The Effects of Information on Credit Market Competition: Evidence from Credit Cards

Fritz Foley Agustin Hurtado Andres Liberman

Alberto Sepulveda*

March 2019†

Abstract

We investigate the effect of credit information on credit market competition. We first present a simple model that shows that by reducing asymmetric information across lenders, public credit information leads to more credit to borrowers in good standing but less credit to new, relatively riskier borrowers with limited credit histories. Using individual by lender level data for the universe of credit card borrowers in Chile, we compare the initial credit card contracts offered by banks, which share their borrowers' credit histories through credit bureaus, with non-bank issuers, which only share information about borrowers who default and are thus relatively better informed about their own borrowers' creditworthiness. Consistent with the simple model, non-bank issuers lend lower amounts to riskier borrowers and increase limits over time, while banks lend higher amounts to safer borrowers. To identify the causal effect of information sharing on ex post competition, we exploit a natural experiment where a non-bank lender's credit card portfolio was sold to a bank. After the transaction, the lender's borrowers receive higher credit limits from other banks. The lender responds by increasing credit limits to existing borrowers and starts originating new cards with higher limits to observably safer borrowers. Our results imply that public credit information increases competition in credit markets but can hinder financial inclusion.

Keywords: Information, Consumer Credit, Financial Intermediaries

JEL codes: G21, D12, D82, D40

*Foley is at Harvard Business School, email: ffoley@hbs.edu. Hurtado is at University of Chicago Booth School of Business, email: ahurtado@chicagobooth.edu. Liberman is at New York University, email: aliberma@stern.nyu.edu. Sepulveda is at SBIF, email: asepulveda@sbif.cl. We thank Gonzalo Maturana, Alejandro Micco, Scott Nelson, Jose Tessada, Luigi Zingales, and seminar participants at the SBIF and SSEM meetings for comments and suggestions. Dalya Elmalt provided outstanding research assistance. All errors and omissions are our own.

†First version: October 2018.

I. Introduction

In the presence of asymmetric information between borrowers and lenders, incumbent lenders can earn rents from their more creditworthy borrowers. This market power arises because incumbent lenders become relatively better informed than other lenders about the quality of their borrowers, in particular, by observing whether borrowers repay their debts on time (Sharpe (1990), Petersen and Rajan (1995), Dell'Ariccia, Friedman, and Marquez (1999), Dell'Ariccia (2001)). Public credit information can thus reduce incumbent lenders' market power and improve credit allocations. However, lenders may choose to lend to riskier populations and initially lose money only if ex post rents are large enough to compensate for these losses. Public credit information, which reduces lender's ex post market power, may thus reduce or completely eliminate lenders' incentive to grant credit to observably riskier populations (Petersen and Rajan (1994)).[1] The net welfare effect of public credit information on competition is therefore ambiguous and depends on the trade-off between the distortions induced by incumbent lenders' market power ex post and financial inclusion ex ante.

To date, there is no direct evidence of the effect of credit information on competition in credit markets. To provide this evidence, the econometrician must overcome two empirical challengers. First, data must track lending outcomes across two different information regimes, one where credit information is public and another where it is private to incumbent lenders. However, even when such data are available, a naïve comparison of the lending policies of lenders that operate under different information regimes is unlikely to lead to causal inference. For example, cross-country studies, which show that credit information systems are in general associated with better functioning credit markets, cannot identify a causal effect of credit information on allocations through increased competition (Djankov, McLiesh, and Shleifer (2007), Brown, Jappelli, and Pagano (2009), Bruhn, Farazi, and Kanz (2013)). This is partly because lenders that share credit information are likely to have

[1] This parallels the use of patents, which grant market power ex post, as a way to encourage innovation (e.g. Mansfield (1986)).

different lending policies or to operate in different environments than those that do not, irrespective of their information setting, and partly because credit information may have direct effects apart from changing the degree of competition between lenders.[2]

This paper studies empirically the effect of credit information on competition in credit markets. We focus our analysis on the Chilean credit card market, which presents a unique opportunity to overcome the above empirical challenges. In this market there are two types of lenders, banks and non-bank retailers. Retailer credit cards were initially offered as a way to facilitate payments exclusively within the originating retailer. Over time, however, their credit offering has expanded to become virtually indistinguishable from traditional bank cards, that is, unsecured revolving credit cards with low minimum required monthly payments. Crucially for the purposes of this study, retailers and banks in Chile operate in distinct information environments. Banks report to credit bureaus information on the outstanding balance and repayment status of each bank borrower, while retailers only report whether an individual is in default. In particular, outside lenders, banks or retailers, cannot distinguish *retail* borrowers who are not in default, i.e., those who have repaid their debt on-time, from individuals who do not borrow. If lenders and borrowers are asymmetrically informed about their future probability of repayment and if past repayment predicts future repayment, then retailers hold an informational advantage relative to banks over their borrowers. We exploit this asymmetry between banks and retailers to understand how information affects competition and credit allocations.

We perform our empirical analysis using panel data collected by the Chilean banking regulator, SBIF, on the universe of retail and bank credit card borrowers in Chile. The data are broad, encompassing more than 8 million borrowers and 627 million borrower by lender by month observations between 2014 and 2017, and in our empirical analysis we work with a 10% random sample at the individual level. For each individual by lender by month we

[2]For example, public credit information reduces information asymmetries between lenders and borrowers and may provide a disciplining device that increases repayment (e.g., Pagano and Jappelli (1993), Padilla and Pagano (1997), Padilla and Pagano (2000)).

observe credit limits, usage (actual debt balances), and default status. Although in recent years researchers have been able to access and work with large consumer credit micro-level data (e.g., Agarwal, Chomsisengphet, Mahoney, and Strobel (2015)), these data are unique in allowing researchers to observe cards issued to the same individual by multiple lenders for the universe of Chilean credit card borrowers

We begin our analysis by comparing new retailer and bank borrowers in the credit card market. We focus on new borrowers who do not have any credit card at the beginning of our panel and who receive their first credit card during our analysis period. We find that new retailer borrowers are observably riskier: they have lower incomes and, controlling for income, are older than new bank borrowers. New retailer borrowers are also unobservably riskier, at least with respect to the information set available to the econometrician: controlling for observables at origination, retailer borrowers default at a higher rate. We further find that initial credit limits are significantly lower for new retailer borrowers, but retailer borrowers who remain in good standing see a relatively larger increase in their limits over times.

Next, we develop a simple framework of a credit card market with adverse selection that rationalizes the observed stylized facts for first-time borrowers. In the model there are two types of borrowers, G and B. Offering credit cards to G-type borrowers is profitable, but these borrowers only take-up loans of a certain limit or higher. In contrast, lending to B-types, who take up credit cards with any limit, is not profitable.[3] The asymmetric take-up rule induces adverse selection on credit limits. When lenders cannot distinguish types, they offer a pooling contract to all new borrowers only if the degree of information asymmetry, measured as the proportion of B-types in the population, is sufficiently low.[4] In a regime where credit information is not shared, we assume that only incumbent lenders learn their borrowers' type after they lend. We show that in this setting, lenders serve riskier

[3] This asymmetric take-up rule is akin to a more standard story of adverse selection on interest rates where good types drop out of borrowing at relatively high rates (e.g., Stiglitz and Weiss (1981)).

[4] A similar mechanism is discussed in Nelson (2018) and Liberman, Neilson, Opazo, and Zimmerman (2018).

populations because the expected ex post profits from lending to G-types without fear of poaching from other lenders compensate initial losses from lending to the pooled population. We then assume that a credit bureau allows other lenders to also learn a borrower's type. If lenders know a borrower's type, competition drives ex post profits to zero. As a result, in this setting lenders exclude populations that are riskier, that is, with a higher degree of asymmetric information.

The model delivers predictions that are consistent with the stylized facts. Due to their informational advantage, retailers have an incentive to lend to observably riskier populations in order to acquire information about their borrowers. As a result, retailers can offer a low-credit limit card to first-time borrowers because the losses that come from a higher charge-off rate are compensated by offering higher limits to relatively more creditworthy borrowers ex post. The retailer's ex post profits, when they lend larger amounts to less risky borrowers, are not competed away by banks or other retailers due to the presence of adverse selection. The source of this ex-post monopolistic power is the information generated in the first period of lending (e.g., Sharpe (1990), Petersen and Rajan (1994), Padilla and Pagano (1997), Marquez (2002)). Effectively, private information induces adverse selection among ex post entrants, which acts as a barrier to entry in credit markets (e.g., Dell'Ariccia, Friedman, and Marquez (1999), Dell'Ariccia (2001)).[5]

Even though the stylized facts described for the population of first-time borrowers are consistent with and rationalized by our model, other differences between retailers and banks could drive heterogeneous outcomes even in the absence of differences in their credit information disclosure requirements. For example, banks and retailers differ in their management practices, sources of funding, and in their distribution network. Although these differences can be understood as being at least partly endogenous to the credit information setting, identification of the direct effect of information on competition is

[5]Pagano and Jappelli (1993) investigate theoretically how this trade off affects lenders' incentives to disclose information, while Liberti, Sturgess, and Sutherland (2017) shows evidence consistent with this mechanism.

compromised. To circumvent this identification concern, in an idealized experimental setting the econometrician would observe a retailer lender, operating under a limited information sharing environment, that exogenously transitions to a bank-like informational environment. In this case, any change in the lending outcomes of the lender's borrowers can be causally assigned to the change in the informational environment.

In our final empirical analysis we exploit a natural experiment that closely resembles the idealized setting just described. In this experiment, one of the largest Chilean retailers (henceforth, the "Lender") sold its entire credit card portfolio and card origination business to a bank (henceforth, the "transaction"). As a result of the transaction, 1.8 million credit card borrowers who were previously under the retailer's informational regime became observable to other banks in the banking sector's credit registry. We exploit the transaction as a shock to the Lender's existing borrowers' informational regime and credit outcomes, and also investigate how the transaction affects the Lender's new originations.

We have three main findings. First, after the transaction there is an economically large and statistically significant increase in the credit limits of the Lender's borrowers from other banks, relative to the same change for other retail borrowers.[6] This increase is precisely timed around the transaction occurs, with no discernible pre-trends across groups. There is no comparable increase in limits from retailer credit cards, which helps rule out a causal effect of credit information on borrowers' probability of default, which would also explain why banks expand lending.[7] Moreover, consistent with a response to the new competitive environment, the Lender significantly increases credit limits of its own borrowers after the transaction. Second, following Liberman, Neilson, Opazo, and Zimmerman (2018), we construct predictions of the future probability of default for bank cards. For each of the

[6] Our focus on limits stems primarily from the fact that we do not observe prices. However, as in Agarwal, Chomsisengphet, Mahoney, and Stroebel (2018), credit limits are typically thought to be the main margin of adjustment in consumer credit markets.

[7] The causal effect of information on repayment may be explained either by information directly affecting borrowers' access to credit in the future (Garmaise and Natividad (2017), Liberman, Paravisini, and Pathania (2017)) or because of coordination problems in a setting with multiple equilibria (Hertzberg, Liberti, and Paravisini (2011)).

Lender's borrowers, we compute how these predictions shift after the transaction due to the information on the Lender's card that is revealed to banks. That is, we estimate how the change in the informational environment changes how banks perceive the riskiness of lending to each of the Lender's borrowers. This generates heterogeneity in exposure to the informational shock within the Lender's borrowers. We exploit this heterogeneity in a diff-in-diffs specification and show that credit limits increase significantly more among those whose predicted costs drop following the transaction. This strategy isolates the mechanism by which bank credit limits increase following the transaction–a change in the informational environment for banks— and documents a positive effect of more credit information on the Lender's relatively good borrowers.

Third, we find that the Lender shifts originations to new credit card borrowers, who did not have a credit card with any retailer or bank, who are drawn from observably safer populations: new borrowers have higher incomes and are younger. The Lender doubles credit limits at origination for these new borrowers after the transaction, which also results in balances that also twice as large. New borrowers, however, are not more likely to default even when borrowing larger amounts. These borrowers also have higher credit limits from other banks and from other retailers.

Our framework can parsimoniously explain the empirical findings. Prior to the acquisition, individuals who were in good standing with the Lender were indistinguishable from individuals with no credit card. After the acquisition, banks update the expected costs of lending to these borrowers and contest this market by increasing limits. If this information had been made public initially, the Lender would have had less incentives to lend to a pool of riskier individuals, learn who are the better borrowers within the pool, and offer these good borrowers higher limits. These results show the two sides of information on credit market competition: information reduces information asymmetries across banks, leading to better outcomes for good borrowers, but hinders access to credit to good borrowers who are pooled with riskier populations.[8]

Our paper is connected to several academic literatures. First, our paper relates to the literature on relationship lending and competition (e.g., Petersen and Rajan (1994), Petersen and Rajan (1995), Boot and Thakor (2000)).[9] Our paper contributes to this literature by providing evidence consistent with the predictions of models of asymmetric information and the industrial organization of the banking sector, highlighting a potentially deleterious effect of competition on credit allocations in the presence of asymmetric information. Second, our paper is connected to a literature that studies how information sharing affects credit market equilibria, both theoretical (e.g., Pagano and Jappelli (1993), Padilla and Pagano (1997)) and empirical (e.g., Jappelli and Pagano (2002), Djankov, McLiesh, and Shleifer (2007), Bos and Nakamura (2014), Liberman (2016), Dobbie, Goldsmith-Pinkham, Mahoney, and Song (2016)). We show how the structure of credit information impacts banking competition. More broadly, our paper is consistent with a relatively large theoretical literature that studies information problems in credit markets (e.g., Jaffee and Russell (1976) and Stiglitz and Weiss (1981)).

II. Data and Empirical Facts

In this section we introduce the empirical setting, discuss our data, and present relevant summary statistics. We then present a set of stylized facts comparing first-time retailer and bank borrowers.

[8] A similar point is made in Liberman, Neilson, Opazo, and Zimmerman (2018) for deletion of credit information and in Agan and Starr (2017) and Doleac and Hansen (2016) for criminal records in labor markets.

[9] In a recent contribution, Gissler, Ramcharan, and Yu (2018) investigate how more competition may induce more risk-taking by banks in search of profits.

A. The Chilean Credit Card market

Our empirical analysis is set in the Chilean credit card market. In this market there are two main types of credit card lenders, banks and retailers (see Liberman (2016) for background on the Chilean consumer credit market). As of January 2015, there are 17 banks and 6 retailers in Chile. Banks fund themselves primarily through deposits and are subject to regulation from SBIF. As of January 2015, Chilean banks held total assets of $300 billion, approximately 1.3 times GDP.[10] Retailers are typically funded through commercial paper and are not subject to regulation by SBIF. Indeed, until 2014, SBIF had no data on retailers' lending activities at the micro-level, and could only observe consolidated lending volumes across all retailers. Our primary data concerns the universe of credit card borrowers across bank and retail, in Chile. We defer summary statistics to the next subsection.

B. Data

Our data corresponds to a 10% random sample at the individual level of the full SBIF regulatory dataset from 2014 to 2017, which contains retail and bank lenders. We obtain for each individual a full panel at the lender by month level for lenders at which the individual had an active credit card (i.e., cards with a non-zero credit limit or with an active balance). Lenders are categorized into banks and retailers. The categorization emerges directly from the regulatory nature of SBIF: banks fall under the regulatory purview of SBIF, while retailers are not regulated. An individual can borrow from many retailers and many banks in a given month. For each individual-lender-month we observe the credit limit, which corresponds to the total card limit including any amount already used, amount of the limit used, whether a borrower is in default by 30, 60, 90, or 120 days, and whether a particular card was renegotiated.[11] Our data were collected from July 2014 to October 2017 and

[10] All aggregate statistics computed from publicly available data downloaded from www.sbif.cl.

[11] The Internet Appendix contains all variable names and descriptions. Renegotiation is only partially observed for some lenders, hence we do not focus our analysis on it.

contain 62.7 million individual-lender-month observations with a positive credit limit, for 849,449 individuals and 23 lenders.

In Table I we present summary statistics of the full dataset of 62.7 millions observations. On average, credit limits are equal to 1.4 million Chilean pesos, or $2,800, across all lender-individual pairs. On average, individuals hold 373,283 pesos in actual debt balances (close to $750) in each account. The average default rate measured as the proportion of observations with a 90+ days delinquency is 2.2%.

There is substantial heterogeneity in credit limits across types of lenders. We exclude from this categorization borrower-lenders corresponding to the retail lender whose portfolio was sold to a bank in May 2015, which explains the fact that the number of bank observations plus retail observations is less than the total of the two. The average bank account has a limit of 2.4 million pesos, while the average retail account has a limit of 700 thousand pesos, about one third as large. The differences in borrowing are less salient: average bank usage is 523,107 pesos while average retailer usage is 254,975. Retailer borrowers are about 3 times more likely to be delinquent by more than 90 days.

In terms of demographics, 53% of all account-month observations are held by female borrowers. The average borrower age is 47 years old and 65% are married. Individuals are binned into eight categories according to their income as reported by the Chilean IRS, where 1 is the lowest income group and eight is the highest. The average income bin is 1.6, while 60% of the sample belongs to the first bin. Consistent with the differences in limit and usage, there is some heterogeneity in observables across types of lenders: bank account holders are less likely to be female and have higher incomes. In contrast, average age is almost identical across types of lenders.

C. Facts about first-time retail and bank borrowers

We begin our empirical analysis by focusing on the sample of first-time retail and bank borrowers. We define first-time borrowers in our sample as those who do not have a credit card with any lender, bank or retail, prior to October 2014. We also restrict the timing of new borrowing to occur at least 15 months before the last month in our sample. We exclude from the analysis all new borrowers from the lender involved in the transaction documented in Section IV below. This selection procedure leaves us with a total sample of 36,614 first-time bank borrowers and 74,080 first-time retail borrowers between October 2014 and May 2016. This is the analysis sample for this entire section.

In Table II we present summary statistics for first-time borrowers across both types of lenders. Column 4 presents the difference between retail and bank averages, where three stars represent a 1% level of significance of this difference (all differences are significant at this level). First-time retail borrowers are significantly less likely to be women, are older, and are more likely to be married. Crucially, new bank borrowers earn higher incomes, measured both by the level of their income bin and by the fraction of new borrowers who belong to bin one, the lowest income bin. These facts imply that new bank borrowers are observably less risky than new retail borrowers.[12]

We study the dynamic evolution of limits and repayment of first-time borrowers for both types of lenders. We define "event time" in terms of month since the first-time origination (event time zero corresponds to the month in which first-time borrowers obtained their credit card). Figure 1, Panel A, presents the event time evolution of the number of borrowers who have a positive credit limit as a fraction of the event time zero number, for both types of lenders. Most account closures are driven by the lender: credit cards transition to a zero limit when individuals are in default, so this graph is effectively the inverse of the cumulative default rate of initial borrowers. Indeed, Panel B, which shows cumulative default

[12]Internet Appendix Figure A.2 presents histograms of age and income bin, which confirm the different distributions across bank and retail new borrowers.

rates for new borrowers for both types of lenders, confirms the higher default rate of new retail borrowers. The graphs demonstrates that first-time retail borrowers are riskier than first-time bank borrowers: after 15 months, 85% of first-time bank borrowers still have a credit limit, while this fraction is 70% for first-time retail borrowers.[13]

Can differences in observables at origination explain the heterogeneity in future default rates? Table III presents the output of a regression of a dummy that equals one for any default that occurs in the first 12 months, on a dummy that equals one for first-time retail borrowers, and zero for first-time bank borrowers. Column 1 presents the regression output with no controls, which shows that first-time retail borrowers have a 10% higher probability of defaulting in the first year. In column 2 we include fixed effects for month of origination, 5-year age bins, female borrowers, married borrowers, income bin, and county. The difference in default rate between first-time retail and bank borrowers drops to 8.6%, but continues to be statistically significant at the 1% level. Finally, in column 3 we include 5-year age bin by female by month by income bin and by county fixed effects. Note that the inclusion of this fixed effect raises the R^2 of the regression from 7% to 39%. However, first-time retail borrowers still default at an 8.5% higher rate than first-time bank borrowers. This result suggests that first-time retail borrowers are both observably and unobservably riskier. Put differently, the result suggests that lenders know less about borrowers' risk when borrowers are drawn from observably riskier segments of the population.

Figure 2 shows the event time evolution of average credit limits for first-time retail and bank-borrowers. The figure conditions the average on individuals who have positive credit limits, and scales the average limit by the event time zero average. Over the first 6 months both lenders adjust their limits similarly, but after 15 months, first-time retail borrowers who continue to have positive limits have had their limit increased by approximately 70%, while banks have increased limits by approximately 50%.[14]

[13] Internet Appendix Table A.I shows a regression version of these results, which confirms these differences across retail and bank borrowers are statistically significant.

[14] Column 3 in Internet Appendix Table A.I shows the regression version of this analysis, which suggests

Finally, we obtain access to a separate dataset that contains interest rates for all credit card originations in 2015. In Internet Appendix Table A.II we present summary statistics for interest rates measured at the monthly level for all credit card originations in this period, as well as separately for bank and retailer originations (we exclude the Lender involved in the transaction documented in Section IV below). The table shows that retailers issue credit cards that are higher by on average one percentage point at the monthly level, 12 percentage points in yearly terms. This effect is consistent with the fact that banks lend to observably riskier populations, although it is to a large degree mediated by the difference in the proportion of individuals who get a promotional "zero-rate" type card with a fixed number of installments.

We summarize the findings of this section as follows. First, retailers lend to observably and unobservably riskier populations, who are significantly more likely to default on their new credit cards. Retailers charge higher interest rates for these loans. Second, retailers originate cards with lower limits but increase credit limits to individuals who are not in default by a larger fraction than banks. In the next section we develop a stylized framework based on the different informational environment in which both lenders operate that rationalizes this set of facts.

III. Framework

In this section we develop a simple model of a credit card market with asymmetric information. Then, we derive empirical predictions that rationalize the findings of the empirical analysis in sections II and IV.

that the increase in limits is in the order of 12% higher for retail borrowers and highly statistically significant.

A. Setup

There are two periods and three dates, $t = 0, 1$, and 2. Interest rates are fixed conditional on a vector of observables X_i. [15] In the first part of our analysis we drop all reference to X_i, and assume that the analysis occurs for individuals with equal values for this set of observables.

A.1. Borrowers

There is a continuum of individuals of mass 1 indexed by i. Individuals need a credit card and will accept any credit card with a limit that is higher than a threshold. There are two types of individuals, B and G, who differ in the limit threshold and in the profits they generate to banks, as detailed below. B-type individuals accept a card offer with any positive credit limit, while G-type individuals only accept a credit limit above a threshold $L^{\star}$. Individuals know their type, but banks only know that there is a fraction θ of B-type individuals. This generates adverse selection on credit limits. In particular, θ can be interpreted of as a measure of adverse selection in the market.

A.2. Lenders

There are N>>1 lenders who offer credit cards contracts under a zero-expected profits assumption. All lenders have access to the same cost of funds, which we normalize to zero, and have the same information about borrowers initially. Lenders take rates as given and offer cards with an individual limit up to a total capacity per card of C. Lenders make simultaneous offers for one-period credit card contracts, competing on credit limits (i.e.,

[15] As in Agarwal, Chomsisengphet, Mahoney, and Stroebel (2018) and Liberman, Neilson, Opazo, and Zimmerman (2018), we assume that limits are the main margin of adjustment for the supply of credit cards. Our results assume rates are fixed within a set of observables, and do not preclude variation in rates across groups with different observable characteristics, consistent with the fact that retailers charge higher rates, as shown in Internet Appendix Table A.II. We provide evidence in favor of this assumption in subsection C.1 below.

they have zero expected profits). A lender's expected net benefit of offering a credit line L is equal to RL for G type borrowers, and $-L$ for B types. Borrowers observe all lender offers, and decide whether to accept one offer. Because all lenders are symmetric initially, contract offers will be equivalent, and borrowers choose their contract randomly.

A.3. Equilibria with a credit registry

We study sequential Nash equilibria under different information settings. As a benchmark, under symmetric information about types, all lenders offer G-type individuals a card with a limit equal to C in both periods. G-type borrowers randomly choose which bank to accept an offer from. Banks do not offer credit cards to B borrowers.

Next, we assume that banks do not initially observe borrower type but learn the type of all borrowers in the next period. This is akin to a setting with credit information. A credit card offer to a randomly selected individual from the population for a limit that is higher than $L^\star$ has expected profits equal to $(1-\theta)\,R-\theta$ per dollar of limit in period 1.

We define the parameter $\theta^\star = \frac{R}{1+R}$, and note that the equilibrium depends on the relation between θ and $\theta^\star$. If $\theta < \theta^\star$, lenders offer credit cards to *all* individuals in $t=0$ and $t=1$ with limits equal to the average capacity C. In this economy, adverse selection is low but not very costly, and credit is maximized but misallocated as banks lend to bad types who always default. Conversely, when $\theta \geq \theta^\star$, banks lose money from offering any credit line. Intuitively, when adverse selection is high, no bank lends and the market unravels as in Akerlof (1970).

A.4. Lenders' informational advantage

Next we assume that incumbent lenders are able to observe their own borrowers' type in the next period and that other lenders can never observe borrowers' type. Empirically, this can be thought of as a lender observing past repayment of its own borrowers in a setting with no credit information. This implies that in $t=1$ lenders can offer their $t=0$ borrowers contracts that are contingent on their type.

In a symmetric equilibrium, incumbent banks offer each of their G-type borrowers a credit line of size C in $t = 1$ and make positive profits, while denying credit to all B type borrowers. Thus, banks' expected profits from offering a credit card limit $L > L^{\star}$ to an average individual in $t = 0$ equal:

$$\underbrace{L \times [(1-\theta)\,R - \theta]}_{t=0} + \underbrace{(1-\theta) \times R \times C}_{t=1} = 0.$$

When $\theta > \theta^{\star}$, in $t = 0$ banks lend no more but no less than $L^{\star}$ (to guarantee high types do not drop out of the pool of borrowers) and make negative profits, which they can compensate in $t = 1$ as long as:

$$\theta \leq \theta^{POOLING} = \frac{R}{\frac{L^{\star}}{L^{\star}+C} + R}$$

Intuitively, when adverse selection is not too high ($\theta \leq \theta^{POOLING}$) incumbent lenders invest in $t = 0$ to acquire information about their high-type borrowers. This allows lending to riskier populations with a degree of information asymmetry θ such that $\theta^{\star} \leq \theta \leq \theta^{POOLING}$. Note that these riskier populations would not be offered credit cards unless lenders hold an informational advantage ex post.

A.5. Empirical predictions

The analysis thus far assumes borrowers belong to a population determined by a vector of observable characteristics X_i. For simplicity, we collapse the vector to one observable variable x_i (e.g., income). We assume:

$$\frac{d\theta}{dx} < 0 \qquad (1)$$

Assumption 1 implies that the proportion of B type individuals, and thus the degree of information asymmetry of a particular market, decreases with income. This implies that in a setting with no credit registry, lenders' informational advantage decreases with x_i. In a setting with a credit registry, where there is full competition ex post, individuals with higher income are likely to receive credit cards with larger limits initially. Individuals with lower incomes will not be served. In a setting with no information sharing, poorer individuals may receive a credit card with a lower initial limit, which then increases among good type borrowers.

In the empirical setting, banks observe the repayment of defaulters and non-defaulters at all banks. Thus, banks operate in what we refer to in our model as the full credit information setting. At the same time, retailers operate in a setting where only defaults are observed. Because outside lenders cannot distinguish non-defaulters from the pool of non-borrowers, the market for non-defaulters is similar to the setting with no credit information where retailers hold an informational advantage relative to other lenders. Comparing the no credit information (retailers) and credit information (banks) settings, the framework delivers the following testable implications:

- New retail borrowers have a higher default rate conditional on all observables: this follows from the correlation between observable risk and the fraction of B-types in the economy.

- New retail borrowers have lower incomes and are observably riskier: this follows from the assumption that lenders' informational advantage decreases with observable risk.

- When they lend, banks lend up to their full capacity in $t = 0$ and $t = 1$. Retailers lend a lower initial limit in $t = 0$, and subsequently increase their limit to their full capacity for borrowers who are not in default. Retail limits are thus initially lower but increase proportionally more over time.

- A retail borrower in good standing who becomes identified as such will see an increase in her credit limits from all lenders. In equilibrium, the incumbent lender will also increase the borrower's limit.

In the next section we study a natural experiment that allows us to study the effects of different information regimes on credit market equilibria.

IV. Natural Experiment

In this section we study the transaction by which a large retailer's existing credit card portfolio and new originations were transferred to a bank. We interpret the results of this analysis with the framework developed in section III

A. The transaction

In May 2015, a large Chilean retailer completed of the sale of its credit card portfolio to a bank. The sale had been announced as of June 2014 and was subject to regulatory approval by the local banking regulator. The outcome and timing of such regulatory approval were uncertain. Approval was granted in late April, and the transaction occurred in May. While it is possible that the timing of the transaction may have been anticipated by the Lender or by its borrowers, in our empirical tests we present pre-trends and interpret our results accordingly.

As a result of the transaction, the Lender's credit card portfolio and new originations were transferred to a separate subsidiary of the bank and consolidated into the bank's balance sheet as of May 2015.[16] At that time, the Lender's credit card borrowers were reported by

[16]Formally, the acquiring bank's regular credit card business was maintained separate from the Lender's credit card business. In our data we identify separately the Lender as a stand-alone entity and the bank that acquired it, and focus only on the Lender. The acquiring bank ex-Lender has a relatively small market share in the credit card business, and all the effects documented below are net of the effects on this bank. Additionally, the Lender's parent company owned a bank prior to the transaction, and a small fraction of the Lender's borrowers were clients of this bank. We exclude this bank from the analysis as well.

SBIF's regulatory data to all other banks. The transaction increased the total number of bank credit cards by about 30%, as can be seen in Internet Appendix Figure A.1.

We first study the effects of this transaction on the Lender's existing borrowers. Then we study how the transaction affected the Lender's originations.

B. The effect of the transaction on existing borrowers

The transaction affected all of the Lender's borrowers. Hence, there is no direct control group to which compare the outcomes of this group over time. To construct a reasonable counterfactual for the evolution of bank credit limits among the Lender's existing borrowers, we include in our sample individuals who had a positive credit limit from other retailers as of October 2014. We then collapse our individual-lender-month level sub-sample to the individual-lender type (bank or retailer)-month level, adding up each individual's total bank and retail credit limits each month.[17]

Internet Appendix Table A.III presents preperiod summary statistics for the analysis sub-sample, broken down for all borrowers, and for Lender and non-Lender borrowers. Lender borrowers have higher bank and retail credit limits, and borrow from more lenders. They also have a higher usage and significantly lower default rates. The Lender's borrowers are wealthier, more likely to be female and married, and are older.

B.1. The Lender's credit limits

We first describe the time-series evolution of credit outcomes for the Lender's own credit card. Figure 3 shows the fraction of individuals with positive credit limits with the Lender and the average credit limit by month. The sample corresponds to the Lender's borrowers with a positive credit limit as of October 2014. As the figure shows, the fraction of individuals

[17]In this collapsed dataset, each individual has two observations per month, one for banks and one for retailer credit cards. We balance the individual-month panel by including months in which the individual had a zero bank or retail limit. This setup avoids concerns of selection of those accounts in which an individual will eventually have a credit limit.

with a positive credit limit trends smoothly downwards, with attrition being driven primarily by defaults. But, there is a significant and discontinuous increase in the average credit limit in August 2015, four months after the transaction occurred.

Formally, we run a regression of outcomes of the Lender's own card on event quarters dummies centered at zero around the May-July 2015 quarter in which the transaction is completed,

$$y_{i,t} = \alpha_i + \delta_t + \epsilon_{i,t}.$$

We omit quarter -2, and include up to three quarters after the transaction. Table IV reports the coefficients. In column 1 we confirm the evidence in Figure 4, as the Lender increased credit limits for its own borrowers by approximately 260,000 pesos by quarter 1, a 30% increase relative to the preperiod mean. Column 2 shows that there is no break on the preperiod downward trend on the extensive margin of credit.

The table includes three more outcomes to investigate borrowers' response to the increase in credit limits. The results are largely inconclusive, as we see a slight break in the downward trend of usage, both in terms of balance (column 3) and balance divided by limit, and a modest increase in the propensity to default but no break in trend. These results suggest that the Lender targeted credit limit increases to its best borrowers who do not generally take on more credit, a result that is consistent with Agarwal, Chomsisengphet, Mahoney, and Stroebel (2018).

B.2. Competitive reaction by banks

We exploit the sale of the Lender's portfolio as a shock to the information regime of its existing borrowers. Prior to the transaction, other lenders (banks and retailers) would be able to learn whether a Lender's borrower had defaulted, as the Lender reported its defaulters to the credit bureaus. However, other lenders would not be able to observe the Lender's non-defaulters. After the transaction, the Lender's non-defaulters would be observable by other banks, due to banks regulatory obligation to report all of their borrower's debt balances and repayment status. As a result, after the transaction, other lenders can contest the market for the Lender's non-defaulters. Thus, other banks should increase their credit limits to the Lender's borrowers.

We construct a difference in differences regression to compare the time series evolution of bank credit limits for the Lender's pre-transaction borrowers relative to the evolution of bank credit limits for other retail pre-transaction borrowers. To the extent that in the absence of the transaction, bank credit limits of other retail borrowers would have evolved in parallel to the bank credit limit of the Lender's borrowers, this comparison uncovers the causal effect of public information on banks' credit limits.

In Figure 4 we plot the average bank credit limit of Lender and non-Lender borrowers in our sub-sample by month, normalized to zero as of their beginning of sample levels. The graph suggests that after the transaction occurred in May 2015, other banks increased their credit limits to the Lender and non-Lender borrowers, but the increase is larger for the Lender's borrowers. Moreover, prior to the transaction, both graphs move in parallel, consistent with the identification assumption that underlies a difference-in-differences analysis. The graph also shows an increase in bank limits to non-Lender borrowers after the transaction. Although out of the scope of the analysis in this paper, one way to rationalize this is that banks respond to the presence of the new large bank lender by increasing limits to their own borrowers.[18] Interestingly, this mechanism is also consistent with a change in the competitive environment, but it is not mediated by credit information.

We run the following regression:

$$Limit_{i,t} = \alpha_i + \delta_t + \sum_{\tau=-1}^{3} \beta_\tau \left(Lender_i \times \delta_t\right) + \epsilon_{i,t}, \tag{2}$$

[18]This effect for the control group also confirms the need to use an appropriate control group in any diff-in-diff analysis (i.e., it is typically not correct to assume a counterfactual flat trend for the treated group following the transaction).

where $Limit_{i,t}$ is the individual-level credit limit, α_i and δ_t are individual and quarter fixed effects, where the quarter are centered around May-July 2015, the first quarter post-transaction. $Lender_i$ is a dummy that equals one for individuals who had a positive credit limit with the Lender as of October 2014 and zero for individuals who had a positive credit limit with other retailers as of the same month. Our data include two full quarters pre-transaction. For ease of exposition, we restrict the sample to three quarters post-transaction. We omit the dummy for the first quarter in the sample (quarter minus 2).[19] Thus, the coefficients of interest, β_τ , measure the average change in bank credit limits for the Lender's pre-transaction borrowers relative to pre-transaction non-Lender retail borrowers relative to the November 2014-January 2015 quarter.

Table V formalizes the intuition conveyed by figure 4. Column 1 shows the coefficients of regression (2) using credit limit as the outcome. The preperiod coefficient is negative, but starting in quarter 0, there is an increasing trend in the bank card limits for the Lender's borrowers relative to non-Lender borrowers. The coefficient implies that three quarters after the transaction occurs, bank issued credit limits for the Lender's borrowers increase by 156,000 pesos (approximately $310) more than for other retail borrowers, a 6.7% increase relative to the pre-period mean of 2.3 million pesos.[20]

The results suggest that banks react to the transaction by learning new information from their existing customers who had a Lender card, and, as a result, increasing the credit limits. In column 2 of Table V we use the total number of bank lenders as the outcome variable. The coefficients on this variables suggest that the Lender's borrowers are on a slightly different trend in terms of the number of cards, with no discernible break around the

[19]The choice of months in the sample is inconsequential for the measured effects. The choice of omitted category shifts the level of the coefficient but does not affect the post-period increasing trend.

[20]As our model predicts, the effects of information sharing should be largest for individuals who are revealed to be good after the transaction, i.e., those who are not in default. In Internet Appendix Table A.IV we run a robustness test where we restrict the sample to individuals who are not in default with the Lender or with any retailer prior to the transaction. The results are almost indistinguishable from the main analysis, mostly due to the small number of defaulters in the sample. Below we explore the heterogeneity by predicting how banks react to the information that is revealed.

transaction quarter. We rationalize this as follows. First, as Internet Appendix Table A.III shows, the Lender's borrowers have a significantly higher probability of having a bank credit card than other retail borrowers. Thus, these borrowers are likely to have a higher attrition rate over time. Second, banks react to the new informational environment by increasing credit limits. This anticipates any poaching from other lenders. In equilibrium, the Lender's borrowers have more credit from their existing bank lenders. Third, precisely because of the transaction, the Lender's borrowers have a new bank credit card after the transaction, and thus, a lower demand for new bank cards. Moreover, as we show in Section B.1, the Lender increases their credit limit, which also reduces borrowers incentive to accept a different bank card.

We examine whether borrowers make use of this existing increase in credit. For this, we run regression (2) with credit card usage and usage divided by limit as the outcome variables, shown in columns 3 and 4 of Table V, respectively. In the short-run, the Lender's borrowers do not increase their use of bank credit. Towards the last quarter in the post period there appears to be an increase in usage, probably due to seasonal shopping, but it is small in magnitude, in particular when compared to the increase in credit limits. Moreover, as column 4 shows, there is an immediate and persistent decrease in the ratio of usage to limit. This fact suggests banks increase their credit lines to clients who are already deemed to be good, and who, as a result, do not borrow much. Finally, column 5 of Table V shows that the transaction had no noticeable effect on the propensity of the Lender's borrowers to be in default.

We conduct additional tests that underscore the robustness of our results. First, the Lender's borrowers are wealthier and have more credit before the transaction. To alleviate the concern that the results in Table V are driven by time-series differences in access to credit as a result of this heterogeneity, in Internet Appendix Table A.V we conduct a robustness test where we replace the individual fixed effects in regression (2) with fixed effects formed by the interaction of 5-year age bins, marital status, income bin, retail default status, retail credit limit deciles, bank credit limit deciles, number of bank accounts, and total number of accounts. The results are almost indistinguishable to the main specification.[21] Second, Internet Appendix Table A.VI repeats regression (2) but focusing on retail lending instead of bank lending. The results show a positive pre-trend prior to the transaction, but, contrary to the main results, we see no increase in retail limits on average. The number of retail lenders trends upward, again consistent with the fact that non-Lender borrowers have more retail lenders. This finding, together with the lack of a significant change in the trend of default (column 5 in table V) suggests that the change in the informational environment does not affect borrowers' creditworthiness, e.g. because of a causal effect of credit information on access to credit as in Garmaise and Natividad (2017) and Liberman, Paravisini, and Pathania (2017), or due to banks lack of coordination in a multiple equilibria setting as in Hertzberg, Liberti, and Paravisini (2011). Indeed, a causal effect of information on ability to repay would induce higher limits from retailers and from banks, and not only from banks as we find in our main analysis.

Summarizing, we find that after the transaction, other banks increase lending to the Lender's borrowers. Based on the framework developed in Section III, this result is consistent with a competitive response to the fact that the Lender's "good types" become observable after the transaction, which leads to a contestable market.

B.3. Changes in predicted default

After the transaction other banks are able to observe the Lender's borrowers credit limit and usage. Other banks use the new information revealed from the transaction together with information that is available throughout the sample period (e.g., default on the Lender's card) to re-assess their prediction of the profitability of extending a credit card to an

[21] Panel B in Internet Appendix Table A.V repeats the robustness test of Internet Appendix Table A.IV, which restricts the sample to individuals who are not in default with a retailer or the Lender.

individual. Thus, following the approach in Liberman, Neilson, Opazo, and Zimmerman (2018), we expect a stronger positive effect of the transaction on individuals for whom predicted profitability drops the most after the transaction.[22]

We implement this test by computing two sets of predictions of the probability of default on any bank credit card for the next 6 months at the beginning of the sample period (August 2014). We construct one prediction that uses all information available to banks before the transaction, which includes age, gender, marital status, income bin, bank limit, usage and default status, and retail default status, including the Lender as a retailer. We refer to this prediction for individual i as $\hat{C}_{i,pre}$. Next, we construct a second prediction, referred to as $\hat{C}_{i,post}$, which incorporates all the information used to predict $\hat{C}_{i,pre}$, and adds the Lender's card credit limit and usage. We then compute a measure of change in predicted probability of default for the Lender's existing borrowers as the difference in the (log) predicted default rates,

$$\text{Change in predicted default}_i = ln\left(\hat{C}_{i,post}\right) - ln\left(\hat{C}_{i,pre}\right).$$

We use log differences to account for the different magnitudes of predicted defaults. For example, an individual whose predicted default increases from 1% to 2% will have the same change in predicted default as one for whom predicted default increases from 10% to 20%.

We construct the predictions by training a probit model on a randomly selected 30% sub-sample of the Lender's August 2014 cross-section of borrowers. We then predict the two probabilities of default in the remaining 70% of the data, the testing sample. Internet Appendix Figure A.3 shows a histogram of the change in log predicted default as computed in the testing sample, trimmed at the 1st and 99th percentiles. The distribution is highly negatively skewed, with an average drop of 48.2%, consistent with the average increase in

[22] In our analysis we compare how predicted probabilities of default change among individuals who already have any credit card. Given the data and empirical setting–i.e., because we do not observe individuals without a credit card–, we cannot test the first-order informational effect of the transaction on predicted default, which is to allow banks to distinguish the Lender's borrowers who were not in default from other individuals who were not borrowing at all.

bank credit limits documented in Table V. However, the median borrower only sees a 0.2%% drop in the predicted probability of default.

Internet Appendix Figure A.4 splits the sample of Lender borrowers by decile of the change in log predicted default, and plots the average of several characteristics within each decile. The top four panels exhibit V-patterns, where individuals with increases and decreases in predicted default are similar in age, proportion of female, bank, and retailer limits. These are characteristics that are observable by banks before and after the transaction. The bottom two panels show that individuals with increases and decreases in predicted costs differ in two key characteristics. First, individuals with the largest drops in predicted default have large limits with the Lender, and second, they are much less likely to be in default with the Lender. This is intuitive, as the new information available to banks, their limit (and usage, which shows a pattern very similar to limits), and conditional on limit (and usage) whether they are in default, separates the Lender's good borrowers from the bad.

We implement a difference-in-differences test where we compare the evolution of the Lender's borrowers whose prediction of default drops relative to those whose prediction increases following the transaction. We interact the dummy Predicted Drop with quarter dummies centered at zero as of the May-June 2015 quarter. We then regress the same outcomes as in previous subsection–bank limit, balances, balances over limit, and default–on these interactions and control for individual and month fixed effects:

$$y_{i,t} = \alpha_i + \alpha_t + \sum_{\tau=-1}^{3} \beta_\tau \left(Predicted\ Drop_i \times \delta_t \right) + \epsilon_{i,t}, \tag{3}$$

The omitted category is Lender borrowers whose predicted costs increase and quarter minus 2. Thus, the coefficients measure the relative change in the outcome, e.g. limits, on the Lender's borrowers for whom predicted defaults drop relative to those for whom predicted defaults increase relative to quarter minus 2. The standard identification assumption of this test is that in the absence of the transaction, the trends of individuals with predicted increases and decreases remain flat after the transaction, which we verify with pre-trends. We expect to see no differences in the coefficients prior to event quarter zero, and limit increases after quarter zero.

Table VI presents the results. Column 1 shows that prior to the transaction, bank limits for individuals with predicted increases and decreases are not in different trends. However, after the transaction, there is a sharp increase in limits for individuals for whom predicted defaults decrease. Although pre-trends do not allow for a clean causal interpretation, column 2 shows a small but statistically significant effect on the number of bank lenders for individuals for whom predicted defaults drop, which is contrary to the results in our main test above. This suggests that the average masks heterogeneous effects for individuals with predicted increases and decreases in the cost of lending, as proxied by the default rate. Columns 3 through 5 show that balances increase slightly after the transaction for borrowers with drop in predicted defaults, but balances still are lower as a fraction of limits. In Internet Appendix Table A.VII we present the outcome of regression (3) when the outcomes are now for the Lender's credit card, i.e., as in Table IV. Interestingly, we see that the Lender increases credit limits significantly more for individuals whose prediction of bank default drops as a result of the transaction. This is again consistent with the idea that lenders choose to lend to borrowers who do not necessarily have the highest marginal propensities to borrow.

In sum, the evidence in this section confirms that the informational effects of the transaction are likely to drive the observed effects on credit limits. In particular, this subsection exploits variation within the set of the Lender's borrowers and is thus subject to a different set of identification assumptions relative to the main test above.

C. The effect of the transaction on new borrowers

In this subsection we exploit the transaction to study how the Lender's origination policies change due to the different informational setting. We motivate this analysis with Figure 5, which shows the number of new cards issued by the Lender by month, and the average initial credit limit. New borrowers are defined in the same manner as in Section II as those individuals whose first credit card was issued by the Lender. There is a significant reduction in the number of credit cards issued in the transaction month, but this can be attributed to the transaction affecting normal operations within the Lender. After the transaction, the monthly number of new borrowers remains at roughly 200 (or 2,000 in the full sample from which our data is a 10% random sample). Importantly, the average credit limit to new borrowers rises from approximately 200,000 pesos before the transaction to more than 400,000 pesos after it. According to our framework, this effect can be explained by the fact that banks target observably and unobservably safer borrowers, and by the fact that banks offer initially larger credit limits that do not increase as much over time. We investigate each of these effects separately.

We begin by presenting a regression that compares the origination-time evolution of credit outcomes for Lender borrowers compared to other new retail borrowers. The regression model is:

$$y_{i,t} = \alpha_t + \sum_{\tau=-1}^{3} \beta_\tau \left(Lender_i \times \delta_t \right) + \epsilon_{i,t}, \tag{4}$$

where here t denotes the origination quarter centered at zero in the May-July 2015 quarter, and $y_{i,t,}$ is the origination quarter outcome. The coefficients of interests are β_t, which measure the difference in origination quarter outcomes for the Lender's new borrowers relative to other retail new borrowers, both relative to quarter minus 2.

Table VII presents the regression output for several credit outcomes. Column 1 confirms Figure 5 and shows that after the transaction, new Lender borrowers receive a credit limit that is 249,000 pesos larger, relative to a pre-period mean of 209,000 pesos. As Internet Appendix Figure A.5 shows, initial credit limits for the Lender's new credit cards are in line with other retailers prior to the transaction, and become more like a bank after the transaction.

Column 2 of Table VII shows that the Lender's new borrowers carry larger balances, although column 3 shows that balance do not increase as a fraction of the new, higher credit limits. Column 4 shows that these new borrowers are unconditionally not more likely to default, although they carry a larger balance. Next we investigate whether this effect corresponds to a change in the Lender's lending policy, resulting in a selection of safer borrowers.

In Table VIII we present the output of regression (4) where we focus on observable characteristics of the Lender's new borrowers. There is a discontinuous shift in age notable in column 1, as the Lender shifts originations to new individuals who are two years younger after the transaction. Further, these individuals earn higher incomes. The income bin category is too coarse to capture a significant difference after the transaction, but the coefficients of the interactions of the Lender dummy by event quarter dummies are positive after event quarter zero. Moreover, the fraction of new borrowers who belong to the lowest income bin becomes smaller, and this result is statistically significant at event quarter 3. Finally, the Lender shifts originations from female to male borrowers.

In Table IX we show the results of regression (4) using bank and retail limits. Columns 1 and 3 show that one month after their Lender card is originated, the Lender's new borrowers have significantly higher credit limits. This result persists for at least 12 months after origination, which implies that this effect does not stem from an overall increase in credit limits to all retail borrowers following the transaction–it is only an increase to the Lender's new borrowers whose loan was originated after the transaction. Moreover, the transaction does not change the credit registry information that retailers observe about the Lender's new borrowers (i.e., whether an individual is in default). Thus, this effect can only be explained by differential selection: the Lender starts issuing loans to borrowers who are more likely to receive credit cards from other retail lenders.

Columns 5 and 7 of table IX show that the Lender's new borrowers are slightly more likely to have other bank cards one month after origination by the Lender, and significantly more likely to do so, by 5 to 7 percent, 12 months after origination. This effect is consistent with differential selection of new borrowers after the transaction. Notably, this effect is not consistent with the differential information setting of banks, because borrowers whose loans originate prior to the transaction also become observable by banks after the transaction. Hence, twelve months after the transaction, these borrowers also benefit from the increased competition associated to being bank borrowers.

In sum, the evidence suggests that once the Lender becomes a bank, it originates loans to safer borrowers. In particular, borrowers whose Lender card is issued prior to the transaction are significantly more likely to receive a bank credit card than those whose card is issued prior to the transaction. Together with the evidence on the new contract terms, usage, and default behavior, the results are consistent with the framework developed in Section III. In particular, once the Lender becomes a bank, it's ex post informational advantage is reduced because banks observe all bank debt and defaults for all bank borrowers. This reduces incentives to lend to riskier populations.

We caveat our results by recognizing that, aside from the informational structure, the transaction probably involves other changes to the Lender's management and operations. For example, the availability of funding based on deposits may change the Lender's incentives to screen borrowers over time.[23]

Taken together with the results in the previous subsection, the results in this section of the paper suggest a causal effect of information on the competition for consumer credit borrowers, in the form of higher credit limits from banks for existing Lender borrowers, and with a shift towards safer originations after the transaction. These are consistent with the stylized facts presented in section II, and are rationalized by existing models of competition

[23] However, the Lender's physical distribution network remains intact: the Lender's card is maintained as a separate product from the acquiring bank's pre-existing card, and the Lender's card can only be obtained in the Lender's stores. This remains unchanged from before and after the transaction, and implies that the Lender's pool of potential borrowers who shop at the Lender's stores remains fixed. This does not preclude, however, a shift in originations through mailing campaigns.

in credit markets, as well as by our simple framework in section III.

C.1. Interest Rates

Throughout our analysis we've assumed that credit limits are the main margin of adjustment for credit card contracts, as highlighted in Section III. To validate this assumption, we obtain access to a separate dataset that contains interest rates for all credit card originations during 2015. We cannot merge these data to our main dataset, but we can identify the lender associated with each new origination. In Internet Appendix Table A.VIII we run a diff-in-diffs specification similar to the one shown in equation 2, where the outcome is the monthly interest rate at origination. Each observation corresponds to a new credit card. In columns 1 and 2 we compare the Lender's new originations to Retailer cards, while in columns 3 and 4 the comparison group corresponds to Bank cards. We include month of origination fixed effects (columns 1 and 3), or a more stringent fixed effect that groups cards of the same type (revolving or fixed duration), duration, and month of origination (columns 2 and 4). In all columns we see that after the transaction date, the Lender issues loans at 0.1%-0.2% lower rates, although the results are not statistically significant. Overall, the results support the assumption that in credit card markets the main margin of adjustment is credit limits rather than interest rates.

D. Discussion

The empirical facts derived from the cross section of new borrowers across banks and retailers and from the acquisition of the Lender's portfolio can be parsimoniously explained by the differences in the credit information shared by both types of lenders. As our framework highlights, when lenders are less informed than potential borrowers about their repayment prospects and when past repayment predicts future repayment, credit information provides incumbent lenders with monopoly power over its borrowers. Adverse selection prevents good borrowers from shopping around for a better deals, which as our framework shows typically means a higher credit limit. As a result, credit information improves allocations for good borrowers with good track records. On the other hand, credit information may case good borrowers who have more limited credit histories and are pooled with riskier (e.g. poorer) populations to have less access to credit. The reason is that lenders may only choose to serve riskier populations only when they can compensate initial losses with non-zero profits ex post. This explains the fact that banks lend lower amounts that stay relatively flat over time to safer borrowers. It also explains the fact that, following the transaction, the Lender's existing borrowers see higher credit limits from other banks and that the Lender starts originating cards to safer borrowers.

Alternative stories fail to explain parsimoniously all the empirical findings. Here we show how the evidence helps rule out some of these stories as the single parsimonious explanation behind our findings.

A first alternative story is that credit information causally leads to better repayment, which leads to more credit from banks. Information may improve repayment directly by reducing future liquidity constraints, which reduces their probability of default (Garmaise and Natividad (2017), Liberman, Paravisini, and Pathania (2017)). Information may also improve repayment if banks use public signals to coordinate their lending decisions (Hertzberg, Liberti, and Paravisini (2011)). Although this mechanism is likely to exist, it cannot explain all of our findings. First, if the Lender's borrowers' become more creditworthy, then all other lenders should increase their limits, not only banks. However, in the Internet Appendix Table A.VI we show that other retailers do not increase credit limits to the Lender's borrowers following the transaction. Second, this mechanism also predicts that individual's probability of default decreases. But Table IV reports that the Lender's default probability does not decrease or change trends after the transaction, although pretrends complicate inference. There is a slight downwards trend in defaults noticeable in Table V, which refers to bank lenders, and in the Internet Appendix Table A.VI for retailer lenders, but there does not seem to be a break in the pretrend.

Second, banks and retailers have different sources of funding. In particular, banks can take deposits, which might shift a bank's incentives to lend to riskier populations (e.g., Ioannidou and Pena (2010)). However, we document that other banks change their lending decisions to some clients once information on these clients becomes public. That is, there is no change over time in the fixed characteristics of banks (or retailers). This mechanism may, however, explain partly the effects on credit limits of the Lender and on the change in originations following the transaction.

A third story is that retailers bundle credit with purchases of products and offer discounts for the use of the card internally at their stores. This would induce selection on borrowers irrespective of the informational regime. But, same as above, there is no change in the fixed characteristics of retailers that would explain how lending from banks would change to the Lender's borrowers. Moreover, after the transaction, the Lender remains connected to the actual retailer: most of its originations are conducted at the stores, and the use of the card is incentivized as a means of payment for purchases in these stores.

In sum, although these alternative mechanisms may be present they fail to explain all our findings from both empirical strategies. In contrast, the effect of credit information on competition can parsimoniously explain the totality of our findings.

V. Conclusion

In this paper we show that credit information directly affects competition and the industrial organization of credit markets. We first show cross sectional facts about the contracts offered to new borrowers for lenders who differ in their information structure. We complement this analysis with causal evidence that exploits a causal experiment that approximates an idealized setting to study the interaction of information and competition.

As a result of our analysis, several conclusions emerge. First, retailers, who enjoy rents provided by the structure of their information sharing mechanism enable individuals who are not served by traditional banks to access credit markets. Forms of information other than what is typically captured in a bank's credit score facilitate this enhanced access to credit. Other differences across lenders may emerge endogenously as a result of this difference. For example, retailers may also endogenously set up structurally lower costs to serve these riskier populations, such as a broader branch network located in shopping malls and lower income neighborhoods.

Second, lenders can learn about the creditworthiness of individuals through lending, screening out bad borrowers, and expanding credit availability to others. Third, the private information developed through this lending process is valuable, and other lenders respond to it when it becomes public by adjusting their credit offerings.

Our findings imply a tradeoff of increased information sharing: reforms with this objective might reduce rents, but they could also reduce financial inclusion. Our study provides evidence on the trade offs that should be considered in the design of information systems that affect lender competition. We leave a full blown welfare analysis of these trade offs for future research.

References

Agan, Amanda, and Sonja Starr, 2017, Ban the box, criminal records, and racial discrimination: A field experiment, *The Quarterly Journal of Economics* 133, 191–235.

Agarwal, Sumit, Souphala Chomsisengphet, Neale Mahoney, and Johannes Strobel, 2015, Regulating consumer financial products: Evidence from credit cards, *The Quarterly Journal of Economics* pp. 111–164.

Agarwal, Sumit, Souphala Chomsisengphet, Neale Mahoney, and Johannes Stroebel, 2018, Do banks pass through credit expansions to consumers who want to borrow?*, *The Quarterly Journal of Economics* 133, 129–190.

Akerlof, George A, 1970, The market for "lemons": Quality uncertainty and the market mechanism, *The Quarterly Journal of Economics* 84, 488–500.

Boot, Arnoud WA, and Anjan V Thakor, 2000, Can relationship banking survive competition?, *The journal of Finance* 55, 679–713.

Bos, Marieke, and Leonard I Nakamura, 2014, Should defaults be forgotten? evidence from variation in removal of negative consumer credit information, .

Brown, Martin, Tullio Jappelli, and Marco Pagano, 2009, Information sharing and credit: Firm-level evidence from transition countries, *Journal of Financial Intermediation* 18, 151–172.

Bruhn, Miriam, Subika Farazi, and Martin Kanz, 2013, Bank competition, concentration, and credit reporting, Discussion paper The World Bank.

Dell'Ariccia, Giovanni, 2001, Asymmetric information and the structure of the banking industry, *European Economic Review* 45, 1957 – 1980.

———, Ezra Friedman, and Robert Marquez, 1999, Adverse selection as a barrier to entry in the banking industry, *The RAND Journal of Economics* 30, 515–534.

Djankov, Simeon, Caralee McLiesh, and Andrei Shleifer, 2007, Private credit in 129 countries, *Journal of financial Economics* 84, 299–329.

Dobbie, Will, Paul Goldsmith-Pinkham, Neale Mahoney, and Jae Song, 2016, Bad credit, no problem? credit and labor market consequences of bad credit reports, Discussion paper National Bureau of Economic Research.

Doleac, Jennifer L., and Benjamin Hansen, 2016, Does "ban the box" help or hurt low-skilled workers? statistical discrimination and employment outcomes when criminal histories are hidden, Working Paper 22469 National Bureau of Economic Research.

Garmaise, Mark, and Gabriel Natividad, 2017, Consumer default, credit reporting, and borrowing constraints, *The Journal of Finance* 72.

Gissler, Stefan, Rodney Ramcharan, and Edison Yu, 2018, The effects of competition in consumer credit markets, Discussion paper.

Hertzberg, Andrew, Jose Maria Liberti, and Daniel Paravisini, 2011, Public information and coordination: Evidence from a credit registry expansion, *The Journal of Finance* 66, 379–412.

Ioannidou, Vasso P., and Maria Fabiana Pena, 2010, Deposit insurance and bank risk-taking: Evidence from internal loan ratings, *Journal of Financial Intermediation* 19.

Jaffee, Dwight M, and Thomas Russell, 1976, Imperfect information, uncertainty, and credit rationing, *The Quarterly Journal of Economics* pp. 651–666.

Jappelli, Tullio, and Marco Pagano, 2002, Information sharing, lending and defaults: Cross-country evidence, *Journal of Banking & Finance* 26, 2017–2045.

Liberman, Andres, 2016, The value of a good credit reputation: Evidence from credit card renegotiations, *Journal of Financial Economics* 120, 644–660.

——— , Christopher Neilson, Luis Opazo, and Seth Zimmerman, 2018, The equilibrium effects of information deletion: Evidence from consumer credit markets, Discussion paper.

Liberman, Andres, Daniel Paravisini, and Vikram Pathania, 2017, High-cost debt and perceived creditworthiness: Evidence from the uk, .

Liberti, Jose, Jason Sturgess, and Andrew Sutherland, 2017, Economics of voluntary information sharing, Discussion paper.

Mansfield, Edwin, 1986, Patents and innovation: an empirical study, *Management science* 32, 173–181.

Marquez, Robert, 2002, Competition, adverse selection, and information dispersion in the banking industry, *Review of Financial Studies* 15, 901–926.

Nelson, Scott, 2018, Private information and price regulation in the us credit card market, .

Padilla, A.Jorge, and Marco Pagano, 2000, Sharing default information as a borrower discipline device, *European Economic Review* 44, 1951–1980.

Padilla, A. Jorge, and Marco Pagano, 1997, Endogenous communication among lenders and entrepreneurial incentives, *Review of Financial Studies* 10, 205–236.

Pagano, Marco, and Tullio Jappelli, 1993, Information sharing in credit markets, *The Journal of Finance* 48, 1693–1718.

Petersen, Mitchell A., and R.G. Rajan, 1995, The effect of credit market competition on lending relationships, *The Quarterly Journal of Economics* 110, 407–43.

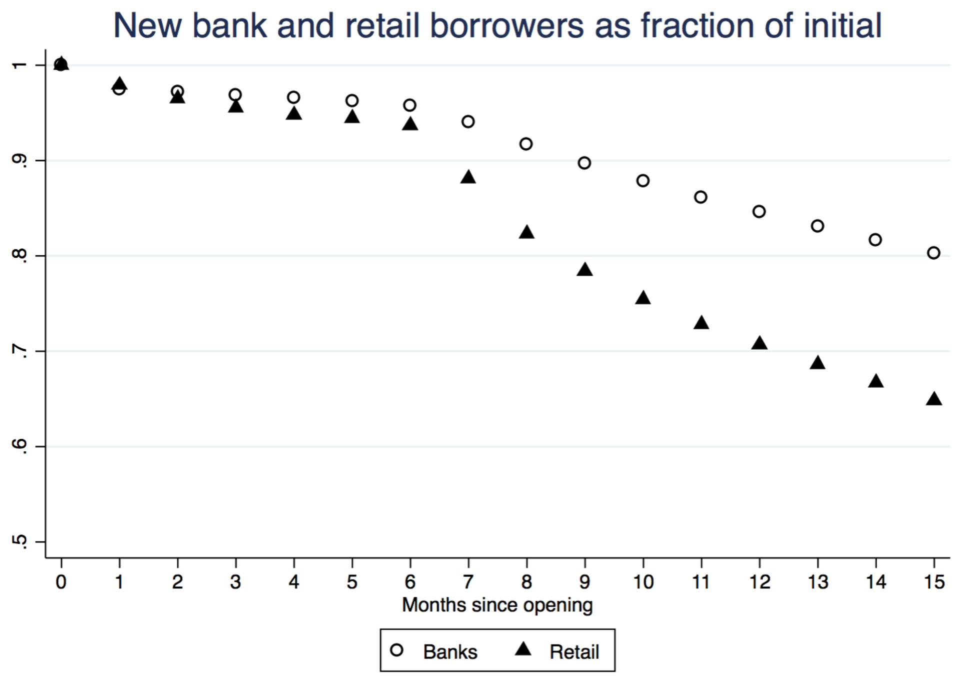
Petersen, Mitchell A., and Raghuram G Rajan, 1994, The benefits of lending relationships: Evidence from small business data, *The Journal of Finance* 49, 3–37.

Sharpe, Steven A., 1990, Asymmetric information, bank lending, and implicit contracts: A stylized model of customer relationships, *The Journal of Finance* 45, 1069–1087.

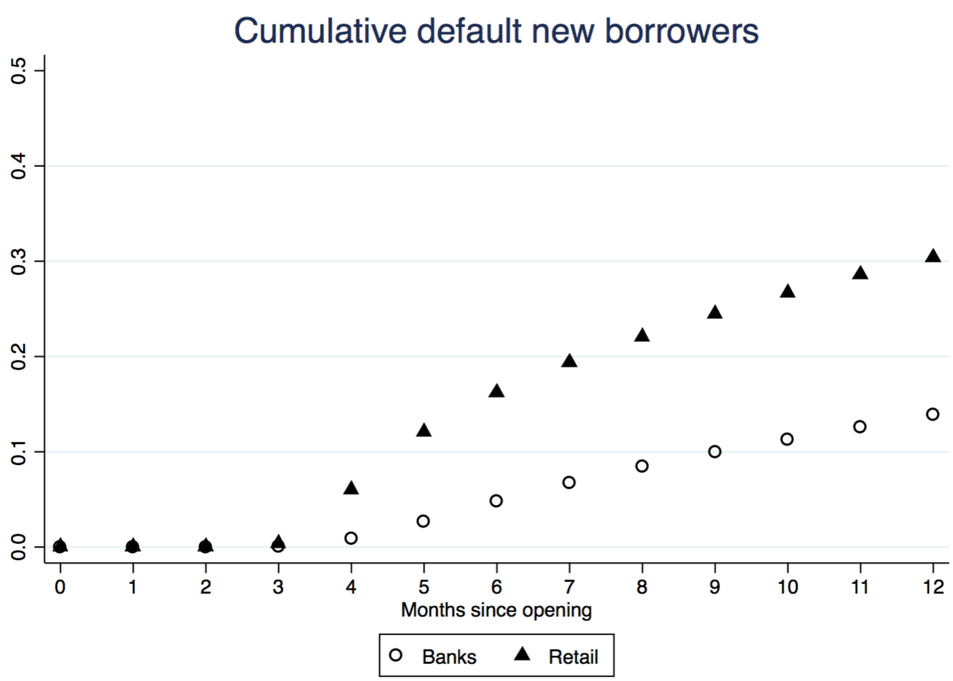
Stiglitz, J.E., and A. Weiss, 1981, Credit rationing in markets with imperfect information, *American Economic Review* 71, 393–410.

Figure 1: Number and cumulative default of new retail and bank borrowers by month since origination

This figure shows the number (Panel A) and cumulative default rate with their initial lender (Panel B) of new retail and bank borrowers by month since origination, scaled by the initial month.



Panel A



Panel B

Figure 2: New borrowers: evolution of credit limits

This figure shows the average credit limit of new retail and bank borrowers by month since first having a positive credit line as a fraction of initial credit limit.

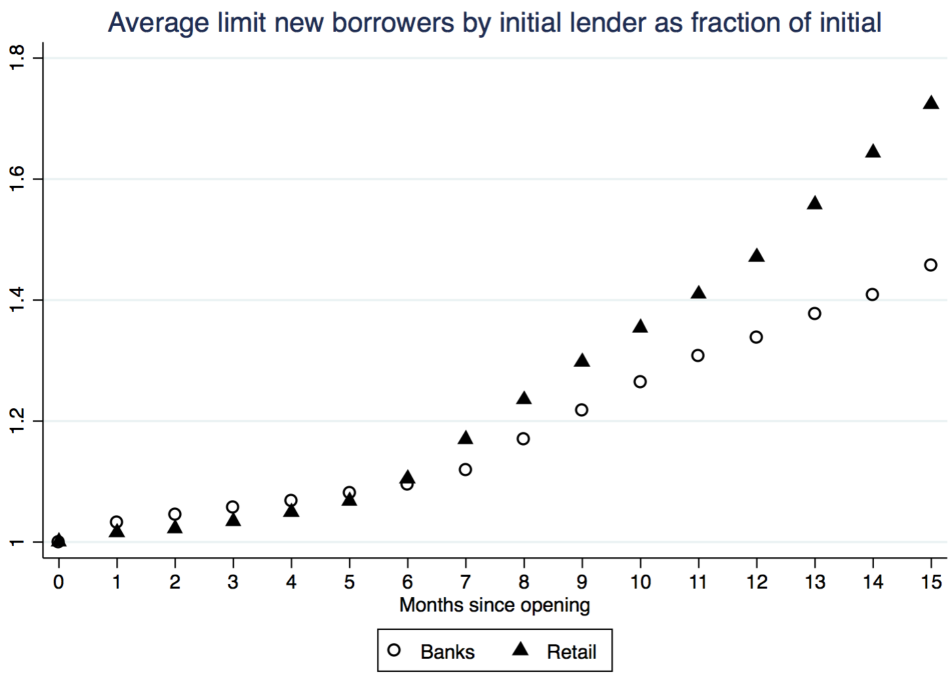


Figure 3: Lender credit limits

This figure shows the evolution of the Lender's average credit limit and the fraction of individuals with positive credit limit. The dashed vertical line represents the month of the transaction.

Figure 4: Bank credit limits for Lender borrowers

This figure shows the time-series evolution of average credit limits from bank credit cards for Lender borrowers and non-Lender retail borrowers. Series are normalized to zero as of their November 2014 level. The dashed vertical line represents the month of the transaction.

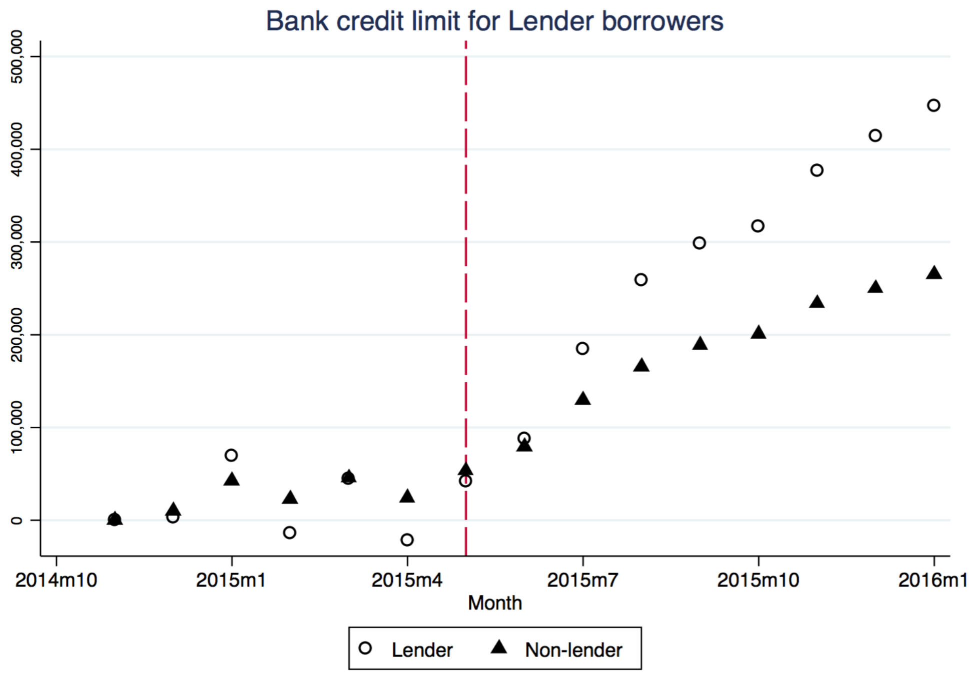


Figure 5: Average credit limit of new Lender borrowers

This figure plots the average credit limit at origination for the Lender's credit card and the number of new Lender borrowers by month of origination. The dashed vertical line represents the month of the transaction.

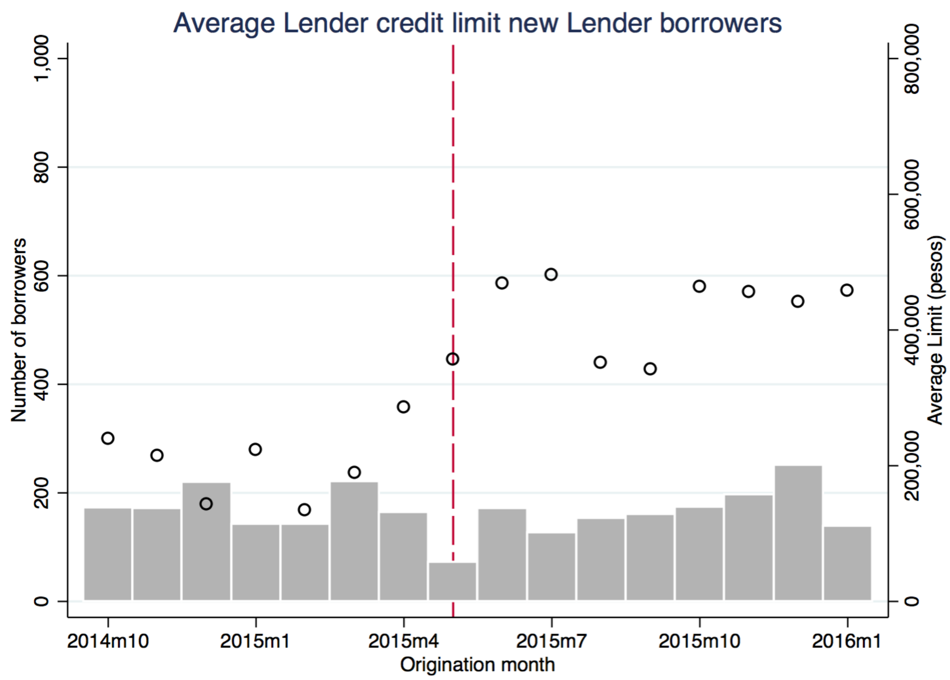


Table I: Summary statistics

This table shows summary statistics of our sample as of August 2014. Sample includes all individuals with a credit card from a bank or a retailer, and excludes borrowers from the Lender involved in the May 2015 transaction as described in the text.

	(1)	(2)	(3)
	All	Bank	Retail
Panel A: Credit Card Characteristics			
Credit Card Limit	1,437,031	2,371,160	699,395
Credit Card Usage	373,283	523,107	254,975
Credit Card Balance/Limit	0.3310	0.2548	0.3912
Number Lenders	2.0777	2.0231	2.1208
Number Lenders with Balance	1.3182	1.1160	1.4778
Credit Card Default	0.0218	0.0103	0.0309
Panel B: Borrower Characteristics			
Monthly income bin	1.64	1.85	1.47
Fraction in income bin 1	0.60	0.52	0.67
Female	0.5304	0.4907	0.5617
Married	0.6582	0.6486	0.6658
Age	47.35	46.49	48.02
Individuals	657,856	434,276	521,904

Table II: Observables at origination

This table shows the mean of selected statistics for all new borrowers (column 1), new bank (column 2) and new retail (column 3) borrowers, and the difference between columns 2 and 3 (column 4). New borrowers are defined as individuals who first appear in the credit card data on or after October 2014. *** represents a 1 percent significance level.

	(1)	(2)	(3)	(4)
	All	Bank	Retail	Retail minus Bank
Monthly income bin	1.0792	1.1160	1.0576	-0.0584^{***}
Fraction in income bin 1	0.8765	0.8602	0.8865	0.0263^{***}
Female	0.5061	0.5267	0.4973	-0.0294^{***}
Married	0.3860	0.3052	0.4268	0.1217^{***}
Age	38.11	34.46	39.95	5.4872^{***}
Individuals	252,992	86,808	160,521	

Table III: New borrowers default

This table presents the output of a regression of default, defined as a payment that is 90 days late or more, on a dummy for new retail borrowers. New borrowers are defined as individuals who first appear in the credit card data on or after October 2014. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)
	Default in 1 year	Default in 1 year	Default in 1 year
New Retail Borrower	0.1003***	0.0864***	0.0847***
	(0.0016)	(0.0040)	(0.0080)
Fixed Effects:			
Month		Y	
5-year age bin		Y	
Female		Y	
Married		Y	
Income bin		Y	
County		Y	
Age bin x Female x Month x Income bin x County			Y
Dep. variable Mean	0.20	0.20	0.20
Observations	247,329	247,329	247,329
R-squared	0.01	0.07	0.39

Table IV: Transaction: Lender Outcomes

This table reports the average difference in credit outcomes for the Lender's own credit among its borrowers relative to event quarter -2. Event quarter is centered at zero around the quarter in which the transaction is announced (May-June 2015). The sample corresponds to all Lender borrowers with a positive credit limit prior to event quarter -2. The data is a balanced panel. Standard errors clustered at the individual level. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)	(5)
	Limit	Has Card	Balance	$\frac{\text{Balance}}{\text{Limit}}$	Default
t_{-1}	$-23,136.54^{***}$	-0.0223^{***}	$-9,319.92^{***}$	0.0006	0.0117^{***}
	(454.06)	(0.0003)	(377.28)	(0.0004)	(0.0003)
t_0	$-18,733.42^{***}$	-0.0419^{***}	$-16,018.72^{***}$	-0.0001	0.0182^{***}
	(663.95)	(0.0004)	(544.88)	(0.0006)	(0.0003)
t_1	$258,318.89^{***}$	-0.0582^{***}	$-17,610.90^{***}$	-0.0057^{***}	0.0220^{***}
	(2,994.57)	(0.0005)	(701.15)	(0.0006)	(0.0003)
t_2	$257,882.71^{***}$	-0.0758^{***}	562.35	0.0160^{***}	0.0245^{***}
	(3,051.73)	(0.0006)	(890.12)	(0.0007)	(0.0003)
Dep. variable Mean	852,809	0.9377	200,998	0.3217	0.0194
Observations	2,696,190	2,696,190	2,696,190	2,501,668	2,501,668
R-squared	0.83	0.75	0.83	0.78	0.44
Clusters	179,746	179,746	179,746	174,458	174,458

Table V: Transaction: Bank Outcomes

This table shows the output of regression (2). The coefficients of interest correspond to the difference in outcome for Lender borrowers relative to non-Lender borrowers. The coefficients of interest correspond to the difference in outcome for Lender borrowers relative to non-Lender borrowers, relative to event quarter -2. Event quarter is centered at zero around the quarter in which the transaction is announced (May-June 2015). The data is a balanced panel with one observation per individual-month. Standard errors clustered at the individual level. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)	(5)
	Limit	Number Lenders	Balance	$\frac{\text{Balance}}{\text{Limit}}$	Default
Lender x t_{-1}	$-34,154.68^{***}$	-0.0057^{***}	$2,441.10$	-0.0000	-0.0047^{***}
	(3,308.29)	(0.0005)	(2,349.96)	(0.0005)	(0.0003)
Lender x t_0	$11,064.13^{*}$	-0.0123^{***}	819.55	-0.0028^{***}	-0.0073^{***}
	(5,755.04)	(0.0009)	(3,609.71)	(0.0007)	(0.0003)
Lender x t_1	$99,830.17^{***}$	-0.0196^{***}	$-5,395.71$	-0.0028^{***}	-0.0088^{***}
	(7,555.16)	(0.0011)	(4,287.44)	(0.0007)	(0.0003)
Lender x t_2	$156,269.53^{***}$	-0.0254^{***}	$10,143.54^{**}$	-0.0028^{***}	-0.0110^{***}
	(12,128.98)	(0.0013)	(4,820.12)	(0.0007)	(0.0004)
Dep. variable Mean	2,383,359	0.9499	548,984	0.2819	0.0109
Observations	7,569,285	7,569,285	7,569,285	4,310,800	4,310,800
R-squared	0.95	0.96	0.87	0.83	0.36
Clusters	504,619	504,619	504,619	305,165	305,165

Table VI: Transaction: Heterogeneity by predicted default

This table shows the output of regression (3), which studies the evolution of bank credit card outcomes for Lender borrowers with decreases in predicted default rate relative to those with predicted increases, relative to event quarter zero. Event quarter is centered at zero around the quarter in which the transaction is announced (May-June 2015). The data is a balanced panel with one observation per individual-month. Standard errors are clustered at the individual level. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)	(5)
	Limit	Number Lenders	Balance	$\frac{\text{Balance}}{\text{Limit}}$	Default
Pred. Def. Drops $\times t_{-1}$	$7,172.54$	0.0114^{***}	38.05	-0.0066^{***}	-0.0056^{***}
	(5,976.89)	(0.0009)	(4,214.01)	(0.0008)	(0.0004)
Pred. Def. Drops $\times t_0$	$90,747.69^{***}$	0.0215^{***}	$-1,081.09$	-0.0108^{***}	-0.0078^{***}
	(10,492.30)	(0.0015)	(6,531.98)	(0.0010)	(0.0004)
Pred. Def. Drops $\times t_1$	$195,166.12^{***}$	0.0322^{***}	$1,181.02$	-0.0117^{***}	-0.0102^{***}
	(13,714.48)	(0.0019)	(7,709.91)	(0.0011)	(0.0005)
Pred. Def. Drops $\times t_2$	$288,236.26^{***}$	0.0437^{***}	$16,488.04^{*}$	-0.0149^{***}	-0.0119^{***}
	(23,948.84)	(0.0022)	(8,682.72)	(0.0012)	(0.0005)
Dep. variable Mean	3,641,122	1.3307	810,628	0.2542	0.0080
Observations	2,500,260	2,500,260	2,500,260	1,825,368	1,825,368
R-squared	0.93	0.96	0.86	0.82	0.34
Clusters	166,684	166,684	166,684	126,252	126,252

Table VII: Transaction: New borrowers credit outcomes

This table reports the average difference in credit outcomes at origination by origination quarter for the Lender's new borrowers relative to new retail borrowers. Event quarter is centered at zero around the quarter in which the transaction is announced (May-June 2015). The sample corresponds to new retail or Lender borrowers. New borrowers are defined as individuals who first appear in the credit card data on or after October 2014.. The data is a cross section, with one observation for each new origination. Standard errors are robust to heteroskedasticity. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)
	Limit	Balance	$\frac{\text{Balance}}{\text{Limit}}$	Default in 1 year
Lender x t_{-1}	$3,514.58$	$-17,840.03^{***}$	-0.0589^{**}	-0.0452^{*}
	(22,726.29)	(6,053.82)	(0.0248)	(0.0255)
Lender x t_0	$249,640.27^{***}$	$37,172.81^{***}$	-0.0662^{**}	-0.0196
	(31,057.14)	(9,805.51)	(0.0259)	(0.0281)
Lender x t_1	$186,294.05^{***}$	$49,366.56^{***}$	-0.0282	-0.0500^{*}
	(24,463.20)	(9,184.43)	(0.0238)	(0.0256)
Lender x t_2	$241,275.00^{***}$	$61,213.59^{***}$	-0.0552^{**}	-0.0344
	(24,478.14)	(9,958.12)	(0.0225)	(0.0248)
Dep. variable Mean	209,596	92,618	0.4840	0.2856
Observations	70,363	70,363	70,363	70,363
R-squared	0.0246	0.0056	0.0131	0.0025

Table VIII: Transaction: Characteristics of new borrowers

This table reports the average difference in characteristics observable at origination by origination quarter for the Lender's new borrowers relative to new retail borrowers. Event quarter is centered at zero around the quarter in which the transaction is announced (May-June 2015). The sample corresponds to new retail or Lender borrowers. New borrowers are defined as individuals who first appear in the credit card data on or after October 2014. The data is a cross section, with one observation for each new origination. Standard errors are robust to heteroskedasticity. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)	(5)
	Age	Income bin	In income bin 1	Female	Married
Lender x t_{-1}	0.05	−0.0158	0.0004	−0.0244	0.0021
	(0.96)	(0.0281)	(0.0197)	(0.0305)	(0.0305)
Lender x t_0	−2.18**	0.0232	−0.0316	−0.0593*	−0.0285
	(1.02)	(0.0359)	(0.0229)	(0.0329)	(0.0325)
Lender x t_1	−2.04**	0.0162	−0.0252	−0.1041***	0.0104
	(0.90)	(0.0275)	(0.0201)	(0.0302)	(0.0301)
Lender x t_2	0.90	0.0448	−0.0532***	−0.1671***	0.0449
	(0.87)	(0.0302)	(0.0201)	(0.0285)	(0.0288)
Dep. variable Mean	40	1.0737	0.9007	0.5115	0.4560
Observations	69,805	67,735	70,363	70,363	70,363
R-squared	0.0034	0.0021	0.0020	0.0026	0.0024

Table IX: Transaction: New borrowers other outcomes

This table reports the average difference in the level of retail and bank credit limits as well as dummy variables that indicate any retail or bank credit limit as of one and twelve months after origination by origination quarter for the Lender's new borrowers relative to new retail borrowers. Event quarter is centered at zero around the quarter in which the transaction is announced (May-June 2015). The sample corresponds to new retail or Lender borrowers. New borrowers are defined as individuals who first appear in the credit card data on or after October 2014. The data is a cross section, with one observation for each new origination. Standard errors are robust to heteroskedasticity. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Retail Limit		Has Retail Limit		Bank Limit		Has Bank Limit	
	Month 1	Month 12	Month 1	Month 12	Month 1	Month 12	Month 1	Month 12
Lender x t_{-1}	$-3,857.21$	-334.61	-0.0304^{**}	0.0410^{*}	$13,776.23$	$-2,295.32$	-0.0095	-0.0008
	(5,812.67)	(13,773.50)	(0.0154)	(0.0236)	(34,184.26)	(54,772.11)	(0.0100)	(0.0191)
Lender x t_0	$19,885.77^{**}$	$39,855.89^{**}$	0.0359^{*}	0.1065^{***}	$-8,290.42$	$44,049.83$	-0.0008	0.0531^{**}
	(8,857.62)	(16,849.47)	(0.0208)	(0.0280)	(13,446.94)	(60,414.38)	(0.0125)	(0.0242)
Lender x t_1	$20,446.06^{**}$	$42,544.41^{**}$	0.0203	0.0928^{***}	$74,181.40^{*}$	$199,431.05^{**}$	0.0285^{**}	0.0624^{***}
	(9,286.87)	(17,912.39)	(0.0185)	(0.0253)	(40,345.74)	(88,945.67)	(0.0136)	(0.0222)
Lender x t_2	$21,121.84^{***}$	$30,699.00^{*}$	0.0441^{**}	0.0625^{***}	$38,840.45$	$70,090.91$	0.0068	0.0756^{***}
	(7,414.50)	(15,858.09)	(0.0188)	(0.0235)	(48,118.55)	(45,636.63)	(0.0116)	(0.0214)
Dep. variable Mean	23,238	65,131	0.0955	0.2023	22,390	116,729	0.0293	0.1413
Observations	70,383	70,475	70,383	70,475	70,383	70,475	70,383	70,475
R-squared	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00