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

Fritz Foley Agustin Hurtado Andres Liberman

Alberto Sepulveda*

July 2019†

Abstract

We show empirically that credit information increases competition in credit markets. We access data that cover all credit card borrowers in Chile and include details about relationship borrowers have with each lender. We exploit a natural experiment whereby a non-bank lender's portfolio was sold to a bank. Because of this transaction, the lender's borrowers, who were previously not identifiable unless in default, become observable by banks through the credit bureau but remain unobservable to other non-bank lenders. Using a difference-in-differences strategy, we find that after the transaction the lender's borrowers receive higher credit limits from other banks relative to other non-bank borrowers. This result is mediated by individuals whose predicted probability of bank default drops as a result of the change to banks' information set. After the transaction, the lender shifts originations to safer borrowers with higher initial limits, a result that is consistent with cross sectional evidence that banks tend to lend to safer borrowers. Our results imply that by increasing competition, public credit information can reduce lenders' incentive to "learn by lending", potentially excluding riskier populations from access to credit.

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 Comision para el Mercado Financiero, email: aasepulveda@cmfchile.cl. We thank Mark Garmaise, Neale Mahoney, Gonzalo Maturana, Alejandro Micco, Scott Nelson, Enrique Seira, Jose Tessada, Luigi Zingales, and numerous seminar participants for comments and suggestions. Dalya Elmalt provided outstanding research assistance. All errors and omissions are our own.

†First version: October 2018.

I. Introduction

When borrowers and lenders are asymmetrically informed about the formers' repayment prospects, incumbent lenders can exert market power over their more creditworthy borrowers because of adverse selection should borrowers choose to switch lenders. As a result, lenders have an incentive to "learn by lending" to riskier populations, losing money initially that can be compensated ex post due to the lack of competition (see e.g. Sharpe, 1990; Petersen and Rajan, 1995; Dell'Ariccia, Friedman, and Marquez, 1999; Dell'Ariccia, 2001).[1] Public credit information can therefore reduce the adverse selection problem and increase ex post competition. However, if credit information is public, then lenders have less incentive to make risky loans that might reveal which borrowers are creditworthy, thus excluding potentially riskier populations from access to credit.

Evaluating the effects of credit information on competition has proven to be challenging for two main reasons. First, data must track credit outcomes across two different information regimes, one where credit information is public and another where it is private to incumbent lenders. Second, 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 the 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 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]

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

[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

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 provides 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 at the retailer's physical stores. 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.[3] 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 over their borrowers relative to other banks. We exploit this asymmetry to study how information affects competition and credit allocations.

We perform our empirical analysis using panel data collected by the Chilean banking regulator, Comision para el Mercado Financiero (CMF), on the universe of retail and bank credit card borrowers in Chile. The data cover each credit card borrower's relationship with each lender in each month, encompassing more than 8 million borrowers and 627 million 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 observe credit limits, usage (actual debt balances), and default status. Although in recent years researchers have been able to access and work with micro-level consumer credit data (e.g.,

Pagano, 1997, Padilla and Pagano, 2000).

[3] We refer to institutions that collect and disseminate credit information interchangeably as credit bureaus or credit registries.

Agarwal, Chomsisengphet, Mahoney, and Strobel, 2015), these data are unique in allowing researchers to track outcomes for cards issued to the same individual by multiple lenders for the universe of Chilean credit card borrowers.

To identify the causal effects of credit information on competition we exploit a natural experiment whereby one of the largest Chilean retailers sold its entire credit card portfolio and card origination business (henceforth, the "Lender") to a bank (henceforth, the "transaction"). As a result of the transaction, 1.8 million credit card borrowers who were previously under the retailers' 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 borrowers' informational regime and credit outcomes, and also investigate how the transaction affects the Lender's new originations.

We implement two related empirical strategies to document how credit information increases competition for the Lender's borrowers. First, using a difference-in-differences strategy, we find that 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 among other retailer borrowers.[4] This increase is timed precisely around when the transaction occurs, with no discernible pre-trends across groups. There is no comparable increase in limits from retailer credit cards, which acts as a placebo test because retailers do not have access to the banking credit registry information about the Lender's borrowers, and also helps rule out a story whereby credit information causally improves borrowers' creditworthiness. We combine the results for banks and retailer cards and find the same results with a triple-differences specification that compares the evolution of bank limits for the Lender's borrowers, before and after the transaction, relative to the same difference for retailer credit card limits.

[4] As in Agarwal, Chomsisengphet, Mahoney, and Stroebel (2018) and Liberman, Neilson, Opazo, and Zimmerman (2018), we focus on credit limits as the main margin of adjustment in consumer credit markets. We show evidence that the Lender's interest rates are almost constant after the transaction. This fact can be partially explained by the presence of rate caps that are typically binding in the Chilean credit card market (see Cuesta and Sepulveda, 2018).

Second, following Liberman, Neilson, Opazo, and Zimmerman (2018), we construct ex ante predictions of the probability of default for bank cards. For each of the Lender's borrowers, we compute how banks' beliefs would shift after the transaction due to the information on the Lender's card that is revealed to banks. Intuitively, the difference in banks' beliefs should be correlated with changes in credit supply differentially only after the transaction has happened, when individuals whose prediction of bank default decreases should receive more credit. We implement a difference in differences specification that shows that bank credit limits increase significantly more among the Lender's borrowers whose predicted costs drop following the transaction. As before, retailer limits do not exhibit this pattern, and the result is also detectable in a triple-diffs specification, that compares the time-series evolution of bank limits relative to retail limits for the Lender's borrowers whose predicted default drops relatives to those for whom it increases. This test isolates the mechanism by which bank credit limits increase following the transaction–a change in the informational environment for banks. Moreover, this strategy implies similar estimates for the effects of information while relying on a completely different identification assumption from the first diff-in-diffs.

Next we study how the informational environment affects credit card originations at the Lender around the transaction. Using a diff-in-diffs strategy that compares the Lender cards to other new retail cards we find that immediately after the transaction the Lender, who becomes a bank, shifts originations to borrowers who have higher incomes. The Lender doubles credit limits at origination after the transaction, but new borrowers are not more likely to default even when borrowing cards with larger limits. These borrowers also receive higher credit limits from other banks and from other retailers, whose information structure remains intact, which is consistent with the Lender selecting more creditworthy borrowers after the transaction.

The differences in the initial contracts and characteristics of the Lender's new borrowers can be rationalized as a consequence of differences in market power induced by the credit information setting. When credit information is private, the Lender serves riskier populations with lower credit limits on average, as the expected profits from lending higher limits to good types in the future, without fear of poaching from other lenders, compensate initial losses from lending initially to a riskier population. The source of this market power ex post is the information generated in the first period of lending, as in the models in Sharpe (1990), Petersen and Rajan (1994), Padilla and Pagano (1997), and Marquez (2002).[5] In a setting with public credit information, similar to banks in Chile, other lenders learn a borrower's type, and ex post competition drives profits to zero in every period. As a result, the Lender, who must break even initially, serves safer borrowers and offer cards with larger limits.[6] While we recognize that our tests may be confounded by the fact that the Lender itself becomes a bank, with all the organizational changes that this may imply, we emphasize that the shift of originations to safer populations is not a mechanical consquence of the transaction.

We also test for the effects of credit information using the entire cross section of credit card originations to new borrowers during our sample period. This allows for a broader study of the effects of information but comes at the cost of a relatively worse identification. Consistent with the effects that we document for the Lender, we find that new retailer borrowers are observably and unobservably riskier, and receive credit card with limits that are significantly lower. Further, retailer borrowers who remain in good standing see a relatively larger increase in their limits over time, as the retailer is able to screen the good types. Our results therefore suggest a trade-off of credit information, whereby increased information

[5]See also Dell'Ariccia, Friedman, and Marquez (1999), and Dell'Ariccia (2001). 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. Dell'Ariccia and Marquez (2006) investigate how banks' lending standards vary with the market's information structure. Darmouni (2016) shows that these informational frictions limited credit reallocation during the 2007-2009 recession.

[6]In the Appendix we present for completeness a simple model in the style of Akerlof (1970) that illustrates this ideas.

leads to better outcomes for relatively safer populations but may come at the cost of financial exclusion for relatively riskier groups (see e.g. Castellanos, Jiménez-Hernandez, Mahajan, and Seira (2018)).

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).[7] 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. Indeed, an implication of our results is that credit information can hinder access to credit to good borrowers who are pooled with riskier populations.[8] 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; Bouckaert and Degryse, 2004; Bouckaert and Degryse, 2006) 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 directly 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).

[7] In related contributions, Darmouni and Sutherland (2018) show how lenders react to information about their competitor's actions and Gissler, Ramcharan, and Yu (2018) investigate how more competition may induce more risk-taking by banks in search of profits.

[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.

II. Empirical setting and data

In this section we introduce the empirical setting, discuss our data, and present relevant summary statistics.

A. The Chilean credit card market

Our empirical analysis is set in the Chilean credit card market. In this market there are two types of 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 CMF on their capital ratios and information disclosure. Retailers are not regulated on their capital structure and do not share information on their borrowers who are not in default with other lenders. As of January 2015, Chilean banks held total assets of $300 billion, approximately 1.3 times GDP.[9] Retailers are typically funded through commercial paper, bank debt, and equity. Both bank and retailer debts are treated symmetrically by the personal bankruptcy law implemented in Chile in 2014 (there is no difference in the expected priority of recoveries of retailers versus banks). Our primary dataset 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 correspond to a 10% random sample at the individual level of the full CMF regulatory dataset from 2014 to 2017, which contains retailer and bank lenders. We obtain for each individual a full panel at the lender by month level for all cards with positive credit limits. Lenders are categorized into banks and retailers. An individual can borrow from many retailers and many banks in a given month. For each individual by lender relationship

[9] All aggregate statistics computed from publicly available data downloaded from http://www.cmfchile.cl.

we observe monthly values of the credit limit, which corresponds to the total card limit including any amount already used, amount of the limit used, and whether a borrower is in default by 90 days.[10] Our data were collected from July 2014 to October 2017 and contain 62.7 million individual-lender-month observations with a positive credit limit, for 849,449 individuals and 23 lenders.

III. Measuring the effect of credit information on competition

In this section we describe the transaction whereby a large retailer's existing credit card portfolio and new originations were sold to a bank. We then exploit this transaction to identify the causal effects of credit information on credit market competition.

A. The transaction

In May 2015 a large Chilean retailer chain completed the sale of its credit card business, the Lender, to a bank. After the sale, the Lender's credit card name remained associated to the retailer's business and the primary source of originations remained at the retailer's physical stores. 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 regulatory approval were uncertain. Approval was granted in late April 2015, and the transaction occurred in May 2015. 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

[10]The Internet Appendix contains all variable names and descriptions.

sheet as of May 2015.[11] At that time, the Lender's credit card borrowers were reported by CMF's regulatory data to all other banks. Retailers do not have access to the regulatory banking data. As a result, there is no change after the transaction in the information that retailers observe about the Lender's clients. The transaction increased the total number of bank credit cards by about 30%, as can be seen in Internet Appendix Figure A.1. We study the effects of this transaction on the Lender's existing borrowers and on the Lender's originations.

B. Identifying the effects of information on competition

The transaction affected all of the Lender's borrowers. To construct a reasonable counterfactual for the evolution of bank credit limits among the Lender's existing borrowers, we focus on an analysis sample that includes all individuals who had a positive credit limit from the Lender or from other retailers as of the first month in our data, October 2014. We then collapse our individual-lender-month level analysis sample to the individual-lender type (i.e., bank or retailer)-month level, adding up each individual's total bank and retail credit limits each month. In this collapsed dataset each individual has two observations per month, one for banks and one for retailer credit cards. We exclude the Lender's own card from either bank or retailer cards. We balance the individual by type of lender by month panel by including months in which the individual had a zero bank or retail limit. This setup avoids concerns of selection of accounts from lenders in which an individual will eventually have a credit limit.

Table I presents preperiod summary statistics for the analysis sample, broken down for Lender and non-Lender retailer borrowers. Lender borrowers have an overall credit card

[11]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 any 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.

limit of 4.6 million pesos (roughly $9,200) while non-Lender borrowers have an overall limit of about 2.4 million pesos. The difference is more pronounced among bank cards, where Lender borrowers have an average limit that is twice as large. We select the sample so that all non-Lender borrowers have at least one retailer credit card with a positive limit, while Lender's borrowers may or may not have a retailer or bank credit card outside from the Lender's. However, the Lender's borrowers are much more likely than the average retailer borrower to have a bank card (74.5% to 47.9%). Lender borrowers also have higher usage and significantly lower default rates. In terms of demographic characteristics, the Lender's borrowers are wealthier, more likely to be female and married, and are older.

B.1. Effect of the transaction on bank card limits relative to other retail borrowers

We use a difference in differences strategy 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. The summary statistics in Table I suggest that Lender and non-Lender borrowers are different. However, to the extent that in the absence of the transaction bank credit limits of non-Lender 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 credit information on credit market competition on limits. We provide the standard evidence in support of this identification assumption in the form of pre-trends.

In Figure 1 we plot the average bank credit limit of Lender and non-Lender borrowers in our subsample by month normalized to zero as of their beginning of sample levels. The graph shows 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.

The graph also shows an increase in bank limits to non-Lender borrowers after the transaction. Although out of the scope of our analysis, one way to rationalize the trend for non-Lender borrower is that banks respond to the presence of a new large bank lender (the Lender) that is presumably interested in increasing its market share by increasing limits to their own borrowers.[12] 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:

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

where $Limit_{i,t}$ is the individual-level credit limit across all bank or retail cards, α_i and α_t are individual and quarter fixed effects, where the quarters are centered at zero 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).[13] 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 (quarter minus 2).

Table II, column 1, which shows the results of specification (1) on the sample of bank credit cards, formalizes the intuition conveyed by figure 1. All units are expressed in thousand

[12] This effect for this 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).

[13] 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.

of Chilean pesos. The preperiod coefficient in period minus 1 is negative (relative to minus 2), and 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. This evidence combines the effect of the transaction on both the intensive and extensive margins.

In column 2 of Table II we present the coefficients of specification (1) where the sample is now limited to retailer limits. All coefficients are small but positive, including in the pretrend. But, importantly, there is no discernible break in the trend after the transaction. Thus, contrary to the effect observed for banks, the transaction generated no change in credit limits for retailer credit cards, which is consistent with the fact that credit information about the Lender's borrowers remains unchanged for retailers. The absence of an effect among retailers also helps rule out stories based on differential demand for credit across the Lender and non-Lender borrowers, and stories based on different credit supply effects due to changes in the risk profile across these two groups.[14]

Finally, in column 3 of Table II we combine these two effects into a triple-differences specification,

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

that compares the evolution of limits issued by banks ($Bank_j = 1$) relative to retailers ($Bank_j = 0$), for the Lender's borrowers relative to other retail borrowers, relative to quarter minus 2. The specification is saturated with fixed effects that absorb all double interactions (individual by month, lender type by month, and lender type by individual). The results

[14]Information can have a causal effect 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).

confirm the intuition of the first two columns, and imply large increases in the bank credit limits of Lender borrowers. These 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, increase the credit limits of their cards.[15]

One potential concern with the results in Table II is that, as shown in Table I, the Lender's borrowers are wealthier and have more credit before the transaction. To alleviate the concern that the results in Table II are driven by time-series differences in access to credit as a result of this heterogeneity, in Internet Appendix Table A.I we conduct a robustness test where we replace the individual fixed effects in regression (1) 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, where all credit outcomes are measures as of the first month of the sample (in the pre-period). It is reassuring that the results are almost indistinguishable from Table II.

In principle, the effects of more competition could also be evident in the extensive margin. To test this, in Internet Appendix A.II we present the output of regressions (1) and (2) where the outcome is a dummy for whether individuals have any credit card. The table shows, however, that this is not true: the Lender's borrowers seem to be on different trends with respect to the probability of having a bank or a retailer card, relative to other retailer borrowers. We rationalize this by observing that the Lender's borrowers are much more likely to have a credit card in the pre-period, which suggests limited scope for an observable effect in the extensive margin. Further, this effect masks heterogeneous effects among individuals who will have fewer cards, both because of attrition due to default and because the Lender is now a bank. We also interpret these results cautiously, as they suggest that in terms of this outcome, retailer borrowers may not form a good counterfactual for the Lender's

[15] In Internet Appendix Figure A.4 and Internet Appendix Table A.IV we present credit card outcomes for the Lender's own credit card. These tests suggest an increase in the average credit limit for the Lender's own credit card in August 2015, four months after the transaction occurred. While this evidence is consistent with the Lender increasing the limits of its own card in response to the increased competition for its borrowers, its also consistent with the change in organizational form from a a retailer to a bank.

borrowers. This fact motivates the need for a second identification strategy that relies on variation within the Lender's own borrowers, which we present next.

B.2. Effect of transaction within the Lender's borrowers: changes in predicted default

We construct a second empirical test to study the effects of credit information on competition that relies on a different identification assumption. The test relies on the following intuition. 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, which is always observed) to re-assess their prediction of the profitability of extending a credit card to an 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.[16] Indeed, as Dobbie, Liberman, Paravisini, and Pathania (2018) show, credit supply typically exhibits a strong correlation with banks' beliefs about future default.

We implement this test within the set of the Lender's borrowers by computing two sets of predictions of the probability of default on any bank credit card for the next 6 months as of the beginning of the sample period. 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 $\hat{C}_{i,pre}$,

[16] In our analysis we compare how predicted probabilities of default change among individuals who already have at least one credit card (from a retailer or a bank). 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.

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%.

To construct the predictions we estimate a probit model of a dummy for bank default in the bext 6 months on the predictors listed above. We randomly select a 30% sub-sample of the Lender's cross-section of borrowers in the first sample month (October 2014) to train the model. We then predict the two probabilities of default and calculate the change in log predicted default. Internet Appendix Figure A.2 shows a histogram of the change in log predicted default 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 bank credit limits documented in Table II. However, the median borrower only sees a 0.2% drop in the predicted probability of default.

Internet Appendix Figure A.3 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 that is 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. To motivate the test, Figure 2 presents average credit limits among bank cards for Lender borrowers whose predicted bank default decreases and those whose predicted bank default increases, both normalized to their level as of November 2014. Prior to the transaction, both series move in parallel, which validates the empirical strategy. Moreover, after the transaction, credit limits increase significantly more among borrowers whose predicted default drops relative to those for whom it increases. As in Figure 1, average limits for individuals whose predicted default drops are also increasing, consistent with the fact that banks expand their credit supply after the transaction.

To construct regression estimates we interact the dummy Predicted Drop with quarter dummies centered at zero as of the May-June 2015 quarter. We then regress card limits on these interactions and control for individual and month fixed effects:

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

The omitted category corresponds to Lender borrowers whose predicted costs increase, and quarter minus 2. Thus, the coefficients measure the relative change in 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 support with pre-trends analysis. We expect to see no differences in the coefficients prior to event quarter zero, and limit increases after quarter zero.

Table III 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. Column 2 shows that the effect is absent for retailer limits, which suggests that the change in predicted default does not capture an overall shift in credit limits from all lenders, which helps rule out differential changes in credit demand. Combining the results in columns 1 and 2 of Table III, column 3 presents the output of a triple diffs specification that includes the triple interaction of time dummies δ_t, Predicted Drop, and the bank cards dummy ($Bank_j$), with fixed effects that absorb all double interactions,

$$Limit_{i,j,t} = \alpha_{i,j} + \alpha_{t,j} + \alpha_{i,j} + \sum_{\tau=-1}^{3} \beta_\tau \left(Predicted\ Drop_i \times Bank_j \times \delta_t \right) + \epsilon_{i,t}. \tag{4}$$

The results confirm that among the Lender's borrowers, bank limits increase substantially more than retailer limits for individuals whose predicted probability of default drops as a result of the change in the information set triggered by the transaction. We note that the two identification strategies in this section rely on different assumptions, and as such underscore the robustness of our findings. Indeed, our tests exploit variation across borrowers from different Lenders *as well* as variation within the Lender's borrowers, and show remarkably consistent estimates of the effects of information on bank competition.

As in Table II, the evidence in Table III combines effects along the intensive and extensive margins. We present in Internet Appendix A.III the outputs of regressions (3) and (4) using a dummy for having any credit card as the outcome, which parallels Internet Appendix Table A.II. We see that the effect on the extensive margin goes in the same direction as the result using credit limits, suggesting small but noticeable effects of the transaction on the probability of having a card. Contrary to Internet Appendix Table A.II, all individuals in this subsample experience the same shock of borrowing from the Lender, that is, a retailer lender that becomes a bank. Therefore, there is no demand effect that would potentially muddle the inference on the number of bank and retail cards that individuals hold. Nonetheless, as in Internet Appendix Table A.II, we interpret this result cautiously and prefer to focus on the results using card limits, which combines the extensive and intensive margins, as the main outcome.

IV. The effect of credit information on originations and borrower outcomes

In this section we evaluate whether the increased competition reduces banks' incentive to lend to riskier populations. In theory, retailer lenders can target riskier populations because of their superior information of the repayment of non-defaulters relative to all other lenders. Further, retailer limits can experiment with limits that are initially lower but increase proportionally more over time. Intuitively, because banks have to break even on every period, as they have no informational advantage after lending, they select safer populations ex ante.[17]

We first exploit the transaction to study how the Lender's origination policies change due to the different informational setting. Next, we expand the scope of the analysis to a broader cross-section of all credit card originations to new borrowers in our data. This broader analysis allows us to study a much larger population but comes at the cost of a stronger identification assumption.

A. Change in the Lender's origination policies

We study how the Lender changes its origination policies as a result of the transaction. We present the output of a regression that compares the origination-time evolution of credit

[17] In the Internet Appendix we present a simple framework based on Akerlof (1970) that shows the theoretical effects of differences in credit information on credit card contracts and lending policies. The framework delivers implications that are consistent with stylized facts shown in the paper.

outcomes and characteristics for Lender borrowers compared to other new retail borrowers. The regression model is:

$$y_{i,t} = \alpha_t + \sum_{\tau=-1}^{3} \beta_\tau \left(Lender_i \times \delta_t \right) + \epsilon_{i,t}, \tag{5}$$

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 IV presents the regression output. Columns 1 and 2 show that the Lender shifts originations to individuals who 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. There is also discontinuous shift in age notable in column 3, as the Lender shifts originations to new individuals who are two years younger after the transaction.

Column 4 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. This result is presented graphically in Figure 3, which shows the average initial credit limit by month of origination.[18] This effect is consistent with the fact that banks target safer borrowers because increased competition reduces ex post profits among good borrowers.[19] Finally, column 5 shows that these new borrowers are unconditionally not more likely to default, although they

[18] The number of credit cards issued in the transaction month drops, which can be attributed to the transaction affecting normal operations within the Lender. After the transaction the monthly number of new borrowers remains as in the preperiod at roughly 200 (or 2,000 in the full sample from which our data is a 10% random sample).

[19] This effect is also consistent with the Lender being constrained prior to the transaction. To distinguish this hypothesis we focus on the observable quality of the new borrowers.

carry a larger balance.

In Internet Appendix Table A.VI we show the results of regression (5) where the outcomes are limits for bank and retail cards. The results suggest that the Lender's new borrowers have significantly higher contempraneous credit limits from other Lenders, a result that persists for at least 12 months after origination. This result is consistent with the Lender targeting safer populations that are more creditworthy as these other lenders' information set remains unchanged after the transaction.

In sum, the evidence suggests that once the Lender becomes a bank, it originates larger loans to safer borrowers. In particular, borrowers whose Lender card is issued after 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, the results suggest that 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.[20] Nonetheless, we point out that the shift of originations to safer populations is not a mechanical consequence of the transaction. Instead, we interpret the results as broadly consistent with the effects of information on competition.

A.1. Interest Rates

Throughout our analysis we've assumed that credit limits are the main margin of adjustment for credit card contracts. To validate this assumption, we obtain access to a

[20] 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.

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. Each observation in the data corresponds to a new credit card.

In Internet Appendix Table A.V we present the output of a diff-in-diffs specification similar to the one shown in equation 1, where the outcome is the monthly interest rate at origination. This specification allows us to measure the change in the interest rate of new credit cards for the Lender after the transaction relative to other bank or retailer credit cards. We see that after the transaction date, the Lender does issue loans at slightly lower rates, although the results are not statistically significant, which is consistent with the assumption that the main margin of adjustment is credit limits rather than interest rates.

B. Credit contracts across banks and retailers in the cross section

Next, we expand the scope of our analysis to complement the findings of the previous section on new credit card contracts. Patterns in cross sectional data also inform the role that information plays in credit market competition. Bank borrowers appear safer; retailer lenders offer lower initial credit limits, gather information beyond observables that inform which borrowers are creditworthy, and increase credit limits to those who prove to be good credit risks. Although a cross sectional analysis is subject to concerns about unobserved variation, these patterns are consistent with the idea that because information is kept private, retailers take on credit risk and learn by lending to risky populations.

We focus 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. 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 subsection.

In Table V we present summary statistics for first-time borrowers across both types of lenders. Column 4 presents the difference in the means of new retailer and bank borrower. First-time 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 (all differences are significant at the 1% level). These facts imply that new bank borrowers are observably less risky than new retail borrowers.

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 where event time zero corresponds to the month in which first-time borrowers obtained their credit card. Figure 4 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. Indeed, Panel B, which shows cumulative default 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 card, while this fraction is 70% for first-time retail borrowers.[21]

Can differences in observables at origination explain the heterogeneity in future default rates? Table VI 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

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

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 5 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%.[22]

Finally, we obtain access to a separate dataset that contains interest rates for all credit card originations in 2015. In Internet Appendix Table A.VIII 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). 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.

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

[22]Column 3 in Internet Appendix Table A.VII shows the regression version of this analysis, which suggests that the increase in limits is in the order of 12% higher for retail borrowers and highly statistically significant.

larger fraction than banks. These findings are similar in nature and complement those found for the Lender following the transaction.

C. Discussion

The empirical facts derived from the acquisition of the Lender's portfolio and from the cross section of new borrowers across banks and retailers can be parsimoniously explained by the differences in the credit information shared by both types of lenders. 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 market power over its borrowers. Adverse selection prevents good borrowers from shopping around for a card with 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 cause good borrowers who have more limited credit histories and who are pooled with riskier (e.g. poorer) populations to have less access to credit. The reason is that lenders may choose to serve riskier populations only when they can compensate initial losses with positive 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.

In general, alternative stories fail to explain parsimoniously all the empirical findings. Here we discuss how the evidence helps rule out some of these stories as the single 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. Second, this mechanism also predicts that individual's probability of default decreases. In Internet Appendix Table A.IX we show that the Lender's borrowers' default probability does not decrease or change trends after the transaction, although pretrends complicate inference.

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 there is no change in the 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.

A fourth alternative is that for reasons unrelated to their information set, banks only lend to other bank borrowers and have little incentives to invest in lending to other populations. As a result, after the transaction, banks would start lending more to the Lender's borrowers because they ar enow bank clients. However, this fails to explain the heterogeneous results among the Lender's borrowers whose predicted default increases and decreases after the change in the information set. This effect is, in fact, only consistent with the mechanism in thie paper, which is that credit supply depends on Lender's beliefs about future default, which are in turn mediated by lenders' information set.

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 exploit a natural experiment to show how credit information increases competition for borrowers. We then show theoretically and empirically how new credit card contracts vary depending on the informational setting.

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 through the learning by lending mechanism. 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 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, .

Bouckaert, Jan, and Hans Degryse, 2004, Softening competition by inducing switching in credit markets, *The Journal of Industrial Economics* 52, 27–52.

——— , 2006, Entry and Strategic Information Display in Credit Markets, *The Economic Journal* 116, 702–720.

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.

Castellanos, Sara, Diego Jiménez-Hernandez, Aprajit Mahajan, and Enrique Seira, 2018, Financial inclusion and contract terms: Experimental evidence from mexico, .

Cuesta, José Ignacio, and Alberto Sepulveda, 2018, Price regulation in credit markets: A trade-off between consumer protection and credit access, .

Darmouni, Olivier, 2016, Informational frictions and the credit crunch, Discussion paper.

———, and Andrew Sutherland, 2018, Learning about competitors: Evidence from sme lending, Discussion paper.

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.

Dell'Ariccia, Giovanni, and Robert Marquez, 2006, Lending booms and lending standards, *The Journal of Finance* 61, 2511–2546.

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.

Dobbie, Will, Andres Liberman, Daniel Paravisini, and Vikram Pathania, 2018, Measuring bias in consumer lending, 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, .

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, Discussion paper.

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.

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: 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.

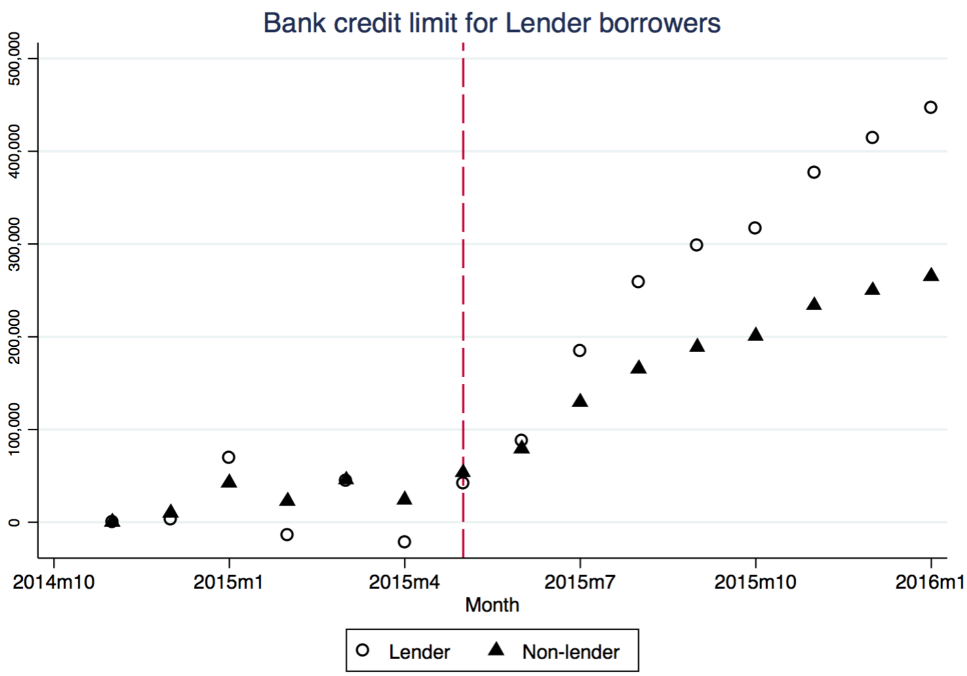


Figure 2: Bank credit limits heterogeneity

This figure shows the time-series evolution of average credit limits from bank credit cards for Lender borrowers whose predicted bank default drops relative to those whose predicted bank default increases. See paper for details on construction of predictions. Series are normalized to zero as of their November 2014 level. The dashed vertical line represents the month of the transaction.

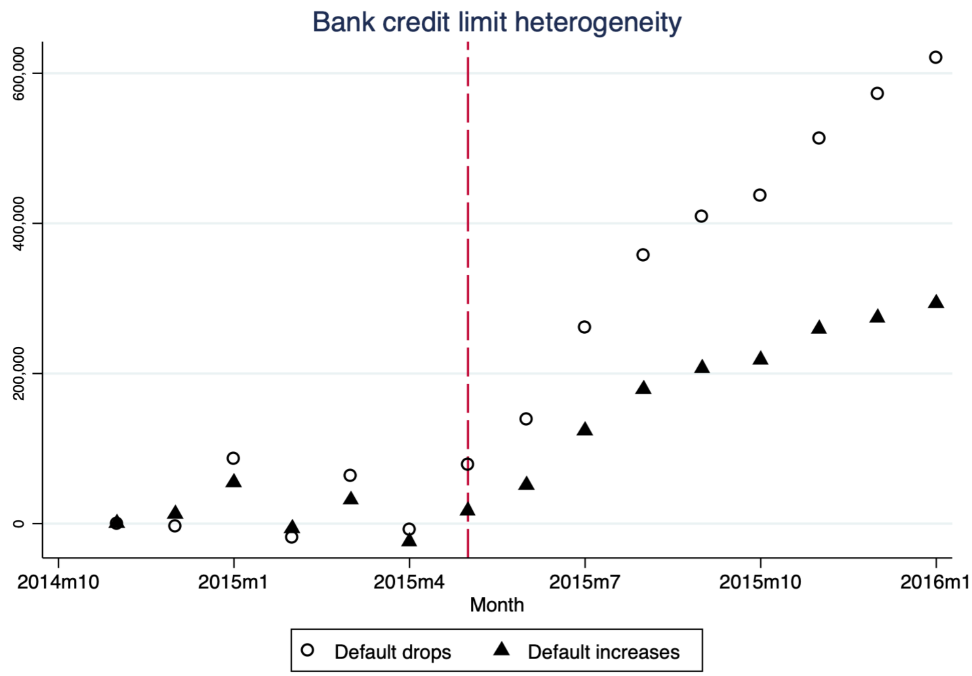


Figure 3: Average credit limit of new Lender borrowers

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.

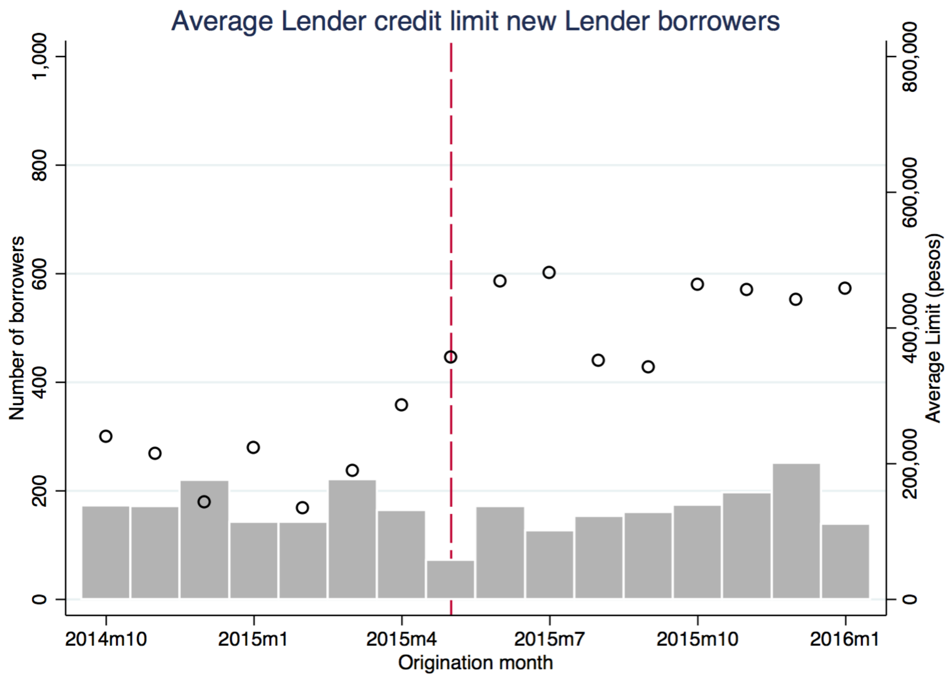
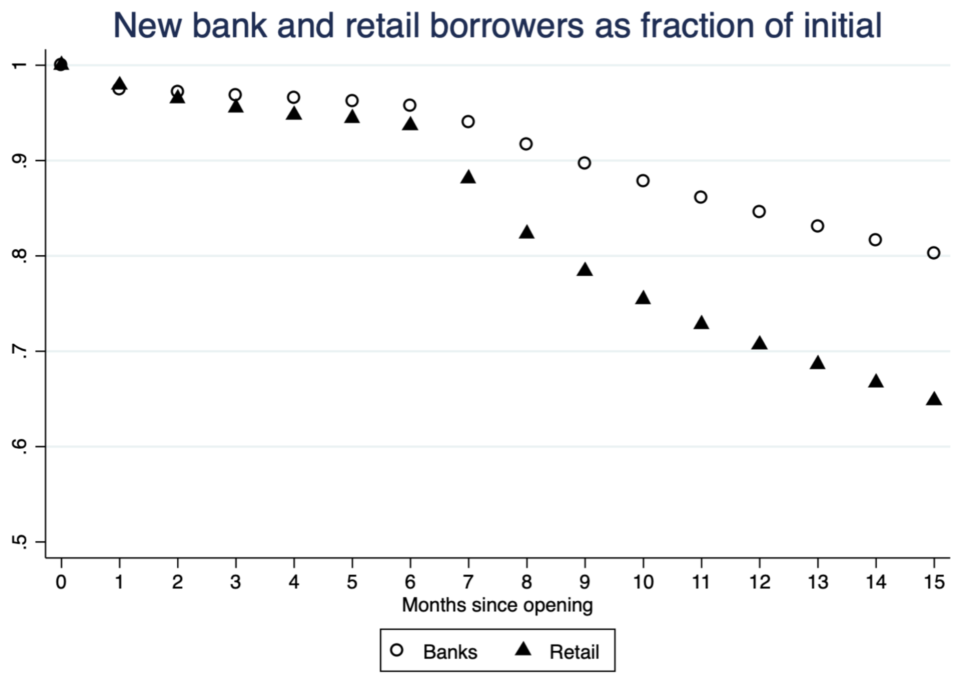
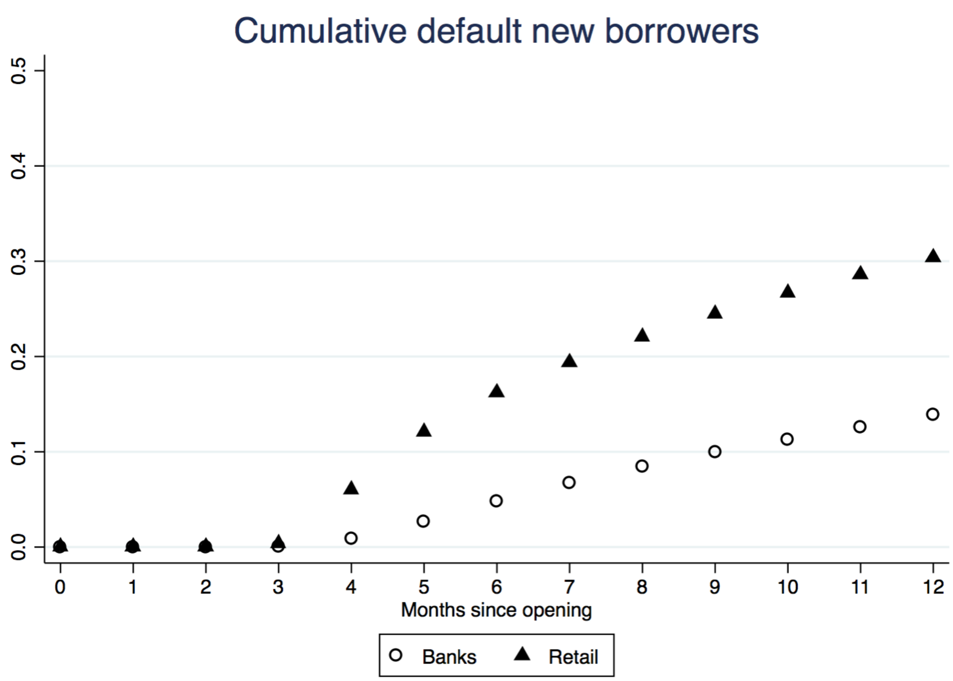


Figure 4: Number and cumulative default of new retail and bank borrowers by month since origination

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 5: 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.

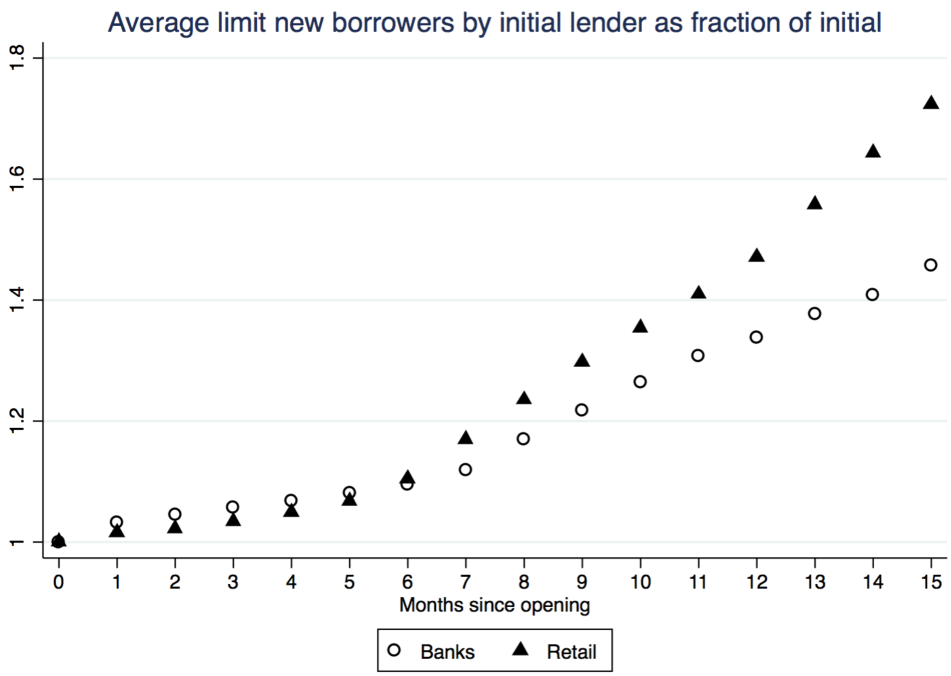


Table I: Preperiod summary statistics for analysis sample

This table shows summary statistics of the sample of who have a retail credit card have a credit as of August 2014. Individuals who have a card with a positive limit with the Lender are labeled as Lender, and individuals who have a card with a positive limit with other retailers are labeled non-Lender.

	(1)	(2)
	Lender borrowers	Non-Lender borrowers
Panel A: Outside Credit Card Characteristics		
Credit Card Limit	4,678,069	2,401,954
Bank Credit Card Limit	3,564,118	1,656,261
Retail Credit Card Limit	1,113,951	745,693
Has Credit Card	0.9013	1.0000
Has Bank Credit Card	0.7450	0.4791
Has Retail Credit Card	0.7665	1.0000
Credit Card Balance	1,161,896	688,890
Bank Credit Card Balance	754,837	375,561
Retail Credit Card Balance	407,059	313,329
Credit Card Default	0.0211	0.0574
Bank Credit Card Default	0.0080	0.0076
Retail Credit Card Default	0.0146	0.0523
Panel B: Lender Credit Card Characteristics		
Lender Credit Card Limit	766,089	0
Has Lender Credit Card	1.0000	0.0000
Lender Credit Card Balance	207,001	0
Lender Credit Card Default	0.0239	0.0000
Panel C: Borrower Characteristics		
Monthly income	957,750	787,206
Income bin	1.6335	1.3256
Female	0.5842	0.5218
Married	0.7021	0.6152
Age	49.66	46.12
Individuals	191,190	328,829

Table II: Change in credit limits following the Transaction

This table shows the effect of the Transaction on credit limits for the Lender's borrowers. Columns 1 and 2 show the output of regression (1), where the coefficients of interest correspond to the difference in outcome for Lender borrowers relative to non-Lender borrowers, relative to event quarter -2. Column 1 reports coefficients for bank issued cards and column 2 reports coefficients for retailer issued cards. Column 3 reports the output of regression (2), where the coefficients of interest correspond to the difference in bank cards relative to retailer cards 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)
	Limit	Limit	Limit
Lender x t_{-1}	-34.15^{***}	26.04^{***}	
	(3.31)	(0.96)	
Lender x t_0	11.06^{*}	26.97^{***}	
	(5.76)	(1.38)	
Lender x t_1	99.83^{***}	15.15^{***}	
	(7.56)	(1.67)	
Lender x t_2	156.27^{***}	24.02^{***}	
	(12.13)	(2.07)	
Lender x Bank x t_{-1}			-60.19^{***}
			(3.43)
Lender x Bank x t_0			-15.91^{***}
			(5.88)
Lender x Bank x t_1			84.68^{***}
			(7.67)
Lender x Bank x t_2			132.25^{***}
			(12.20)
Sample	Banks	Retail	All
Dep. variable Mean	2,383.36	933.02	1,208.27
Observations	7,569,285	7,569,285	15,138,570
R-squared	0.95	0.93	0.98
Clusters	504,619	504,619	504,619

Table III: Heterogeneity by changes in predicted probability of default

Columns 1 and 2 show the output of regression (3), which measures the evolution of credit card limits for Lender borrowers with decreases in predicted bank default rate relative to those with predicted increases, relative to event quarter -2. Column 1 reports coefficients for bank issued cards and column 2 reports coefficients for retailer issued cards. Column 3 reports the output of regression (4), where the coefficients of interest correspond to the difference in bank cards relative to retailer cards for Lender borrowers with decreases in predicted bank default rate relative to those with predicted increases, 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 are clustered at the individual level. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)
	Limit	Limit	Limit
Pred. Def. Drops $\times t_{-1}$	7.04	20.71^{***}	
	(5.98)	(1.68)	
Pred. Def. Drops $\times t_{0}$	90.49^{***}	19.62^{***}	
	(10.49)	(2.43)	
Pred. Def. Drops $\times t_{1}$	194.94^{***}	9.37^{***}	
	(13.71)	(2.92)	
Pred. Def. Drops $\times t_{2}$	288.01^{***}	46.28^{***}	
	(23.95)	(3.63)	
Pred. Def. Drops $\times$ Bank $\times t_{-1}$			-13.68^{**}
			(6.19)
Pred. Def. Drops $\times$ Bank $\times t_{0}$			70.87^{***}
			(10.71)
Pred. Def. Drops $\times$ Bank $\times t_{1}$			185.57^{***}
			(13.92)
Pred. Def. Drops $\times$ Bank $\times t_{2}$			241.73^{***}
			(24.07)
Sample	Banks	Retail	All
Dep. variable Mean	3,641.12	1,195.67	1,897
Observations	2,500,260	2,500,260	5,000,520
R-squared	0.93	0.94	0.53
Clusters	166,684	166,684	166,684

Table IV: Originations after the transaction

This table reports the average difference in credit outcomes at origination and 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)
	Income bin	In income bin 1	Age	Limit	Default
Lender x t_{-1}	−0.0005	−0.0175	−0.11	1.31	−0.0407
	(0.0197)	(0.0281)	(0.96)	(22.65)	(0.0254)
Lender x t_0	−0.0318	0.0244	−2.55**	237.07***	−0.0159
	(0.0229)	(0.0359)	(1.02)	(30.95)	(0.0281)
Lender x t_1	−0.0233	0.0172	−2.20**	175.55***	−0.0384
	(0.0200)	(0.0278)	(0.90)	(23.68)	(0.0258)
Lender x t_2	−0.0518**	0.0492	0.88	239.33***	−0.0331
	(0.0202)	(0.0316)	(0.87)	(24.51)	(0.0248)
Dep. variable Mean	0.9011	1.0732	40	210	0.2846
Observations	70,337	67,708	69,779	70,337	70,337
R-squared	0.0020	0.0022	0.0034	0.0236	0.0025

Table V: 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^{***}
Age	38.11	34.46	39.95	5.4872^{***}
Individuals	252,992	86,808	160,521	

Table VI: 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