Information Sharing Regimes and Financial Inclusion*

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Abstract

We study how credit information sharing regimes affect credit access. Chile's information-sharing regime in the credit-card market features banks sharing full credit histories, while retailer card issuers keep histories proprietary. Using borrower-level panel data, we document three facts. First, retailers disproportionately serve lower-income and first-time borrowers. Second, conditional on good repayment performance, retailers increase credit limits faster than banks, consistent with *learning by lending* when information remains private. Third, when a major retailer sells its credit card portfolio to a bank, making those histories observable to banks, other banks increase limits for those borrowers, especially for higher-income, lower-risk individuals. Taken together, our findings are consistent with a trade-off between full information sharing regimes and financial inclusion.

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I. Introduction

Well-functioning credit markets require that lenders distinguish between borrowers who are likely to repay and those who are not. A common presumption in the literature on information and credit is that more information about borrower credit histories reduces information asymmetries, leading to more efficient credit allocation. However, this view overlooks the incentives embedded in the information regime itself. When credit histories remain private to the insider lender, that lender may be more willing to extend credit to risky or opaque borrowers because it can *learn by lending* without fear of competitors poaching good clients. This is akin to a patent, as it encourages investment in information production ex-ante by granting an informational monopoly ex-post (Mansfield, 1986). By contrast, when credit histories are public, lenders know any valuable information they produce will be immediately shared, so they have less incentive to lend to borrowers of uncertain quality, knowing that future profits could be competed away.

Empirically evaluating how information regimes shape financial inclusion is challenging for two primary reasons. First, an ideal test requires observing lenders operating under information regimes and data that allow tracking credit outcomes across these regimes. Second, a simple comparison between institutions subject to different regimes may capture differences unrelated to the information regime itself. For example, lenders that share credit information may differ systematically in their lending policies or operate in distinct market environments.[1] Moreover, a public information regime might directly influence borrower behavior by reducing information asymmetries and strengthening repayment incentives, as emphasized by Pagano and Jappelli (1993) and Padilla and Pagano (1997, 2000).

In this paper, we study the effect of information regimes on credit market inclusion using a unique dual-regime setting in the Chilean credit card market. In Chile, two types of credit card lenders coexist: banks and retailers. Crucially, they operate under distinct credit information regimes. Banks are required to share both defaults and repayment behavior in a bank-only credit registry, while retailers report only default information to a retailer-only credit bureau, retaining private information about repayment and credit use. This asymmetry means that non-defaulting retailer borrowers are indistinguishable from individuals with no credit history to outside lenders. In addition, only the originating retailer can observe whether its borrowers repay on time and their credit use, which might create

[1] See, for example, Djankov, McLiesh, and Shleifer (2007); Brown, Jappelli, and Pagano (2009); Bruhn, Farazi, and Kanz (2013). These cross-country studies show that public information regimes are associated with better-functioning credit markets, but cannot identify the causal impact of information regimes on credit allocations.

incentives for retailers to lend in order to learn about creditworthiness—a mechanism we refer to as *learning by lending*. In contrast, banks know that any borrower's performance will be visible to all banks, so *learning by lending* to less creditworthy, informationally opaque borrowers might be less attractive because any gains from identifying a good borrower would be shared with competitors.

We begin by documenting three facts consistent with the view that private information regimes might expand financial inclusion by incentivizing lenders to provide credit in ways that reveal borrower quality. First, retailers disproportionately serve borrowers with lower formal-sector income, underscoring their role in reaching populations with limited access to credit. Second, retailers play a central role in providing credit to first-time borrowers, who comprise 13.5 percent of their clients, compared to 8.1 percent for banks. These borrowers exhibit substantially lower incomes and higher ex-post default rates than their bank counterparts. Third, retailers extend significantly lower initial credit limits to first-time borrowers but increase those limits more rapidly, conditional on good performance. By month 24, average retail limits grow to 250 percent of the initial amount for nondefaulters, compared to approximately 170 percent for banks.

While these facts are consistent with the incentives embedded in private information regimes, they are observational and may reflect differences unrelated to the information environment. For instance, lenders that share credit information may also differ systematically in their lending policies or target markets. Moreover, public information regimes may improve repayment by reducing information asymmetries between borrowers and lenders.

To identify the effect of information regimes on financial inclusion, we exploit a natural experiment in which a large Chilean retailer, henceforth "the Retailer", transferred its credit card portfolio and origination operations to a commercial bank. This transaction shifted the information regime for the Retailer's borrowers from private to public among banks. We study this shift using two complementary empirical strategies. Both strategies draw on a novel dataset constructed from regulatory filings submitted to the Chilean banking authority. The data include the universe of credit card borrowers, covering credit limits, balances, defaults, and lender identity at the borrower-lender-month level. Borrowers can hold multiple accounts across banks and retailers, allowing us to track individual borrowing across lender types and over time. We link these records to administrative tax data, which provide demographic and income information and mitigate concerns about self-reported data.

First, we implement a difference-in-differences design comparing the evolution of bank credit limits for the Retailer's pre-transaction borrowers to those of borrowers from other retailers, whose information regime remained unchanged. Following the transaction, we find

a statistically and economically significant increase in bank credit limits for the Retailer's borrowers. The increase is tightly aligned with the transaction's timing and is not preceded by differential trends across groups. In contrast, credit limits from retailers, which lack access to the newly public information, do not change, serving as a placebo test that strengthens identification.

Second, we exploit within-Retailer heterogeneity in the informativeness of newly available credit information. Specifically, we estimate how bank credit supply responds across borrowers whose predicted default probabilities either rise or fall once the Retailer's data become observable to banks. The intuition is that if banks learn that a borrower's default risk is lower than previously believed, they will increase credit limits, but only after the information becomes public. Using a second difference-in-differences design, we find that bank credit limits increase significantly more for borrowers whose predicted default declines, with no corresponding pattern for retailer-issued credit. This second design isolates the mechanism at work and yields similar estimates to the first design while relying on a distinct identification assumption.

We further show that the now-bank-affiliated Retailer alters its lending policies following the transaction. Compared to the pre-period, it targets higher-income, lower-risk borrowers and extends cards with substantially larger initial credit limits. These changes are consistent with a decrease in financial inclusion driven by the information shift.

Our findings can be parsimoniously explained by the incentives created by differences in information regimes. When lenders are less informed than potential borrowers about repayment prospects, private information regimes grant inside lenders an informational monopoly over their borrowers. In such environments, information frictions prevent good borrowers from shopping around for a card with a higher credit limit. Good borrowers with more limited credit histories can be pooled with riskier populations because lenders can recoup initial losses with positive profits ex-post. In contrast, when credit histories are made public, these informational rents dissipate, weakening incentives to serve borrowers with limited or uncertain repayment prospects. This explanation accounts for both the expansion in bank credit to the Retailer's existing borrowers following the information shock and the Retailer's post-transaction pivot toward safer borrowers and higher initial limits.

Alternative interpretations fail to explain the complete set of empirical results. One possibility is that the acquiring bank simply sought to signal a more aggressive competitive stance, prompting other banks to raise credit limits. However, this narrative cannot explain why the increase in bank credit is concentrated among the Retailer's borrowers, nor why it is limited to those whose newly observable histories suggest lower risk. A second hypothesis

is that credit information improves borrower repayment by relaxing liquidity constraints or coordinating lender behavior. While plausible, this channel would likely induce broad-based increases in credit across all lenders, not just banks. Moreover, we find no evidence that default probabilities declined following the transaction. Other potential explanations, such as differences in funding costs between banks and retailers, bundling of credit with consumption, or mechanical changes in client status once the Retailer became a bank, fail to match the heterogeneity in the credit supply response or the timing of the effects.

Taken together, our results underscore how credit information shapes the incentives of lenders to acquire and act on borrower-specific information. The findings highlight a key trade-off: while public information may enhance competition and borrower discipline, it can also reduce lenders' willingness to serve opaque or high-risk borrowers. This tension has first-order implications for financial inclusion and the design of information-sharing regimes in consumer credit markets.

II. Setting, data, and observational evidence

In this section, we introduce the empirical setting, discuss our data, and present several pieces of observational evidence consistent with the view that allowing lenders to keep credit information private encourages them to provide credit in a manner that reveals creditworthiness and increase financial inclusion.

A. Setting

Our empirical analysis focuses on the Chilean credit card market, where two types of lenders, banks and retailers, operate under distinct regulatory and information regimes. As of January 2015, the market includes 17 banks and 6 retailers that issue credit cards. Banks are required to report both outstanding balances and defaults on all their borrowers to the banking authority. This information is publicly accessible to other banks. In contrast, retailers report only defaults and do so exclusively through privately-owned credit bureaus. As a result, non-defaulting borrowers with retailer credit histories remain invisible to outside lenders, who cannot distinguish them from the unbanked.

Beyond information disclosure, banks and retailers also differ in their funding sources and regulatory oversight. Banks are primarily funded through deposits and are subject to capital regulation by the banking authority, while retailers rely on commercial paper, bank loans, and equity, and are not regulated on capital structure. In terms of legal treatment, bank-

and retailer-issued credit cards are treated symmetrically under Chile's personal bankruptcy law, with no distinction in recovery priority. As of January 2015, all credit cards, regardless of issuer type, are also subject to a uniform interest rate cap of 39%.

B. Data

We construct a novel dataset from regulatory filings to the banking authority. The original data were not research-ready and had to be standardized and cleaned through on-site collection. The resulting panel covers the universe of credit card borrowers between July 2014 and October 2017, at the individual-lender-month level, across bank and retailers.

For each borrower-lender relationship, we observe the total credit limit, the balance, and whether the borrower is in default. Borrowers can hold multiple relationships with both banks and retailers, and our data allow us to track individuals over time and across cards and lender types. We link these data to administrative tax records, which provide demographics and high-quality measures of borrower income, and mitigate concerns about misreporting in self-declared income to lenders.

To facilitate computation in some empirical exercises, we draw a 10% random sample containing 62.7 million individual-lender-month observations, covering 849,449 individuals and 23 lenders. Variable definitions and further details are provided in the Internet Appendix.

C. Observational evidence

Using our data, we show that retailers disproportionately serve borrowers whose creditworthiness is difficult to assess using standard observables, who are less creditworthy on average, and who exhibit greater underlying variation in repayment behavior. These individuals are more likely to be female, have low levels of formal sector income, and be first-time borrowers relative to their bank counterparts. Retailers extend low initial credit limits to these borrowers and increase those limits more steeply conditional on good repayment behavior. These patterns suggest that retailers specialize in lending to borrowers without strong observables, often outside the formal credit system, and rely on dynamic information, accumulated throughout the relationship, to screen and retain good credit risks. The evidence is consistent with the idea that private information regimes incentivize lenders to *learn by lending*, particularly to borrowers with limited credit histories. We now examine this evidence in greater detail.

First, using the entire cross-section of borrowers during our sample period, we find that retailers have a larger fraction of female borrowers and a much larger fraction of borrowers

with low levels of formal sector income. Panel A of Table I shows that close to 54 percent of retail-only borrowers are female, compared to 49 percent among bank-only borrowers. The differences are even more pronounced for formal sector income: nearly 90 percent of retail-only borrowers fall into the first income tax bracket, relative to 75 percent of bank-only borrowers. Panel A of Figure 1 further illustrates the strong income gradient in retail credit ownership. The retail card share, defined as the fraction of a borrower's cards issued by retailers, exceeds 60 percent in the lowest income bracket and declines monotonically across higher brackets. These patterns highlight the central role of retailers in extending credit to low-income borrowers who may have limited access to bank credit.

Second, retailers play a central role in providing credit cards to individuals who have not previously held one. We define first-time borrowers as individuals with no credit cards issued by any lender, bank, or retailer prior to July 2014, one month after the start of our sample period. We further restrict new borrowing to occur at least 24 months before December 2017, the end of our sample, in order to observe two-year dynamics for the final cohort entering in December 2015. As shown in Figure 2, first-time borrowers account for 13.5 percent of all retail borrowers, compared to 8.1 percent among bank borrowers, which corresponds to over 130,000 first-time accounts per retailer and just 18,900 per bank. Panel B of Table I shows that first-time retail borrowers have much lower formal sector income: 92 percent fall into the lowest income tax bracket, compared to 85 percent for banks. Consistent with this pattern, Panel B of Figure 1 shows that the retail card share exceeds 70 percent in the lowest bracket and declines steadily across the income distribution. These borrowers also appear less creditworthy: as shown in Figure 3, default rates among first-time retail borrowers exceed 16 percent within six months of origination, nearly four times the rate observed for banks.

Third and last, retailers originate credit cards with much lower initial limits for first-time borrowers but increase those limits more steeply for those who perform well. Panel A of Figure 4 shows that first-time retail borrowers start with average credit limits around 200,000 pesos, far below the 700,000 pesos granted to first-time bank borrowers, and that this gap persists over time. Panel B shows that limit growth is more pronounced among nondefaulting retail borrowers. After the first year, once retailers have closed accounts for early defaulters, average retail limits rise sharply, reaching 250 percent of the initial limit by month 24, compared to approximately 170 percent for banks.

III. Measuring the effect of information regimes on financial inclusion

The evidence presented in the previous section suggests that private information regimes incentivize lenders to *learn by lending*. However, this evidence is observational and may reflect differences unrelated to the information regime itself. For example, lenders that share credit information may differ in their lending policies or operate in distinct market environments. Moreover, public information regimes reduce information asymmetries between lenders and borrowers and may serve as a disciplining device that increases repayment, as emphasized by Pagano and Jappelli (1993), Padilla and Pagano (1997), and Padilla and Pagano (2000). In this section, we exploit a natural experiment to identify the effect of information regimes on financial inclusion. The setting is a transaction in which a large retailer transferred its existing credit card portfolio and the origination of new accounts to a bank, shifting the information regime from private to public for both existing and new borrowers.

A. Natural experiment

In May 2015, a large Chilean retail chain, referred to as the Retailer in the introduction, finalized the sale of its credit card business to a bank. Following the sale, the credit card retained the Retailer's brand and continued to be originated exclusively at the Retailer's physical stores. The transaction was announced in June 2014 and was subject to regulatory approval by the banking authority. Both the outcome and timing of approval were uncertain. Approval was granted in late April 2015, and the transaction closed in May. While it is possible that the timing of the transaction was anticipated by the Retailer or its borrowers, we document pre-trends and interpret the results accordingly.

As a result of the transaction, the credit card portfolio and new originations business previously managed by the Retailer were transferred to a separate subsidiary of the acquiring bank and consolidated into the bank's balance sheet as of May 2015.[2] At that time, the Retailer's credit card borrowers were reported through the banking authority's regulatory data to all other banks. Because retailers do not have access to these regulatory banking

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

data, the information observed by retailers about their own clients did not change following the transaction. The transaction increased the total number of bank credit cards by approximately 30 percent, as shown in Internet Appendix Figure A.1. We study the effects of this transaction on the Retailer's existing borrowers and on its new originations.

B. Identifying the effect of information regimes on financial inclusion

The transaction affected all of the Retailer's borrowers. To construct a reasonable counterfactual for the evolution of bank credit limits among the Retailer's existing borrowers, we focus on an analysis sample that includes borrowers with the Retailer or other retailers as of the first month in our data, July 2014. We then collapse our individual-lender-month level analysis sample to the individual-lender type (i.e., bank or retailer)-month level, aggregating 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 Retailer's own card from either bank or retailer cards.

To facilitate computation, all analyses from this point forward rely on a 10 percent random sample of the universe of credit card borrowers. Table II presents pre-period summary statistics for the analysis sample, disaggregated by Retailer and non-Retailer borrowers. The Retailer's borrowers have an overall credit card limit of 4.6 million pesos, while non-Retailer borrowers have an overall limit of about 2.4 million pesos. The difference between the Retailer's and non-Retailer borrowers is more pronounced among bank cards, where the Retailer's borrowers have an average limit that is twice as large. We select the sample so that all non-Retailer borrowers have at least one retailer credit card with a positive limit, while the Retailer's borrowers may or may not have retailer or bank cards outside of the Retailer's. However, the Retailer's borrowers are substantially more likely to hold a bank card (74.5 percent versus 47.9 percent). The Retailer's borrowers also have higher usage and significantly lower default rates. In terms of demographics, the Retailer's borrowers have higher incomes, are more likely to be female and married, and are older.

B.1. Across-lenders design

The summary statistics in Table II suggest that the Retailer's and non-Retailer borrowers are different. Our identification strategy must therefore account for this borrower heterogeneity, as well as for potentially confounding time-series variation.[3] We do so by

[3]E.g., the banking authority modified the formula used to set interest rate caps in 2013, which is well before the transaction was even announced.

implementing a difference-in-differences design that compares the time-series evolution of bank credit limits for the Retailer's pre-transaction borrowers relative to the evolution of bank credit limits for pre-transaction borrowers from other retailers. To the extent that, in the absence of the transaction, bank credit limits of non-Retailer borrowers would have evolved in parallel to those of the Retailer's borrowers, this design identifies the effect of the shift from a private to a public information regime on financial inclusion. We provide standard evidence in support of this identification assumption in the form of a lack of pre-trends.

In Figure 5, we plot the average bank credit limit of the Retailer's and non-Retailer borrowers in our subsample by month. To account for the differences across borrower observables in Table II, we residualize monthly credit limits using fixed effects constructed by the interaction of five-year age bins, marital status, income bin, retail default status, retail credit limit quartiles, bank credit limit quartiles, number of bank and retail accounts, and bank default. The figure shows that, prior to the transaction, both Retailer and non-Retailer's (residualized) bank credit card limits move in parallel, consistent with the identification assumption. After the transaction, the credit limits of bank cards grew faster for Retailer's borrowers. Figure 5 also shows an upward trend in bank limits to non-Retailer borrowers after the transaction, consistent with a growing economy. This pattern underscores the importance of using a control group that absorbs economy-wide secular trends like we do, rather than assuming a flat counterfactual trajectory.

We run the following regression:

$$Limit_{i,t} = \alpha_i + \alpha_t + \beta \times Retailer_i \times Post_t + \epsilon_{i,t}, \tag{1}$$

where $Limit_{i,t}$ is the individual-level credit limit across all bank or retailer cards, α_i and α_t are individual and month-year fixed effects. $Post_t$ is a dummy that equals one after May 2015, when the transaction occurred, and $Retailer_i$ is a dummy that equals one for individuals with the Retailer as of July 2014 and zero for individuals with other retailers as of the same date. The coefficient of interest β measures the average change in bank credit limits for the Retailer's pre-transaction borrowers relative to non-Retailer pre-transaction borrowers, relative to the pre-transaction period. Our data include six months pre-transaction and nine months post-transaction. The choice of post-transaction window does not affect the results.

In column 1 of Table III, we show the results of specification (1) on the sample of bank credit cards, formalizing the intuition conveyed by Figure 5. All units are expressed in thousands of Chilean pesos. The coefficient on $Retailer \times Post$ implies that bank issued

credit limits for the Retailer's borrowers increase by 106,000 pesos more than for other retail borrowers, a 4.4% increase relative to the pre-period mean of 2.3 million pesos.

In column 2 of Table III we present the coefficients of specification (1) where the sample is now limited to cards issued by retailers. There is a small increase of 9,000 pesos in total credit after the transaction. The small effect among retailers suggests that the demand for credit across the Retailer's and non-Retailer borrowers is similar.

Finally, in column 3 of Table III we combine these two effects using the following triple-differences specification,

$$Limit_{i,j,t} = \alpha_{j,i} + \alpha_{j,t} + \alpha_{i,j} + \beta \times Bank_j \times Retailer_i \times Post_t + \epsilon_{i,t}, \tag{2}$$

that compares the evolution of bank limits ($Bank_j = 1$) relative to retailer limits ($Bank_j = 0$), for the Retailer's borrowers relative to other retail borrowers, and relative to the pre-period. The specification includes fixed effects that absorb all double interactions (individual by month, lender type–bank or retailer–by month, and lender type by individual). The results confirm the intuition of the first two columns and imply large increases in the bank credit limits of the Retailer's borrowers, on the order of 100,000 pesos on average. These results suggest that banks responded to the transaction by learning new information from their existing customers who had a Retailer card, and, as a result, increased the credit limits of their cards.[4]

As shown in Table II, the Retailer's borrowers have higher incomes before the transaction. Thus, one potential concern with the results in Table III is that they may reflect time-series variation in access to credit for higher income borrowers. To alleviate this concern, in Internet Appendix Table A.I we conduct a robustness test where we replace the individual fixed effects in regression (1) with fixed effects of the interaction of five-year age bins, marital status, income bin, retail default status, retail credit limit quartiles, bank credit limit quartiles, number of bank accounts, total number of accounts, and bank default, 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 nearly identical to those in Table III.

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

B.2. Within-Retailer design

We use a second research design to study the effect of information regimes on financial inclusion that relies on a different identification assumption. The idea is as follows: after the transaction, other banks can observe the Retailer borrowers' credit limits and balances, which were not observable prior to the transaction. Other banks can use this new information to recalculate their predicted probability of future default. Thus, we expect a stronger positive effect of the transaction for individuals whose predicted probability of future bank default drops the most after the transaction.[5]

We implement this test within the set of the Retailer's borrowers by computing two sets of predictions of the probability of default in the next six months on any bank credit card as of the beginning of the sample period. The first prediction uses all information available to banks before the transaction, which includes age, gender, marital status, income bin, bank limit, balances, and bank and retailer default status. We refer to this prediction for individual i as $\hat{C}_{i,pre}$. The second prediction, referred to as $\hat{C}_{i,post}$, incorporates all the information used to predict $\hat{C}_{i,pre}$, and adds the Retailer's card limit and balance. We then compute the change in predicted probability of default for the Retailer's existing borrowers as the difference in the (log) predicted default rates,

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

To construct the predictions we run a probit of a dummy for bank card default in the next six months on the predictors listed above. We randomly select a 30 percent sub-sample of the Retailer's cross-section of borrowers in the first sample month 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. The distribution is highly negatively skewed, with an average drop of 48.2 percent, consistent with the average increase in bank credit limits documented in Table III.

Internet Appendix Figure A.3 splits the sample of the Retailer's borrowers by decile of

[5]In our analysis, we compare how predicted probabilities of default change among individuals who already have at least one credit card, from either a retailer or a bank. Given the data and empirical setting–that is, 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 would allow banks to distinguish the Retailer's borrowers who were not in default from other individuals who were not borrowing at all. This relies on the assumption that credit supply is correlated with banks' beliefs about future default, which is supported by evidence presented in Dobbie, Liberman, Paravisini, and Pathania (2018). As Agarwal, Chomsisengphet, Mahoney, and Strobel, 2015 document, one of the main drivers of credit card profitability is default or charge-offs, so we can interpret the reassessment of default prediction after new information arrives as a reassessment of the profitability of extending a credit card.

the change in log predicted default and plots the average of several characteristics within each decile. The top four panels exhibit U-shaped patterns, suggesting that borrowers with both large increases and large decreases in predicted default are similar in demographics as well as in their bank and retailer credit 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 defaults differ in two key characteristics. First, individuals with the largest drops in predicted default have higher credit limits with the Retailer, and second, they are much less likely to be in default with the Retailer. This is intuitive, as the new information available to banks consists of the Retailer's credit limits and usage and whether the borrower is in default. These borrowers (and their characteristics) were not observable to banks prior to the transaction.

The change in lenders' predictions induces variation across the Retailer's borrowers for whom the new information leads to positive and negative updating of beliefs about future default. For example, lenders always observe the Retailer's borrowers' default but do not observe their limits and balances. The new information may lead to positive updating among those for whom no information was observed because they were not in default. It may also lead to negative updating among those whose limit and default with the Retailer is likely to represent a large fraction of their total oustanding debt. In principle we could also use the group with small or no change in lenders' beliefs as a placebo group. However, we do not observe the counterfactual evolution of the credit outcomes of the zero group in the data, and it is misguided to assume that the counterfactual time series evolution for this group would be flat in the absence of the transaction.

We implement a difference-in-differences design where we compare the evolution of the Retailer's borrowers whose prediction of default drops relative to those whose prediction increases following the transaction. To motivate this test, Figure 6 presents average credit limits among bank cards for the Retailer's borrowers whose predicted bank default decreases and increases, respectively, both normalized to their level as of August 2014. Prior to the transaction, both series move in parallel, which validates our empirical strategy. Moreover, the post-transaction increase in credit limits is more pronounced for borrowers whose predicted default drops relative to those for whom it increases.

To construct the difference-in-differences estimates we regress card limits on the interaction between, *Predicted Drop*, a dummy that equals one for individuals who experience a drop in predicted default, and *Post*, a dummy that equals one after May 2015, and control for individual and month-year fixed effects:

$$Limit_{i,t} = \alpha_i + \alpha_t + \beta \times Predicted\,Drop_i \times Post_t + \epsilon_{i,t}. \quad (3)$$

The omitted category corresponds to Retailer borrowers whose predicted default increase. Thus, the coefficients measure the relative change in limits on the Retailer's borrowers for whom predicted defaults drop relative to those for whom predicted defaults increase relative to the pre-transaction period. 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 parallel after the transaction, which we support with the visual pre-trends analysis from Figure 6.

Table IV presents the results. After the transaction, there is a sharp increase of 187 thousand pesos in the card limits of individuals for whom predicted defaults decrease, relative to those whose predicted defaults increase. Column 2 shows that the effect is smaller at 14 thousand pesos, although statistically significant for retailer limits. Combining the results in columns 1 and 2 of Table IV, column 3 presents the output of a triple-differences specification that includes the triple interaction of the *Post* dummy, *Predicted Drop*, and the bank cards dummy ($Bank_j$), with fixed effects that absorb all double interactions,

$$Limit_{i,j,t} = \alpha_{i,j} + \alpha_{t,j} + \alpha_{i,j} + \beta \times Predicted\,Drop_i \times Bank_j \times Post_t + \epsilon_{i,t}. \quad (4)$$

The results confirm that among the Retailer's borrowers, bank limits increase substantially more, approximately 180 thousand pesos, 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 with different lenders *as well* as variation within the Retailer's borrowers, and demonstrate a remarkable consistency among the estimates of the effects of information on bank's competitive reaction.

B.3. Change in the Retailer's lending policies

Does a public information regime reduce banks' incentives 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, as shown in Section II, retailers can experiment with limits that are initially lower but increase more over time for performing borrowers. Intuitively, because banks have no informational advantage after

lending, they have to break even on every period and thus select safer populations ex-ante.[6]

We study how the Retailer 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 outcomes and characteristics for Retailer borrowers compared to other new retailer borrowers. The regression model is:

$$y_{i,t} = \alpha_t + \beta \times Retailer_i \times Post_t + \epsilon_{i,t}, \tag{5}$$

where here t denotes the origination month centered at zero as of May 2015, and $y_{i,t,}$ is the origination month outcome. The coefficients of interest is β, which measures the difference in the origination month outcome for the Retailer's new borrowers relative to other retailer new borrowers, both relative to the pre-transaction period.

Table V presents the regression output. Columns 1 and 2 show that the Retailer shifts originations to individuals who earn higher incomes. The income bin category is coarse but captures a difference in average income bin after the transaction that is significant at the 10% level. Moreover, in column 2 we see that the fraction of new borrowers who belong to the lowest income bin becomes significantly smaller.

Column 3 shows that after the transaction, new Retailer borrowers receive a credit limit that is 216,000 pesos higher, a large increase relative to the pre-period mean of 210,000 pesos. Figure 7 presents this result graphically, plotting the average initial credit limit by month of origination.[7] This effect is consistent with the fact that banks target safer borrowers to whom they lend higher limits because increased competition reduces ex-post profits among good borrowers. Finally, column 4 shows that these new borrowers are unconditionally not more likely to default.

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

[6]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.

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

In sum, the evidence suggests that once the Retailer becomes a bank, it originates larger loans to safer borrowers. In particular, borrowers whose Retailer 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. The results suggest that once the Retailer becomes a bank, its 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.

One caveat of our results is that, aside from the informational structure, the transaction probably involves other changes to the Retailer's management and operations. However, the Retailer's physical distribution network remains intact: the Retailer's card is maintained as a separate product from the acquiring bank's pre-existing card, and the Retailer's card can only be obtained in the Retailer's stores. This remains unchanged from before and after the transaction, and implies that the pool of potential borrowers who shop at the Retailer's stores remains fixed. This does not preclude, however, a shift in originations through mailing campaigns. 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.

IV. Interpreting the Evidence: Alternative Stories

The empirical facts derived from the cross section of first time borrowers and from the acquisition of the Retailer's portfolio can be parsimoniously explained by differences in information regime of banks and retailers. When lenders are less informed than potential borrowers about their repayment prospects and when past repayment predicts future repayment, a private information regime provides incumbent lenders with an informational monopoly over its borrowers. Information frictions prevent 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 Retailer's existing borrowers see higher credit limits from other banks and that the Retailer starts originating cards to safer borrowers.

Alternative explanations fail to parsimoniously explain all the empirical findings. A first

possibility is that the bank that acquired the Retailer is less financially constrained and signals its intent to start competing with other lenders, irrespective of credit information. Other lenders then respond to this new competitive pressure by raising credit limits. Two key results in the paper are inconsistent with this alternative story. First, if other lenders consider the post-transaction Retailer to be a competitive threat for *all* borrowers, then this mechanism cannot explain the differential increase in credit limits for the Retailer's borrowers following the transaction. In particular, the Retailer's borrowers are only distinguishable after the transaction: banks can only choose to increase their credit limit after they become observable in the data, which highlights the role of information in making a market contestable. Second, it is unclear from this alternative story why the increase in bank credit limits is concentrated among the Retailer's borrowers for whom banks' predicted default decreases. It is important to note that the heterogeneity in exposure to the information shock that this test exploits does not sort individuals by the *level* of their predicted probability of default, but rather by the *change* in banks' beliefs about future default driven by the change in the information set.

A second alternative story is that credit information causally leads to better repayment, which leads to more credit from banks. Information may improve borrowers' repayment directly by reducing future liquidity constraints, which in turn 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 Retailer'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.IV we show that the default probability of the Retailer's borrowers does not decrease or change trends after the transaction, although pretrends complicate inference.

Third, banks and retailers have different sources of funding. In particular, banks can take deposits, which might shift the Retailer'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 these other banks (or retailers). This mechanism may, however, explain partly the effects on credit limits of the Retailer and on the change in originations following the transaction.

A fourth 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. However, there is no change in the characteristics of other retailers that would explain how lending from banks to the Retailer's borrowers would change. Moreover, after the transaction, the Retailer 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 fifth 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 Retailer's borrowers because they are now bank clients. However, this alternative fails to explain the heterogeneous results among the Retailer's borrowers whose predicted default increases and decreases after the change in the information set. This effect is, in fact, only consistent with the fact that credit supply depends on Retailer's beliefs about future default, which are in turn mediated by changes to the 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

This paper examines how information regimes affect financial inclusion by shaping lenders' incentives to screen and serve borrowers. In a setting where banks share both defaults and repayment histories while retailers keep this information private, we show that private regimes foster lending relationships with low-income and first-time borrowers. Retailers extend low initial limits but rapidly expand those limits for borrowers who perform well.

We exploit a natural experiment in which a large retailer became a bank, shifting its borrower histories from private to public. We find that banks increase limits to the retailer's existing borrowers only after the information becomes public. Opaque borrowers receive less credit, and the now-bank-affiliated retailer reallocates toward observably safer borrowers. These patterns are not easily reconciled with alternative explanations such as capital constraints, competition, or borrower selection unrelated to information.

The findings underscore a central trade-off in credit markets: while public information can improve allocative efficiency for borrowers with favorable credit histories, it can also weaken lenders' incentives to originate loans to borrowers whose risk must be learned over time. This tension has broad implications for understanding how information structures shape credit supply, especially at the margins of financial access.

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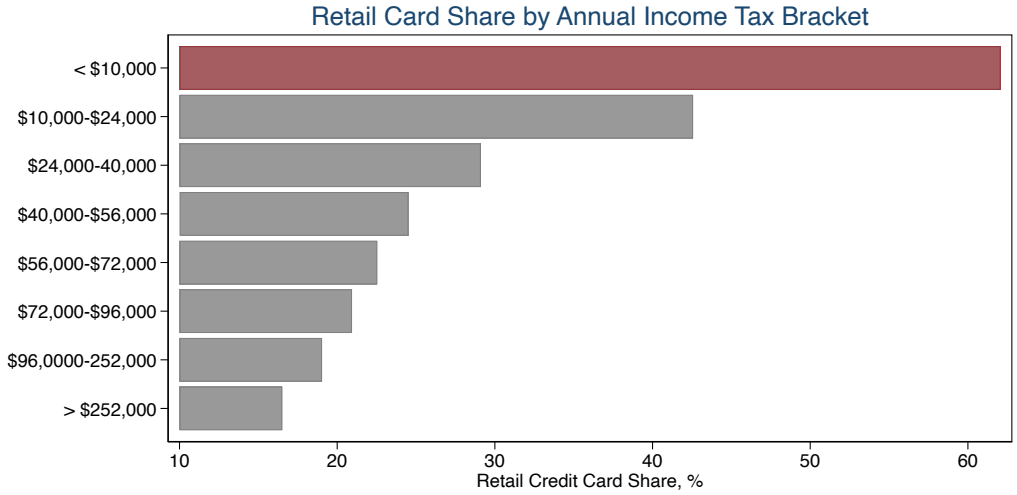
Figure 1: Retail Credit Card Share by Income Tax Bracket

This figure shows the share of retail credit cards by borrower income. Income brackets are expressed in U.S. dollars and derived from individual-level administrative tax returns. We compute the retail card share as the number of retailer cards divided by total number of credit cards. We then average this retail share across borrowers within each income bracket. Panel A reports results for all borrowers, while Panel B restricts the sample to first-time borrowers.

Panel A: All Borrowers



Panel B: First-Time Borrowers

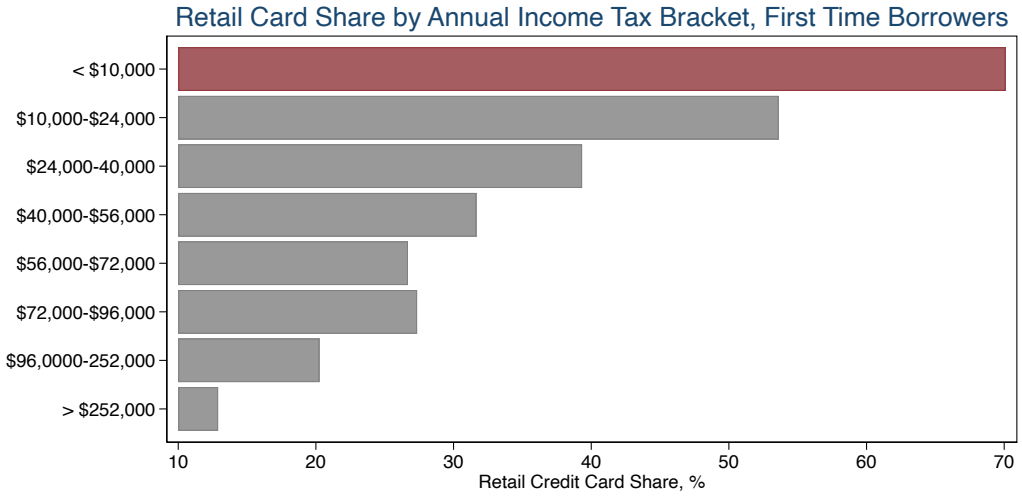


Figure 2: Share of First-Time Credit Card Borrowers

This figure compares the share of first-time credit card borrowers across lender types. For each lender type, we compute the share of total cardholders who are first-time borrowers and average.

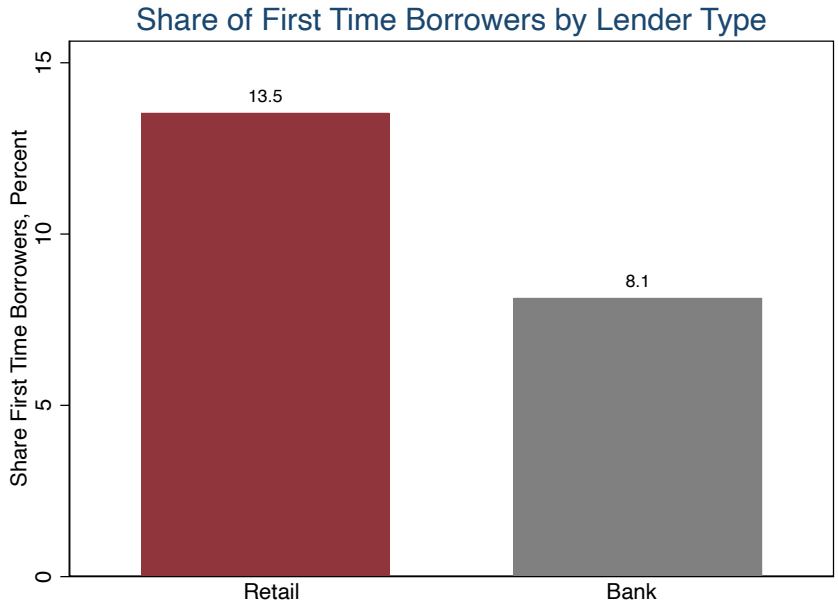


Figure 3: Credit Card Defaults, First Time Borrowers

This figure plots the share of credit card accounts in default (90+ days past due) over time since first time borrowing, separately for borrowers with bank and retailer cards. We compute the default status for each account, then average across borrowers by months since first time borrowing.

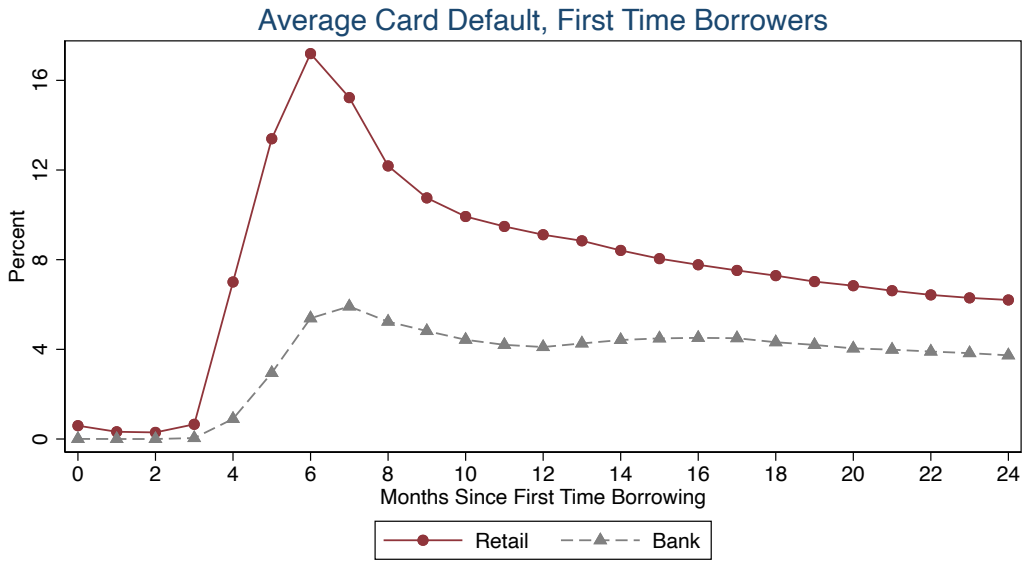
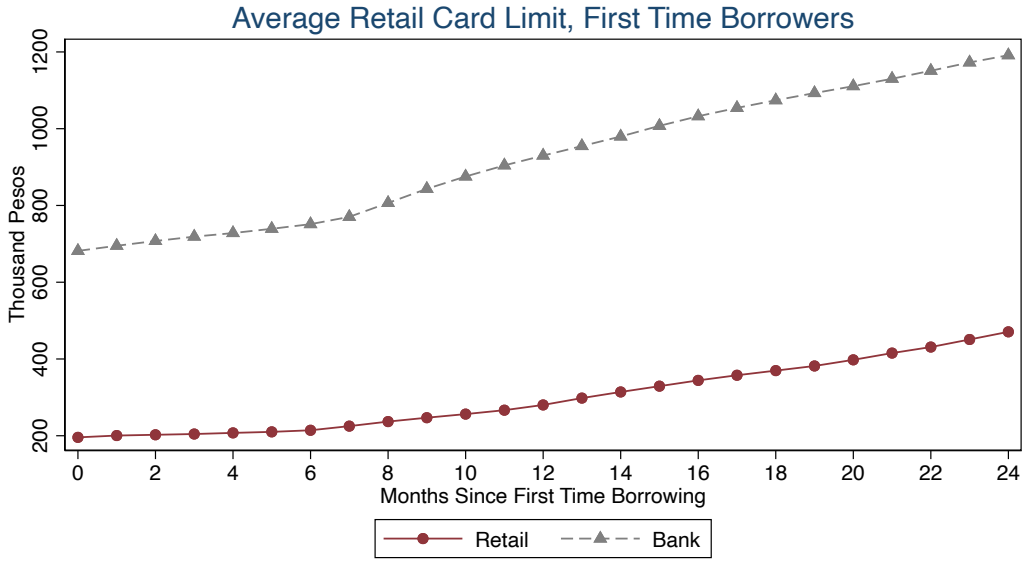


Figure 4: Evolution of Credit Limits for First-Time Borrowers

This figure reports the evolution of credit card limits in the 24 months following account origination for first-time borrowers, separately by lender type. Panel A plots the average nominal credit limit in thousands of pesos by months since first time borrowing, while Panel B normalizes limits to 100 percent in the first month to track relative growth over time.

Panel A: Levels



Panel B: Growth

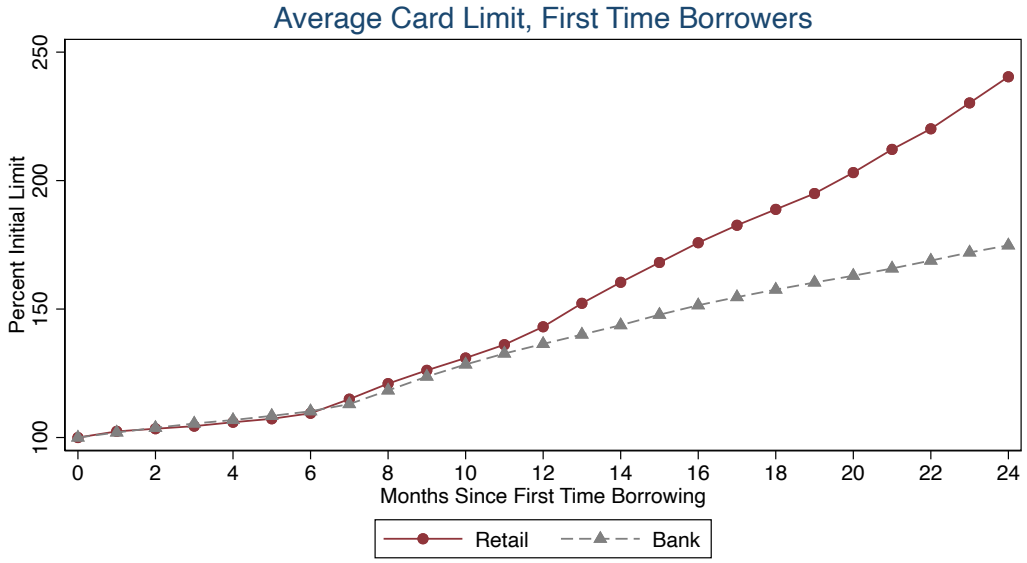


Figure 5: Bank credit limits for Lender borrowers

This figure shows the time-series evolution of average credit limits from bank credit cards for Lender borrowers and non-Lender retailer borrowers. Monthly credit limit is residualized by fixed effects constructed from the intersection of quartiles of bank limits, quartiles of retailer limit, 5-year age group, gender, income bin, number of bank and number of retailer cards, and bank default status, all measured as of November 2014. The dashed vertical line represents the month of the transaction.

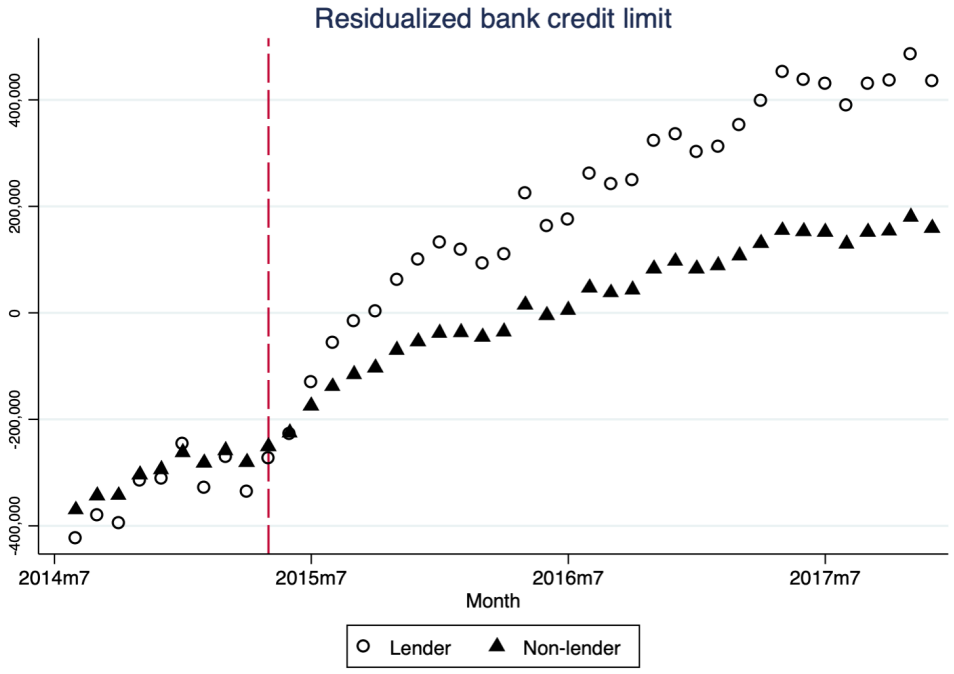


Figure 6: Bank credit limits heterogeneity

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. Both series are normalized to zero as of their November 2014 level. The dashed vertical line represents the month of the transaction.

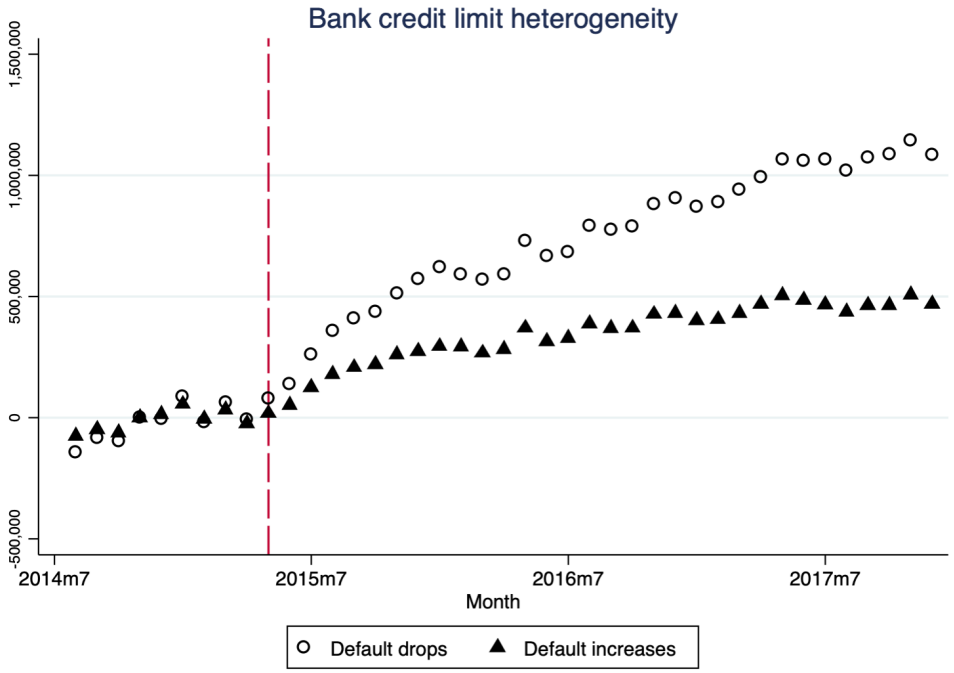


Figure 7: Average credit limit of new Lender borrowers

This figure plots the average credit limit at origination for the Lender's credit card (in circles, right axis) and the number of new Lender borrowers by month of origination (in bars, left axis). The dashed vertical line represents the month of the transaction.

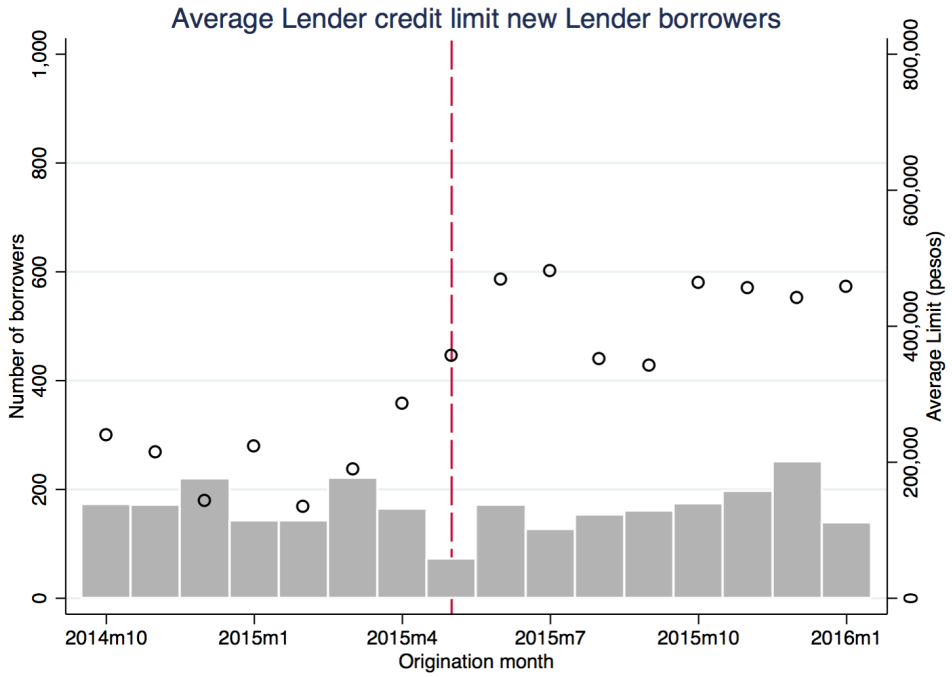


Table I: Borrower Demographics by Lender Type

This table reports the demographic composition of credit card borrowers in our analysis sample, separately by lender type and borrower segment. Panel A includes all borrowers, while Panel B restricts the sample to first-time borrowers. For each group, we report the fraction of borrowers who are female and the fraction classified as low-income based on income tax return data. The rightmost column reports the number of borrowers used to compute each statistic.

Panel A: All Borrowers

	Retailers		Banks	
Fraction Female Borrowers	53.86	2,947,773	49.15	1,866,356
Fraction Low-Income Borrowers	89.74	2,947,773	75.35	1,866,356

Panel B: First-Time Borrowers

	Retailers		Banks	
Fraction Female Borrowers	51.78	634,907	54.89	286,975
Fraction Low-Income Borrowers	91.55	634,907	85.17	286,975

Table II: pre-period summary statistics for analysis sample

This table shows summary statistics of the sample of individuals with a retailer credit card 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 III: 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 limits for Lender borrowers relative to non-Lender borrowers, relative to the pre-transaction period. 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 the pre-period. The data are a balanced panel with one observation per individual-month for columns 1 and 2, and two observations for column 3. 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 Post	106.13***	9.03***	
	(6.67)	(1.31)	
Lender x Bank x Post			97.10***
			(6.74)
Sample	Banks	Retail	All
Dep. variable Mean	2,383.36	933.02	1,658.19
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 IV: Heterogeneity in credit limits by changes in predicted bank 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 the pre-period prior to the transaction. 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 the pre-period. The data are a balanced panel with one observation per individual-month for columns 1 and 2, and two observations for column 3. 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 × Post	187.63***	14.74***	
	(12.52)	(2.29)	
Pred. Def. Drops × Bank × Post			172.89***
			(12.64)
Sample	Banks	Retail	All
Dep. variable Mean	3,641.12	1,195.67	1,896.53
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 V: Originations after the transaction

This table reports the average difference in borrower characteristics and credit outcomes at origination for the Lender's new borrowers relative to new retail borrowers, after the transaction. The sample corresponds to new retailer or Lender borrowers. New borrowers are defined as individuals who first appear in the credit card data on or after October 2014. The data are 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)
	Income bin	In income bin 1	Limit	Default
Lender × Post	0.0391^{*}	-0.0368^{***}	216.64^{***}	-0.0132
	(0.0202)	(0.0136)	(16.80)	(0.0170)
Dep. variable Mean	1.0732	0.9011	210	0.2846
Observations	67,708	70,337	70,337	70,337
R-squared	0.0021	0.0019	0.0232	0.0025

Internet Appendix for "Information Regimes and Financial Inclusion"

by Foley, Hurtado, and Sepulveda

Figure A.1: Total bank credit cards

This figure shows the number of bank credit cards by month in 2015. The dashed vertical line represents the date of the transaction.

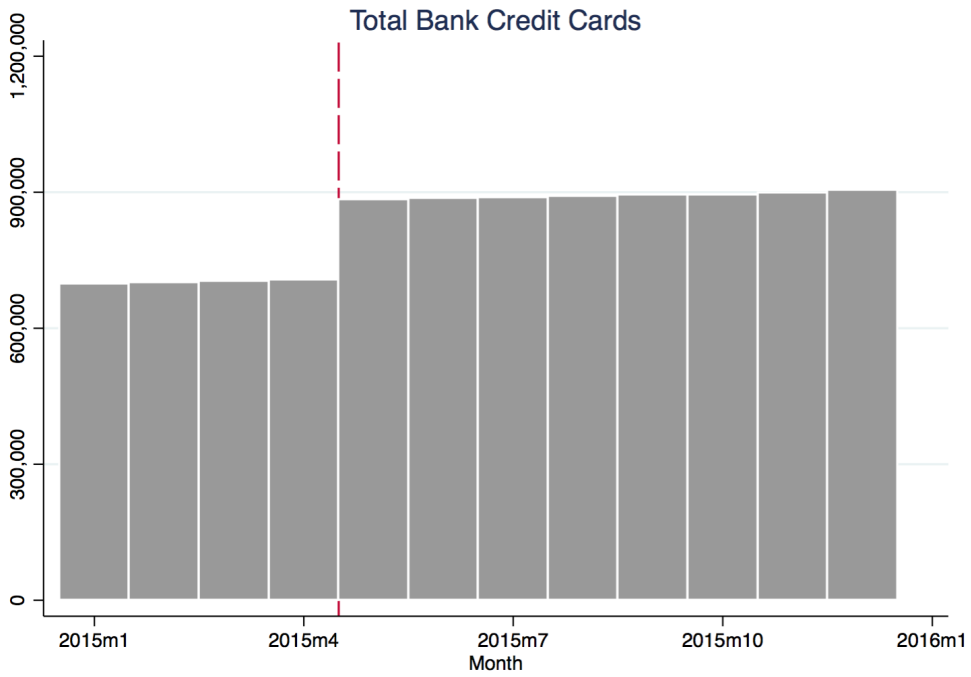


Figure A.2: Histogram of changes in predicted bank default

This figure shows the histogram of the changes predictions of the logarithm of bank default in the next 6 months as of August 2014, trimmed at -300% and +150%. See text for details.

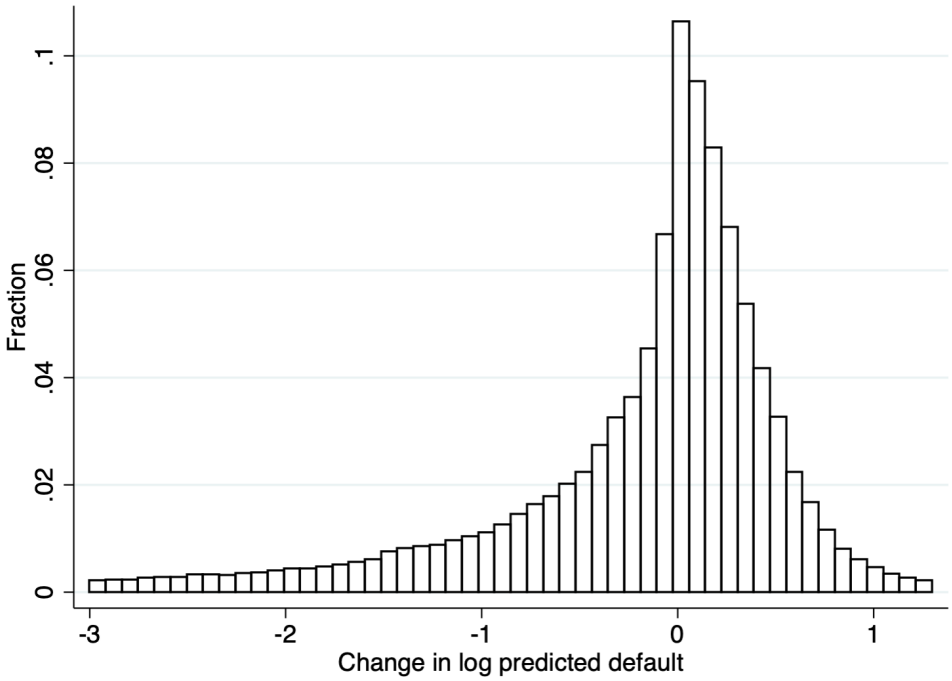


Figure A.3: Characteristics across deciles of changes in predicted bank default

This figure shows panels of average characteristics of the Lender's borrowers grouped according to the change in logarithm of predicted default as defined in the text.

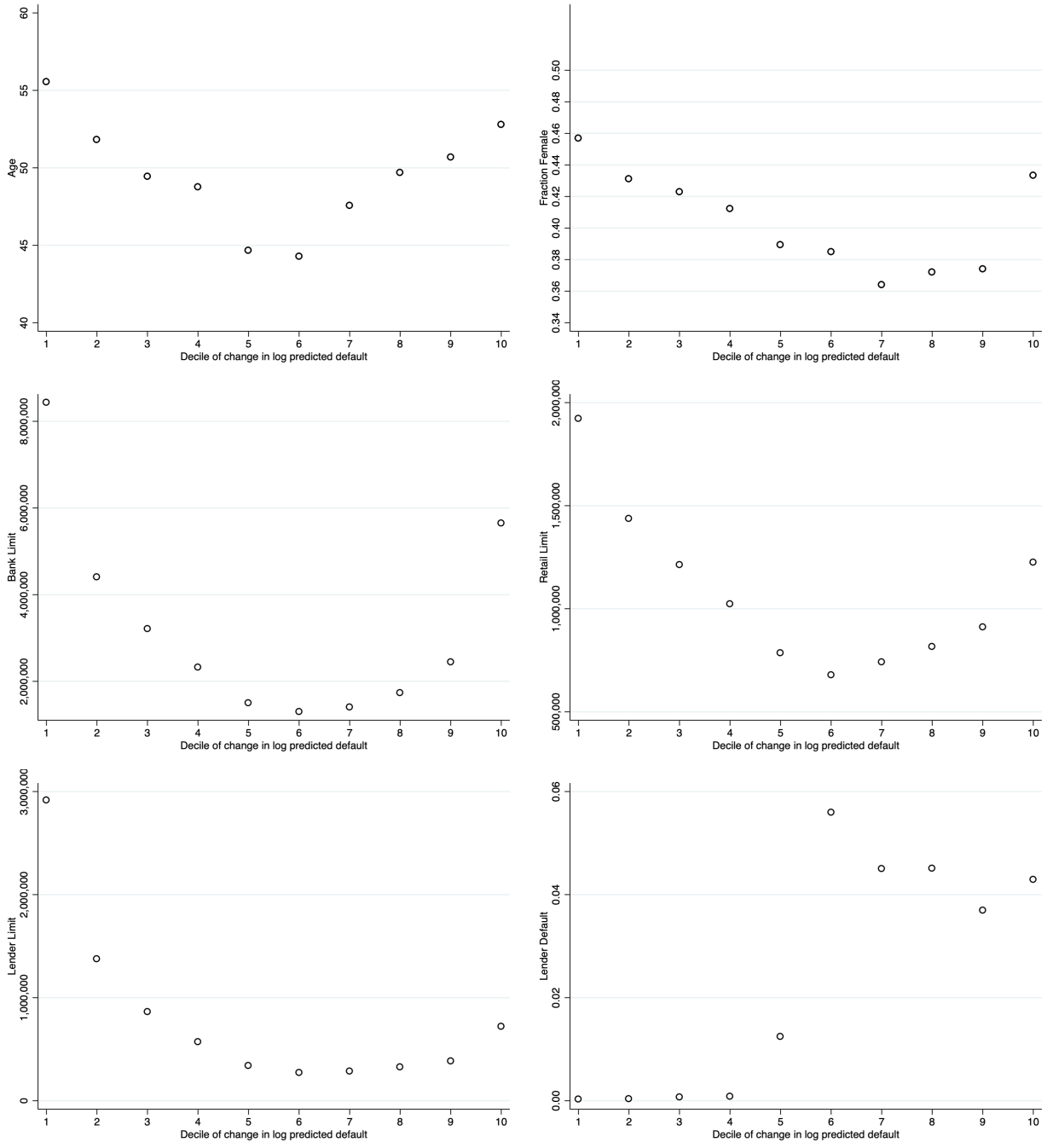


Figure A.4: Lender credit limits

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.

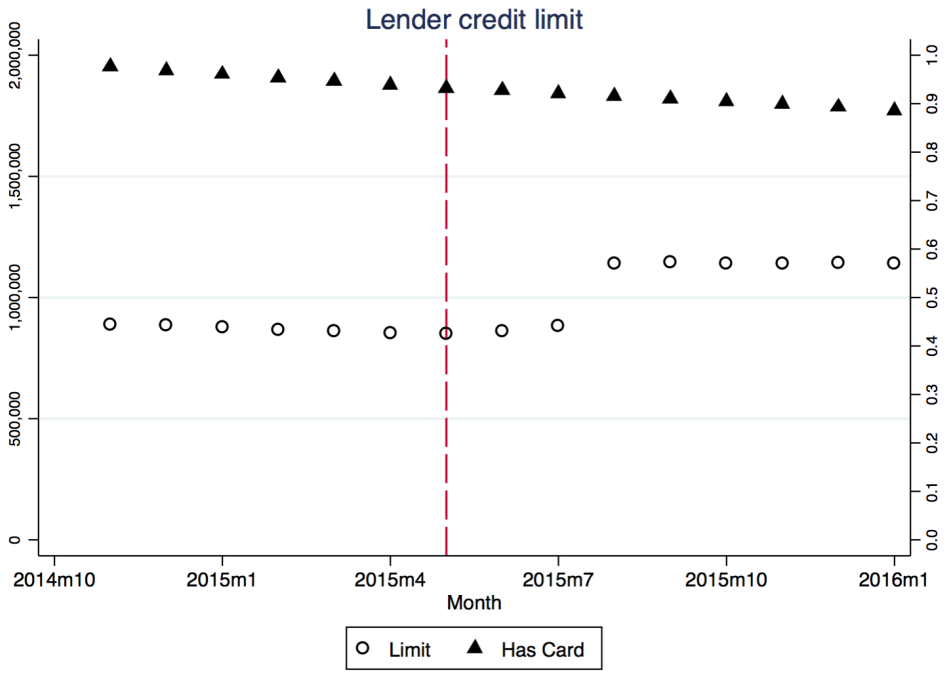


Table A.I: Transaction analysis: robustness using fixed effects

This table shows the output of regression (1) (columns 1 and 2) and regression (2), where individual fixed effects are replaced by fixed effects constructed 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 sample corresponds to retail or Lender borrowers. The data are a balanced panel with one observation per individual-month for columns 1 and 2, and two observations for column 3. Standard errors are robust to heteroskedasticity. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)
	Limit	Limit	Limit
Lender x Post	106.16***	9.02***	
	(6.68)	(1.31)	
Lender x Bank x Post			97.13***
			(6.77)
Sample	Banks	Retail	All
Dep. variable Mean	2,383.36	933.02	1,658.19
Observations	7,560,495	7,560,495	15,120,990
R-squared	0.58	0.49	0.35
Clusters	504,033	504,033	504,033

Table A.II: Transaction: Lender Outcomes

This table reports the average difference in credit outcomes for the Lender's own credit card 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 are 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	Balance / Limit	Default
Post	$177,391.00^{***}$	-0.0475^{***}	$-6,362.47^{***}$	0.0029^{***}	0.0156^{***}
	(2,039.60)	(0.0004)	(555.61)	(0.0005)	(0.0002)
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	0.82	0.74	0.83	0.78	0.43
Clusters	179,746	179,746	179,746	174,458	174,458

Table A.III: Outcomes from other lenders for new borrowers after the transaction

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. 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 are 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 Post	$23,442.98^{***}$	$35,257.51^{***}$	0.0511^{***}	0.0612^{***}	$32,628.87$	$117,222.21^{**}$	0.0200^{**}	0.0732^{***}
	(5,292.12)	(11,862.99)	(0.0120)	(0.0184)	(27,565.72)	(48,056.05)	(0.0078)	(0.0163)
Dep. variable Mean	22,686	77,651	0.0954	0.2447	22,561	136,694	0.0287	0.1692
Observations	70,080	57,589	70,080	57,589	70,080	57,589	70,080	57,589
R-squared	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.01

Table A.IV: Transaction analysis: default

This table shows the output of regression (1) (columns 1 and 2) and regression (2), where the outcome is a dummy for whether the individual is in default in any card by more than 90 days. The sample corresponds to retail or Lender borrowers. The data are a balanced panel. Standard errors are robust to heteroskedasticity. *, **, and *** represent 10, 5, and 1 percent significance level, respectively.

	(1)	(2)	(3)
	Default	Default	Default
Lender x Post	0.0004**	0.0099***	
	(0.0002)	(0.0003)	
Lender x Bank x Post			−0.0095***
			(0.0003)
Sample	Banks	Retail	All
Dep. variable Mean	0.0062	0.0263	0.0163
Observations	7,569,285	7,569,285	15,138,570
R-squared	0.18	0.22	0.63
Clusters	504,619	504,619	504,619

Framework

In this appendix we develop a simple model of a credit card market with asymmetric information. The purpose of the model is to formalize in a simple way the differences in credit card contracts emerging from different information regimes. With this in mind, the model makes stark assumptions, particularly about borrower behavior. Throughout we assume that parameters are chosen so that equilibria exist.

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.[8] 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.

There is a continuum of individuals of mass 1 (indexed by i) who want a credit card, and who 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. In particular, θ can be interpreted as a measure of adverse selection in the market.

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.[9] Lenders make simultaneous offers for one-period credit card contracts, competing on credit limits. Lenders can offer cards with an individual limit up to a total capacity per card of C. 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 unique card randomly.

[8] 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.

[9] In the empirical setting it is likely that different lenders, e.g. retailers and banks, have different cost of funds. We abstract from this heterogeneity to focus on the predictions of a model with differences in the informational environment across markets. Retailers' higher cost of funds would, for example, rationalize their reluctance to voluntarily make their information public in a setting where they compete with banks.

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.

We assume first that banks learn the type of all borrowers from all banks 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).

Lenders' informational advantage

Next we assume that incumbent lenders are only 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, e.g., among retailer borrowers who are not in default. 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 lenders 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, lenders' 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 $t=0$ lenders 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.

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:

$$\frac{d\theta}{dx} < 0 \tag{6}$$

Assumption 6 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 implications, which are consistent with stylized facts shown in the paper:

- 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.

Data Appendix

Credit Card Limit or Limit: We construct Credit Card Limit at the individual-level as the sum of limits from all bank and retail credit cards in Chilean pesos.

Bank Credit Card Limit or Bank Limit: We construct Bank Credit Card Limit at the individual-level as the sum of limits from all bank credit cards in Chilean pesos.

Retail Credit Card Limit or Retail Limit: We construct Retail Credit Card Limit at the individual-level as the sum of limits from all retail credit cards, at the individual-level, in Chilean pesos.

Lender Credit Card Limit or Lender Limit: The individual-level limit of credit cards issued by the Lender, at the individual-level, in Chilean pesos.

Credit Card Balance: We construct Credit Card Usage at the individual-level as the sum of debt balances from all bank and retail credit cards, in Chilean pesos.

Lender Credit Card Balance: The individual-level usage or debt balance of credit cards issued by the Lender, in Chilean pesos.
Credit Card Balance/Limit or Balance/Limit: We construct Balance/Limit at the individual-level as the quotient of Credit Card Balance and Credit Card Limit.

Has Credit Card: An indicator for an individual having a credit card. Has Credit Card is set to one for individuals who have a credit card and zero otherwise.

Lender: An indicator for an individual having a credit card with the Lender. Has Lender Credit Card is set to one for individuals who have a credit card with the Lender and zero otherwise.

Default: An indicator for an individual defaulting on her bank or retail credit card. Default is set to one for bank or retail borrowers with a 90+ days delinquency and zero otherwise.

Bank Credit Card Default: An indicator for an individual defaulting on her bank credit card. Bank Credit Card Default is set to one for bank borrowers with a 90+ days delinquency

and zero otherwise.

Retail Credit Card Default: An indicator for an individual defaulting on her retail credit card. Retail Credit Card Default is set to one for retail borrowers with a 90+ days delinquency and zero otherwise.

Income Bin: A discrete variable indicating an individual's IRS income bin, where one and eight are the lowest and highest, respectively. As of May 2015 (the date of the transaction) individuals in bin one and eight earn less than 606,893 and more than 6,743,250, respectively (http://www.sii.cl/valores_y_fechas/impuesto_2da_categoria/impuesto2015.htm).

Fraction in Income Bin One: An indicator for individuals with incomes in bin one. Fraction in income bin 1 is set to one for individuals with a monthly income lower than 606,893 by May 2015 and zero otherwise.

Female: An indicator for whether the individual is female. Female is set to one for female individuals and zero for male individuals.
Married: An indicator the applicant being married. Married is set to one for married individuals and zero otherwise.

Age: The individual's age in years.

New Retail Borrower: An indicator for individuals being new retail borrowers, defined as individuals whose first credit card appears in the data as of October 2014. New Retail Borrower is set to one for new retail borrowers and zero for new bank borrowers.

Pred. Def. Drops: An indicator for individuals experiencing a drop in default predicted by a Probit model on a randomly selected 30% sub-sample of the Lender's August 2014 cross-section of borrowers.

Interest rate: The individual-level credit card's monthly interest rate in percentage.