms may need to lower their prices as an alternate means of delivering the subsidy when trade credit is no longer available. We should also expect this mechanism to be strongest for non-perishable goods that may be held in inventory for more than 30 days.

Fourth, the bargaining and market power of the large retailer could provide motives for small suppliers to extend trade credit. If purchasers have asymmetric bargaining power relative to suppliers, then suppliers may choose to either cut prices or to offer other enticements such as trade credit. Extension of trade credit may also signal a willingness of firms to invest in specific supplier-purchaser relationships.[29]

Finally, trade credit may help to mitigate information asymmetries in product quality.

[29]Fabbri and Klapper (2008) suggests that in China, suppliers with lower market power do in fact extend more trade credit. Fisman and Raturi (2004) find that suppliers of products in more competitive markets offer more trade credit than suppliers with strong market power.

Quality may not be verifiable at the time the supplier delivers the products, but may be learned over time. Trade credit provides buyers extended time to learn about quality before being required to pay the supplier.[30] Under this theory, trade credit restrictions may result in buyers switching to firms unaffected by the Agreement, or it may result in lower-quality products being supplied by affected firms. If product quality decreases, then we should again expect to see a decreases in price.[31]

3 Empirical Setting

3.1 The supermarket industry in Chile and the Agreement

The Chilean supermarket industry is composed of two large firms (one of which corresponds to the Supermarket) and a host of smaller, geographically concentrated firms. The two large Chilean supermarket chains have some characteristics in common with US supermarkets and discount retailers, including store format (large superstores), means of payments (own credit cards), and relationship with suppliers.[32] Through a series of aggresive acquisitions and organic growth, these two large chains acounted for 63% of total supermarket revenues during 2006.[33] As a comparison, the third largest firm accounted for less than 3% of total sales. In contrast, the US supermarket sector exhibits a relatively lower degree of concentration, with the 4 (8) largest firms representing 32% (46%) of total industry revenues.[34]

In August 2006 the Chilean government's pro-competition agency forced the two large supermarket chains to modify the terms of their relationships with small suppliers. In its report, the agency expressed its concern over the industry's concentration as a source of

[30] Giannetti, Burkart, and Ellingsen (2011) find suggestive evidence that firms producing more differentiated goods offer more trade credit to buyers.

[31] However, we should note that while trade credit may be restricted by the Agreement in the Chilean context, firms may still have the ability to write long-term contracts, which could also allieviate quality concerns. It is also not so clear that goods sold by supermarkets that typically engage in long-term relationships with their suppliers are plagued by unobservable quality.

[32] Indeed, one of these two large Chilean supermarket chains is currently a subsidiary of Walmart.

[33] Information taken from Chilean pro-competition agency website, www.fne.cl.

[34] Figure taken from 2007 Economic Census, NAICS code 44511 in factfinder2.census.gov

monopsonistic power.[35] The government agency's strategy consisted on denying both supermarkets its regulatory approval to their latest acquisitions until the modifications were put in place. This prompted both supermarkets to agree to these modifications in December 2006 and July 2008.

Before the Agreement, the Supermarket frequently used terms of 90 days payable, especially with its smaller suppliers. However, larger suppliers are typically able to negotiate shorter days payable. The Agreement was mainly motivated by the Chilean government's concern over the large differences in size between the Supermarket and its smaller suppliers. Indeed, there is no evidence that the Agreement might have been motivated by secular trends in the retail sector that would have differentially affected small and large firms. In particular, the Agreement's precise timing and chosen cutoff of 60,000 UF would have been nearly impossible to anticipate for firms.

The Agreement established new terms of payments from the Supermarket to its small suppliers. For the purpose of the Agreement, small suppliers were defined by the following criteria:

1. Suppliers who in the last 12 months had sold no more than 60,000 UF to the Supermarket,

2. Suppliers who in the last 12 months had sold no more than 100,000 UF to all their clients, as accredited by their sales tax information.

3. For new suppliers, maximum monthly revenues no higher than 5,000 UF.

Firms that met the three selection criteria were eligible to be paid by the Supermarket in 30 days. Prior to the Agreement, the Supermarket had a flat days payable policy of 90 days with all its suppliers (except for a small group of very large firms). Hence, the Agreement reduced the account payable days for eligible firms by 60 days at most.

[35] See "Requerimiento contra Cencosud y D&S", www.fne.cl.

Since the Agreement was put in place, the Chilean government has actively monitored its implementation. Publicly available reports suggest that the Supermarket has indeed complied with the shorter payment period for small firms.[36]

3.2 Data and Summary Statistics

We obtain from the Supermarket a proprietary dataset that summarizes all the transactions with its suppliers between January 2006 and August 2011 and contains observations at the supplier-product-month level. Several of the mechanisms outlined in Section 2 suggest that prices may provide a margin for adjustment in response to the restriction of trade credit terms. The rich data set allows us to analyze the price effects of trade credit, something that not many other studies have been able to do. We also examine quantities procured, the likelihood of supplying to the Supermarket in any given year, and revenues from business with the Supermarket. The product-level information also provides scope to compare the heterogeneous effects of restricting trade credit terms over different types of products or supplier-Supermarket relationships.

Panel A in Table 1 shows some summary statistics for the entire datatset at the finest level provided. Not surprisingly, there is tremendous heterogeneity in both prices and quantities sold by the supermarket. Part of this heterogeneity is a result of the large number of different products sold. Panel B details the 16 overall product departments. The Supermarket uses four categories to describe their inventory: department, line, section, and product. The number of products ranges from 10 in the restaurant department to 387 in hardlines. Hardlines includes electronics, sporting goods, and toys, for example, soccer balls. Columns 1 and 2 indicate the importance of each of these major departments by sales both in units and in value. General Food and Consumer products are the largest categories and together represent over half of the value of all goods sold to the Supermarket.

Figure 6 plots the distribution of revenues firms received from the Supermarket and

[36] See, for example, www.fne.gob.cl/wp-content/uploads/2012/03/arch_054_2009.pdf and www.fne.gob.cl/wp-content/uploads/2011/11/arch_022_2011.pdf

highlights the large degree of heterogeneity in the size of the Supermarket's relationship with various suppliers. The 60,000 UF cutoff, which is relevant for the Agreement, falls around the 91st percentile of the firm sales distribution and is denoted by the dashed line. For most of our analysis, we restrict the sample to only those firms close to this cutoff (see explanation below). This restriction (40,000 UF to 90,000 UF) is denoted by the solid bars, and corresponds to the firms between the 88th and 94th percentiles of the sales distribution to the Supermarket. Thus, by making this restriction we discard observations from 94% of the suppliers and avoid making comparisons between firms with vastly different characteristics.

4 Empirical Strategy

Before outlining our empirical strategy in depth, it is useful to observe the overall trends in prices, our key outcome, for treatment firms (40,000 - 60,000 UFs) compared to control firms (60,000 - 90,000 UFs) over the horizon of the data. Figure 7 compares the log prices of each group, normalizing the level of log prices to be 1 in the last quarter of 2006, our last observation before the law change. The picture is quite striking. While prices evolved similarly in the preperiod, prices begin to diverge after the Agreement was instituted in the beginning of 2007. This figure is the basis for our identification strategy.

4.1 Identification

We exploit the cross-sectional variation induced by the 60,000 UF cutoff to identify the effect of the Agreement's change in days payable on the procurement outcomes between the Supermarket and its suppliers. First, we limit our sample to those firms that made sales to the Supermarket during 2006. To comply with the restriction for the Agreement's criteria for new firms, we further limit our sample to firms whose sales to the Supermarket during any month in 2006 were no larger than 5,000 UF. Within this subsample, the Agreement only reduced days payable to suppliers that during 2006 sold 60,000 UF or less to the supermarket

and that simultaneously had total revenues of no more than 100,000 UF during 2006. Since we do not observe total 2006 revenues, we focus on the first criteria and denote a firm whose 2006 sales to the Supermarket were less than 60,000 UF as Treated.

The effect of the reduction in days payable will be confounded with contemporaneous shocks in the time series. We use firms whose 2006 sales to the Supermarket were above 60,000 UF, which were unaffected by the Agreement, to construct a counterfactual for Treated firms. We denote these firms as Control. By restricting our sample to firms whose 2006 sales to the Supermarket are within 40,000 UF and 90,000 UF, we may assume that Treated and Control firms would have evolved in a similar manner absent the reduction in days payable. We conduct our main analysis in this restricted sample.

Note that, by construction, Control firms do not have access to the Agreement's shorter days payable, whereas Treated firms may be erroneously classified if their unobserved total revenues were above 100,000 UF during 2006. To partially address this concern, we obtain a broad classification of each supplier's total revenues (i.e., too all clients) during 2007 and 2008. We verify that no firm that sold less than 100,000 UF in 2007 sold more than 600,000 UF in 2008. Hence, even though 2007 revenues may be an endogenous outcome of the Agreement, we further restrict our sample to firms whose total revenues during 2007 were below 600,000 UF. This also helps make Treated and Control firms more comparable. Because we do not fully observe total revenues during 2006, our results only provide a lower bound of the effect of shorter days payable on contracting outcomes.

Table 2 shows descriptive statistics for our sample during 2006, before the Agreement. The sample includes 73 firms, 30 Treated and 43 Control. Panel A shows monthly statistics at the firm level. On average, firms in our sample had revenues of approximately US$192,000 coming from the sale of 9.4 products. These firms have roughly 1.3 departments, 1.7 lines, and 3.9 sections. The table also shows the same statistics for Treated and Control firms. The statistics confirm that, by construction, Control firms are larger than Treated firms. In particular, Control firms have higher monthly revenues and have roughly three more products

than Treated firms.

Table 2 (Panel B) provides sample statistics at the firm-product level during 2006. There are 877 firm-product pairs, of which 548 correspond to Treated firms and 329 to Control firms. The table shows that Treated firms sell less (in $ and units) of each of their products than Control firms. Interestingly, the average price of products sold by both groups is similar (and, based on a simple hypothesis test, not statistically different), even though Supermarket margins are 3% lower for Control firms. Finally, Control firms exhibit a higher probability of having a sale on any given month during the year.

Table 2 (Panel C) reports the concentration of product-firm transactions each month (by number) at the department level, the broadest level used by the Supermarket, for all months in the sample. This Table allows us to refine our sample selection. Because no Treated (Control) firms sell Health (Pets) products, we exclude these departments from our analysis. We also exclude Restaurant products because all pre-period revenues are made by Control firms, and Impulsive Shopping because Control firms only sold 4 products during the entire sample period. These four excluded departments represent less than 4.5% (3.0%) of all product-firm-month transactions by number (amount). The remaining 12 industries are fairly diversified in terms of the distribution between Treated and Control firms, although, most notably, Control firms sell a higher fraction of General Food products

Notwithstanding these differences in the cross-section, the identification assumption is the the trends of Treated and Control firms would have evolved in the same manner absent the reduction in days payable. Figure 8 provides evidence that supports this assumption. In particular, average log revenues, log units, number of products, and number of departments for Treated and Control firms have similar pre-Agreement trends at the firm level, by quarter.

Under the assumption of similar trends, we identify the effects of the reduction of days payable on contracting outcomes through a differences-in-differences (diffs-in-diffs) strategy. Intuitively, we compare how the outcome changes for Treated firms before and after the Agreement is in place (January 2007) with how the outcome changes for Control firms for

the same period. We implement this comparison with the following baseline specification,

$$outcome_{ijt} = FE(i,j,t) + \omega_0 post_t + \omega_1 treated_t + \beta post_t \times treated_i + \epsilon_{ijt}, \tag{5}$$

for firm i, product j, and month t, where $post_t$ is a dummy that equals 1 if the monthly observation occurs on or after January 2007, $treated_i$ equals 1 if firm i is in the Treated group, and $FE\,(i,j,t)$ is a set of fixed effects at the firm, product, and time levels (and their interactions). The coefficient of interest is β, the interaction of $post_t$ and $treated_i$. To assess how the effects of the policy evolve over time, we modify specification (5) to

$$\begin{aligned} outcome_{ijt} = FE(i,j,t) + \sum_{year=2007}^{2009} \omega_{year} 1\,(year_t) + \omega_1 treated_i + \ldots \\ \ldots + \sum_{year=2007}^{2009} \beta_{year} 1\,(year_t) \times treated_i + \epsilon_{ijt}, \end{aligned} \tag{6}$$

where the coefficients of interest are ω_{year}, the interaction of the yearly dummies and the $treated_i$ variable. Finally, we restrict the sample and focus on the outcomes during the first three years after the agreement (2007, 2008 and 2009), for both specifications.

4.2 Mechanisms and Heterogeneous Effects

We also use the differences-in-differences framework to attempt to disentangle the mechanisms driving the impacts of trade credit on contract structure. The theories of trade credit suggest that both reliance on inventories (longer than 30 days) and relative market power might impact the need or the value of trade credit. For example, if inventory costs are the main motivation for trade credit terms, then we should expect to see a larger price adjustment for those products that tend to be held by the Supermarket for more than 30 days (1 month). To test this hypothesis, we run a heterogeneous effects regression allowing for different treatment effects based on product departments, noting that perishable goods cannot generally be held by the supermarket for more than 30 days.

Our heterogeneous effects specification by firm or product characteristic H_{ijt} (where, for example, H_{ijt} is a dummy for perishable products) is

$$outcome_{ijt} = FE\left(i,j,t\right) \times g\left(H_{ijt}\right)) + \omega_0 post_t + \omega_1 treated_i + \omega_2 H_{ijt} + \omega_3 post \times H_{ijt} + \ldots \tag{7}$$

$$\ldots + \omega_4 treated_i \times H_{ijt} + \beta post_t \times treated_i + \gamma post_t \times treated_i \times H_{ijt} + \epsilon_{ijt}.$$

The term $FE\left(i,j,t\right) \times g\left(H_{ijt}\right))$ allows for differential time trends by function $g\left(\right)$ of the variable H_{ijt}. In many cases, H_{ijt} is discrete, and we add the full set of $t \times H_{ijt}$ fixed effects to the specification. In these regressions the coefficient of interest is γ.

In addition to indicators for perishable products, we also use characteristics of the relationships between each supplier firm and the Supermarket that may proxy for relative bargaining power. These include market share of each supplier vis-a-vis the Supermarket and the number of products sold by each supplier to the supermarket.

5 Results

5.1 The effect of the reduction of days payable on prices

Table 3 reports the results of the OLS differences in differences regression (5) when the outcome is $log\left(prices\right)$, the natural logarithm of the price of each product sold to the Supermarket every month. On average, after the Agreement is implemented on January 2007, Treated firms sell their products at significantly lower prices than Control firms relative to their 2006 prices. The coefficient varies between a minimum of -9.9% when including firm, month and product fixed effects, to a maximum of -5.5% when including month, product-firm and department-year fixed effect. This last regression accounts for the fact that Treated and Control firms sell products in different departments, which may exhibit differential time-trends that are unrelated to the Agreement.

It is the case that $\log(price)$ is only defined in our data in those months when there is a sale in a given product. The likelihood of making a sale is an interesting outcome in itself and is missing from the $log\,(prices)$ regressions. However, if anything, this omission will bias our results toward zero. In general, suppliers will not make a sale if the prices demanded by the Supermarket are lower than what they are willing to accept. Thus, we are potentially missing the lowest latent prices for the treatment group in this set of specifications. This will hold in general unless the distribution of latent prices of those products that were not sold was significantly higher than those that did exhibit sales. We show results for the likelihood of making a sale in Section (5.2) below.

These results strongly suggest that supermarkets value trade credit. When suppliers lose the ability to extend it, as under the Agreement, they must adjust through other margins. We document that contracts are adjusted through prices. The magnitudes of the price changes appear on the surface to be much larger than a reasonable 60-day interest rate for external financing. For example, the 5% to 10% price reductions are equivalent to a range of monthly interest rates of 2.6% to 5.4% (annualized 31.2% to 64.8%). This compares to the Chilean banking sector reported yearly rates for the same period of 7% to 11%.[37]

We provide two different types of robustness checks. First, we attempt to correct for mis-classification of control firms in the treatment group. While we are able to precisely identify whether firms meet the first criterion for the Agreement (sales to the Supermarket $<$ 60,000 UF), we are not able to separate firms based on the second criterion (total sales $<$ 100,000 UF). We received data for a subsample of firms detailing total firm sales, but for 2007, not for 2006. While 2007 size is endogenous to prior trade credit terms, we check that our results do not vary dramatically when we restrict treatment firms to also have sales $<$ 100,000 UF in 2007. Results are shown in column (6). We lose much of the sample size and have reduced power, but the price effect of the Agreement is of a similar magnitude and is still significant at the 10% level.

[37]Figure taken from "Tasa de Interés Corriente y Máxima Convencional" in www.sbif.cl, for "Operaciones Reajustables" (i.e., UF-denominated) for less than a year, as of January 1, 2007.

Our second robustness check runs a placebo regression, comparing our Control firms with even larger firms before and after the Agreement. The results are in column (7). We find no evidence of differential price effects as a result of our placebo treatment.

5.2 The effect of the reduction of days payable on other outcomes

We study the effects that the Agreement had on other outcomes. To this extent, we analyze how these effects evolved over time during 2007, 2008, and 2009 using regression (6) for a set of different outcomes. Table 4 reports the results. Column (1) replicates the results from Table 4, by showing that prices are lower each year after the Agreement and each year by a higher magnitude. One explanation for the increasing magnitudes over time could be that some suppliers sign longer term contracts with the Supermarket. Therefore, the effects of the days payable change become stronger over time as more suppliers modify their contracts.

Column (2) studies whether the Agreement modifies the supplier's revenues through the extensive margin. The outcome is a dummy variable that equals one if the firm sold a particular product to the firm on that particular month (note that the panel is fully time-balanced). During the first year after the Agreement, Treated firms reduce the probability that they make a sale on each of their products by approximately 4.4% (significant at the 10%). This finding suggests that the Supermarket values the longer days payable, and, therefore, may choose to buy the same product from a supplier who can provide the longer terms. Eventually, the adjustment in prices increases the relative likelihood of making a sale. Indeed, the differential probability is zero in 2008, while it is positive but insignificant in 2009.[38] Another interpretation of columns (1) and (2) may be that the prices demanded by the Supermarket fell by even more than 3.5% in 2007, but some suppliers chose instead not to make a sale. In subsequent years, suppliers decide to sell their products, but at even lower prices.

Column (3) reports the treatment effects of the reduction in trade credit on $\log(units)$

[38]The 2009 coefficient is positive and strongly significant in an alternative specification of this same regression without department-year fixed effects.

sold to the Supermarket at the product-firm level. These results imply that conditional on making a sale, treatment firms sold relatively fewer units to the Supermarket after the Agreement. Column (4) describes a similar outcome variable, normalized units, which equals the number of units sold in each post-Agreement period divided by 2006 units sold. Thus, this specification conditions on the set of products sold by each firm before the Agreement and contains observations even for those months where no sale was made. Under this specification, units sold after the Agreement by the treatment firms dropped by 27%, reflecting in part the drop in the likelihood of making any sale. However, as the probability of making a sale increases in 2008 and 2009, so does the normalized units sold.

Columns (5) and (6) show revenues at the supplier-product level in two different ways. In column (6), $\log(amount)$ refers to the log of revenues in a specific product for a given firm each month. Again, taking the log results in any periods with no sales to be dropped from the regression. These results look much like units, with conditional revenues falling substantially after the Agreement. However, the picture in column (7) looks a bit different. Here, revenues are normalized by their 2006 levels and show a large initial decrease. However, the magnitude of this decrease is lower over time. While the standard errors are large, the coefficient for treated firms in 2009 implies that the mean firm was able to regain its pre-Agreement level of profits three years later.

These results suggest that Treated suppliers initially face a reduced likelihood of making a sale, and, conditional on making a sale, sell less units right after the Agreement. Thus, they are forced to reduce prices in future years to remain competitive. Indeed, Treated suppliers are able to return to their pre-Agreement normalized revenues. The results are ambiguous in terms of welfare because we do not observe the evolution of the supplier's cost structure over time. Hence we are unable to establish if, for example, these changes make firms more efficient, or improve the quality of the products sold.

5.3 Heterogeneity: Testing theories of trade credit

Table (5) presents heterogeneous treatment effects by product characteristics. This exercise is helpful in trying to disentangle the mechanisms behind the large price decreases after the Agreement.

We first compare the treatment effects by whether the product is perishable (an indicator variable). Perishable goods are generally held in inventory for fewer than 30 days. Under the inventory theory of trade credit, changing the terms from 90 to 30 days should not be marginal for perishable goods. We find support for this theory in column (1). The negative price impact appears to be coming entirely from non-perishable goods, which may sit in inventory for longer than 30 days.

We next look for potential indicators of bargaining power strength. We rank firms within each product by the share of total Supermarket purchases of that product in 2006. Firms above the median are denoted to have a high fraction of sales for that product. We find that practically all of the treatment effects are coming from product-firm combinations where the supplier firm is a minor supplier of that good to the Supermarket. Firms with high market shares in a given product only experience a 1.4% decreases in prices (not significant) compared to an 8.9% decline for firms with low market share, recorded in column (2).

Finally, we also separately estimate the treatment effects by the diversity of each supplier's product offerings. These results are displayed in column (3). Firms with an above-median number of products sold to the Supermarket in 2006 experience the bulk of the price declines, while firms that are more specialized do not experience a decline. A priori more diversified firms would appear to be in a stronger bargaining position with respect to the Supermarket. However, holding the size of the Supermarket-supplier relationship constant, more diversified firms will have a smaller market share on each product.[39] Therefore, more diversified firms would be in a weaker bargaining position in each product market. Our result

[39]We find a strong negative correlation between firm-level average product market share and firm diversification for 2006.

is consistent with this interpretation.

6 Discussion

We document a strong substitution between the availability of trade credit and the prices at which goods are supplied to a large retailer. As a lower bound, reducing the days payable of procurement contracts from 90 to 30 days causes a decrease in product prices of 5%-10%. At the midpoint of this range, the implied monthly interest rate earned on accounts payable is 3.68% (or ~54% annualized). In comparison, the standard yearly market interest rate on UF-denominated bank loans is 7% to 11%. Further the two large Chilean supermarkets pay approximately 5% per year on their publicly traded debt. Thus, it appears that the large supplier is willing to pay a relatively large premium for its trade credit.

Under a financial constraints theory of trade credit, the Supermarket should not be willing to pay such a high premium for credit above its cost of external financing. Some of the alternate theories of trade credit appear to better fit our causal estimates. Our results are consistent with theories in which trade credit is extended as a means to offset inventory costs, and theories of asymmetric bargaining power.

Our results are ambiguous in terms of welfare. The inability to extend longer trade credit terms initially reduces the small suppliers' revenues. But over time these suppliers are able to increase their probability of making a sale and thus their product revenues return to their initial level. Further, it is unclear whether the Supermarket's final consumers are better off when less trade credit is extended to the Supermarket. This effect could arise through product price reductions, or through other investments in product quality that are outside the scope of this study.

Part IV

Tables and figures

1 Tables

1.1 Chapter 1

Table 1: Summary statistics

	Full sample		Renegotiation offer sample	
	Mean	St. Dev.	Mean	St. Dev.
Balance ('000 pesos)	397.07	720.94	587.87	995.53
Age (years)	44.72	13.43	41.78	13.38
Tenure (months)	96.35	73.65	78.97	65.23
Female	0.571	0.495	0.532	0.499
Monthly income ('000 pesos)	341.68	475.76	285.15	462.67
Credit Score	578.14	132.80	474.95	56.21
Late $>$ 90 days	0.062	0.240	0.450	0.497
Fraction late	0.104	0.261	0.474	0.350

This table reports means and standard deviations (SD) as of January 2010: Balance is the debt balance at The Store in thousands of pesos (approximately 500 pesos per dollar), Age is the age in years, Tenure is the number of months since the client first bought on credit at The Store, Female is the fraction of female borrowers, Monthly income is the borrower's reported monthly income in thousands of pesos at the time of application for credit from The Store, Credit Score is The Store's internal credit score, "Late $>$ 90 days" represents the fraction of borrowers that are late by more than 90 days, Fraction late represents the fraction of the borrower's balance that is late.

Table 2: Effect of phone campaign on probability of renegotiation

month n	(1)	(3)	(6)	(12)	(20)
π	0.0740***	0.1129***	0.1204***	0.1182***	0.1144***
	(0.0052)	(0.0074)	(0.0080)	(0.0081)	(0.0085)
ω	0.0069***	0.0181***	0.0248***	0.0380***	0.0501***
	(0.0013)	(0.0023)	(0.0025)	(0.0027)	(0.0032)
R^2	0.032	0.062	0.076	0.075	0.070

This table shows the effect of the phone campaign on the fraction of renegotiations. The table reports the constant $\omega(n)$ and the coefficient $\pi(n)$ of the regression:

$$renegotiation(n) = \omega(n) + \pi(n)\, treated + f(amount - 50000) + \epsilon,$$

where the outcome $renegotiation(n)$ is a dummy variable that equals 1 if borrower renegotiates between one and n months after January 2010, for months $n \in \{1, 3, 6, 12, 20\}$, on *treated*, a variable that equals one if the borrower owes more than 50,000 pesos as of Jan 2010 and zero if she owes less, and $f(amount - 50000)$, a 4th degree polynomial of the outstanding balance at The Store as of January 2010. Regressions are ran on the sample of borrowers in the renegotiation offer sample who as of January 2010 owe between 10,000 and 200,000 pesos ($N = 51,622$). Standard errors are clustered at the "comuna" level (332 comunas). *, ** and *** represent significance at the 10%, 5%, and 1%, respectively.

Table 3: Summary statistics for renegotiations

Variable	Mean	Median	St. Dev.
% Installment increase	-0.335	-0.417	0.486
Number of installments before offer	6.27	5.00	20.24
Number of installments after offer	11.66	12.00	3.38
% Amount increase	0.315	0.343	0.184
Upfront payment as % of amount	0.059	0.000	0.120
Frac. borrowers with no upfront	0.641	1.000	0.480
Implied interest rate	0.040	0.031	0.033
Number of renegotiations		2,735	

This table shows means, medians and standard deviations for variables related to the renegotiations that occur within six months after the campaign starts of borrowers in the renegotiation offer sample whose outstanding balance as of January 2010 is between 50,000 and 100,000 pesos. % installment increase is the increase in installment amount from previous to new, Number of installments before offer is the estimated average number of installments remaining before the renegotiation offer. Number of installments after offer corresponds to the average number of installments of the renegotiated debt, amount increase is the percentage increase of the debt balance due to the renegotiation, Upfront payment as a percentage of amount corresponds to the up front payment as a fraction of the renegotiated outstanding balance, Frac. borrowers with no upfront payment corresponds to the fraction of borrowers that renegotiate their loan and do not make an up-front payment, implied interest rate corresponds to the interest rate that makes the discounted value of instalments equal to the loan balance as of January 2010.

Table 4: OLS correlations between renegotiation and ex post debt repayment

monthly discount rate (r)	0%	1%	2%	3%	4%
renegotiation	0.4476***	0.4055***	0.3678***	0.3337***	0.3028***
	(0.0096)	(0.0087)	(0.0079)	(0.0073)	(0.0068)
constant	0.4819***	0.4476***	0.4175***	0.3914***	0.3688***
	(0.0090)	(0.0083)	(0.0077)	(0.0072)	(0.0067)
R^2	0.0540	0.0560	0.0550	0.0540	0.0520
monthly discount rate (r)	5%	6%	7%	8%	9%
renegotiation	0.2750***	0.2498***	0.2271***	0.2067***	0.1881***
	(0.0063)	(0.0060)	(0.0057)	(0.0054)	(0.0052)
constant	0.3486***	0.3311***	0.3154***	0.3013***	0.2887***
	(0.0064)	(0.0061)	(0.0059)	(0.0057)	(0.0055)
R^2	0.0490	0.0460	0.0420	0.0390	0.0350

This table reports the results of the OLS regression

$$npv\,(r) = \alpha\,(r) + \beta\,(r)\,renegotiation + \epsilon,$$

of the outcome $npv\,(r)$, the discounted sum of payments net of new credit at The Store for 20 months after January 2010 as a fraction of the January 2010 outstanding balance, as a function of the discount rate r, on *renegotiation*, a dummy that equals 1 if a borrower in the renegotiation offer sample renegotiates her debt within 6 months after selection. Outcome variable is winsorized at the 1st and 99th percentiles. The sample corresponds to borrowers in the renegotiation offer sample, as defined above, with oustanding balances between 50,000 and 200,000 pesos as of January 2010 ($N = 33,636$). Standard errors are clustered at the "comuna" level (316 clusters). *, ** and *** represent significance at the 10%, 5%, and 1%, respectively.

Table 5: Balancing of covariates

	(1) $npv\,(4\%)$	(2) discontinuity	(3) $p-value$
Tenure	0.0059	-0.0097	0.712
Fraction Late	-0.0421***	0.0060	0.735
Days late	-0.0603***	0.0136	0.605
Default90	0.0089	0.0341	0.214
Income	0.0107*	-0.0361	0.154
Female	-0.0199***	-0.0300	0.201
Age	-0.0039	-0.0160	0.480

This table shows that variables that are predetermined as of January 2010 do not exhibit a discontinuity at the campaign cutoff. Column (1) reports results of the OLS regression of $npv\,(4\%)$on the listed variables. The regressions include controls for "comuna" (a local geographic division similar to a U.S. county) and for major Store branches (17 branches). Column (2) reports the coefficient π of the individual regression of each variable on the *treated* dummy that equals 1 if the balance as of January 2010 is higher than 50,000 and a 4th degree polynomial of the January 2010 balance,

$$variable = \alpha + \pi treated + f\,(amount - 50,000) + \epsilon.$$

Column (3) shows the p-values of the test that each coefficient equals zero. $npv\,(4\%)$ is winsorized at the 1st and 99th percentiles. The sample corresponds to borrowers in the renegotiation offer sample, as defined above, with oustanding balances between 20,000 and 200,000 pesos as of January 2010 ($N = 45,428$). Standard errors are clustered at the "comuna" level (327 clusters). *, ** and *** represent significance at the 10%, 5%, and 1%, respectively.

Table 6: Causal effect of renegotiation on debt repayment using a fuzzy RD design

monthly discount rate (r)	0%	1%	2%	3%	4%
renegotiation	0.4383**	0.3968**	0.3583**	0.3266**	0.2880**
	(0.1838)	(0.1633)	(0.1483)	(0.1372)	(0.1280)
constant	0.5723***	0.5269***	0.4875***	0.4539***	0.4272***
	(0.0205)	(0.0182)	(0.0165)	(0.0152)	(0.0142)
R^2	0.028	0.028	0.027	0.026	0.025
monthly discount rate (r)	5%	6%	7%	8%	9%
renegotiation	0.2496**	0.2170*	0.1832	0.1537	0.1296
	(0.1217)	(0.1168)	(0.1130)	(0.1103)	(0.1075)
constant	0.4039***	0.3843***	0.3675***	0.3523***	0.3387***
	(0.0135)	(0.0129)	(0.0125)	(0.0121)	(0.0118)
R^2	0.023	0.021	0.018	0.016	0.014

This table estimates a lower bound on the willingness to pay for a good credit reputation on debt repayment. Estimation is conducted using a fuzzy RD design, implemented with a 2SLS procedure, where

$$renegotiation = \omega + \pi treated + f\left(amount - 50000\right) + \eta,$$

is the first stage and

$$npv\left(r\right) = \alpha\left(r\right) + \beta\left(r\right) renegotiation + \epsilon,$$

is the second stage. The table shows the coefficient $\beta\left(r\right)$ where the outcome is $npv\left(r\right)$, the discounted sum of payments net of new credit at The Store for 20 months after January 2010 as a fraction of the January 2010 outstanding balance. The instrumented variable, *renegotiation*, is a dummy that equals 1 if a borrower renegotiates within six months after January 2010, and $f\left(amount - 50000\right)$ is a fourth-degree polynomial of the January 2010 balance. The coefficients $\beta\left(r\right)$ correspond to a lower bound on the borrower's willingness to pay for a temporary good reputation. The sample corresponds to borrowers in the renegotiation offer sample, as defined above, whose oustanding balances as of January 2010 is between 10,000 and 200,000 thousand pesos (N=51,622). *, ** and *** represent significance at the 10%, 5%, and 1%, respectively. Standard errors are clustered at the "comuna" level (332 comunas), a Chilean geographical division similar to a U.S. county.

Table 7: Likelihood of distribution of characteristics of compliers relative to renegotiation offer sample

Variable:	(1) First stage $\pi(6)$	(2) Median	(3) Relative Likelihood
Fraction late	0.123	0.969	1.021
Days late	0.128	87.0	1.066
Internal credit score	0.126	472.0	1.046
Age (yrs)	0.116	37.0	0.964
Tenure (months)	0.093	49.0	0.776
Income	0.096	150,000	0.797
Female	0.130	1.000	1.076

This table is based on Angrist and Pischke (2008), section 4.4.4. For each listed variable, the table reports whether compliers of the *treated* instrument (as defined above) are more likely to have a value of that variable that is higher than its median. Compliers are not identifiable from the data, and are defined as those borrowers in the renegotiation offer sample who renegotiate before write off because their balance as of January 2010 is higher than 50,000 pesos. The size of the group of compliers is the unrestricted first stage coefficient shown on Table 2, approximately 0.1204 of the renegotiation offer sample right above the cutoff (for renegotiations that occur within 6 months). Column (1) shows the first stage regression coefficient $\pi(6)$ of regression

$$renegotiation(6) = \alpha(6) + \pi(6)\, treated + f(amount - 50000) + \eta,$$

for renegotiations that occur within 6 months after January 2010, restricting the sample to borrowers whose value of the variable is higher than the median. Column (2) shows the median of the variable for borrowers whose balance as of January 2010 is within 10,000 pesos (US$20) of the cutoff (40,000 pesos to 60,000 pesos). Column (3) shows whether compliers of the *treated* instrument are more likely (> 1 is more likely) to have a value of the variable that is higher than the median. This is calculated as the first stage coefficient of the restricted sample (column (1)) divided by the unrestricted first stage, 0.1204.

Table 8: Effect of renegotiation campaign on propensity to default over time

month (m)	1	2	3	4	5	6
renegotiation	-0.1614*	-0.4498***	-0.4430***	-0.4236***	-0.3987***	-0.2691***
	(0.0912)	(0.0997)	(0.0813)	(0.0894)	(0.0842)	(0.0828)
constant	0.5022***	0.5698***	0.5478***	0.5237***	0.5167***	0.5068***
	(0.0105)	(0.0118)	(0.0100)	(0.0108)	(0.0104)	(0.0101)
R^2	0.033	0.084	0.123	0.143	0.149	0.111
month (m)	7	8	9	10	11	12
renegotiation	-0.1498*	-0.1631*	-0.1123	-0.0217	0.0261	0.0066
	(0.0863)	(0.0852)	(0.0864)	(0.0864)	(0.0850)	(0.0858)
constant	0.5086***	0.5176***	0.5210***	0.5194***	0.5233***	0.5319***
	(0.0100)	(0.0101)	(0.0100)	(0.0101)	(0.0104)	(0.0106)
R^2	0.068	0.067	0.053	0.020	0.006	0.014

This table shows that borrowers who renegotiate before write off reduce their propensity to default at The Store for a period of eight months. Thus, renegotiation allows borrowers to acquire a good credit reputation for this time period. Estimation is conducted using a fuzzy RD design, implemented with a 2SLS procedure, where

$$renegotiation = \omega + \pi treated + f\left(amount - 50000\right) + \eta,$$

is the first stage and

$$default\left(m\right) = \alpha\left(m\right) + \beta\left(m\right) renegotiation + \epsilon,$$

is the second stage. The table reports the coefficient $\beta\left(m\right)$ where the outcome is $default90\left(m\right)$, a dummy that equals 1 if a borrower is late by more than 90 days on month m after January 2010, for months $m \in \{1, \ldots, 12\}$ .The instrumented variable, *renegotiation*, is a dummy that equals 1 if a borrower renegotiates within six months after January 2010, and $f\left(amount - 50000\right)$ is a fourth-degree polynomial of the January 2010 balance. The sample corresponds to borrowers in the renegotiation offer sample, as defined above, whose oustanding balances as of January 2010 is between 10,000 and 200,000 thousand pesos(N=51,622). *, ** and *** represent significance at the 10%, 5%, and 1%, respectively. Standard errors are clustered at the "comuna" level (332 comunas), a Chilean geographical division similar to a U.S. county.

1.2 Chapter 2

Table 1: Descriptive statistics - January 2010 cross section

Sample	All ($N = 6,837$)		Treated ($N = 3,427$)		Control ($N = 3,410$)		
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.	$t - stat$
As of 01/2010							
Balance ('000 pesos)	49.92	5.74	54.86	2.88	44.96	2.92	-140.00**
Tenure (months)	70.72	63.42	70.77	64.13	70.69	62.71	-0.06
Credit score	483.0	59.6	480.8	63.4	485.3	55.5	3.16**
Late>90 days	0.429	0.495	0.434	0.496	0.424	0.494	-0.83
As of 12/2009							
Bank Debt ('000 pesos)	3,655.68	14,478.04	3,568.20	16,197.74	3,743.59	12,515.73	0.50
Consumer	1,329.19	3,404.88	1,372.52	3,412.92	1,285.65	3,396.73	-1.05
Commercial	511.50	9,736.04	569.53	12,215.91	453.19	6,332.63	-0.49
Mortgage	1,814.97	7,905.63	1,626.15	7,195.44	2,004.74	8,557.07	1.98**
Fraction bank debt	0.518	0.500	0.521	0.500	0.515	0.500	-0.56
Fraction in default	0.326	0.469	0.344	0.475	0.309	0.462	-2.21**
Amount in default	0.088	0.238	0.091	0.238	0.086	0.238	-0.59

This table shows summary statistics of the cross section of borrowers in the renegotiation selection sample that owe between 40,000 and 60,000 pesos at The Store as of January 2010 (All), and by sub-sample of Treated (owe between 50,000 and 60,000 pesos) and Control (owe between 40,000 and 50,000 pesos). Store Debt ('000 pesos), Tenure (months), Credit score and Late>90 days are self explanatory. Bank Debt is the aggregate debt with banks; Consumer, Commercial and Mortgage correspond to the average aggregate (across banks) debt broken down by type of debt; Fraction bank debt is the fraction of borrowers that has an outstanding balance with banks; Fraction in default is the fraction of borrowers with outstanding bank debt that are in default; Amount in default is the borrower's total debt in default divided by the borrower's outstanding balance. All Store variables are as of January 2010, while all bank debt variables are as of December 2009. The t-stat column shows the t-stat of the difference between the average variable for the Control group and the average variable for the Treated group; ** represents significance at the 95% level.

Table 2: The effect of a good reputation on access to credit and default rates

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. variable	debt GS	consumer	commercial	mortgage	default	frac_default
post	-81.11	-22.63	34.17	23.47	0.0379***	0.0798***
	(72.95)	(26.55)	(28.80)	(46.51)	(0.0112)	(0.0066)
$post \times treated$	209.87*	42.02	-26.87	206.37**	0.0285*	0.0230**
	(114.39)	(46.88)	(39.63)	(84.65)	(0.0161)	(0.0096)
R^2	0.002	0.001	0.000	0.002	0.008	0.085
Obs.	27,348	27,348	27,348	27,348	13,681	13,681
Clusters	6,837	6,837	6,837	6,837	3,893	3,893

This table shows that a good reputation is valuable because it allows borrowers to have more debt with banks, and that ex-post, renegotiated borrowers have a higher propensity to default on their bank debt. The table shows the coefficients on *post* and $post \times treated$ for the regressions of bank debt in good standing (1), consumer (2), commercial (3) and mortgage debt (4) with banks, a dummy that equals 1 if the borrower is in default (5) (default), and the fraction of outstanding debt that is in default (6), using the differences in differences specification,

$$outcome_{it} = \alpha_i + \xi_t + \omega post_t + \beta post_t \times treated_i + \epsilon_{it}.$$

All regressions include month and individual fixed effects. Standard errors are clustered by individual. *, ** and *** represent significance at the 10%, 5%, and 1%, respectively.

1.3 Chapter 3

Table 1: Summary statistics

This table presents summary statistics of the dataset. Panel A shows statistics of each transaction from a supplier to the supermarket at the product-firm-month level. Amount in US$ calculated using approximations of 500 pesos/US$ and 20,000 pesos/UF exchange rates.

Panel A: Supplier sales to Supermarket at the product-firm-month level (422,174 observations)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variable	Mean	St. Dev.	Min	P10	P50	P90	Max
Amount (UF)	1,006.9	4,251.6	<0.01	3.8	115.1	2,029.6	293,405.6
Amount (thousand US$)	40.3	170.1	<0.01	0.2	4.6	81.2	11,736.2
Units	31,682	155,759	1	90	2,220	56,576	9,003,029
Price (UF)	0.16	0.87	<0.01	0.01	0.04	0.21	78.39

Panel B: Number and amount of transactions at the product-firm-month level, and number of sections, lines and products by department.

	(1)	(2)	(3)	(4)	(5)
Name	Percentage (number)	Percentage (amount)	# Lines	# Sections	# Products
GENERAL FOOD	29.88	41.96	7	57	261
MEAT AND FISH	5.40	10.88	2	7	89
HOME	3.36	1.69	6	36	169
CONSUMER	11.59	15.08	5	43	129
ENTERTAINMENT	5.41	3.77	7	42	189
DELI	7.58	8.44	2	12	36
FRUITS & VEGETABLES	10.08	3.24	3	7	76
HARDLINES	8.80	2.74	9	81	396
PETS	1.02	1.37	1	6	23
IMPULSIVE SHOPPING	1.06	0.38	1	15	39
BABY	2.87	2.25	1	17	88
BUSINESS	3.69	1.46	1	2	28
BAKERY	3.87	3.47	2	5	28
RESTAURANT	1.82	0.24	1	3	10
HEALTH	0.82	0.24	1	11	32
CLOTHING	2.76	2.99	12	84	278
TOTAL			61	428	1,871

Table 2: Cross sectional differences for Treated and Control firms

This table shows the mean and standard deviation of variables for Treated and Control firms as defined above.

Panel A: Firm level average monthly variables

	All		Treated		Control	
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
Revenues (UF)	4,808.8	3,253.0	4,022.2	2,393.6	5,962.5	3,935.7
Revenues (th. US$)	192.4	130.1	160.9	95.7	238.5	157.4
# Departments	1.261	1.215	1.083	0.829	1.522	1.588
# Lines	1.748	2.151	1.515	1.215	2.089	3.011
# Sections	3.866	7.218	3.472	3.339	4.444	10.573
# Products	9.440	17.552	8.134	7.112	11.356	26.090
N	74		44		30	

Panel B: Product-firm level average monthly variables

	All		Treated		Control	
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
Revenues (UF)	235.51	814.75	196.64	624.56	292.76	1,030.36
Revenues (US$)	9,420.3	32,590.0	7,865.6	24,982.5	11,710.4	41,214.3
Units	9,641.45	91,248.92	5,645.50	24,437.92	15,527.46	140,195.70
Price	0.1002	0.3206	0.0995	0.2510	0.1009	0.3802
Makes sale	0.4623	0.4986	0.3977	0.4894	0.5576	0.4967
Supermarket mark-up (%)	0.3198	0.2254	0.3325	0.2493	0.3013	0.1834
N	877		548		329	

Panel C: Industry distribution of product transactions

	All	Treated	Control
GENERAL FOOD	2,338	649	1,689
MEAT AND FISH	337	159	178
HOME	404	275	129
PERISHABLES	1,106	640	466
ENTERTAINMENT	353	305	48
DELI	327	132	195
FRUITS & VEGETABLES	823	262	561
HARDLINES	958	827	131
PETS	186	186	0
IMPULSIVE SHOPPING	108	104	4
BABY	232	139	93
BUSINESS	196	69	127
BREAD & BAKING	549	298	251
RESTAURANT	105	14	91
HEALTH & WELLBEING	12	0	12
CLOTHING	349	236	113
TOTAL	8,383	4,295	4,088

Table 3: Diffs in diffs estimates of the effect of the reduction of days payable on prices

This table presents the estimated coefficient of interest of regression (5), β, which measures the relative change in each outcome variable for Treated firms relative to Control firms, before and after the reduction in days payable for Treated firms. The outcome variable is the natural logarithm of the price ($log\,(price)$). The data is a balanced panel at the monthly-firm-month level, as described above. Each column specifies the structure of fixed effects included in the regression. "ANALYSIS" sample considers the full sample as described above. "Small 2007" sample considers firms that as of 2007 have total sales below 100,000 UF. "EX POST" sample includes only product-firm pairs that have at least one observation after December 2006. "Placebo" sample includes only Control firms. Standard errors are clustered at the firm level. *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(6)
VARIABLES	$log\,(price)$	$log\,(price)$	$log\,(price)$	$log\,(price)$	$log\,(price)$	$log\,(price)$	$log\,(price)$
Sample	ANALYSIS	ANALYSIS	ANALYSIS	ANALYSIS	Small 2007	EX POST	Placebo
$post \times treated$	-0.0992**	-0.0974**	-0.0554**	-0.1045**	-0.0981*	-0.0554**	-0.0003
	(0.0396)	(0.0375)	(0.0275)	(0.0439)	(0.0540)	(0.0275)	(0.0224)
R^2	0.885	0.079	0.108	0.651	0.090	0.108	0.136
Obs.	27,881	27,881	27,881	27,881	5,260	27,244	162,838
Firms	73	73	73	73	21	72	173
Product-firms	1,333	1,333	1,333	1,333	321	1,154	4,838
Fixed effects:							
Month	YES	YES	YES	YES	YES	YES	YES
Firm	YES			YES			
Product	YES						
Firm-product		YES	YES		YES	YES	YES
Department-year			YES			YES	YES
Line-year				YES			

Table 4: Diffs in diffs estimates of the effect of the reduction of days payable on other variables

This table presents the estimated coefficients of interest of regression (6), β_{2007}, β_{2008}, and β_{2009}, which measure the relative change in each outcome variable for Treated firms relative to Control firms as defined above, for each year 2007, 2008, and 2009, relative to 2006, respectively. The outcome variables are the natural logarithm of the price ($log\,(price)$), a dummy that equals 1 if there is a sale of that product on a given month ($makes\ sale$), the logarithm of units sold ($log\,(units)$), monthly product-firm units sold divided by average 2006 monthly product firm units sold ($norm\ units$), the logarithm of the amount of each transaction ($log\,(amount)$), and monthly product-firm revenues divided by average 2006 monthly product firm revenues ($norm\ revenues$). The data is a balanced panel at the monthly-firm-month level, as described above. All regressions include month, firm-product and department-year fixed effects. Standard errors are clustered at the firm level. *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	(1)	(2)	(4)	(5)	(6)	(7)
VARIABLES	$log\,(price)$	$makes\ sale$	$log\,(units)$	$norm\ units$	$log\,(amount)$	$norm\ revenues$
Sample	ANALYSIS	ANALYSIS	ANALYSIS	ANALYSIS	ANALYSIS	ANALYSIS
$1\,(2007) \times treated$	-0.0347*	-0.0436*	-0.1199**	-0.2745***	-0.1546***	-0.3149***
	(0.0190)	(0.0260)	(0.0550)	(0.1015)	(0.0510)	(0.0957)
$1\,(2008) \times treated$	-0.0506	0.0048	-0.1750**	-0.1700	-0.2256***	-0.2211
	(0.0324)	(0.0391)	(0.0826)	(0.1636)	(0.0750)	(0.1462)
$1\,(2009) \times treated$	-0.0943**	0.0799	-0.1413	0.0534	-0.2357***	0.0051
	(0.0382)	(0.0484)	(0.0886)	(0.1663)	(0.0836)	(0.1579)
R^2	0.109	0.034	0.024	0.015	0.024	0.015
Obs.	27,881	69,312	27,881	50,064	27,881	50,064
Firms	73	73	73	73	73	73
Product-firms	1,333	1,333	1,333	1,333	1,333	1,333

Table 5: Heterogeneity

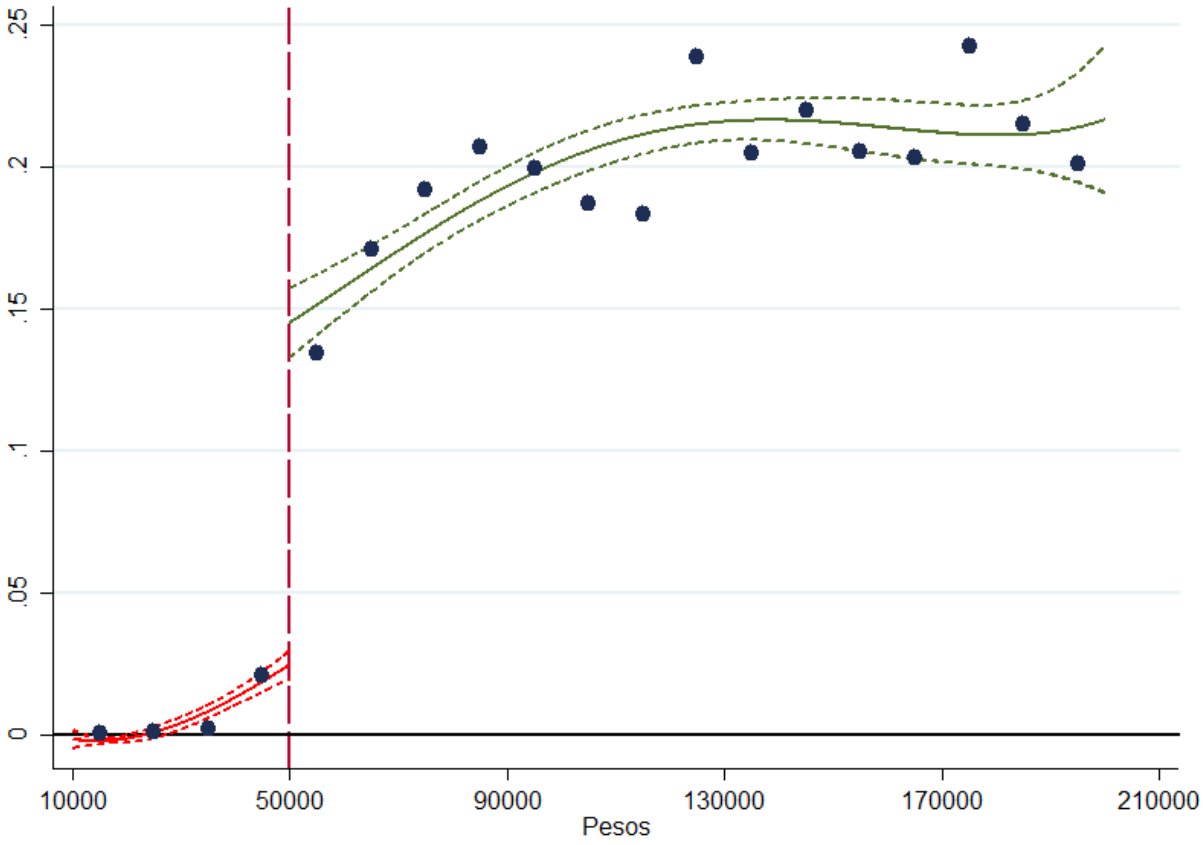
This table reports how the estimated effects of the change in days payable on transaction prices vary based on pre-determined characteristics. Each column shows the estimated coefficient β of regression (5) for different subsamples, High corresponds to borrowers whose value of the sorting variable is above the sample median and Low corresponds to borrowers whose value of the sorting variable is below the sample median. The sorting variables are: Perishables, a dummy that equals one for products whose department is either Meat and Fish or Fruit and Vegetables and zero otherwise; Fraction of sales, a product-firm-level variable that measures the fraction of sales to the Supermarket of the product that were made by the firm; Diversification, a firm-level variables that measures the average number of different products sold by the firm to the Supermarket during 2006. The column also shows the estimated coefficient γ of regression (7) ran on the full sample. This coefficient measures the difference between the High and Low coefficients from the separate regressions. All regressions include month, firm-product and department-year fixed effects. Standard errors are clustered at the firm level. *, ** and *** represent significance at the 10%, 5%, and 1% respectively.

	(1)	(2)	(3)
Outcome		$log\,(prices)$	
Interaction	Perishables	Fraction of sales	Diversification
		Subsample regressions	
High	0.0303	-0.0136	-0.0675**
	(0.0439)	(0.0191)	(0.0322)
Low	-0.0724***	-0.0896**	0.0112
	(0.0269)	(0.0354)	(0.0307)
		Pooled regression	
$high \times treated \times post$	0.1350***	0.0765**	-0.0787*
	(0.0413)	(0.0372)	(0.0441)

2 Figures

2.1 Chapter 1

Figure 1: Effect of phone campaign on probability of renegotiation

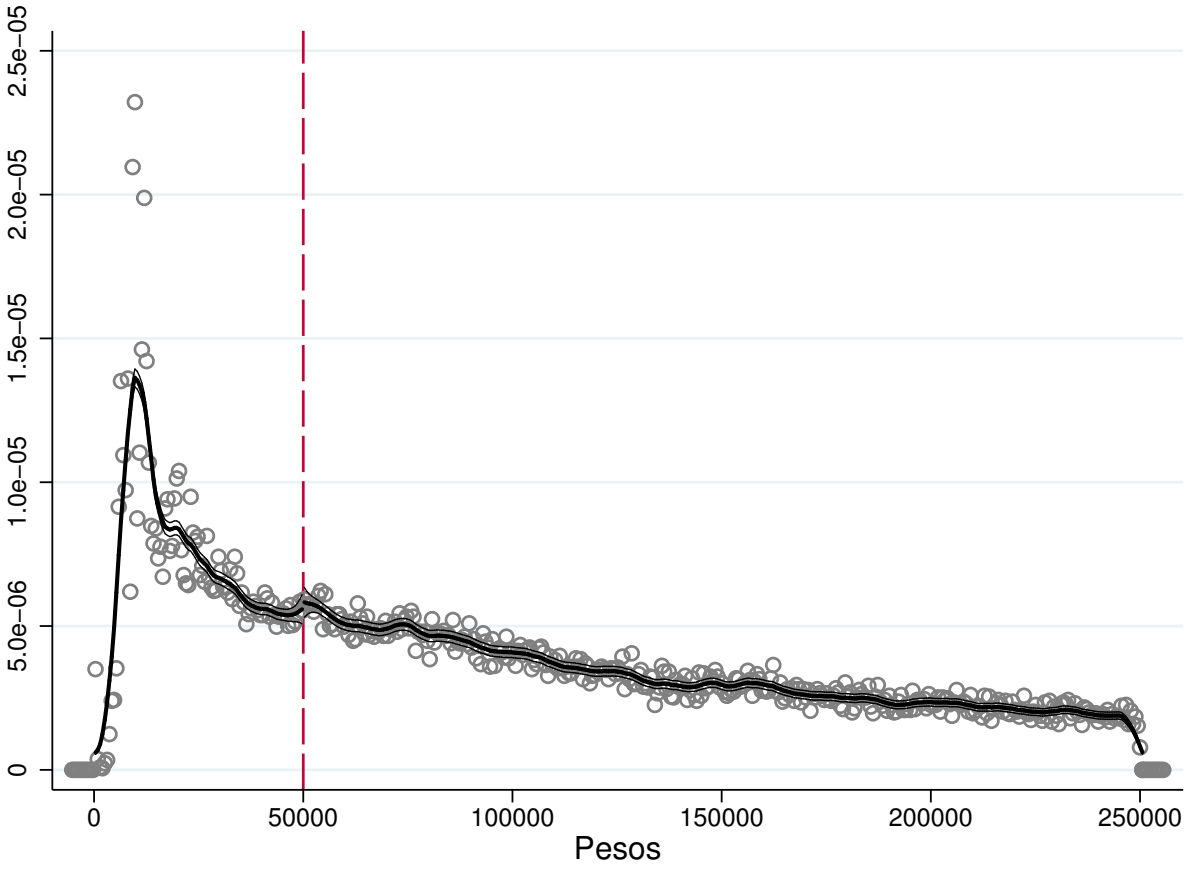


This figure examines how the renegotiation campaign impacts the fraction of borrowers who renegotiate. The figure shows the average fraction of borrowers in the renegotiation offer sample (as defined above) who renegotiate their loans between one and six months after January 2010, in bins of 10,000 Chilean pesos (USD20), and the fitted values and 90% confidence intervals from the regression model,

$$renegotiation = \alpha_0 + \pi treated + f\left(amount - 50000\right) + \epsilon,$$

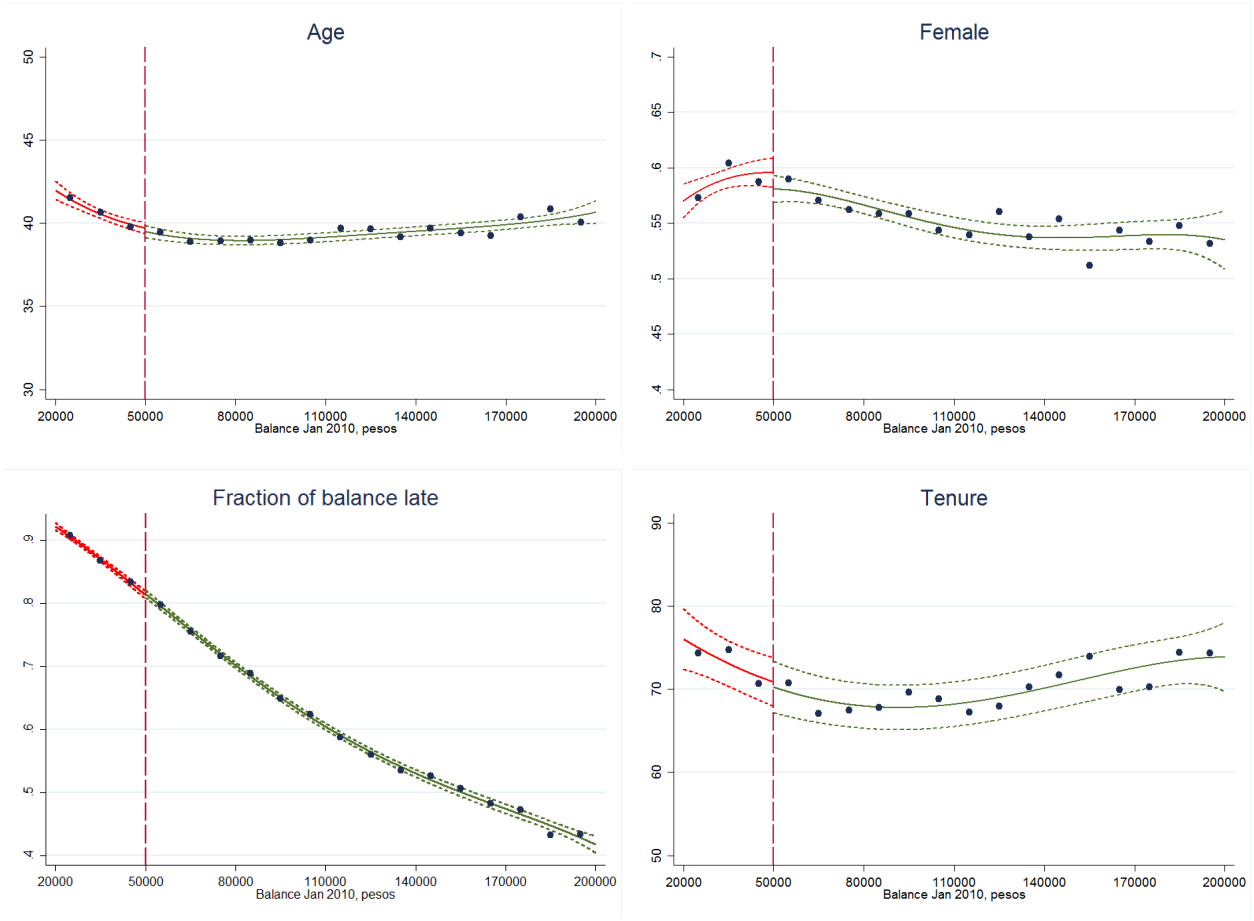
where the outcome variable *renegotiation* is an indicator that equals 1 if the borrower renegotiates the loan within six months after selection into the renegotiation offer sample, on *treated*, a variable that equals 1 if the borrower owes more than 50,000 pesos as of Jan 2010 and 0 if she owes less, and $f\left(amount - 50000\right)$, a 4th degree polynomial of the outstanding balance at The Store as of January 2010. The dashed red line represents the 50,000 pesos campaign cutoff. Standard errors are clustered at the "comuna" level (332 comunas), a Chilean geographical division similar to a U.S. county.

Figure 2: Histogram by January 2010 store debt balances



This figure shows that borrowers do not manipulate the amount they owe to be on either side of the cutoff before the renegotiation campaign. The figure is a histogram of borrowers in the renegotiation offer sample by the balance as of January 2010, generated using code by Justin McCrary, for balances from 0 to 250,000 pesos (USD0 to USD500). The dashed red line represents the 50,000 pesos threshold. Algorithm and methodology found in McCrary (2008).

Figure 3: Cross-sectional covariates

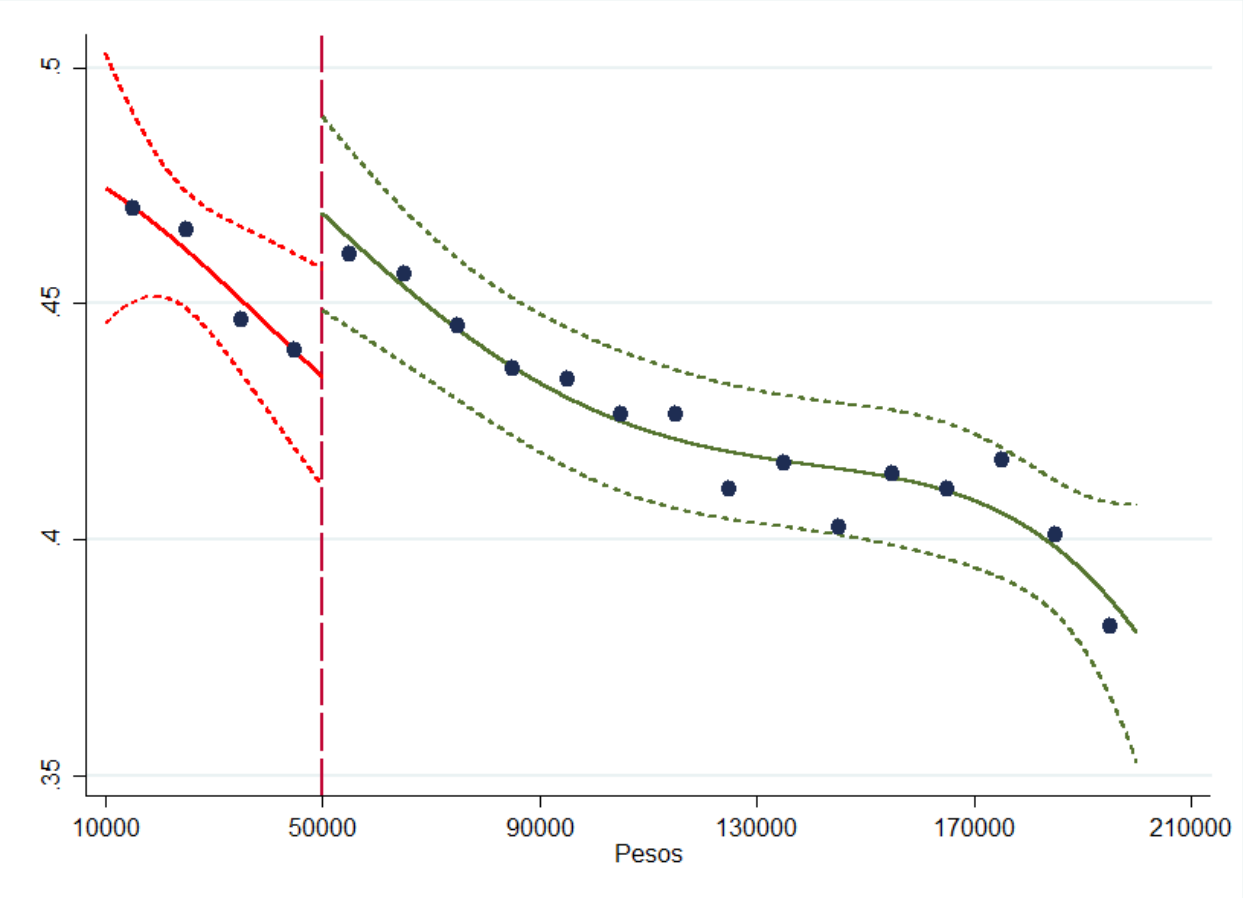


This figure shows that predetermined variables are continuous at the cutoff for borrowers in the renegotiation offer sample. Four plots are shown for the age, fraction of balance in arrears, a dummy for female borrowers, fraction of balance that is late, and tenure at Store, respectively. All variables as of January 2010. Plots show averages grouped in bins of 10,000 pesos (USD20) of the balance at The Store as of January 2010. The plots also show the fitted values and 90% confidence interval of a modified version of the regression in equation (1),

$$outcome = \alpha_0 + \beta treated + f\left(amount - 50000\right) + \epsilon,$$

with each of these variables as outcomes, on *treated*, a variable that equals 1 if the borrower owes more than 50,000 pesos as of Jan 2010 and 0 if she owes less, and $f\left(amount - 50000\right)$, a 4th degree polynomial of the balance as of January 2010. The dashed red line represents the 50,000 pesos campaign cutoff. Standard errors are clustered at the "comuna" level (332 comunas), a Chilean geographical division similar to a U.S. county.

Figure 4: Effect of renegotiation campaign on ex post repayment



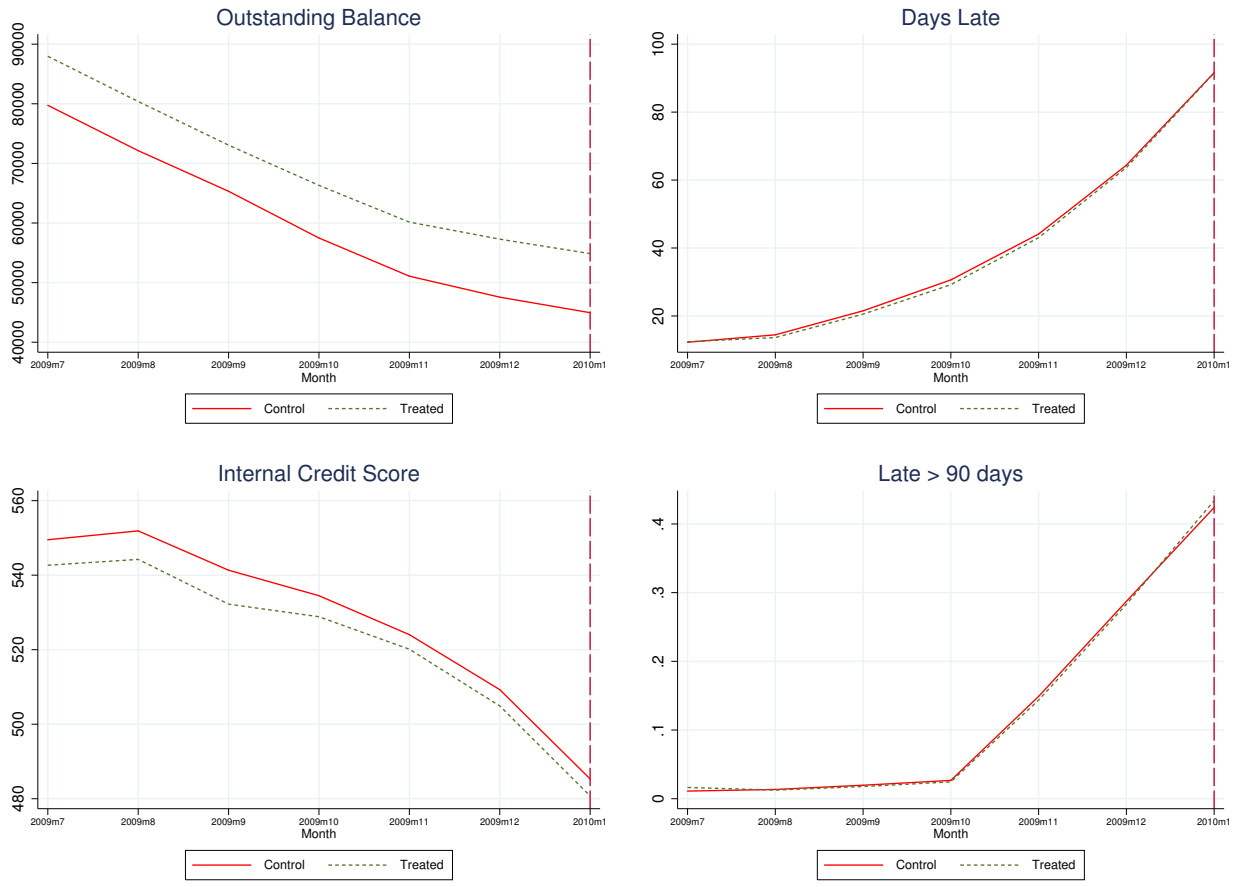
This figure examines the effect of the renegotiation campaign on ex post repayment for all borrowers who were eligible for the renegotiation irrespective of whether they renegotiated or not (i.e., the reduced form estimates). The figure shows the average value of $npv\,(4\%)$, the discounted sum of payments net of new credit at The Store for 20 months after January 2010 discounted using a 4% monthly rate, as a fraction of the January 2010 outstanding balance, for borrowers in the renegotiation offer sample in bins of 10,000 Chilean pesos (USD20), and the fitted values and 90% confidence intervals from the regression model,

$$npv\,(4\%) = \alpha_0\,(r) + \beta\,(r)\,\pi treated + f\,(amount - 50000) + \epsilon,$$

of $npv\,(4\%)$ on $treated$, a variable that equals 1 if the borrower owes more than 50,000 pesos as of Jan 2010 and 0 if she owes less, and $f\,(amount - 50000)$, a 4th degree polynomial of the outstanding balance at The Store as of January 2010. The dashed red line represents the 50,000 pesos campaign cutoff. Standard errors are clustered at the "comuna" level (332 comunas), a Chilean geographical division similar to a U.S. county.

2.2 Chapter 2

Figure 5: Time series plots for the differences in differences specification



The figures show plots of outstanding balance at The Store, days late at The Store, credit score at The Store, and fraction of borrowers late by more than 90 days at The Store for the Treated and Control groups. Treated borrowers are borrowers in the renegotiation offer sample who as of January 2010 owed between 50,000 and 60,000 pesos; Control borrowers owed between 40,000 and 50,000 pesos.

2.3 Chapter 3

Figure 6: Histogram of firms by log 2006 revenues

This figure presents a histogram of the Supermarket's suppliers during 2006, by their log revenues to the Supermarket. The dashed line represents 60,000 UF, the cutoff point for the Agreement. The dotted lines represent 40,000 UF and 90,000 UF, the min and max values of 2006 revenues included in the main sample.

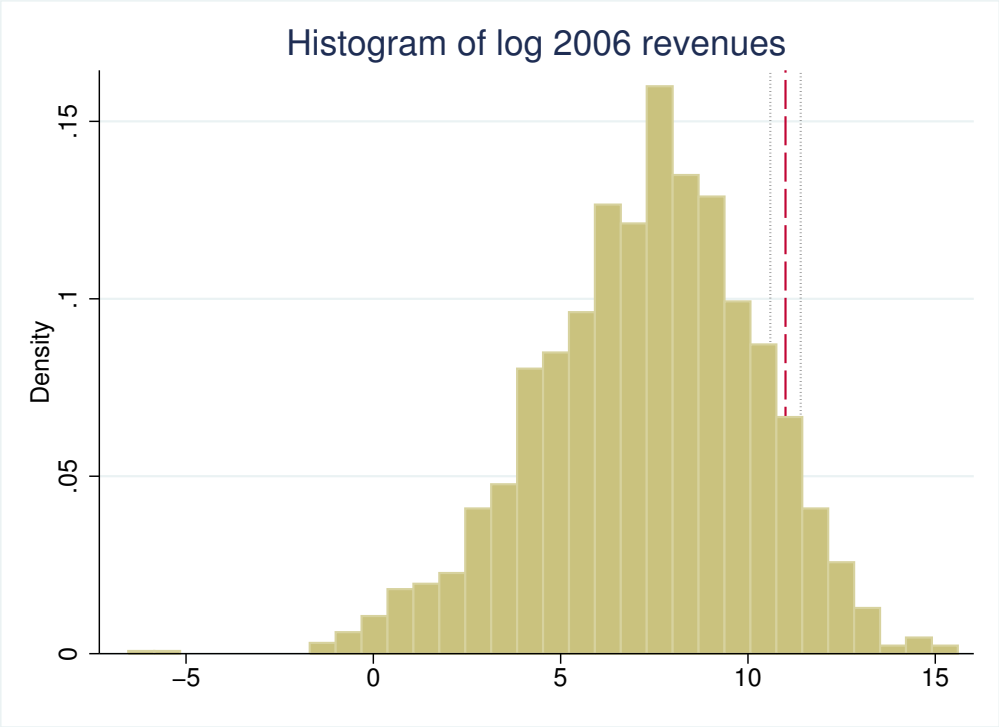


Figure 7: Log Prices for Treated vs. Control Firms

This figure presents a simple graph of average log prices over all goods supplied to the Supermarket from 2006 through 2010 for Treated vs. Control firms. Log prices are normalized to 1 for the 4th quarter of 2006. Note that aside from the normalization, no further adjustments have been made to the data.

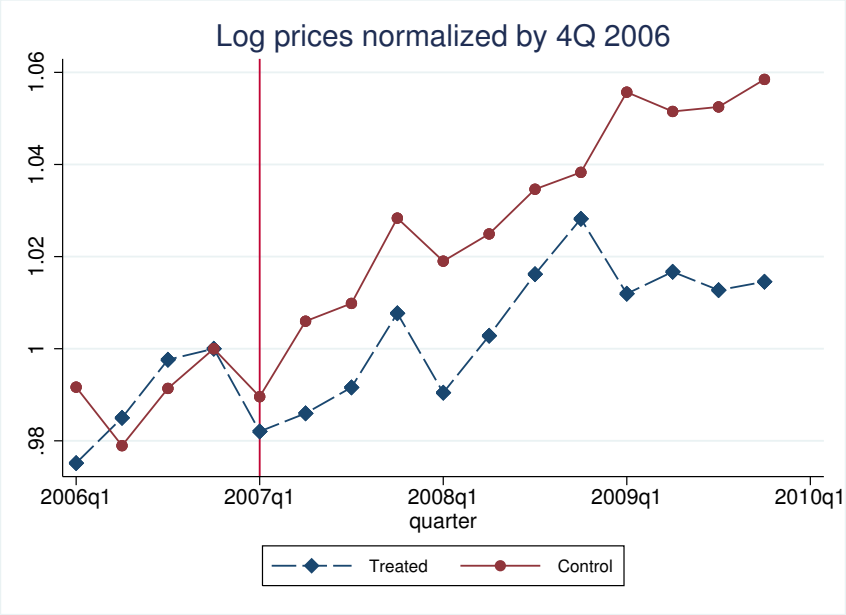
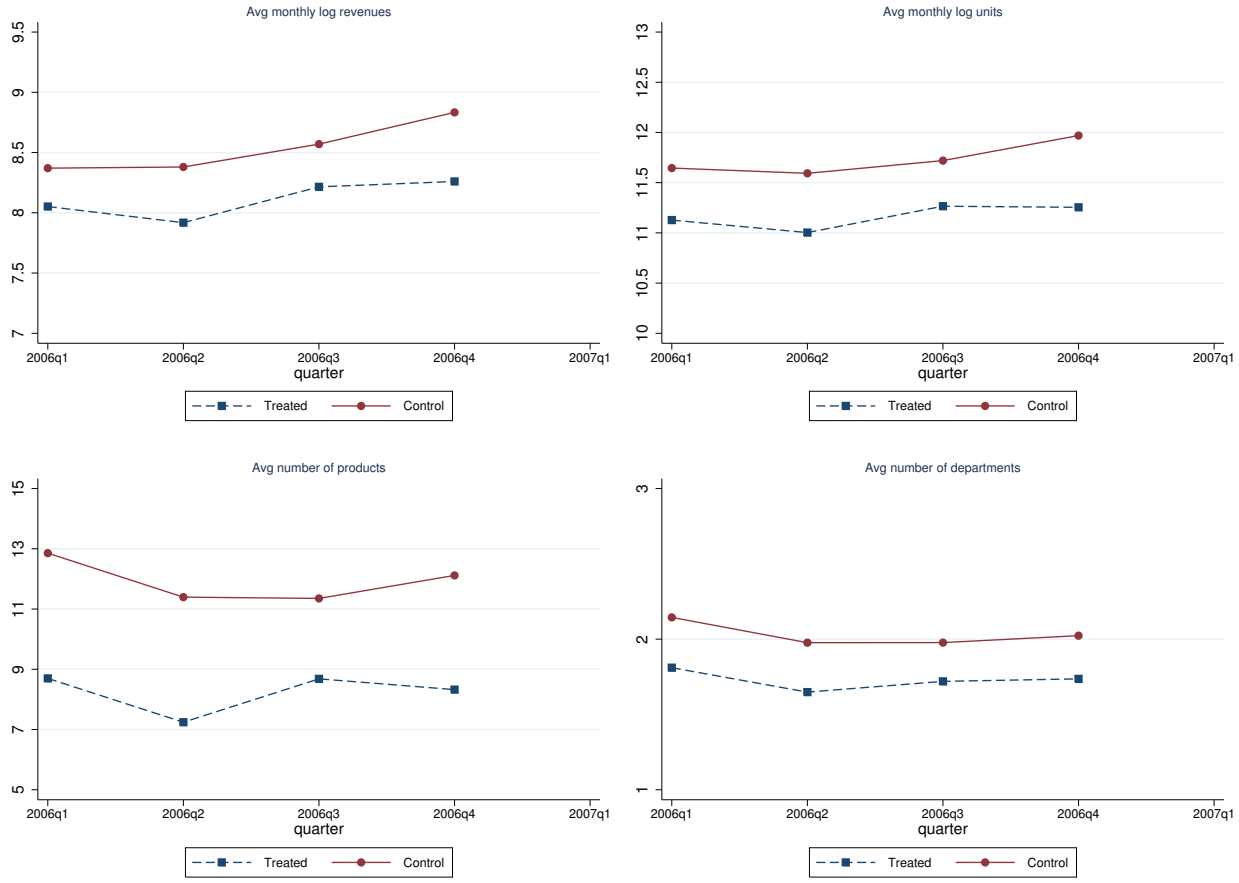


Figure 8: Firm-level Pre-period trends for Treatment vs. Control Firms

This figure presents four graphs of pre-period trends for firm-level average monthly revenues, log units, number of products and number of departments for Treatment and Control firms as defined above, at the quarter level.



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Part V

Appendix

Other potential discontinuity margins

Aside from the 50,000 pesos outstanding balance cutoff, the criteria for selection into the renegotiation offer sample offers three additional margins that are suitable candidates for discontinuities: (i) borrowers who as of January 2010 are above and below the 30 days late threshold, (ii) borrowers who have tenures at The Store above and below six months, and (iii) borrowers around the threshold number of renegotiations in the past years. The data suggests that the identification assumption for a regression discontinuity analysis, that is, that the distribution is continuous at the threshold, can be rejected for the first and third margins. In particular, I discard the third margin because the number of borrowers who have more than the maximum number of renegotiations in the last one or three years is substantially lower than the number that have less than the maximum. I also discard the 30 days late threshold based on the histogram of borrowers by days late (not presented in this paper), which shows a large mass of borrowers immediately before 30 days. Officers at The Store relate this pattern to the fact that many borrowers that are late by less than 30 days would repay their debt precisely before 30 days to avoid being in arrears one full billing cycle.

The six-month minimum tenure at The Store offers some potential for a discontinuity analysis. However, who as of January 2010 have tenures with The Store of six or less months eventually cross the 6 month threshold and become eligible for the renegotiation campaign in subsequent months. Although the identification assumption may hold at this threshold, that is, borrowers with tenures of six months may be comparable to borrowers with tenures

of seven months, this very high slippage greatly reduces statistical power.

Bias due to differential access to new credit at The Store

The outcome variable measures payments net of new credit as a fraction of the initial balance. One potential problem with this measure is that the dataset is right truncated at 20 months. Therefore, it may be the case that borrowers whose balance as of January 2010 is below the cutoff assume relatively more new credit in the final months of the sample, and their repayment of this new credit does not show up in the data. This would result in an upward bias in the measure of repayment of the initial outstanding balance.

I investigate this potential bias by looking at the cumulative value of new credit as a fraction of the initial outstanding balance, from months 8 to 20. If there is a differential access to credit before these months, a repayment time of at least 12 months would ensure the bias is minimized. Figure 9 shows a plot of this variable, averaged in bins of 10,000 pesos, and the fitted values and standard errors of the fuzzy RD regression (3). There is no discontinuity at the cutoff in the value of new credit in later months, which implies that at least by month 8, when there are 12 months left to observe repayment, borrowers above and below the cutoff have the same relative access to credit within The Store. This evidence reduces the probability that the results are biased.

Placebo test for discontinuities

I investigate whether the fuzzy RD framework generates discontinuities in other points of the distribution of outstanding balances as of January 2010, in particular for outstanding balance close to the 50,000 pesos cutoff. Results are shown in Figure 10. No discontinuity is significant at the 10% level on any of these placebo cutoffs.

Robustness of RD specification

In order to assess if the specification choices of the degree of the polynomial and length of the amount interval materially affect the results, I present in Table 6 the fuzzy RD coefficient estimates of β for $npv\,(4\%)$, $default90\,(3)$ and $default90\,(12)$. I also show the first stage *renegotiated* for six months after selection for a number of different specification choices. The last rows present a local linear estimation in the spirit of Imbens and Lemieux (2007). The results suggest that the main results hold irrespective of the particular choice of specification.

Cross sectional variation of the willingness to pay for a good credit reputation

I study whether the willingness to pay for a good credit reputation varies with some predetermined characteristics. In particular, I explore whether borrowers in the renegotiation offer sample whose value of each of a list of predetermined variables is higher than the median are willing to pay a different fraction of their balance for a good credit reputation. I report results for the outcome variable calculated using my preferred monthly discount rate of 4% per month, $npv\,(4\%)$.

I run separate regressions for the subsamples of borrowers whose values of the outcome are higher and lower than the sample median. For example, let $D = 1\,(age > age_{median})$, a dummy that equals 1 if the borrower is older than the sample median age. Then, I run the first stage regression (2) separately for the subsamples with $D = 0$ and $D = 1$, and in each case I report the coefficient π_D. I also run the 2SLS estimation with equation (2) as the second stage and present the coefficient β_D.

In order to verify whether the coefficients π and β are statistically different in both subsamples, I interact equation (2) with a dummy that equals one if the variable is higher than the sample median and run a pooled regression that includes both subsamples with

$D = 0$ and $D = 1$. The regression model is

$$npv\,(4\%) = \alpha + \beta renegotiation + \gamma renegotiation \times D + \epsilon. \tag{8}$$

As in the fuzzy RD setting, I use the variable *treated* (conditional on a flexible polynomial of the balance as of January 2010) to instrument for *renegotiation*. In the same spirit, I allow the subsample of borrowers with $D = 1$ to have a different value of the discontinuity at the cutoff (π_D) and functional relationship between the January 2010 balance and the *renegotiation* variable. In this case, the reduced form equation is

$$\begin{aligned} npv\,(4\%) = \alpha + \alpha_1 D + \beta\pi treated + \gamma\pi_D treated \times D \\ + \tilde{f}\,(amount - 50,000) + \tilde{f}_D\,([amount - 50,000] \times D) + \chi. \end{aligned}$$

The statistical significance of γ can be used to assess whether borrowers in both subsamples are willing to pay different fractions of their initial balance to have a good credit reputation. I also present the results for an "expanded" first stage regression that includes the possibility that the discontinuity in the fraction of renegotiations and the polynomials used to estimate it differ for borrowers in the different subsamples,

$$\begin{aligned} renegotiation = \omega + \omega_1 D + \pi treated + \pi_D treated \times D + \\ + f\,(amount - 50,000) + f_D\,([amount - 50,000] \times D) + \eta. \end{aligned}$$

Similarly, the statistical significance of π_D can be used to infer whether the phone campaign affects the fraction of renegotiations differentially for both subsamples.

Table 7 shows that the fraction of borrowers who renegotiate is significantly higher for borrowers whose tenure is shorter than 58 months, for borrowers whose fraction of the balance that is late is above 47%, and for borrowers whose monthly income is less than 201,000 pesos. Borrowers above and below median age, internal credit score, a dummy for female borrowers,

and days late do not exhibit a significant difference in the fraction of renegotiations. The results for the fraction of the balance that is late should be taken with extra caution do to the high correlation of this variable with the *treated* instrument.

With respect to the willingness to pay for a good credit reputation, borrowers who are older, who have been clients of The Store for a longer period, who have a higher internal credit score, who are male, who are late by less days, who are late by a larger fraction of their balance, and who earn a higher monthly income seem to be willing to pay more for a good credit reputation. However, the results are only significantly different for the internal credit score, gender and monthly income.

Some of these results are consistent with models of life cycle reputation acquisition such as Diamond (1989). For example, older borrowers are willing to pay more to restore their good credit reputation because they may have less time left to do so. Borrowers with longer tenures and higher internal credit scores presumably value their good credit reputation relatively more for similar reasons. Further, it is likely that in Chile most households are headed by males, which may therefore be relatively more interested in obtaining a good credit reputation in order to, for example, access credit markets in the future. Finally, borrowers who are late by a larger fraction of their balance are those who would most benefit from a renegotiation that reduces the price of a good credit reputation.

Nevertheless, these results must be taken with caution: since the variable used to sort the subsamples is not randomly assigned, any difference in the estimated coefficients may not be interpreted causally. For example, age may be correlated with other variables, such as income and education so that the differences in repayment may be rather driven by these variables

Tables

Table 6: Robustness of fuzzy RD specification

(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Coefficient on *treated*		
Interval	Degree	Obs	*renegotiation*	*npv* (4%)	*default*90 (3)	*default*90 (12)
$[0, 500k]$	6	82,188	0.1291***	0.3040***	-0.3204***	0.1611**
$[10k, 400k]$	6	71,699	0.1217***	0.3364***	-0.4643***	0.0226
$[20k, 300k]$	6	57,107	0.1095***	0.2317*	-0.5681***	-0.1638*
$[20k, 80k]$	2	21,371	0.1102***	0.2974*	-0.4146***	0.0320
$[10k, 100k]$	Linear	33,023	0.1186***	0.2499*	-0.4224***	0.1308
$[10k, 80k]$	Linear	30,043	0.1090***	0.2851*	-0.4074***	0.1371

This table shows that the particular choice of the degree of the fuzzy RD polynomial and the outstanding balance interval used to select the sample do not have a material effect on the main results of the paper. The table shows the fuzzy RD regression coefficients β, where the outcomes are *renegotiation* six months after selection into the campaign , $default90\,(3)$ and $default90\,(12)$, and $npv\,(4\%)$ on *treated* and a polynomial of varying degree. "Linear" represents a local linear approximation. The amount owed as of January 2010 varies in column 1 (Interval), while the degree of the polynomial varies in column 2 (Degree). Standard errors used to assess significance (not shown) are clustered at the "comuna" level. *, ** and *** represent significance at the 10%, 5% and 1% respectively.

Table 7: Heterogeneity of results in the cross section

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variable	Age	Tenure	Credit score	Female*	Days late	Fraction late	Income
Median	40	58	463	1.0	87	0.4741	201,000
Outcome variable: *renegotiation*							
Separate regressions:							
$treated_{D=0}$	0.1200***	0.1444***	0.1093***	0.1073***	0.1148***	0.0466**	0.1319***
	(0.0087)	(0.0093)	(0.0095)	(0.0099)	(0.0092)	(0.0227)	(0.0087)
$treated_{D=1}$	0.1207***	0.0890***	0.1274***	0.1295***	0.1281***	0.1211***	0.0980***
	(0.0103)	(0.0094)	(0.0093)	(0.0090)	(0.0095)	(0.0072)	(0.0102)
Pooled regressions:							
$treated \times D$	0.0007	-0.0554***	0.0181	0.0223	0.0133	0.0745***	-0.0339**
	(0.0128)	(0.0131)	(0.0140)	(0.0140)	(0.0123)	(0.0213)	(0.0135)
			(0.0105)				
Separate regressions:							
$renegotiation_{D=0}$	0.1600	0.1810	0.0839	0.6084***	0.4315**	-2.0526	0.1238
	(0.1638)	(0.1305)	(0.1560)	(0.2316)	(0.2036)	(1.9295)	(0.1333)
$renegotiation_{D=1}$	0.4816**	0.5044*	0.4858***	0.1064	0.1753	0.3467***	0.6905**
	(0.1981)	(0.2795)	(0.1811)	(0.1480)	(0.1279)	(0.1309)	(0.2883)
Pooled regressions:							
$renegotiation \times D$	0.3216	0.3233	0.4019*	-0.5020**	-0.2562	2.3993	0.5667*
	(0.2549)	(0.3130)	(0.2229)	(0.2299)	(0.2758)	(1.9695)	(0.2945)

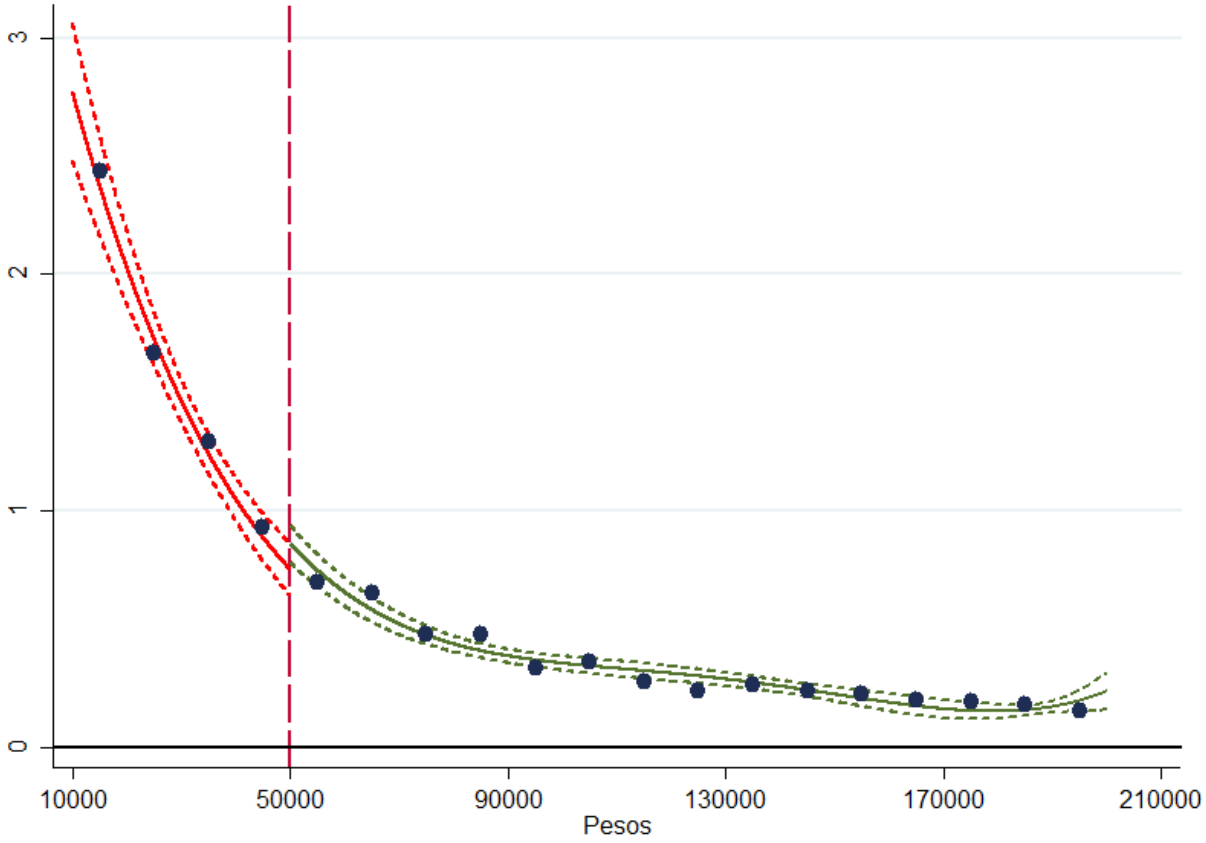
This table shows the estimated π and β coefficients of the fuzzy RD estimation procedure as defined above, run separately for subsamples of borrowers in the renegotiation offer sample whose value of each variable in the table is higher than the sample median. Let $D = 1\,(variable > median)$, the table also presents the interaction coefficients of $treated \times D$ and $renegotiation \times D$ of a regression ran on the full sample,

$$npv\,(4\%) = \alpha + \beta renegotiation + \gamma renegotiation \times D + \epsilon,$$

where *treated* and $treated \times D$ are used to instrument for *renegotiation* and $renegotiation \times D$. The flexible polynomial $f\,(amount - 50,000)$ is also alllowed to vary for borrowers with $D = 1$. These interaction coefficients show whether the coefficients estimated from the separate regressions are statistically different. *, ** and *** represent significance at the 10%, 5%, and 1%, respectively. Standard errors are clustered at the "comuna" level (332 comunas), a Chilean geographical division similar to a U.S. county.

Figures

Figure 9: Cumulative value of new credit in The Store as a fraction of initial balance for 8 to 20 months after January 2010

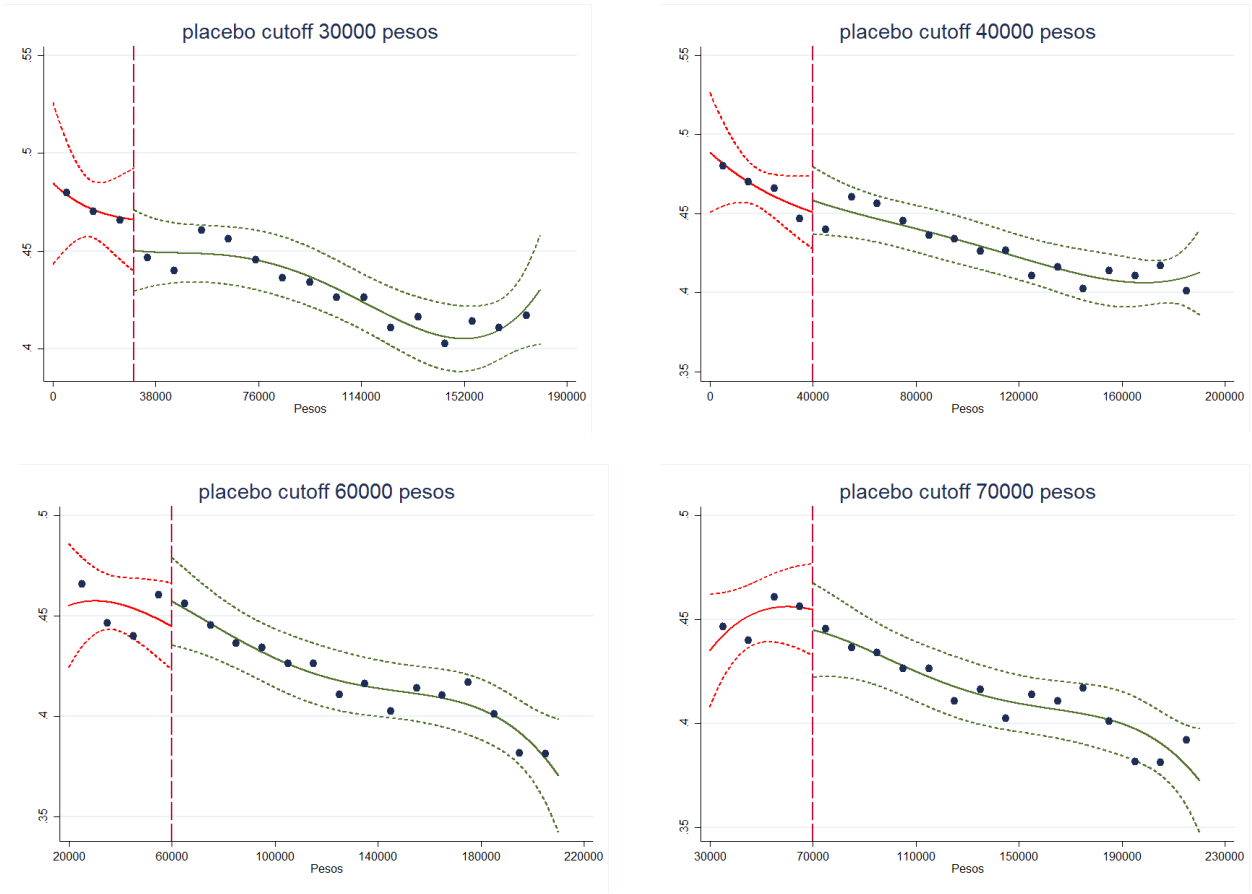


This figure shows that there is no discontinuity on the cumulative value of new credit at The Store as a fraction of initial balance at the phone campaign cutoff, which, if present, would bias the $npv\,(r)$ measure of debt repayment. The figure shows the average of $valuenewcredit8-20$, tthe discounted sum of the value of new credit, including new products bought on credit and cash advances taken by the borrower, between months 8 and 20, divided the outstanding balance as of January 2010 for borrowers in the renegotiation offer sample, in bins of 10,000 Chilean pesos (USD20), and the coefficient β and 90% confidence intervals in the fuzzy RD regression model,

$$valuenewcredit8-20=\alpha_0+\beta\pi treated+f\,(amount-50000)+\epsilon,$$

where $valuenewcredit8-20$ is regressed on $treated$, a variable that equals 1 if the borrower owes more than 50,000 pesos as of Jan 2010 and 0 if she owes less, and $f\,(amount-50000)$, a 4th degree polynomial of the outstanding balance at The Store as of January 2010. The dashed red line represents the 50,000 pesos campaign cutoff. Standard errors are clustered at the "comuna" level (332 comunas), a Chilean geographical division similar to a U.S. county.

Figure 10: Tests for discontinuities in $npv\,(4\%)$ at other balance cutoff



These figures show four plots for the average $npv\,(4\%)$, as defined before, grouped in bins of 10,000 pesos (USD20) of the balance at The Store on January 2010, and the fitted values and a 90% confidence interval of the regression model in equation (1),

$$npv\,(4\%) = \alpha_0 + \beta treated + f\,(amount - 50000) + \epsilon$$

of each of these variables as outcomes, on *treated*, a variable that equals 1 if the borrower owed more than 30,000, 40,000, 60,000 and 70,000 pesos as of January 2010, respectively, and a 4th degree polynomial for the amount owed as of January 2010. Standard errors are clustered at the "comuna" level (332 comunas).