Credit Supply Shocks, Consumer Borrowing and Bank Competitive Response: Evidence from Credit Card Markets*

Sergio Correia
Duke University, Fuqua School of Business

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Abstract

I study local shocks to consumer credit supply arising from the opening of bank-related retail stores. Bank-related store openings coincide with sharp increases in credit card placements in the neighborhood of the store, in the months surrounding the store opening, and with the bank that owns the store. I exploit this relationship to instrument for new credit cards at the individual level, and find that obtaining a new credit card sharply increases total borrowing as well as default risk, particularly for risky and opaque borrowers. In line with theories of default externality, I observe that existing lenders react to the increased consumer borrowing and associated riskiness by contracting their own supply. In particular, in the year following the issuance of a new credit card, banks without links to stores reduce credit card limits by 24–51%, offsetting most of the initial increase in total credit limits.

Keywords: Banking, Consumer Credit Supply, Household Finance, Credit Cards, Default Externalities, Non–Exclusivity, Common Pool, Credit Rationing

JEL Classification: G21, D12, D62

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1 Introduction

Credit cards are prominently non–exclusive. Lenders interact not only through the market, but affect each other through the creditworthiness and repayment ability of their shared customers. Thus, changes in the supply curve of one lender can induce other lenders to shift their own supply, either exacerbating or ameliorating the initial effects. For instance, default externality theories posit that a new credit card from an outside bank might increase a consumer's indebtedness, which might then raise default rates on her preexisting loans and reduce the profitability of her other credit card contracts. This creates incentives for existing lenders to curtail their credit to the shared customer if a new card is issued. Alternatively, consumer behavior theories suggest that existing lenders might respond to a new card by increasing credit availability, either to prevent the poaching of profitable customers, or if consumers are more likely to carry a balance and pay interest due to increased indebtedness. In this paper, I contrast these two channels and study the effects of new credit cards on consumers and competitors. I find that a new card induces consumers to borrow more and default more frequently. In turn, existing lenders curtail credit credit card limits, suggesting that the default externality channel dominates, in particular for opaque and less educationally attained consumers.

From a theoretical perspective, default externalities have been explored in depth, and have been shown to lead to an inefficient credit supply, above–market interest rates and increased consumer default (Bizer and DeMarzo 1992; Parlour and Rajan 2001; Bisin and Guaitoli 2004). They have also been linked to a lack of competitiveness in the credit card market, as described by Ausubel (1991). However, so far there has been no evidence of its existence or strength in consumer credit markets (Zinman 2014).

I extract six predictions from the theoretical literatures on default externalities and consumer behavior, and test them on a detailed panel dataset of Peruvian credit card borrowers. In default externality models such as Bizer and DeMarzo (1992), consumers are unable to borrow as much as desired and their default rates increase with indebtedness due to e.g. moral hazard channels. Consequently, if outside credit is made available they will (1) borrow more, and (2) default more frequently. In turn, other loans will be less profitable, so inside banks will (3) curtail credit limits on existing credit cards. This is in contrast to behavioral theories, which imply that the optimal bank response would be to expand credit limits, even though default rates and borrowing might also increase. Additionally, I consider three moderator variables and study how the results vary with the consumer's education, employment type, and her borrowing concentration.

To validate these predictions, the empirical setup must satisfy three requirements. First, we need an exogenous shift in credit supply that only affects some consumers, thus providing a counterfactual.[1] Second, insider banks must be able to react to an outside loan by curtailing credit, something not possible in previously granted non–revolving loans.[2] Third, econometricians must be able to observe consumer borrowing not just from one source, but from all banks, as well as default patterns and off-the-balance sheet variables, such as credit limits.

To address the first requirement, I exploit the partial integration between the banking and retail sectors in the Peruvian economy through the 2006–2012 period.[3] During this period, some of the largest consumer banks owned retail conglomerates, such as supermarket and department store chains, while several large banks and retail stores remained independent[4]. I find that when a bank-related retail store opened, the bank in question issued significantly more credit cards to consumers in that district. This is consistent with the existence of banking–commerce synergies (Raskovich 2008; Barth et al. 2012; Blair 2004; Baradaran 2011),[5] and motivates the use of bank-related store openings as an instrument for credit card issuance.

A limitation of using bank-related store openings as instruments is their possible correlation with local economic conditions, which would violate the exclusion restriction of the instrument. For instance, retail stores are more likely to open in districts with above–average growth or a positive outlook. Similarly, a new retail store might increase investment and job creation in the area. To address these issues, I add the opening of *all* retail stores—bank-related and independent—as a control. I also verify that both types of store have similar characteristics, and open in locations with statistically similar conditions. In addition, since bank-related store openings are often accompanied by new bank branches, but independent store openings are not, I also control for the openings of *all* bank branches. Several measures of economic and financial activity at the district level complement the store and bank branch controls, as well as a wide array of fixed effects that help to address remaining concerns. These include individual, district, and time fixed effects, as well as heterogeneous slopes (see Duflo 2004 for the rationale behind these controls), and are only possible thanks to a novel linear fixed effect estimator (Correia 2015) that is computationally feasible for large datasets such as this.

Using all the aforementioned controls, I find that when a bank-related store opens in a district, existing credit card holders see their probability of obtaining a new credit card from a

[1]Note that the issuance of a new credit card to an individual cannot be used as a supply shifter because even though it mechanically increases credit supply it is confounded with hard to observe shocks, such as salary increases, new jobs, and marriages.

[2]Degryse, Ioannidou, and Schedvin (2012) is the only empirical study on the effect of default externalities on corporate lending. They address this issue by measuring how a bank's internal credit limit changed when a firm received a loan from another bank.

[3]Due to prohibitions on commercial ownership of banks and vice versa this might not be possible in other countries; see Baradaran (2011) for a discussion of the U.S. case.

[4]This situation began to change in August 2012, when the largest retail chain acquired a banking license, and as of 2015, the banking–retail integration is almost complete.

[5]See also the online appendix, which contains news articles and other anecdotal evidence for the existence of these synergies.

store–related bank increase from a baseline of 1.76% per month up to 1.91-3.75% per month. The strength and specificity of this link—only for some specific banks, districts, and time periods—as well as its robustness across distinct specifications, is what gives credence to its use as an instrument for credit supply shifts.

The second empirical requirement—that inside banks can react to outside loans—is given by the credit limits embedded in the credit cards, which banks can change unilaterally.[6] Within consumer credit markets, home equity lines (HELOCs) also share this feature, although the existence of collateral and seniority clauses would obfuscate empirical analysis.

The third requirement—availability of system–wide credit data—is obtained via the use of credit bureau records, matched with consumer demographic and location characteristics. Together, these datasets allow for our experimental design.

I find that default externalities play a significant role in credit card markets, through total credit volume, default rates, and the composition of such credit. In response to obtaining a new credit card, consumers credit card borrowing increases by 18–20% across different specifications. Moreover, there is no evidence that consumers substitute away from other types of credit, and total borrowing increases by 10-13%. These results suggest that a significant portion of consumers are credit constrained and have a high marginal propensity to consume, in line with results for U.S. consumers (Gross and Souleles 2002). After a slight initial decline, default rates[7] more than double one year after a new card is issued. This is also consistent with U.S. findings (Domowitz and Sartain 1999; Einav, Jenkins, and Levin 2012), is predicted by the default externality literature (Bizer and DeMarzo 1992; Parlour and Rajan 2001), and is also related to the findings of Karlan and Zinman (2009), who find that moral hazard drives most of the link between the cost of credit and default rates.

Finally, competing banks respond to the issuance of a new card, and the decrease in credit quality that accompanies it, by reducing credit limits. In particular, credit limits from banks without store links are reduced by 27% when the card is issued, and by a further 50% through the following year. This reduction partially offsets the increase in credit supply caused by the new card, with total credit limits increasing by 24% when the new card is issued, but only by 19% one year later. Together, these results provide evidence that default externalities have a first order effect on the volume and composition of consumer credit, as well as on consumer default rates. Moreover, when considering how competitors react to a new credit card, default externality channels dominate alternative channels such as consumer poaching.

The effects also appear to be markedly heterogeneous across different types of borrowers. For instance, the effects of a new card on indebtedness and default are twice as large for sole

[6] Note that resolution SBS 8181-2012 was not in effect during our sample. Beginning in 2013, this resolution reduced banks' ability to unilaterally change credit limits by requiring a minimum of 30 days notice.

[7] Roughly defined as 60 days past due; see the appendix for details.

proprietors than for contractors or firm employees, suggesting that individuals prone to moral hazard might be particularly vulnerable to default externalities. Moreover, in the case sole proprietors, decreases in credit limits from existing lenders completely negate the initial increase in credit limits coming from the new card. I also find larger effects for consumers without a college education, and for consumers with credit card limits concentrated amongst a few lenders. Although causality is less clear for these sets of results—after all, variables such as education and job type are endogenous—they suggest that the curtailing effect of default externalities *vis a vis* the expansive effect of behavioral theories is stronger for potentially subprime borrowers.

These findings stand up to several robustness checks and specifications. For instance, stores are often located exactly at district boundaries, possibly because zoning laws and local regulations preclude them from opening in their target districts. In this case, an instrument that only considers store openings in consumers' districts of residence might be weak and lead to inconsistent estimators. To assess this possibility, I employ an alternative instrument that includes neighboring districts and weights each district by how much it overlaps a 1 km. radius around each store.

Another critique lies in the possibility that customers were not using the cards to borrow, but rather to take advantage of promotional rates and arbitrage against deposit rates. However, regressions against end–of–month accrued interests were positive and significant, showing that the effect was not only due to transaction customers but also to credit card revolvers.

This paper has strong implications in the policy evaluation of credit programs. Increases in credit supply from an outside source—such as a government agency or a microfinance institution—might end up being offset by decreases from existing lenders, who react to larger default rates. Impact evaluations would thus overstate the magnitude of credit supply shifts and understate their impact. In the extreme, if the initial shift in credit is completely offset by the existing lenders, no changes in credit supply would occur and the only empirical studies would only reflect the effects of reallocation of credit amongst lenders.

Further, if default externalities are stronger for some groups than for others, identical treatments would result in disparate credit supply shifts. Studies that show heterogeneous effects of credit supply shifts might only be reflecting heterogeneity in the total credit supply shift that occurred and not in the response.

This paper also contributes to the literature on consumers' marginal propensity to consumer and their reaction to credit supply shifts (Gross and Souleles 2002; Agarwal, Skiba, and Tobacman 2009; Bertrand and Morse 2009; Karlan and Zinman 2009; Mian and Sufi 2011). It reinforces existing evidence on the existence of liquidity constraints, a high marginal propensity to consume, and the effects of indebtedness on default rates. This evidence complements the existing literature on consumer behavior in credit card markets (Gabaix and Laibson 2006) by providing an alternative argument for the existence of high interest rates.

We can also extract two insights related to the regulation of consumer credit (see for instance Agarwal et al. 2015). First, if consumers diversify their borrowing across more credit cards, lenders will face a larger common pool problem and will have less incentives to curtail their credit, in the presence of a new outside loan. This implies that limiting the number of participants in this markets might make them more stable and less prone to oversupply.[8] Second, regulations that alleviate default externalities by modifying the contract space, such as adding bankruptcy provisions (Bisin and Rampini 2006), have the potential of having large effects on consumer credit supply and their overall riskiness.

Finally, this work contributes to the literature on banking–commerce separation (Raskovich 2008; Barth et al. 2012) by providing indirect evidence on the synergies between the retail and credit industries, through the relationship between credit card issuance and store openings.

The paper proceeds as follows. Section 2 presents the relevant theories on default externalities and consumer behavior, and introduces the six testable hypotheses. Section 3 describes the Peruvian credit card market and details the sample selection process and variable definitions. Section 4 develops the econometric methodology and the identification assumptions. Section 5 contains the main results. Section 6 presents robustness checks and additional tests. Section 7 concludes. The Appendix, as well as the Online Appendix include additional robustness tests and methodological details.

2 Framework

In this section I discuss the two main sets of theories that can explain how changes in credit availability from one source affect the supply of credit of competing lenders. In the absence of externalities, the actions of one lender would only affect its competitors through market prices. However, externalities can occur if credit supply shifts affect the profitability of existing loans. If consumers react to an increase in credit supply by borrowing more, they might also experience higher interest charges and a larger probability of default. In the first case, higher interest charges increase lender profitability and create incentives for creditors to expand its credit supply. In the second, a larger probability of default decreases the net margin and might lead to credit curtailing from competing lenders.

Which of these two channels prevails is an empirical question, and might also depend on consumer characteristics. Thus, after presenting the main theories, I outline the six testable hypothesis that will be taken to the data.

[8] On the other hand, if other frictions are causing undersupply problems, having more lenders might fix those

2.1 Default Externalities Theories

Default externalities are a class of contractual externalities caused by common pool problems between lenders.[9] Broadly speaking, they occur if the costs of default are spread across all lenders. When a bank decides its supply of credit, it only considers the default costs that apply to its share of lending, without internalizing the costs incurred by other banks incur due to its action.

Two factors conflate to make default externalities pervasive across credit markets. First, in credit transactions—*vis a vis* spot transactions—lenders are only paid in the future and thus borrowers have the ability to renege on their contracts by defaulting. Second, although the externality could potentially be solved with contract exclusivity clauses or collateral provisions, the contract space is incomplete and neither exclusivity nor secured lending are common features of credit card contracts[10]. Thus, borrowers are able to enter multiple simultaneous contracts, a feature extremely prevalent in practice. For instance, in 2013 only 18% of U.S. credit card–holding households held only one card, and the average card–holding household held 4.2 credit cards simultaneously[11].

The first default externality model—and the one we follow more closely—is the one by Bizer and DeMarzo (1992). Several features of this model are also emphasized in the empirical literature (Ausubel 1991), such a positive probability of default, over–indebtedness, and high interest rates in equilibrium. This economy has an infinite number of risk–neutral banks and a representative consumer that sequentially offers them debt contracts that can be either accepted or rejected. One of its key ingredients—together with disallowing clauses contingent on future actions of the consumer[12]—is the existence of moral hazard: the effort exerted by the consumer affects the distribution of his future income. This moral hazard component will then allow us to state an empirical prediction of the model: everything else kept constant, an increase in debt decreases the optimal effort and thus increases the probability of default.

If we could enforce exclusivity in this economy, we would reach an efficient equilibrium that maximizes the borrower's utility subject to a zero–profit conditions for the bank, and has

[9] Bernheim and Whinston (1986) provide a general discussion of the common agency or common pool problem. David and Lars (2003) discuss the specifics of *intrinsic* common agency and its relation with contractual externalities. Pauly (1974), Hellwig (1983) and Arnott and Stiglitz (1991) discuss contractual externalities in insurance markets, with mechanisms very similar to those underlying default externalities: purchasing additional insurance from new sources induces buyers to exert less effort in mitigating losses, which increases the frequency and the severity of loss events.

[10] Moreover, and as argued by Degryse, Ioannidou, and Schedvin (2012) for firm lending, the costs of writing and enforcing covenants may be such that even if possible, such clauses may prove unfeasible given the small scale of credit card lending.

[11] Data from the 2013 Survey of Consumer Finances.

[12] In this model, seniority clauses are not enough to reach an efficient equilibrium under moral hazard. Nonetheless, other clauses might be able to eliminate the default externality channel, but come at a steep cost as they introduce other inefficiencies, such as debt hold–up problems (see also Attar et al. 2013).

a positive probability of default (for certain parameter values). Moreover, the marginal interest rate would take into account not just the actuarial cost of credit (the risk–free interest rate combined with the probability of default), but also the devaluation of the preexisting debt due to the increase in the probability of default. If instead non–exclusive banking is allowed, the optimal contract would not be sustainable. This is because new banks will determine their marginal interest rates based only on the actuarial cost of credit, and not on the damages caused to preexisting lender because of the larger default rates. Thus additional credit will force the preexisting bank to lose money, violating their rationality constraint. To find the equilibrium in this case, we need to find a set of contracts that are so risky and expensive that new lenders will have to charge very high interest rates, dissuading the borrowers from accepting them. This concept, akin to "salting the earth" to prevent more borrowing, is also a component of the other existing default externality models. Finally, notice that default is not strategic in this setup, but depends instead of bad income draws.

Parlour and Rajan (2001) take a different route, and show how default externalities are possible even under the absence of moral hazard or other sources of asymmetric information. In contrast with Bizer and DeMarzo, lenders are the ones who offer the contracts, and do so simultaneously. This allows for extraordinary bank profits, which Ausubel also emphasizes but that Bizer–DeMarzo preclude. On the other hand, some of the Parlour–Rajan implications are hard to reconcile and to put up to test with credit card markets: there is no default in equilibrium; in some scenarios the non–exclusivity problem might be solved by adding *more* lenders to the market; and one of its key implications is that lenders prefer laxer bankruptcy laws and borrowers prefer stricter bankruptcy laws[13].

Under the Parlour–Rajan model, borrowers choose whether to invest the loaned funds into an asset with a standard production function (increasing, strictly concave) that can be sequestered by lenders in case of default, or into a linear technology that is partially exempt from bankruptcy. This choice implies that consumers will default strategically: if they receive large enough credit offers and the fraction of debt exempt from bankruptcy is high enough, consumers will accept all credit offers and abscond with the funds. Also, and similarly to Bizer–DeMarzo, if exclusivity can be enforced, the competitive outcome will be attained in equilibrium. This outcome can also be reached with non–exclusivity, as long as the fraction of the linear investment exempt from the bankruptcy is low enough. If that is not the case, then to solve the equilibrium one must set the amount of lending so high that all potential lenders will be discouraged from offering credit. Although similar to Bizer–DeMarzo, a crucial difference is that latent contracts do matter for determining the equilibrium outcome, because they are

[13] This is in stark contrasts with the Bankruptcy Abuse Prevention and Consumer Protection Act of 2005, which imposed stricter bankruptcy requirements, and was supported by banks and credit card companies and opposed by consumer advocates (White 2007)

taken into account by the consumer when deciding whether to default or not. This also complicates empirical assessments of the effects of new entrants in the Parlour–Rajan model, as in this setting the mere offer of additional credit might trigger strategic default before existing creditors have time to react and curtail their lending.

The model by Bisin and Guaitoli (2004) is similar to Bizer–DeMarzo in that they rely on moral hazard and unobserved effort to explain positive profits in insurance and unsecured credit contracts. They show that non–exclusivity may lead to low effort, or to an equilibrium with multiple contracting and positive profits for lenders. Additionally, default exists with positive probability in their model but is not strategic. Bennardo, Pagano, and Piccolo (2015) focus instead on an environment with collateralizable assets, and find that as the value of collateral becomes more volatile, interest rates and default rates both increase. Further, they explore the set of possible credit market equilibria as a function of the level of creditor rights, and find that the outcome described by Parlour–Rajan (where banks lend at non–competitive rates and a single bank provides lending) is not the only equilibrium at intermediate levels of protection (at low levels markets break down and at high levels the competitive outcome can be attained). In particular, they describe the rationing equilibrium, where lenders lend with a mixed strategy and where the amount of credit (and of default) depends on the luck of the credit applicant.

All together, these models share the existence of non–competitive equilibria, but may have different implications regarding consumer over– or under–indebtedness and the frequency of default in equilibrium.

2.2 Behavioral Theories

Hsieh (2004) classifies credit card customers in three categories: revolvers, transactors and convenience users. Revolver users use credit cards as a source of financing and carry a balance every month, thus paying interest rates. Convenience users use credit cards to occasionally finance large purchases, but do not permanently carry a balance. Finally, transactors—informally referred to as deadbeats by banks—do not carry a balance and pay the cards in full at the end of each month or before any existing grace period expires. Across these groups, revolvers and convenience users are the most profitable due to the interest charges, while transactors only provide income through interchange fees for lenders.

An important possibility that might arise when consumers receive a new credit card, is that the increased availability of credit might lead consumers to transition from one category into another. For instance, a new credit card might be large enough to allow a transactor to purchase a costly non-durable, transitioning him into a high-profit convenience user. Similarly, if transactors overspend on the new card and are unable to repay the balance in full at the end of the month and turn into revolvers. In these scenarios, a new credit card can increase the interest income of existing lenders, and give them incentives to increase their own supply of credit.

Another alternative is that consumers might perceive credit card limits as a desirable attribute, and thus creditors might increase these limits in order to improve the attractiveness of their card and prevent poaching (see Drozd and Serrano-Padial 2012 for a description of poaching in credit card markets). This effect should be larger for wealthier consumers with a low probability of default, and where most of the income is earned through interchange fees.

It is important to acknowledge that these hypothesis are dependent on the existence of behavioral consumers. For instance, mental accounting costs might explain the preference for only using the credit card with the largest limit. Similarly, budgeting mistakes (Agarwal et al. 2013) might be behind consumers switching from transactors into revolvers. Nonetheless, in private interviews, practitioners reflected a strong belief on these theories. If this is the case, they might cancel out any credit curtailing driven by default externalities. In the event of a new credit card, existing lenders might act as multipliers, further increasing credit limits, instead of as stabilizers, if they contract credit limits.

2.3 Testable Hypothesis

Based on the literature discussed above, I derive six hypothesis on the existence and strength of default externalities, net of the potential effect of behavioral theories. The first three link credit supply, consumer borrowing, and consumer default. I then focus on how consumer characteristics (i.e. the deep parameters of the models) affect the strength of our results. Notice that not all default externality models lead to the same set of implications, so in section Appendix H I derive a simple example that encompasses their main features in an unified setting.

Hypothesis 1. *(Some) consumers are credit constrained and will thus borrow more if given the opportunity*

This result follows directly from the Parlour-Rajan model with high enough bankruptcy protections, where consumers will default strategically if given enough credit access. In the case of the Bizer-DeMarzo model, the equilibrium is such that consumers do not want to borrow more unless at preferred conditions, which can be achieved if the new entrant can translate part of the retail–banking synergies to consumers through lower rates. In addition, as seen in lemma 6 of section Appendix H, it is possible to derive this equation from a very simple utility maximization problem in a setting with pecuniary costs of default.

Hypothesis 2. *The probability of default increases with the amount of debt*

In a moral hazard setting such as Bizer-DeMarzo (section III), as the amount of debt increases, the marginal utility of exerting effort decreases, which raises the likelihood that the debt will not be paid. The PR model also features this prediction albeit in a more extreme fashion: starting from equilibrium, one extra dollar of credit will immediately trigger a strategic default. Again, as seen in section Appendix H lemma 3, this statement can also be derived directly from the utility maximization problem of a representative agent under moral hazard.

Hypothesis 3. *If other banks exogenously increase their credit limit x_{-j}, bank j will reduce his credit to offset the additional risk*

In the Bizer-Demarzo model (section VI), an increase in outside credit would make it immediately unprofitable for existing lenders to maintain their previous credit levels, triggering either a reduction in credit or an exit from the market. In contrast, in the PR model adding a third lender to an equilibrium where only two lenders offer positive credit would break down the existing equilibrium. However, a new equilibrium could be attained if one of the two original creditors reduce their lending by the same amount as the original increase. Notice that in both models the total amount of credit could return to the original amount, with the entrant taking the place of one of the previous creditors. Additionally, we can also derive this result in lemma 7 of section Appendix H, by applying the implicit function theorem to the banks' first order condition. In this case, if an equilibrium exists, it will necessarily a entail lower credit volume for the preexisting creditors.

We now consider variables that could moderate the effects described above:

Hypothesis 4. *The effects described in hypothesis 1–3 will be stronger for borrowers more prone to moral hazard.*

In the Bizer-DeMarzo model, moral hazard is what drives default externalities: if a consumer has more debt, he will have larger future obligations, so a larger share of his future income will be unavailable to him, and he will have less incentives to exert effort. This is particularly detrimental for workers were effort plays a larger role, such as entrepreneurs. On the other hand, individuals where effort does not affect future income will be unaffected by this problem.

In this paper, we compare sole proprietors against contractors and employees, and postulate that the effort made by a firm owner has a more direct link to his future income in comparison with a salaried worker, who affects a smaller fraction of the firm where he works. This can be embedded in the Bizer-DeMarzo model as a disutility of effort v more convex for the case of sole proprietors.

Alternatively, as credit card debt is commonly used to finance startups (Robb and Robinson 2014), regressions on sole proprietors might be capturing a different type of moral hazard, where firm owners redirect credit card funds to their own businesses.

Hypothesis 5. *The effects described in hypothesis 1–3 will be stronger for less wealthy and riskier consumers*

In terms of the Bizer-Demarzo model, the *bad outcome* scenario of a wealth or safe consumer may be such that he never enters the equilibrium with default. Therefore, on average, richer consumers are less likely to be credit constrained and default externalities might apply less to them. I proxied these attributes with education. I separated individuals that only completed secondary education (high school) from those that have at least started college when they were 18 years old (in Peru, students usually start college at age 16 or 17)[14]. Note that this classification actually proxies for multiple variables besides education: ability, income level of the parents, and even age. Nonetheless, this regression is still useful in pointing towards what types of individuals are more susceptible to be affected by default externalities.

Hypothesis 6. *After an exogenous increase in credit access, existing lenders will curtail credit more if credit to a consumer is concentrated in few borrowers. In turn, the total expansion in credit supply for these borrowers will be lower.*

This occurs because lenders with more *skin in the game* benefit more from reducing credit limits. For instance, suppose a consumer with 1000 dollars in debt. Also, a lender can reduce his credit by 100 dollars, and by doing so it can lower the consumer's probability of default from 10% to 9%. If the lender is the only creditor, curtailing credit would accrue the lender an expected 10 dollars ((1000×0.01)), but if it is only one of five equally sized creditors, it would only accrue 2 dollars by doing so ((200×0.01)).

We measure concentration by estimating the Herfindahl-Hirschman Index (HHI) of each individual with respect to credit card limits. Thus, individuals with only one credit card will have an HHI of 1.00, and individuals borrowing from multiple sources will have a lower HHI. Note that a limitation of this approach is that I do not model how the current HHI of each individual arose, and instead take it as given.

3 Market Characteristics and Data Description

3.1 The Peruvian Credit Card and Retail Markets

Through the Jan2006–Aug2012 sample period, the Peruvian credit card and retail markets expanded substantially (Figure 1). The number of credit cards increased by 87%—from 3.87m to 7.25m—and the number of retail stores increased by 243%—from 67 to 230 stores. Without these increases, our empirical strategy would not have been feasible, as the strength of the first

[14] I exclude elementary education due to lack of observations.

stage regressions depend on the correlation between new bank-related stores and new credit cards from store–related banks. Further, since credit card and retail penetration start from a lower installed base, a large fraction of the population is then *treated* by the store entry, making the instrument more general (i.e. less *local* in treatment effects jargon) than in an setting where most of the population have already received credit cards and the retail market is mature.

[Figure 1 Goes Here]

My empirical strategy depends not only on observing enough store openings and new credit cards in the sample, but on their composition. Bank-related store openings are required in order to have an instrument, and independent store openings are required to control for the possible endogeneity of store openings with respect to local economic activity. Similarly, the credit cards from store–related banks constitute the endogenous variable, but the outcome of interest is changes in credit limits of cards issued by independent banks.

As seen in table 1, the credit card market comprised both bank-related and independent banks in a similar proportion[15]. Similarly (table 2), three of the retail chains were bank-related while two were independent[16]. Notice that this changed after August 2012, when the largest supermarket retailer (Cencosud) launched a sister bank, and also through 2012 and 2013, when a small bank (BanBif) began offering cards and opening branches in the stores of the remaining independent retailer (Makro).

[Table 1 Goes Here]

[Table 2 Goes Here]

Similarly, during our sample period, banks were able to unilaterally change credit card limits, which allows us to estimate the effect of credit shocks on credit limits from competing banks. This situation changed after our sample ended, when several credit card regulations were issued[17], requiring a minimum of 45 days of notice for changes in credit card contracts—such as interest rates and credit limits—as well as consent from the consumer for credit limit increases.

3.2 Data Sources

The main data source for this paper is the credit bureau records from the Peruvian banking regulator (*Superintendencia de Banca, Seguros y AFP*). These contain end–of–month debt balances and credit limits for every borrower and bank between January 2006 and August 2012[18].

[15] As of August 2012, store–related banks had a 60% market share in terms of number of customers, and a 51% market share in terms of credit volume from credit card loans.

[16] Bank-related retail chains comprised 69.5% of the total stores (August 2012) and 71.6% of total sales (2012).

[17] In particular, see the *Resolución SBS 8181-2012* published on October 29, 2012.

[18] See appendix A# for a detailed discussion on the span of data availability.

I augment this dataset with individual-level demographic and geographic characteristics from multiple public records, as well as district–level controls and store opening information.

Sunat, the tax authority, maintains public records of every past and present Employer Identification Numbers (EIN). These comprise all contractors, sole proprietors and firms, and include information such as profession, registration date, and address. Similarly, the *Superintendencia de Banca, Seguro y AFP* maintains a record of all pension fund affiliates, and includes several demographic variables such as registration date, date of birth, and address. Together, the EIN and pension records comprise the universe of all workers and past workers. They are complemented with demographic records from the electoral roll of the Peruvian 2011 election—which include the district of residence for each individual—provided by LineaGIS, a local geocoding firm, which also provided geocoding assistance. By combining records from these three datasets, I am able to assign a district of residence to each individual in the credit bureau records[19].

Store opening records are hand collected from local news sources and validated against totals from the annual reports of each retailer. They comprise the address and date of opening of every store. In most cases, two independent news sources are used to verify the accuracy of the information. These include the records of *Semana Economica*, a specialized local publication that provided access to its extensive news archive. The dataset is available in the online appendix.

Finally, the exact date and location of each bank branch opening was retrieved from *El Peruano*, the official government gazette—where all laws and regulations are published. They were retrieved with text extraction techniques and validated against the monthly counts of bank branches per district from the banking regulator. This dataset is available on request.

3.3 Sampling Process

I build a panel of credit, demographic, and geographic data by matching all the datasets on the national identification numbers of all borrowers. This entails excluding military personnel and foreign citizens, who have different individual identifiers. In order to have a one–to–one match between individuals and credit records, joint debt accounts—1.09% of the remaining credit bureau accounts—are also dropped. Further, I exclude individuals for which no zipcode could be associated (0.5mm of the remaining 18.9mm individuals, or 2.6%)[20].

Once the population panel was constructed, I applied three rules to construct the regres-

[19] The online appendix describes the rules used if the datasets report conflicting information. On unreported regressions, results appeared robust to alternative rules and to subsamples where the records coincided across datasets. This is consistent with the fact that instrumental-variable regressions are still valid under random measurement error of the instruments.

[20] This could occur if the individuals moved out of the country or had passed away by the time the tax, pension and electoral registries were collected in 2011–2012, or if the national identifier provided by the banks was invalid.

sion sample. First, because we are interested on the effect of credit supply shocks on credit limits of existing cards, only individuals that currently held a credit card and had held one twelve months prior were included[21]. Second, individuals that were in default (see definition in the appendix) were excluded because they exhibited a different behavior and are treated differently by banks. Finally, I dropped individuals from districts that at no point in time—including before the start of the sample—had a store opening nor were neighbors of districts that had store openings.

The final sample—excluding singleton groups—consists of 117,309,310 monthly observations and a total of 2,873,208 borrowers.

3.4 Variable Definitions

Total credit is defined as the sum of all credit accounts maintained by the credit bureau for each individual. It excludes mortgages, which are relatively rare in the Peruvian economy in comparison to the U.S. economy, and which also would complicate the analysis as they are backed by collateral. **Store openings** count the number of stores that opened in a district in a given month. I distinguish between bank-related store openings (i.e. from stores related to Interbank, Banco Falabella and Banco Ripley) and independent store openings. In the first stage regressions we might also distinguish between *de novo* store openings, for the first store opened by a retailer[22] in a province, or *repeated* store openings, for subsequent stores. **New credit cards from store–related banks** count the number of new cards issued to an individual by any of the three store–related banks in a month[23].

An individual is in **default** if a non–trivial amount of her debt is at least 60 days past due. This definition is strict when compared to the Federal Reserve definition of 90 days (Federal Financial Institutions Examination Council 2015), but matches the internal criteria employed by the Peruvian banking regulator. Note that I do not distinguish between default and delinquency, as data limitations precludes distinguishing informal defaults versus formal bankruptcy (see Benjamin and Mateos-Planas 2012; Drozd and Serrano-Padial 2014 for a discussion on these definitions). Finally, default is treated in the regressions as an absorbing state, so the variable *default in n months* includes all individuals who defaulted at any month between $t+1$ and $t+n$, and not only those who were in default by $t+n$. For more details on the construction of this variable, see Appendix B.

[21] On 16 occasions the information submitted by a bank to the regulator was not fully compliant (e.g. they failed to report individuals with credit cards that maintained zero balance). Those cases were excluded although results were still robust to keeping them in the sample. See the online appendix for a detailed list.

[22] Only the five retail conglomerates of table 2 are considered, not individual chains within each conglomerate.

[23] Note that although store openings and new credit cards are count variables, due to their sparsity they are in practice closer to indicator variables, and thus results do not change if we treat them as such and truncate them to a maximum value of one.

All the other variables used in the paper are detailed on table A1.

3.5 Summary Statistics

Table 3 contains summary statistics of the variables used in the regression analysis. The average individual in the sample maintained a debt balance of \$2,920[24], \$1130 of which was from credit cards. Additionally, credit card limits averaged \$4500 per individual, with a mean usage ratio of 31.9%. As expected, the credit variables are positively skewed and follow a power law, with low medians and high standard deviations.

In terms of demographics, 45% of the borrowers are female, 44% have at least some level of college education, and the average borrower is 43 years old, with a credit history of 7 years. These figures reflect the pattern that individuals with access to credit markets are usually older and more educated than the average population (Crook and Hochguertel 2007).

[TABLE 3 GOES HERE]

4 Estimation Strategy

Our empirical focus lies on identifying a causal relationship between consumer credit availability and three types of outcome: consumer credit, default, and the credit supply decisions of competing banks. The identification strategy is discussed below for credit outcomes, with the specifics for the other classes discussed in section 4.1.

To measure credit availability, I use the number of credit cards issued to each individual. An alternative is to use total credit card limits, but a lot of the variation in credit card limits comes from mechanical increases (Gross and Souleles 2002) that could be anticipated by both competitors and borrowers, complicating the identification. I estimate the models in first differences to eliminate individual fixed effects, and augment it with additional sets of fixed effects and controls:

$$\Delta \log(100 + Credit_{it}) = \alpha_i + \delta_t + \nu_t C_z + \gamma\, \Delta X_{zt} + \rho\, \Delta Stores_{zt} + \beta\, \Delta Cards_{it} + \epsilon_{it} \quad (1)$$

Our parameter of interest is β, which represents the semi–elasticity of credit with respect to obtaining an additional card. i indexes individuals, t time, and $z = z(i)$ is the zipcode (district) where an individual lives. Since cardholders may have a zero debt balance, I avoid missing

[24] At the average PEN–USD exchange rate of 0.34.

values by adding 100 Peruvian Nuevos Soles to the amount of credit—roughly equivalent to \$34[25].

Endogenity of Credit Card Issuance.— Credit card issuance only occurs if a bank finds it profitable to offer the card and the consumer decides to accept it. Thus, obtaining a new card is endogenous with respect to a myriad of individual characteristics such as income and consumption patterns, which are often unobservable to the econometrician. To address this issue, I use an instrumental variable approach. I exploit the fact that some banks are part of the same economic conglomerates as supermarket and department store chains, and that when new retail stores open, the banks related to these stores issue significantly more credit cards to individuals living in the districts where the store opened[26].

By using store openings of bank-related chains as excluded instruments, I wipe out the endogeneity of new credit cards with respect to idiosyncratic shocks, but endogeneity concerns with respect to local economic conditions remain. In fact, store openings can be thought as anything but random, as retail firms decide the location and timing of store openings in order to maximize profits. For instance, one may be concerned that store openings occur as a response to strong local economic growth, or that the new stores—and the construction activity they entail—may actually spur local growth, at least on the short term.

To eliminate the endogeneity of bank-related store openings, I add as an included instrument a variable indicating *all* store openings (i.e. both bank-related and unrelated). If bank-related stores are comparable to unrelated stores, both will open in locations with similar characteristics and any effect they might have on local activity will be commensurable. To test the validity of this assumption, we explore two pieces of evidence. First, table 4 compares the characteristics of districts with bank-related store openings against districts with unrelated store openings, 12 months before each opening. As we can see, both types of stores opened on districts with similar demographics and access to credit[27]. Additionally, figure 2 shows the geographic distribution of both sets of stores. Both bank-related and unrelated stores open in overlapping locations, and there is no visible pattern that distinguishes them. This addresses concerns that bank-related stores may have a bias towards opening in e.g. different regions of the country, rural areas, or suburbs. Regarding store characteristics, although evidence is more

[25]Results are robust to multiple alternatives specifications, including adding 10 and 1000 Peruvian Nuevos Soles, and completely replacing the log transform with the less arbitrary inverse hyperbolic sine function.

[26]A reason for the strong correlation between store openings and new credit card issuance is the existence of synergies between the retail and credit businesses, due to cross selling between both sectors. For instance, stores may offer discounts to users that pay with the store–related credit cards, which subsequently makes the consumers more likely to continue using the cards for other transactions. This hypothesis is also backed by anecdotal evidence from local news sources, as described in the online appendix.

[27]In contrast, the online appendix reports the same table at the month when the stores opened. Results are very similar except for the frequency of new credit cards, which is substantially higher in districts where a bank-related store opened, in line with the predicted effect of bank-related store openings on credit card issuance.

scarce, we can see from table 2 that bank-related store chains are neither larger nor smaller than unrelated chains. Similarly, the online appendix lists investment and worker count figures in cases where the information was available, and there was no discernible pattern regarding the size of both sets of stores.

[TABLE 4 GOES HERE]

[FIGURE 2 GOES HERE]

Given this, the first–stage equation in differences is:

$$\Delta Cards_{it} = \tilde{\alpha}_i + \tilde{\delta}_t + \tilde{\nu}_t C_z + \tilde{\gamma}\, \Delta X_{zt} + \tilde{\rho}\, \Delta Stores_{zt} + \pi\, \Delta RelatedStores_{it} + \eta_{it} \quad (2)$$

Where π represents the strength of the correlation between store openings and new credit cards issued.

Running the regression in differences allows not just for faster computations, but provides an advantage to our estimation strategy. If instead we would estimate the model in levels, the estimates would be driven not just by the contemporaneous correlation between stores and new credit cards, but also by lagged correlations that may occur due to long term trends in local growth anticipated by the store chains.

***De Novo* Store Openings and Time Bandwidth of Instrument.—** In order to improve the strength of the first–stage regressions[28], I modify store opening instrument in two ways. Instead of counting how many bank-related stores open in a district in one month, I classify store openings into *de novo* openings (for the first opening of a retailer in a province[29]), and repeated store openings (for subsequent ones) and treat them as separate instruments. The rationale behind this is that the effect of *de novo* openings on credit card issuance is an order of magnitude larger than that of repeated openings.[30] Also, since banks might not necessarily issue the cards the same month a store opened[31], I will use a time bandwidth and include in the instruments not just the stores opened in the current month, but openings from the two previous months to the next month (i.e. from $t-2$ up to $t+1$). Note that, as described in the online appendix, results are robust to different instrument definitions, as well as alternative bandwidths and to regressions at a quarterly frequency instead of monthly frequency.

[28] See Wooldridge (2010), section 5.2.6 for a discussion on how the explained variation in the first stage affects the standard errors of the second stage.

[29] Politically, Peru is divided in 24 departments, 194 provinces and 1828 districts.

[30] Alternative specifications used as robustness checks instead interact store openings with the identity of the bank to which the store is related (thus creating three excluded instruments), but due to weak instrument concerns (Imbens and Rosenbaum 2005) I abstain from using many excluded instruments.

[31] Banks may decide to issue the credit cards right before a store opens (so the cards are already in place when it does), or after it opens (using the store infrastructure to market the credit cards).

Additional Controls.— Bank branches are often co-located with retail stores of the same conglomerates, and open simultaneously. Since proximity to bank branches might affect credit (Jayaratne and Strahan 1996; Agarwal and Hauswald 2010; Ergungor 2010), we need to control for the effect of bank branch openings. We also include bank branch density in the district (number of branches per capita) in order to control possible long–term effects of the existence of bank branches.

Additionally, to further control for local economic activity, we set up and include four measures of employment and firm creation. These measure the growth in registrations of i) employees, ii) contractors sole and proprietors, iii) firms, and iv) construction firms, all at the district level. Since retirements and firm closings are poorly recorded in the dataset, our growth variables are constructed by dividing the number of registrations in the last year over the total number of registrations over the last five years.

Unobserved Heterogeneity and Fixed Effects.— We further control for unobserved heterogeneity at the individual, district, and time level, by adding three sets of fixed effects to our specification (see Gormley and Matsa 2014 for a discussion on the advantages and limitations of high–dimensional fixed effect models). Time fixed effects control for aggregate shocks affecting consumer borrowing. However, economic cycles may have an heterogeneous effect depending on the strength of the credit market in each district. Thus, following Duflo (2004), we add time fixed effects interacted with a vector of initial conditions by district ($\nu_t C_z$). We set C_z to be the initial depth of the credit card market, defined as the number of credit cards per capita for each district in January 2005 (one year before the sample started). Finally, although individual fixed effects are already removed by differencing, individuals might be at a different point in their consumption life cycle (Cocco, Gomes, and Maenhout 2005; Fernandez-Villaverde and Krueger 2011; Gourinchas and Parker 2002). For instance, younger and more educated borrowers might be increasing their credit with time, while other consumers further into their life cycle might be reducing or maintaining their indebtedness levels. Alternatively, their level of financial expertise might vary with their lifecycle (Agarwal et al. 2009). To control for this, we add a first order approximation of the individual life cycle by adding heterogeneous time slopes ($\alpha_i t$), which after differencing appear in the model as individual fixed effects (α_i).

Note that until recently, it has been unfeasible to estimate large models with multiple levels of fixed effects. However, thanks to a novel fixed effect estimator (Correia 2015), these models are now possible with large datasets[32].

[32] All results are estimated using the Stata packages *reghdfe* (Correia 2014) and *ivreg2* (Baum, Schaffer, and Stillman 2002). Singleton groups are excluded and the degrees of freedom are adjusted with the procedure from Abowd, Creecy, and Kramarz (2002).

4.1 Externality Effects

I study how incumbent banks react to an increase in credit availability by observing the lagged effect of a new credit card on the credit limits of banks without store links. Two things should be noted of this definition. First, to avoid the problem of a card being both a treatment—when issued—and an outcome—when observing changes in its credit limits—the outcome comprises only credit card limits from cards issued by banks without stores. Second, banks might only have a lagged reaction the issuance of a new credit card. This is because banks only learn about new cards from the competition with a delay[33], and may only react after they learn about the new card.[34] Therefore, we will include new cards as a regressor both concurrently—to estimate any anticipated response—and with a 12 month lag—to absorb the response after the bank has learned which customers acquired a new card. This also implies that we need to set up the model in levels instead of in differences[35].

[FIGURE 3 GOES HERE]

4.2 Assumptions in the Instrumental Variable Estimation

Under homogeneous treatment effects—where the effect of credit access on borrowing and other outcome variables is the same across distinct groups—the excluded instruments must satisfy two conditions in order to be consistent (Wooldridge 2010, chap. 5): i) they must be uncorrelated with the error (exogeneity), and ii) they must be correlated with the endogenous variable (relevance). In our setup, the first condition is achieved if bank-related store openings are comparable to unrelated store openings. Regarding the second condition, the first–stage results reported in table 5 show a very strong link between bank-related store openings and new credit cards.

Under heterogeneous treatment effects, the IV approach instead estimates instead the local average treatment effect (LATE), given that the following conditions are sastified: random assignment, exclusion restriction, and monotonicity (Wooldridge 2010, chap. 21). In our model,

[33] A new credit card must first be reported to the credit bureau at the end of the month where it was issued. The credit bureau then processes the information in 2–3 weeks and sends it to all the banks, which then add it to their own databases.

[34] Customers might perceive credit limit decreases as demotions, which elicit strong negative reactions (Tillmann Wagner, Thorsten Hennig-Thurau, and Rudolph 2009) that translate into lower loyalty and card usage. In such a scenario, a bank may opt to avoid a small nominal credit limit decrease, and replace it with inaction in the next review period, which is equivalent to a decrease in real terms, more so considering the large annual growth in credit card limits that most customers experienced through the sample period.

[35] Estimating the model with two endogenous regressors complicates the application of the over identification and weak identification (Stock Yogo) tests. To address this problem, we apply the double–or–nothing approach of Kovandzic et al. (2015). There, the authors note that testing the strength or weakness of the identification can be done with contemporaneous effects only, even though the first stage regressions might include both contemporaneous and lagged credit cards and store count variables.

the monotonicity assumption requires that even if for some individuals store openings do not affect card issuance,[36] there are no individuals for whom a credit card is less likely due to the store opening.

In order to consistently estimate the standard errors, the standard assumption of homoscedasticity might not hold (Petersen 2009), and White standard errors might be inconsistent (Stock and Watson 2008), so instead I will report standard errors robust to clustering at the individual level.

4.3 Bias in Store Opening Instruments

A possible improvement to the instrumental variable described above consists of considering all neighboring districts and weighting them by measures of store proximity. An important reason for this is the argument that stores intended to serve one district may instead be opened in the limit of a neighboring district, due to zoning regulations or land availability. Visual inspection of figure 4 and figure 5 shows multiple store openings that occur exactly in boundaries between districts. To address this issue, and as reported in the appendix, we replicate our main results with instruments where the instrument is weighted by the fraction of the district (in terms of area or EIN registrations) that falls within a 1 kilometer buffer around each store. In all cases, we obtain similar results.

[Figure 4 Goes Here]

[Figure 5 Goes Here]

5 Results

This section presents the main results of the paper, testing each of the six hypothesis outlined in section 2, and evaluating the strength of the first–stage equations between bank-related store openings and card issuance from store–related banks.

5.1 Credit Availability and Consumer Borrowing

Hypothesis 1 states that consumers react to a new credit card by borrowing more. To test it, we first focus on the effect of new bank-related cards on credit card borrowing from the three store–related banks and on total credit card borrowing (the direct effects on borrowing). Since this might be offset from reduced borrowing from other sources, I also test the effect on credit from other sources (substitution effects) and on consumer total credit (net effects).

[36] This might happen for individuals that already have cards from all store–related banks, or individuals for which the address has a measurement error and do not live in the reported district.

Table 5 reports the first stage regressions between bank-related store openings and new credit cards from store–related banks (these first–stage regressions also apply to all credit outcomes as they share the same sample). The *mean of the dependent variable* row shows that the consumers in our sample have a baseline probability of acquiring a credit card from a store–related bank[37] of 1.76% per month (21.1% annually). Columns (1) and (2)—which use the simple *bank-related store openings* instrument—indicate that a bank-related store opening raises the probability of obtaining a new card from these banks from 1.76% to 2.21%[38]. In contrast, store openings of chains without bank links only increase the probability to 1.79-1.80%, depending on the specification. Columns (3) and (4) report results using two instruments, *de novo* bank-related store openings and repeated bank-related store openings (a *de novo* opening is the first opening of a retail conglomerate in a given province). Regarding *de novo* store openings, bank-related store openings more than double the probability of obtaining a new card from these banks—from 1.76% to 3.97-3.98%—while unrelated store openings only increase it to 1.99-2.00%. For repeated store openings, the effects are smaller although still very significant, raising the probability by more than 10 percent in relative terms. Finally, in columns (5) and (6) we use instruments interacted by the identity of the bank to which the stores are related, and we see that although not all conglomerates have the same effect, it is in all cases both statistically and economically significant. Notice that the second–stage regressions that follow use the instruments of columns (3)-(4) (*de novo* and repeated store openings), although results are robust to the other specifications.

[Table 5 Goes Here]

Table 6 shows the second stage regressions of credit card issuance on credit card debt. For each outcome variable, we select four alternative specifications based on the inclusion of control variables and the fixed effect specification. Columns (1)-(4) show that a new credit card from a store–related bank leads to a significant increase in credit card from these banks, of around 51–53%. Similarly, columns (5)-(8) show that total credit card debt increases contemporaneously by 18-20%, suggesting that consumers have a large marginal propensity to consume, in line with previous research (Gross and Souleles 2002). For every regression, we also report the Kleibergen-Paap weak identification statistics, which strongly reject the null hypothesis of weak identification. Also note that since we have two excluded instruments and only one endogenous regressor, we can compute the Hansen J over–identification statistic, which for columns (1)-(4) rejects the null hypothesis of validity of instruments. This result can be

[37] We cannot distinguish whether a consumer has 1 or more cards with a given bank, so what we study is actually the probability of entering into a new credit card relationship, not of acquiring a new credit card.

[38] This effect applies to consumers living in the district where a bank-related store opens, and in the months surrounding the store opening (two months before the opening up to one month after)

explained by balance transfer campaigns run by the banks in particular for *de novo* store openings, in order to build enough scale in a region. However, the effect of balance transfers is netted out when considering total credit card debt, which makes these estimates more a more reliable measure of the direct effect of store openings, albeit a less direct one[39].

[Table 6 Goes Here]

The effects of new credit cards on sources of credit (auto loans, consumer loans, etc.), as well as the total effect on bank credit, is reported on table 7. We do not observe any evidence of substitution effects, with the coefficients for new cards insignificant for columns (1)-(4). Consistent with this finding, we observe that total credit increases by 10-13% following the new credit card. These figures are similar in magnitude but smaller than the increase in credit card debt, due to the latter being only a fraction of total debt.

[Table 7 Goes Here]

5.2 Credit Availability and Consumer Default

Hypothesis 2 states that default rates are increasing with consumer indebtedness. Given that the above results show that consumer debt is higher following a new credit card, we expect default rates to also increase. In order to assess this, table 8 shows how a new credit card affects default at different horizons[40]. We see that consumers are less likely to default immediately after a new credit card is issued (61% less in relative terms), but after one year default is more than twice as likely. These results suggest that consumers in risk of default might be drawing funds from new credit cards in order to pay existing debts. Subsequently, the increased indebtedness does lead to significantly larger default rates.

[Table 8 Goes Here]

[39] Another potential source of endogeneity can occur if as a response to store openings, banks not only issue more credit cards, but also raise the credit limits of their existing credit card customers. If such a channel exists, then the effect of store openings may be overstated as it is also masking the effect of credit limit increases. However, in the appendix we run the main regressions using both new credit cards and changes in credit limits of preexisting cards as endogenous regressors, and find no evidence of the second channel.

[40] Notice that the baseline frequency of default is relatively high and increases with the horizon (5.% at a six month horizon, 9.5% at a one year horizon); this is because for simplicity we treat default as an absorbing state (in particular, the outcome variable is 1 if the consumer enters default at any point between t and $t+h$, for a given horizon h) and the regulatory definition that we follow (60 days past due) is not particularly strict.

5.3 Strategic Response of Competing Banks

Hypothesis 3 states that banks' optimal level of credit limits is decreasing with the amount of credit available to the consumer from other sources. In particular, following the issuance of a new credit card, existing credit card providers may choose to curtail credit due to the increased riskiness of consumers. In order to observe this effect, we estimate a regression in levels that includes as endogenous regressors the number of store–related credit cards both contemporaneously and with a 12–month lag. As in the regressions in differences, the first stage results (detailed in the online appendix) are also very significant and of the correct signs.

Table 9 reports the effects of credit cards issued by bank-related stores on both credit card limits from banks unrelated to retail stores, and the net effect on total credit limits. As seen in columns (3)–(4), which document regressions where control variables are included, the immediate effect on credit limits from competing banks is between -27% and -35%, and the long effect—slightly larger—is estimated around -51% and -52%.[41]. All in all, the total change in credit limits is still positive on the short term—24-26% in columns (7) and (8)—but is not significantly different from zero after one year.

The strong negative reaction of competing banks provides strong evidence for the existence of a default externality mechanism. Similarly, the fact that the net effects are close to zero are also in agreement with predictions from default externality theories.

[Table 9 Goes Here]

5.4 Heterogeneous Effects by Consumer Characteristics

In this section, we study how our main results vary according to consumers characteristics and their levels of credit access, and relate these characteristics with the structural parameters discussed in section 2.

Hypothesis 4 states that default externalities will be stronger on individuals more prone to moral hazard, who will also exhibit a higher marginal propensity to consume (more credit constrained) and increases in default rates. We proxy this characteristic with the type of employee identification number (EIN) of the individual. If an individual is registered as a sole proprietor, it is likely that he manages his own firm, which makes his cash flows harder to observe for a bank, and increases the scope for moral hazard. On the other hand, individuals without an EIN are likely to be salaried workers, with an income profile more observable and more predictable income for the bank. On the middle, contractors also have riskier income profiles, but have a smaller scope for moral hazard.

[41] A similar credit curtailing effect is also reported by Degryse, Ioannidou, and Schedvin (2012) for firm credit markets, using data from a Swedish bank

As Table 10 shows, sole proprietors register the largest increase in credit following a new card, suggesting they are severely credit constrained. This observation is also repeated with respect to default, with table 11 showing that sole proprietors increase their two year default rates by 28.2% (from 16.5% to 44.8%), an extremely large increase, considering that other groups only increase their rates by 18% (15-16% to 33%). Further, the short and long term net effects of the new credit card on credit availability, reported on table 12, show that sole proprietors experience a sharp contraction in credit availability 12 months later, with the total change in credit limits changing from +23% at the time of the new card to -20%. In contrast the other type of workers only increased credit limits by 19-20% initially but added another 14-20% during the next year. These results strongly suggest that sole proprietors are very special, and might be experiencing much more severe versions of credit constraints and default externalities that other consumers.

Regarding the level of education, which proxies for the relationship of hypothesis 5—income and riskiness, amongst others—we find that less educated consumers experience a very similar change in borrowing than college–educated consumers, but nonetheless they experience a much larger increase in default rates and a stronger credit curtailing from the incumbent banks, that end up offsetting most of the initial expansion in credit limits. This result appears to show that less educated consumers are closer to default, but that nonetheless are able to borrow at similar levels than their college educated peers. Other possible explanations for the differences in default rates include a lack on financial education, as well as the existence of social safety networks that could prevent a well–educated consumer to default.

Finally, hypothesis 6 states that—after a credit supply expansion—existing banks will curtail credit more if the consumer is concentrated across few lenders. The mechanism behind this is another variant of default externalities, where a bank that curtails credit benefits all other credit providers through lower default rates, so the more concentrated the consumer lending, the larger the incentive to curtail credit for the existing credit card providers. We measure this characteristic with the Herfindahl-Hirschman Index (HHI), lagged 18 months[42]. We can see that the effects on credit are not clear, due to high standard errors. In addition, the effects on default are quite surprising as they are the opposite as highly–concentraded individuals increasing more drastically, even though they have a lower default baseline. With respect to the strategic response of other banks, we do see the expected signs, with a large decrease for concentrated consumers. However, the magnitudes are very large and so are the standard errors. A possible explanation may lie in the many closed cards due to default.[43]. In any case, albeit the

[42]Since the HHI of an individual changes with time, we must use a lag larger than the 12–month lag used to study the strategic response of banks

[43]Note that we are not exactly computing a percent transform (which can be fall at most 100%), and instead computing log differences, and that log differences do not have a lower bound.

magnitudes seem extremely high, the results are highly suggestive about how the composition of creditors affects the response to new lenders.

[Table 10 Goes Here]

[Table 11 Goes Here]

[Table 12 Goes Here]

We should observe that several other explanations might be driving these results. Regarding sole proprietors vs. salaried workers and contractors, it is possible that they are more able to divert funds to their firms in case of adverse shocks, thus triggering default. Regarding education, either less educated borrowers have less financial experience, or they might have a higher probability of losing their jobs and not obtaining a new one on time. Finally, the heterogeneity of effects by lender concentration is quite surprising as our prior expected concentrated lenders to react stronger and thus avoid default. A possibility is that the number of cards may be confounded with unobservables such as income and spending patterns, although it should be noted that—as Table 12 reports—average levels of debt did not vary much across categories.

Altogether, we have seen that following the issuance of a new credit card, consumers borrow and default significantly more. This induces existing lenders to curtail their credit limits, in line with predictions from default externality theories. The total effect of a new credit card on credit limits is thus lower than the increase coming from the new credit card. Further, for some groups the curtailing from existing lenders is such that it completely offsets the effects of a new credit card, and there is no long term increase of credit limits.

6 Robustness Checks

In addition to the results already discussed, I perform a series of robustness checks to assess the robustness of our results to different model specifications and assumptions. Due to their extension, most of the results are detailed in the appendix, but nonetheless I discuss their rationale here.

The first robustness check, already briefly discussed on chapter 4, regards a misspecification regarding the store opening instruments. Note that if the misspecification was random—for instance, a certain fraction of the addresses were swapped—all of the estimation assumptions will still hold, as we do not give a structural interpretation to the first–stage results. Similarly, suppose that due to zoning restrictions stores are sometimes unable to open in a wealthier district, opening instead at the boundaries of a neighboring district with laxer zoning regulations. In that case, as long as this bias is common across all types of stores, and not exclusive to e.g. bank-related stores, our results will still be consistent. Nevertheless, we might be able to improve the precision of the first stage by using a better instrument.

In particular, I test specifications where the store opening variable has been improved, by including not just the district where a store opens, but all the districts in a 1 km buffer around the store, and by replacing indicators with variables that weight the effect on each district by the overlap between the 1 km buffer. We use two weights, one that uses the area of each district that overlaps the buffer^[Thus, if a store opens in district A, which has an area of 10 km2, and half the 1 km buffer around the store overlaps district B, with an area of 20 km2, then the weighted instrument will be $\pi \times 0.5 \times 10 = \pi/5$ for district A and $\pi/10$ for district B. A critique to this weighting might be that certain districts might comprise unpopulated areas. Thus, as an alternative, I also weight the district by the count of all geocoded EINs. I then set the instrument to be the number of EINs located in the district in a 1 km buffer around the store, divided by the number of total EINs in the district. This measure has the advantage of being a better match to population density, but at the same time of being a noisier measurement if the geocoding was imprecise. As seen in the appendix, the results with these instruments are extremely similar than with the vanilla ones.

A source of instrument endogeneity is that store openings might affect not only the issuance of new credit cards, but lead to an increase in limits of already existing cards. Therefore, if we only include the former as regressors, we would be overestimating the effect of new cards on our outcome variable. Thus, exploiting the fact that in our main regressions we use two instruments instead of one, I add as a an endogenous regressor the changes in credit limits of existing cards. The results show that this variable has no predictive power, and that our existing results also hold to this.

Similarly, a reader might be concerned about some choices regarding the instrument bandwidth and the log transformation of the credit variables. To address this, I regress the main results using alternative specifications, and find that the results are qualitatively unchanged. In particular, instead of the $(-2, +2)$ bandwidth, I use a $(-1, +1)$ and $(-2, +2)$ bandwidths, and instead of a log transformation that adds a constant of 100 Nuevos Soles, I use several different constants (1 sol, 1000 soles), and the inverse hyperbolic sine function ($log(x + \sqrt{x^2 + 1})$). Again, the results remain mostly unchanged.

A possible critique is that the increase in consumer borrowing might have been driven by convenience or transactional users—who pay the balance at the end of the month or before incurring interests—instead of the so–called revolvers, who use a credit card more closely to a credit instrument, instead of just as a mean of payment. To address this, I perform an instrumental variable regression on end–of–month accrued interests, and observe that the increase in credit limit does have a large effect on revolver customers. Notice that the regression is not done on logarithms but instead in levels, due to the fact that accrued interests often take the value of zero.

Finally, the main regressions were also run for different subsamples (2006-2008 vs 2009–Aug2012) and geographic regions (the capital, Lima Metropolitana vs other cities), as well as with different techniques (such as two–step feasible GMM). In all cases, the conclusions of our analysis hold.

7 Conclusion

In markets with externalities, the actions of an agent can have large equilibrium and welfare repercussions. This is the case for the credit card market, where an expansion of credit from one agent leads consumers to borrow more and to default more frequently. Furthermore, it induces competitors to reduce their own supply of credit as a response to the deterioration in the consumers' credit quality. This curtailing of credit by competitors not only shifts the composition of credit, but acts as a stabilizer of credit supply shocks. I show this by exploiting the partial integration between banks and retail firms in Peru, which allow me to use bank-related store openings as an instrument for credit supply shifts. I also control for the effects of store openings and bank branch openings, thanks to the existence of independent retail chains and independent banks. Further, by applying a novel multiple fixed-effect estimator, I control for several sources of individual and time heterogeneity. This results match the predictions of default externality models, which until now had not been tested empirically in consumer credit markets.

Following a shift in credit supply due to a new credit card, consumer borrowing increases by more than 10%. This is in line with predictions from the default externality literature, and matches results on credit constraints and consumers' marginal propensity to consume. Subsequently, one year after the new card, default rates more than double one, a feature also predicted by default externality theories. Existing lenders respond by curtailing credit limits, which negates most of the initial increase in credit limits associated with the new card.

As a consequence of the deteriorating quality of the customers, existing banks then curtail credit limits, reducing most of the initial increase of credit limits caused by the new cards. I also find evidence of heterogeneous effects on the reactions of both consumers and competitors. Sole proprietors, individuals with lower educational attainment, and those with highly concentrated borrowing, face a larger credit curtailing by existing lenders when provided with a new source of credit.

A key implication of these findings is that shifts in credit supply, including government or microfinance credit programs, might be partially or fully undone by the actions of existing lenders, as long as individuals already had some access to credit. Another implication is that shifts in credit supply might lead to multiple equilibria depending on the degree of market concentration; and in particular, on the concentration of credit for each borrower. If an individual with only one credit card receives a new one, the existing lender will reduce credit card limits and accommodate the new creditor. On the other hand, if the individual has many credit cards, each credit will have lower incentives to accommodate, and total credit will increase more. Given that total credit limits are related to indebtedness and default risk, it is possible that markets with multiple lenders might end up in equilibria with high default risk and an oversupply of credit.

These results are also relevant from a regulatory point of view, as they suggest possible channels for limiting (or encouraging) consumer indebtedness. For instance, adding maximum debt clauses might have the paradoxical effect of increasing competitiveness, as it allows lenders to offer more competitive deals without fears that subsequent lenders will increase the debtor's risk.

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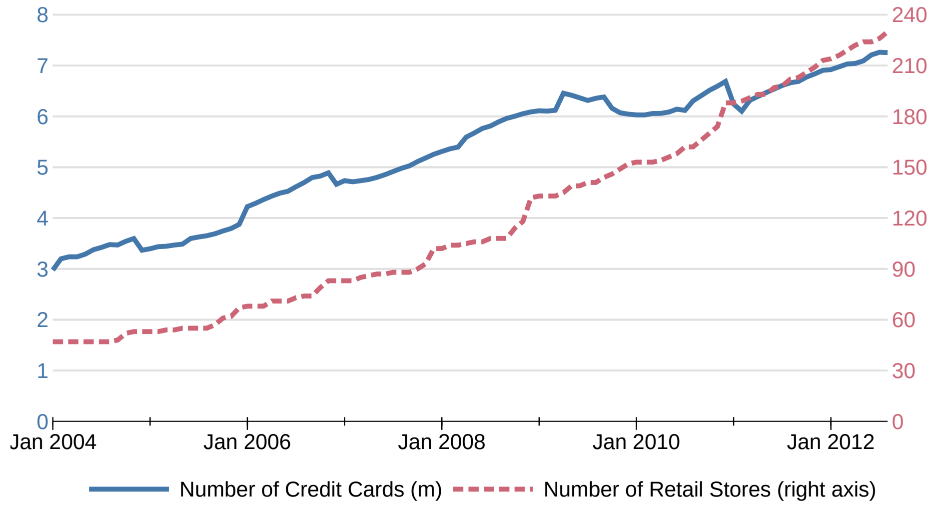


Figure 1: Number of Credit Cards and Retail Stores (Jan2004–Aug2012)

This figure documents the growth of both the consumer financial and retail sectors in Peru through the sample years (Jan2006–Aug2012). Source of credit card records: Asbanc.

Table 1: Credit Cards by Bank (August 2012)

Bank	Retail Sister Company	Number of Cards		Credit Volume	
		Thousands	% of total	$m	% of total
Banco Falabella	Falabella	1296.5	22.9	774.7	19.3
Banco de Crédito	—	1090.5	19.2	962.7	24.0
Interbank[†]	SPSA	1066.3	19.2	889.6	22.2
Banco Ripley	Ripley	1048.4	18.5	305.3	7.6
BBVA Banco Continental	—	498.2	8.8	416.8	10.4
Scotiabank Perú[§]	—	279.6	4.9	360.3	9.0
Banco Financiero	—	141.8	2.5	42.5	1.1
Citibank	—	140.0	2.5	172.8	4.3
Other banks	—	113.4	2.0	81.0	2.0
Total		5674.7		4005.5	

NOTES.— This table reports the number of credit cards (in thousands) and volume of credit card lending (in US dollars) per bank in August 2012, as well as the name of the retail chains belonging to the same conglomerate. *Other banks* comprise Banco Azteca, HSBC Bank, BIF, Mibanco and Banco de Comercio, each with less than 1% market share in terms of both cards and volume. Source: Superintendencia de Banca, Seguros y AFP.

[†] Includes Financiera Uno

[§] Includes Financiera Crediscotia

Table 2: Retail Stores by Chain (August 2012)

Company	Sister Bank	Chain	Store Type	Peru		Lima		Other Cities	
				Total	2006–	Total	2006–	Total	2006–
Falabella	Falabella	Tottus	Hypermarket	28	25	18	15	10	10
		Saga	Dept. Store	23	14	9	5	14	9
		Sodimac	Warehouse	17	15	8	6	9	9
SPSA	Interbank	Plaza Vea	Hypermarket	52	38	35	21	17	17
		Vivanda	Supermarket	8	3	8	3	0	0
		Oechsle	Dept. Store	6	6	2	2	4	4
Cencosud	—	Super Metro	Supermarket	31	30	24	23	7	7
		Hiper Metro	Hypermarket	14	2	14	2	0	0
		Wong	Supermarket	13	3	12	2	1	1
Ripley	Ripley	Ripley	Dept. Store	17	10	12	5	5	5
Makro	—	Makro	Warehouse	8	8	5	5	3	3
Total				217	154	147	89	70	65

NOTES.— This table reports the number of retail stores existing as of August 2012, by store chain and region. Lima refers to *Lima Metropolitan Area*, formed by the conurbation of Lima (Peru's capital) and Callao. The *2006–* columns refer both to store openings between Jan2006–Aug2012 and to existing stores as of Aug2012, since there were no stores closings in that period. As in the rest of the paper, the *Lima Cercado* district, with nine store openings, has been excluded. The full listing of stores is available online.

Table 3: Consumer Summary Statistics

	Mean	Median	Std. Dev.	Obs.
Panel A: Credit Statistics per Consumer				
Total Credit (excl. mortgages)	8595.2	2155.5	37 184.1	122 467 747
Credit Cards	3319.3	755.1	8464.4	122 467 747
From Store-Related Banks	1528.6	27.0	4192.9	122 467 747
From Other Banks	1790.7	0.0	6028.8	122 467 747
Other Sources	5275.8	0.0	35 443.1	122 467 747
Credit Card Limits	13 223.6	4591.1	29 580.1	122 467 747
From Store-Related Banks	6121.7	1725.0	20 856.7	122 467 747
From Other Banks	7101.9	1807.0	15 986.8	122 467 747
Monthly Change in Total Credit	89.4	0.0	8559.9	117 309 310
From Credit Cards	33.7	0.0	2015.3	117 309 310
Monthly Change in Credit Card Limits	172.6	0.0	24 698.9	117 309 310
Usage Ratio (%)	31.9	16.8	1157.6	122 467 747
Default in next 12 months (%)	9.2	0.0	29.0	103 217 785
Default in next 24 months (%)	15.5	0.0	36.2	81 092 561
Number of Credit Cards	2.3	2.0	1.4	122 467 747
From Store–Related Banks	1.2	1.0	0.9	122 467 747
Credit History (in years)§	6.6	6.1	3.6	122 467 747
Panel B: Demographics				
Age (in years)	42.8	40.0	13.0	120 369 770
Female (%)	45.2	0.0	49.8	120 369 873
College Educated (%)	43.6	0.0	49.6	122 467 747
Panel C: District-level Statistics				
Dependent Workers¶	6.2	5.1	3.4	122 467 747
Indep. Wkrs. & Sole Propietors¶	20.6	19.5	4.8	122 467 747
All Firms¶	29.6	29.4	4.6	122 467 747
Construction Firms¶	36.3	34.5	11.6	122 467 747
Number of Branches in District	39.3	35.0	30.1	122 467 747
Branches in District per 100,000 inhabitants	21.7	12.8	30.7	122 467 747
Credit Cards per Capita in 2005 ($\times$100)	12.4	9.4	8.7	122 467 747
Observations	122 467 747			
Number of Individuals	2 873 559			
Number of Districts	221			

Notes.— This table shows summary statistics for Peruvian credit card borrowers. Each observation represents an individual–month pair spanning from Jan2006 to Aug2012. Credit information is from the Superintendencia de Banca y Seguros, demographic characteristics are from Sunat and Reniec and worker and firm registrations are from Sunat. Excludes individuals in default and without a credit card, and untreated districts.

† Credit variables are in Nuevos Soles (1 Nuevo Sol averaged 0.34 USD through the sample, with a 0.30–0.38 range).

§ Available records start in January 2001, so credit history is censored from above.

¶ New registrations in the last year over total registrations in the last 5 years.

Table 4: Average District Characteristics 12 Months Before a Retail Store Opening

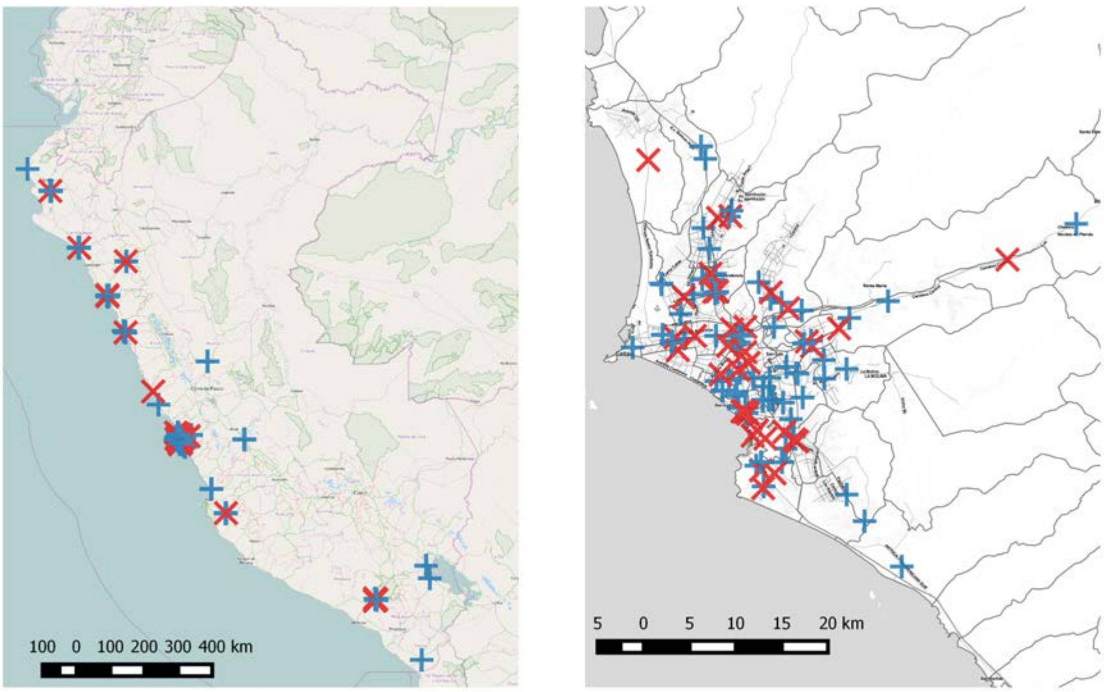
Is the Opening Store Bank–Related?	No	Yes	T–Stat. of Diff.
Total Population (thousands)	279.54	257.33	0.514
Month When Store Opened	2009.82	2009.41	1.154
Total Branches in District	40.31	35.23	0.928
Branches in District per 100,000 inhabitants	20.25	20.50	-0.055
Human Development Index (2007)	0.71	0.69	2.798
Dependent Workers¶	7.20	7.04	0.183
Indep. Wkrs. & Sole Propietors¶	21.06	23.71	-2.899
All Firms¶	28.44	29.40	-1.611
Construction Firms¶	31.49	37.13	-3.285
Total Credit (excl. mortgages)	7711.00	8100.60	-0.621
From Credit Cards	3054.30	2728.38	1.192
Credit Card Limits	11810.52	10354.77	1.055
Monthly Change in Total Credit	20.95	127.96	-2.553
From Credit Cards	22.10	54.05	-2.105
Monthly Change in Credit Card Limits	131.75	279.10	-1.180
College Educated (%)	43.77	47.12	-1.460
Age (in years)	42.27	42.13	0.337
Credit History (in years)§	5.91	5.89	0.161
Registered in Private Pension System (%)	33.75	40.77	-2.636
Has Employer Identification Number (%)	67.17	67.65	-0.609
With Simplified Bookeeping (%)	7.57	8.92	-2.103
Credit Card Usage Ratio (%)	35.26	33.93	0.769
Credit Cards per Capita ($\times$100)	36.88	27.83	1.765
Number of Credit Cards	2.26	2.10	2.536
New Credit Card Frequency ($\times$100)	4.47	4.54	-0.132
Number of Creditor Banks	1.89	1.89	-0.096
Default in next 12 months (%)	10.05	9.26	1.811
Default in next 24 months (%)	17.47	15.92	2.178
Number of Districts	36	82	
Number of Opening Stores	38	102	
Number of Individuals in Sample (thousands)	34.77	28.07	

Notes.— This table reports district characteristics 12 months before a retail store or supermarket opened in a district, for stores that opened between January 2006 and August 2012. Column 2 refers to districts where the opening stores were not bank–related, while Column 1 refers to districts where the opening stores were part of a banking conglomerate. Each cell contains averages of district–level variables, and Column 3 contains the T–Statistic of the differences of each pair of averages (computed assuming unequal variances). The first block of variables contains publicly available district–level information. The second block of variables is based on individual–level information, collapsed as district–level averages (excluding individuals in default or without a credit card). Credit information is from the Superintendencia de Banca y Seguros, demographic characteristics are from Sunat and Reniec, worker and firm registrations are from Sunat, and the human development index is from UNDP and Foncodes.

† Credit variables are in Nuevos Soles (1 Nuevo Sol averaged 0.34 USD through the sample, with a 0.30–0.38 range).

§ Available records start in January 2001, so credit history is censored from above.

¶ New registrations in the last year over total registrations in the last 5 years.



(a) Peru

(b) Lima Metropolitana

Figure 2: Map of Store Openings in Peru between 2005 and 2012

These maps show store openings between 2005 and 2012 of supermarkets and department stores in Peru and the metropolitan area of its capital, Lima. *Legend:* **+**: Stores related to banks. **X**: Unrelated stores.

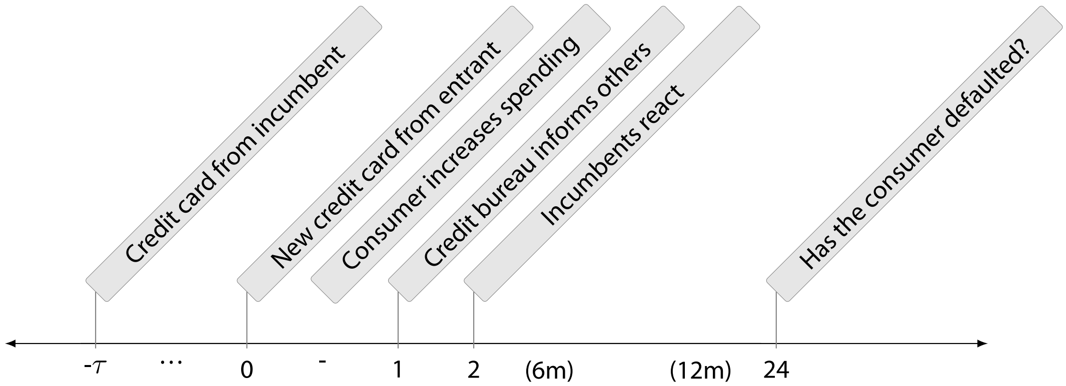


Figure 3: Chronology of events (time in months)

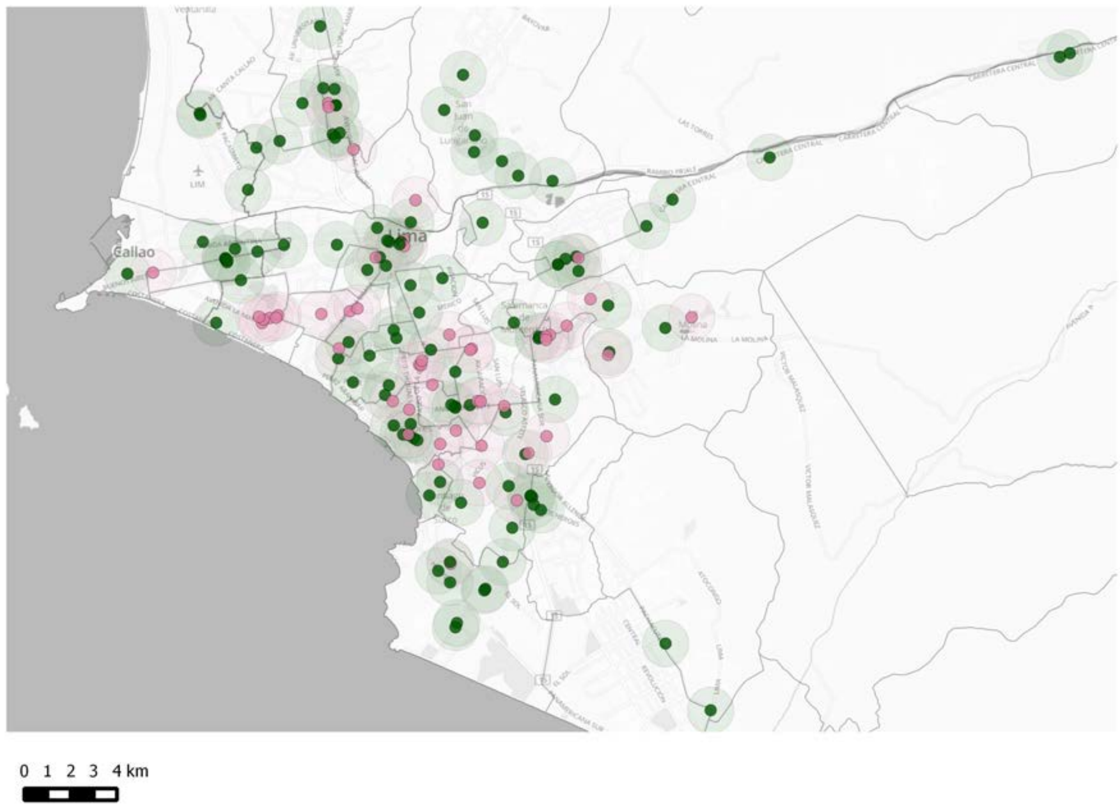


Figure 4: Store Openings in Lima Metropolitana

Green dots represent stores opened during the Jan2006–Aug2012 sample, while red dots represent earlier (–2005) store openings.

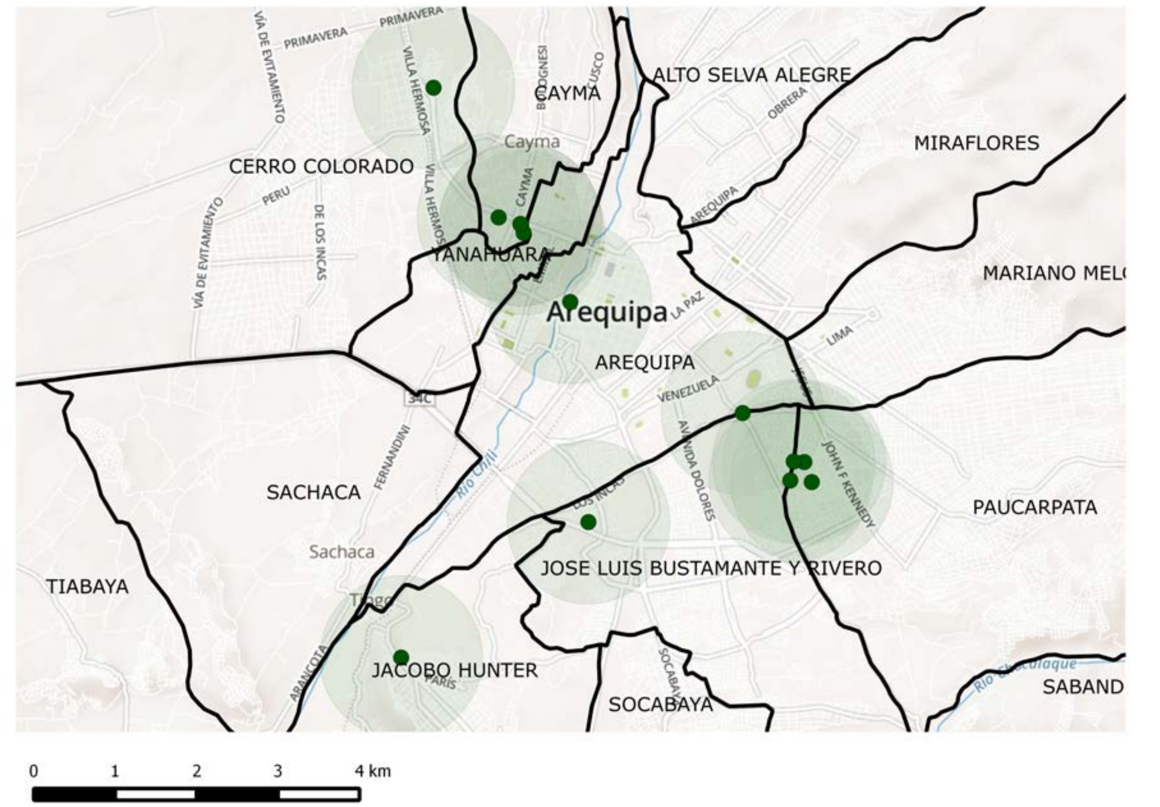


Figure 5: Store Openings in Arequipa

Green dots represent stores opened during the Jan2006–Aug2012 sample; the shaded circle has a radius of 1km. Black lines represent district delimiters.

Table 5: First Stage Regression — Effect of Bank–Related Store Openings on New Credit Cards

Dependent Variable	New Credit Card (store–related banks)					
Type of Store Opening	Pooled Stores		Repeated vs *de Novo*		By Bank	
	(1)	(2)	(3)	(4)	(5)	(6)
Store Opening (bank–related)	0.427***	0.409***				
	(0.007)	(0.007)				
Store Opening (repeat, bank–related)			0.179***	0.161***		
			(0.008)	(0.008)		
Store Opening (de novo, bank–related)†			1.971***	1.985***		
			(0.021)	(0.021)		
Store Opening (bank A)					0.613***	0.598***
					(0.009)	(0.009)
Store Opening (bank B)					1.320***	1.279***
					(0.020)	(0.021)
Store Opening (bank C)					0.189***	0.176***
					(0.008)	(0.008)
Store Opening (all stores)	0.026***	0.038***			0.037***	0.047***
	(0.006)	(0.006)			(0.006)	(0.006)
Store Opening (all stores, repeat)			0.021***	0.038***		
			(0.006)	(0.006)		
Store Opening (all stores, de novo)†			0.253***	0.226***		
			(0.013)	(0.014)		
District–level controls	Y	Y	Y	Y	Y	Y
District Fixed Effect	Y		Y		Y	
Individual Fixed Effect		Y		Y		Y
Time Fixed Effect	Y	Y	Y	Y	Y	Y
Time FE × initial credit cards per capita (by district, 2005)	Y	Y	Y	Y	Y	Y
Mean of Dependent Variable (×100)	1.759	1.759	1.759	1.759	1.759	1.759
Observations	117309310	117305635	117309310	117305635	117309310	117305635
Number of Clusters	2,873,208	2,869,533	2,873,208	2,869,533	2,873,208	2,869,533
F Statistic	2207.8	2215.6	2771.6	2748.8	2175.7	2155.0
R^2	0.002	0.030	0.002	0.031	0.002	0.030

† Denotes the first store ever opened by the conglomerate in a city.

Notes.— This table shows individual–level least squares regressions of bank-related retail store openings on new credit cards issued by store–related banks. The data is a monthly panel from January 2006 to August 2012 and includes all Peruvian consumers with a positive credit history (holding at least one credit card now and 12 months prior, and not in default). Due to computational limitations, the regressions randomly sample 50% of the individuals and exclude districts that never had any store openings (or neighbored districts that had). New Credit Card$_{i,t}$ counts the number of new credit cards issued by store–related banks to the individual i in month t. Store Opening$_{d(i),t-2,t+1}$ counts the number of new retail stores (supermarkets, department stores, etc.) opened in district d (where individual i lives) between months $t-2$ and $t+1$ (results are robust to the bandwidth choice). Additional district–level controls include store openings by any chain (including both bank-related and bank–unrelated stores), four measures of local economic growth (growth rates of firm registrations, construction firm registrations, independent worker registrations, and dependent worker registrations), and two measures of banking growth and activity (new bank branches, branch density). Columns 1–2 report results with all bank-related stores pooled; columns 3–4 report results separating *de novo* store openings (the first opening of a retail conglomerate in a province) with subsequent store openings; columns 5–6 report results separating by the identity of the bank-related store. Singleton groups are excluded. Coefficients and standard errors are scaled by 100. Robust standard errors in parentheses, clustered by individual. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6
IV Regression — Direct Effects of Consumer Credit Supply Shocks on Consumer Borrowing

Dependent Variable	Δ log CC Credit from Store-Related Banks†				Δ log Credit Card Credit†			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
New Credit Card (store–related banks)	53.294***	51.070***	52.336***	50.934***	18.863***	17.635***	19.529***	18.569***
	(2.562)	(2.662)	(2.501)	(2.601)	(2.624)	(2.720)	(2.562)	(2.658)
Store Opening (all stores, repeat)	0.081***	0.099***	0.078***	0.094***	0.076***	0.084***	0.074***	0.080***
	(0.010)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)	(0.011)
Store Opening (all stores, de novo)§	0.044	0.064	0.052	0.060	0.085*	0.094*	0.071	0.077
	(0.046)	(0.047)	(0.045)	(0.047)	(0.047)	(0.048)	(0.046)	(0.048)
District–level controls			Y	Y			Y	Y
District Fixed Effect	Y		Y		Y		Y	
Individual Fixed Effect		Y		Y		Y		Y
Time Fixed Effect	Y	Y	Y	Y	Y	Y	Y	Y
Time FE × initial credit cards per capita (by district, 2005)	Y	Y	Y	Y	Y	Y	Y	Y
Mean of Dependent Variable (×100)	.2666	.267	.2666	.267	.1766	.1769	.1766	.1769
Observations	110087855	110080649	110087855	110080649	110087855	110080649	110087855	110080649
Number of Clusters	2,872,011	2,864,805	2,872,011	2,864,805	2,872,011	2,864,805	2,872,011	2,864,805
Weak identification F stat.	4394.4	4264.8	4654.0	4489.7	4394.4	4264.8	4654.0	4489.7
10% maximal IV size	19.9	19.9	19.9	19.9	19.9	19.9	19.9	19.9
Overidentification J stat.	$\chi^2(1)$=40.5	$\chi^2(1)$=37.2	$\chi^2(1)$=40.1	$\chi^2(1)$=36.5	$\chi^2(1)$=5.2	$\chi^2(1)$=3.9	$\chi^2(1)$=4.8	$\chi^2(1)$=3.6
p value	0.000	0.000	0.000	0.000	0.022	0.049	0.029	0.059

† Used $log(S/.100 + x)$ to avoid missing values

§ Denotes the first store ever opened by the conglomerate in a city.

Notes.— This table shows individual–level instrumental variable regressions of new credit cards from store–related banks on credit card borrowing. Columns 1–4 report how a new credit card affects credit card borrowing from the three store–related banks, while columns 5–8 report the effects on total credit card borrowing. Store openings from store–related banks are used as instrument; see columns 3–4 of table 5. The data is a monthly panel from January 2006 to August 2012 and includes all Peruvian consumers with a positive credit history (holding at least one credit card now and 12 months prior, and not in default). Due to computational limitations, the regressions randomly sample 50% of the individuals and exclude districts that never had any store openings (or neighbored districts that had). New Credit Card$_{i,t}$ counts the number of new credit cards issued by store–related banks to the individual i in month t. Singleton groups are excluded. Coefficients and standard errors are scaled by 100. Robust standard errors in parentheses, clustered by individual. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7

IV Regression — Substitution and Net Effects of Consumer Credit Supply Shocks on Consumer Borrowing

Dependent Variable	Δ log Non–CC Credit†				Δ log Total Credit†			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
New Credit Card (store–related banks)	3.950	2.290	3.872	2.284	12.881***	10.419***	13.082***	11.022***
	(2.973)	(3.072)	(2.902)	(3.001)	(2.638)	(2.727)	(2.575)	(2.664)
Store Opening (all stores, repeat)	–0.010	–0.003	–0.005	0.000	0.049***	0.062***	0.051***	0.061***
	(0.012)	(0.012)	(0.012)	(0.012)	(0.011)	(0.011)	(0.011)	(0.011)
Store Opening (all stores, de novo)§	–0.137***	–0.121**	–0.134***	–0.120**	–0.023	0.018	–0.027	0.008
	(0.052)	(0.053)	(0.051)	(0.053)	(0.047)	(0.048)	(0.046)	(0.047)
District–level controls			Y	Y			Y	Y
District Fixed Effect	Y		Y		Y		Y	
Individual Fixed Effect		Y		Y		Y		Y
Time Fixed Effect	Y	Y	Y	Y	Y	Y	Y	Y
Time FE $\times$ initial credit cards per capita (by district, 2005)	Y	Y	Y	Y	Y	Y	Y	Y
Mean of Dependent Variable ($\times$100)	.4035	.4037	.4035	.4037	.3764	.3767	.3764	.3767
Observations	110087855	110080649	110087855	110080649	110087855	110080649	110087855	110080649
Number of Clusters	2,872,011	2,864,805	2,872,011	2,864,805	2,872,011	2,864,805	2,872,011	2,864,805
Weak identification F stat.	4394.4	4264.8	4654.0	4489.7	4394.4	4264.8	4654.0	4489.7
10% maximal IV size	19.9	19.9	19.9	19.9	19.9	19.9	19.9	19.9
Overidentification J stat.	$\chi^2(1)$=1.8	$\chi^2(1)$=2.2	$\chi^2(1)$=2.1	$\chi^2(1)$=2.3	$\chi^2(1)$=3.5	$\chi^2(1)$=3.7	$\chi^2(1)$=3.5	$\chi^2(1)$=3.7
p value	0.174	0.142	0.146	0.127	0.062	0.054	0.060	0.054

† Used $log(S/.100 + x)$ to avoid missing values

§ Denotes the first store ever opened by the conglomerate in a city.

Notes.— This table shows individual–level instrumental variable regressions of new credit cards from store–related banks on non-credit card and total borrowing. Columns 1–4 report the effect of a new credit card on borrowing from other sources (auto loans, consumer credit, etc. but excluding mortgage loans). Columns 5–8 report the total effect on consumer borrowing. Store openings from store–related banks are used as instrument; see columns 3–4 of table 5. The data is a monthly panel from January 2006 to August 2012 and includes all Peruvian consumers with a positive credit history (holding at least one credit card now and 12 months prior, and not in default). Due to computational limitations, the regressions randomly sample 50% of the individuals and exclude districts that never had any store openings (or neighbored districts that had). New Credit Card$_{i,t}$ counts the number of new credit cards issued by store–related banks to the individual i in month t. Singleton groups are excluded. Coefficients and standard errors are scaled by 100. Robust standard errors in parentheses, clustered by individual. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8
IV Regression — Effects of Consumer Credit Supply Shocks on Consumer Default

Default in…	1 quarter† (1)	2 quarters† (2)	3 quarters† (3)	4 quarters† (4)	5 quarters† (5)	6 quarters† (6)	7 quarters† (7)	8 quarters† (8)
New Credit Card (store–related banks)	−1.89	2.71	7.60**	11.45***	13.44***	14.59***	16.33***	17.82***
	(2.05)	(3.04)	(3.58)	(3.92)	(4.17)	(4.36)	(4.52)	(4.61)
Store Opening (all stores, repeat)	0.01	0.02	0.04**	0.04**	0.04*	0.04*	0.04**	0.06***
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Store Opening (all stores, de novo)§	0.02	−0.08	−0.17**	−0.23***	−0.25***	−0.23**	−0.22**	−0.24**
	(0.05)	(0.07)	(0.08)	(0.09)	(0.09)	(0.10)	(0.10)	(0.10)
District–level controls	Y	Y	Y	Y	Y	Y	Y	Y
Individual Fixed Effect	Y	Y	Y	Y	Y	Y	Y	Y
Time Fixed Effect	Y	Y	Y	Y	Y	Y	Y	Y
Time FE × initial credit cards per capita (by district, 2005)	Y	Y	Y	Y	Y	Y	Y	Y
Mean of Dependent Variable (×100)	3.079	5.511	7.604	9.473	11.17	12.72	14.16	15.5
Relative Effect (%)	-61.3	49.1	99.9	120.9	120.3	114.6	115.3	115.0
Observations	40,544,359	40,544,359	40,544,359	40,544,359	40,544,359	40,544,359	40,544,359	40,544,359
Number of Clusters	1,194,563	1,194,563	1,194,563	1,194,563	1,194,563	1,194,563	1,194,563	1,194,563
Weak identification F stat.	2415.0	2415.0	2415.0	2415.0	2415.0	2415.0	2415.0	2415.0
10% maximal IV size	19.9	19.9	19.9	19.9	19.9	19.9	19.9	19.9
Overidentification J stat.	$\chi^2(1)$=0.0	$\chi^2(1)$=1.3	$\chi^2(1)$=1.2	$\chi^2(1)$=1.9	$\chi^2(1)$=0.7	$\chi^2(1)$=0.4	$\chi^2(1)$=0.0	$\chi^2(1)$=0.8
p value	0.977	0.248	0.271	0.173	0.418	0.529	0.835	0.357

† Default defined as 60 days past due; see appendix for details.

§ Denotes the first store ever opened by the conglomerate in a city.

Notes.— This table shows individual–level instrumental variable regressions of new credit cards from store–related banks on default indicators at a varying horizon. Default is treated as an absorbing state so the frequency of default increases monotonically with the default horizon, as seen in the *mean of dependent variable* row. In turn, the *Relative Effect* row reports the normalized coefficients of interest, by dividing the coefficient for new credit cards by the average default frequency. Store openings from store–related banks are used as instrument; see columns 3–4 of table 5. Note that this is the dynamic counterpart of preceding specifications, and thus specified in levels instead of in differences. The data is a monthly panel from January 2006 to August 2012 and includes all Peruvian consumers with a positive credit history (holding at least one credit card now and 12 months prior, and not in default). Due to computational limitations, the regressions randomly sample 50% of the individuals and exclude districts that never had any store openings (or neighbored districts that had). New Credit Card$_{i,t}$ counts the number of new credit cards issued by store–related banks to the individual i in month t. Singleton groups are excluded. Coefficients and standard errors are scaled by 100. Robust standard errors in parentheses, clustered by individual. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 9
IV Regression — Strategic Effects of Consumer Credit Supply Shocks on Credit Limits from Competing Banks

Dependent Variable	log Credit Card Limits, Unrelated Banks†				log Credit Card Limits, All Banks†			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of Credit Cards (store–related banks)	−68.2***	−54.1***	−35.0***	−27.4***	21.6***	21.3***	26.2***	24.1***
	(5.6)	(4.9)	(5.2)	(4.6)	(2.9)	(2.5)	(2.8)	(2.4)
L12.Number of Credit Cards (store–related banks)	−39.1***	−24.1***	−51.8***	−50.9***	−14.8***	−14.7***	10.2**	−4.8
	(5.3)	(5.0)	(8.0)	(8.0)	(2.9)	(2.7)	(4.5)	(4.4)
Store Opening (all stores, repeat)	1.0***	0.8***	1.0***	0.9***	0.2***	0.2***	0.2***	0.1***
	(0.1)	(0.0)	(0.1)	(0.0)	(0.0)	(0.0)	(0.0)	(0.0)
L12.Store Opening (all stores, repeat)	0.2**	0.2**	−0.1**	−0.1	0.0	0.1***	0.0	0.1**
	(0.1)	(0.1)	(0.1)	(0.1)	(0.0)	(0.0)	(0.0)	(0.0)
Store Opening (all stores, de novo)§	−0.2	0.8***	−0.8**	−0.4	−0.6***	−0.4***	0.3*	−0.1
	(0.3)	(0.2)	(0.4)	(0.3)	(0.2)	(0.1)	(0.2)	(0.2)
L12.Store Opening (all stores, de novo)§	2.8***	2.2***	2.2***	1.9***	0.7***	0.5***	0.6***	0.5***
	(0.2)	(0.2)	(0.2)	(0.2)	(0.1)	(0.1)	(0.1)	(0.1)
District–level controls (incl. lagged)			Y	Y			Y	Y
Individual Fixed Effect	Y	Y	Y	Y	Y	Y	Y	Y
Time Fixed Effect	Y	Y	Y	Y	Y	Y	Y	Y
District–level Slopes	Y		Y		Y		Y	
Individual–level Slopes		Y		Y		Y		Y
Time FE × initial credit cards per capita (by district, 2005)	Y	Y	Y	Y	Y	Y	Y	Y
Mean of Dependent Variable (×100)	733.5	733.5	733.5	733.5	860.5	860.5	860.5	860.5
Observations	85,685,251	85,685,251	85,685,251	85,685,251	85,685,251	85,685,251	85,685,251	85,685,251
Number of Clusters	2,336,927	2,336,927	2,336,927	2,336,927	2,336,927	2,336,927	2,336,927	2,336,927
Weak identification F stat.	402.8	512.5	202.4	192.9	402.8	512.5	202.4	192.9
10% maximal IV size	16.9	16.9	16.9	16.9	16.9	16.9	16.9	16.9
Overidentification J stat.	$\chi^2(2)$=8.2	$\chi^2(2)$=52.4	$\chi^2(2)$=22.7	$\chi^2(2)$=14.6	$\chi^2(2)$=109.6	$\chi^2(2)$=52.8	$\chi^2(2)$=96.1	$\chi^2(2)$=64.3
p value	0.016	0.000	0.000	0.001	0.000	0.000	0.000	0.000

† Used $log(S/.100 + x)$ to avoid missing values

§ Denotes the first store ever opened by the conglomerate in a city.

Notes.— This table shows individual–level instrumental variable regressions of the number of credit cards from store–related banks on credit card limits. Effects are measured at a contemporaneous and at a twelve–month horizon. Columns 1–4 report effects on credit card limits from store–*unrelated* banks. These are intepreted as strategic responses from other banks to the increased credit supply from store–related banks. Columns 5–8 report the net effects on total credit card limits. Store openings from store–related banks are used as instrument; see appendix for the first stage tables (insert link). The data is a monthly panel from January 2006 to August 2012 and includes all Peruvian consumers with a positive credit history (holding at least one credit card now and 12 months prior, and not in default). Due to computational limitations, the regressions randomly sample 50% of the individuals and exclude districts that never had any store openings (or neighbored districts that had). Number of Credit Card$_{i,t}$ counts the number of credit cards from store–related banks available to the individual i in month t. Singleton groups are excluded. Coefficients and standard errors are scaled by 100. Robust standard errors in parentheses, clustered by individual. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 10
IV Regression — Heterogeneous Effects of Credit Supply Shocks on Consumer Borrowing

Dependent Variable	Δ log Total Credit†							
Classification	EIN Type			Schooling Level		HHI Index (t-18)		
	Contractor (1)	Sole Proprietor (2)	No EIN (3)	Secondary (4)	Higher (5)	<0.5 (6)	[0.5, 1) (7)	1.0 (8)
New Credit Card (store–related banks)	8.62**	18.76***	8.35*	10.68**	10.75***	10.91	3.08	0.99
	(3.97)	(5.52)	(4.73)	(4.59)	(3.20)	(7.05)	(5.03)	(5.34)
Store Opening (all stores, repeat)	0.09***	0.04	0.05***	0.08***	0.03*	0.05***	0.07***	0.05***
	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
Store Opening (all stores, de novo)§	0.03	−0.10	0.06	0.04	−0.03	0.01	0.05	0.14*
	(0.08)	(0.10)	(0.07)	(0.07)	(0.07)	(0.09)	(0.10)	(0.08)
District–level controls	Y	Y	Y	Y	Y	Y	Y	Y
Individual Fixed Effect	Y	Y	Y	Y	Y	Y	Y	Y
Time Fixed Effect	Y	Y	Y	Y	Y	Y	Y	Y
Time FE $\times$ initial credit cards per capita (by district, 2005)	Y	Y	Y	Y	Y	Y	Y	Y
Mean of Dependent Variable ($\times$100)	.4276	.2702	.3755	.3595	.4566	-.6543	.0796	1.112
Observations	49,864,911	23,421,466	36,794,272	56,064,899	46,256,893	31,627,218	30,983,067	54,523,566
Number of Clusters	1,260,256	616,848	987,701	1,619,203	992,847	1,125,760	1,599,802	2,559,667
Weak identification F stat.	1888.0	1200.3	1399.8	1554.8	2890.7	603.0	1263.1	1402.7
10% maximal IV size	19.9	19.9	19.9	19.9	19.9	19.9	19.9	19.9
Overidentification J stat.	$\chi^2(1)$=0.4	$\chi^2(1)$=2.8	$\chi^2(1)$=1.8	$\chi^2(1)$=1.4	$\chi^2(1)$=2.5	$\chi^2(1)$=0.0	$\chi^2(1)$=0.0	$\chi^2(1)$=8.3
p value	0.539	0.094	0.184	0.242	0.112	0.901	0.995	0.004

† Used $log(S/.100 + x)$ to avoid missing values

§ Denotes the first store ever opened by the conglomerate in a city.

Notes.— This table shows individual–level instrumental variable regressions of new credit cards from store–related banks on total borrowing, for different samples based on individual characteristics. Columns 1–3 report the effect of a new credit card on total consumer credit, by type of Employer Identification Number (EIN); individuals without an EIN number are usually salaried workers or individuals who have never worked before. Columns 4–5 report the same regressions according to the level of education when the individual was 18 years old: only completed high school vs. at least one term of college or superior education. Similarly, columns 6–8 report data by a Herfindahl-Hirschman Index that measures the degree of concentration of credit card limits for each individual, 18 months lagged. The data is a monthly panel from January 2006 to August 2012 and includes all Peruvian consumers with a positive credit history (holding at least one credit card now and 12 months prior, and not in default). Due to computational limitations, the regressions randomly sample 50% of the individuals and exclude districts that never had any store openings (or neighbored districts that had). New Credit Card$_{i,t}$ counts the number of new credit cards issued by store–related banks to the individual i in month t. Singleton groups are excluded. Coefficients and standard errors are scaled by 100. Robust standard errors in parentheses, clustered by individual. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 11
IV Regression — Heterogeneous Effects of Credit Supply Shocks on Consumer Default

Dependent Variable: 8 quarters†

Classification	EIN Type			Schooling Level		HHI Index (t-18)		
	Contractor	Sole Proprietor	No EIN	Secondary	Higher	<0.5	[0.5, 1)	1.0
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
New Credit Card (store–related banks)	17.62***	28.26***	18.13***	38.40***	6.15*	2.35	10.23**	24.20***
	(4.88)	(6.74)	(5.89)	(6.34)	(3.64)	(9.50)	(5.21)	(4.83)
Store Opening (all stores, repeat)	0.09***	0.05	0.04*	0.06***	0.06***	0.06**	0.02	0.05***
	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)
Store Opening (all stores, de novo)§	−0.31***	−0.53***	−0.35***	−0.52***	−0.16	−0.15	−0.17	−0.45***
	(0.11)	(0.15)	(0.12)	(0.10)	(0.10)	(0.13)	(0.13)	(0.10)
District–level controls	Y	Y	Y	Y	Y	Y	Y	Y
Individual Fixed Effect	Y	Y	Y	Y	Y	Y	Y	Y
Time Fixed Effect	Y	Y	Y	Y	Y	Y	Y	Y
Time FE × initial credit cards per capita (by district, 2005)	Y	Y	Y	Y	Y	Y	Y	Y
Mean of Dependent Variable (×100)	15.17	16.49	15.32	18.68	12.01	16.12	14.67	15.6
Relative Effect (%)	116.2	171.4	118.3	205.6	51.2	14.6	69.8	155.1
Observations	36,899,714	17,457,867	26,710,297	40,620,796	35,153,254	20,673,348	20,469,818	39,756,851
Number of Clusters	1,052,575	522,197	813,956	1,317,278	887,621	867,863	1,246,045	2,062,773
Weak identification F stat.	2071.5	1199.7	1492.2	1541.7	3113.6	549.6	1267.8	1593.9
10% maximal IV size	19.9	19.9	19.9	19.9	19.9	19.9	19.9	19.9
Overidentification J stat.	$\chi^2(1)$=3.9	$\chi^2(1)$=0.1	$\chi^2(1)$=0.0	$\chi^2(1)$=13.0	$\chi^2(1)$=3.8	$\chi^2(1)$=14.8	$\chi^2(1)$=0.0	$\chi^2(1)$=0.0
p value	0.049	0.815	0.880	0.000	0.051	0.000	0.909	0.991

† Default defined as 60 days past due; see appendix for details.
§ Denotes the first store ever opened by the conglomerate in a city.

Notes.— This table shows individual–level instrumental variable regressions of the number of credit cards from store–related banks on credit card limits. Effects are measured at a contemporaneous and at a twelve–month horizon. Columns 1–3 report the effect of a new credit card on total consumer credit, by type of Employer Identification Number (EIN); individuals without an EIN number are usually salaried workers or individuals who have never worked before. Columns 4–5 report the same regressions according to the level of education when the individual was 18 years old: only completed high school vs. at least one term of college or superior education. Similarly, columns 6–8 report data by a Herfindahl-Hirschman Index that measures the degree of concentration of credit card limits for each individual, 18 months lagged. The data is a monthly panel from January 2006 to August 2012 and includes all Peruvian consumers with a positive credit history (holding at least one credit card now and 12 months prior, and not in default). Due to computational limitations, the regressions randomly sample 50% of the individuals and exclude districts that never had any store openings (or neighbored districts that had). New Credit Card$_{i,t}$ counts the number of new credit cards issued by store–related banks to the individual i in month t. Singleton groups are excluded. Coefficients and standard errors are scaled by 100. Robust standard errors in parentheses, clustered by individual. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 12
IV Regression — Heterogeneous Effects of Credit Supply Shocks on Credit Limit Decisions

Dependent Variable	log Credit Card Limits, All Banks†							
Classification	EIN Type			Schooling Level		HHI Index (t-18)		
	Contractor (1)	Sole Proprietor (2)	No EIN (3)	Secondary (4)	Higher (5)	<0.5 (6)	[0.5, 1) (7)	1.0 (8)
Number of Credit Cards (store–related banks)	19.40***	23.06***	19.64***	46.85***	15.43***	10.98	−30.39	−60.22
	(3.36)	(6.09)	(4.21)	(4.80)	(2.64)	(10.43)	(19.49)	(50.94)
L12.Number of Credit Cards (store–related banks)	20.84***	−42.94***	14.65	−17.87***	31.22***	1.34	−26.04***	−107.75***
	(6.13)	(8.61)	(9.21)	(5.77)	(6.72)	(7.71)	(10.05)	(18.49)
Store Opening (all stores, repeat)	0.16***	0.12	0.24***	0.11***	0.04	0.07**	0.33***	0.49**
	(0.03)	(0.08)	(0.04)	(0.03)	(0.04)	(0.03)	(0.10)	(0.20)
L12.Store Opening (all stores, repeat)	0.01	0.27***	−0.05	0.09***	−0.07*	0.02	0.25***	0.22**
	(0.03)	(0.07)	(0.05)	(0.03)	(0.04)	(0.03)	(0.08)	(0.09)
Store Opening (all stores, de novo)§	0.54**	−0.70**	0.30	−0.41**	0.96***	−0.08	−0.53**	−0.70**
	(0.25)	(0.28)	(0.28)	(0.17)	(0.29)	(0.19)	(0.25)	(0.31)
L12.Store Opening (all stores, de novo)§	0.74***	−0.21	0.96***	−0.42***	0.80***	0.42***	−0.13	0.70*
	(0.14)	(0.26)	(0.16)	(0.15)	(0.14)	(0.14)	(0.24)	(0.38)
District–level controls (incl. lagged)	Y	Y	Y	Y	Y	Y	Y	Y
Individual Fixed Effect	Y	Y	Y	Y	Y	Y	Y	Y
Time Fixed Effect	Y	Y	Y	Y	Y	Y	Y	Y
Individual–level Slopes	Y	Y	Y	Y	Y	Y	Y	Y
Time FE $\times$ initial credit cards per capita (by district, 2005)	Y	Y	Y	Y	Y	Y	Y	Y
Mean of Dependent Variable ($\times$100)	881.8	845.3	841	834	901.9	978.1	882.6	766.1
Observations	39,021,267	18,258,805	28,405,179	42,491,489	37,480,549	19,232,231	13,656,274	29,355,876
Number of Clusters	1,040,517	504,707	791,703	1,274,432	882,836	789,074	881,349	1,491,433
Weak identification F stat.	85.6	75.3	44.5	128.1	76.0	16.6	14.0	4.6
10% maximal IV size	16.9	16.9	16.9	16.9	16.9	16.9	16.9	16.9
Overidentification J stat.	$\chi^2(2)$=14.5	$\chi^2(2)$=49.5	$\chi^2(2)$=23.9	$\chi^2(2)$=53.9	$\chi^2(2)$=15.7	$\chi^2(2)$=1.1	$\chi^2(2)$=33.7	$\chi^2(2)$=42.7
p value	0.001	0.000	0.000	0.000	0.000	0.570	0.000	0.000

† Used $log(S/.100 + x)$ to avoid missing values
§ Denotes the first store ever opened by the conglomerate in a city.
Notes.— This table shows individual–level instrumental variable regressions of new credit cards from store–related banks on total borrowing, for different samples based on individual characteristics. Columns 1–3 report the effect of a new credit card on total consumer credit, by type of Employer Identification Number (EIN); individuals without an EIN number are usually salaried workers or individuals who have never worked before. Columns 4–5 report the same regressions according to the level of education when the individual was 18 years old: only completed high school vs. at least one term of college or superior education. Similarly, columns 6–8 report data by a Herfindahl-Hirschman Index that measures the degree of concentration of credit card limits for each individual, 18 months lagged. The data is a monthly panel from January 2006 to August 2012 and includes all Peruvian consumers with a positive credit history (holding at least one credit card now and 12 months prior, and not in default). Due to computational limitations, the regressions randomly sample 50% of the individuals and exclude districts that never had any store openings (or neighbored districts that had). New Credit Card$_{i,t}$ counts the number of new credit cards issued by store–related banks to the individual i in month t. Singleton groups are excluded. Coefficients and standard errors are scaled by 100. Robust standard errors in parentheses, clustered by individual. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix A: Variable Definitions

Table A1: Variable Definitions

Variable	Definition	Source
Total credit	End–of–month debt balance from the *Reporte Crediticio del Deudor* (*Anexo 6*). Sum of all loan accounts (codes 14∗) minus provisions (1409∗) and mortgages (14??04∗)	SBS
Credit from credit cards	Sum of all consumer credit card loan accounts (codes 14??0302∗) excluding provisions (1409∗) and mortgages (14??04∗)	SBS
Credit from other sources	*Total credit* minus *credit from credit cards*	SBS
Credit card limit	End–of–month credit card limit (account 810923) from the *Reporte Crediticio del Deudor* (*Anexo 6*). If such account was not available (before Feb 2006), sum the account 720506 (unused credit limits) with the variable *credit from credit cards*	SBS
Default in # months/quarters	Indicator variable equal to 1 if an individual enters default at any point between t+1 and t+#. For each individual, default is triggered if a material amount of debt (at least 5% of the individual's debt and at least 30 USD) is 60 or more days past due or has been refinanced, restructured or is in judicial collection. See appendix # for more details	SBS
New credit cards	Number of new cards issued to an individual by any of the three store–related banks in a given month. Note: we cannot observe multiple cards per bank; e.g. a Visa and Mastercard are always considered as one card	SBS
Store openings	Number of stores that opened in a district in a given month. See the online dataset and primary sources	SE[†], LineaGIS, author
…(weighted by area)	(Area of the district that intercepts a 1km buffer around a store opening) divided by (land area of a 1km buffer around a store opening)	SE, LineaGIS, INEI, author
…(weighted by EINs)	(Number of EINs in a district within 1km of a store opening) divided by (Total number of EINs within 1km of a store opening). Employer identification numbers (EINs) are provided to every firm, sole proprietor and individual contractor. Two local GIS firms (LineaGIS and BP Geocad) geocoded the addresses attached to the EINs	SE, LineaGIS, BP Geocad, Sunat, author
Bank branches in district	Number of bank branches in the district.	SBS, El Peruano
Branch density	Number of bank branches per 100,000 inhabitants, at the district level. Population figures are end–of–year estimates from the statistical agency.	SBS, El Peruano, INEI
Workers (growth)	Number of pension system registrations in the district over the last year, divided by all the registrations in the district over the last five years	Sunat
Contractors & Sole Prop. (growth)	Number of EIN registrations of contractors and sole proprietors in the district (last year over sum of last five years). These comprise all EIN identifiers that start with 10.	Sunat
All Firms (growth)	Number of firm registrations in the district (last year over sum of last five years). These comprise all EIN identifiers that start with 20.	Sunat
Construction Firms (growth)	Number of construction–related firm registrations in the district (last year over sum of last five years). These comprise all EIN identifiers that start with 10; and with a primary or secondary ISIC code of 45xx, 70xx or 7421 (rev. 3)	Sunat
District of residence	District of residence of each individual. Does not vary with time. See appendix # for the tiebreaking rules in case the different sources conflict.	Sunat, SBS, LineaGIS/Reniec
Schooling level	Last known schooling level registered by Reniec in 2011. We group all education levels above secondary school into *higher*. This is because Reniec records are seldom updated after an individual obtains his national ID card at age 18 (note that in Peru individuals usually start college by age 16–17).	LineaGIS/Reniec
EIN Type	Type of employer identification number. Individuals with an EIN are classified as either contractors ("persona natural sin negocio") or sole proprietors ("persona natural con negocio").	Sunat
Credit card concentration (HHI)	Herfindahl–Hirschman index of the credit card limits from each bank at the individual level For instance, if an individual has a \$1000 credit card limit from bank A and a \$2000 credit card limit from bank B, his HHI index will be $0.33^2 + 0.66^2 = 0.55$	SBS

[†] *SE* refers to Semana Económica, a Peruvian weekly news magazine.

Appendix B: Definition of Default

The definition of default employed through this paper matches the one used internally by the Peruvian banking regulator[44]. Broadly speaking, default is triggered when a non–trivial amount of debt is at least 60 days past due. In addition, it is defined at the individual level, instead of for each bank or for each credit product (i.e. *global* default instead of per–product or per–bank).

More specifically, there are two steps involved in this classification. First, for every debtor, each debt tranche[45] is classified as in default if any of these three criteria occurs:

1. The debt is rated as *doubtful* (60 days past due) or *loss* (120 days past due). This criteria has remained stable for the duration of our sample, and is specified in the resolutions SBS-RES 808-2003 and SBS-RES 11356-2008.
2. The debt has been refinanced, restructured, or is in judicial collection.
3. In the event where a borrower disappears from the credit records—which could happen if the bank sells it portfolio of bad loans to a third party—a stricter criteria is used. In particular, for the last month in record, the first condition is amended to included debt rated as *deficient* (30 days past due).

Second, if at least 5% of an individual's debt[46] is in default, and that amount is at least 100 Peruvian Nuevos Soles (roughly USD 30), then the individual is classified in default.

Appendix C: Geolocation of individuals to a district

Three different sources report individuals' district of residence. The tax authority (Sunat) reports them as part of the employer identification number (EIN) records, which include contractors and sole proprietors. These were collected on November 2012 so they reflect the information available to Sunat as of that date. The banking and pension fund regulator (SBS) reports them as part of the pension records, publicly available for every individual affiliated to a pension fund (which includes all workers). They were also collected on November 2012 so reflect information known to the SBS at that time. Finally, publicly available information from the national identification office (Reniec) published for the 2011 national election reflected electoral records as of May 2010. The biggest limitation of these records is that individuals might move to different districts so the records might not correspond to the facts at the time where

[44]Specifically, it follows the definition used by the Credit Risk Unit of the Superitendencia de Banca y Seguros, as of August 2012.

[45]Public records are not kept for every loan, but instead banks reports individual debt grouped by i) currency, ii) ledger account, iii) days past due, iv) debt status (normal, refinanced, restructured, in judicial collection), and iv) rating type (normal, with potential problems, deficient, doubtful, loss).

[46]Mortgage debt has been excluded from this analysisis—and from the rest of the paper—due to its high collateral and different dynamics.

the stores opened and the credit cards were granted. Since these sources might contradict each other, we record the district of residence based on our priors regarding the data quality of each source (these were obtained from conversations with executives of two GIS firms, LineaGIS and Guia de Calles, although the results are robust to alternative orderings). The selection criteria between these three sources is as follows:

1. If available, we use Sunat records for registrations that occurred from 2004 onwards.
2. Else, we use SBS records for registrations that occurred from 2004 onwards.
3. Else, we use Sunat records for registrations that occurred from 2000 onwards.
4. Else, we use SBS records for registrations that occurred from 2000 onwards.
5. Else, we use Reniec records.
6. Else, we use Sunat records from registrations prior to 2000.
7. Finally, we use SBS records from registrations prior to 2000.

Appendix D: Availability of Credit Bureau Data

The *Reporte Crediticio de Deudores* comprises the information reported monthly by banks to the regulator for each debtor. Within this report, Annex 6 contains the credit accounts of interest to our study. As described by figure Appendix D.1, the report itself started on January 2001. However, available credit limits were not reported until October 2004, and total credit limits were not reported until February 2006 (although they can be backtracked from the sum of credit card debt and unused credit limits). Nonetheless, not all banks reported credit limit information until the end of 2005, and even afterwards, there were some instances were banks failed to report credit limits for a particular month (see the online appendix for the detailed list).

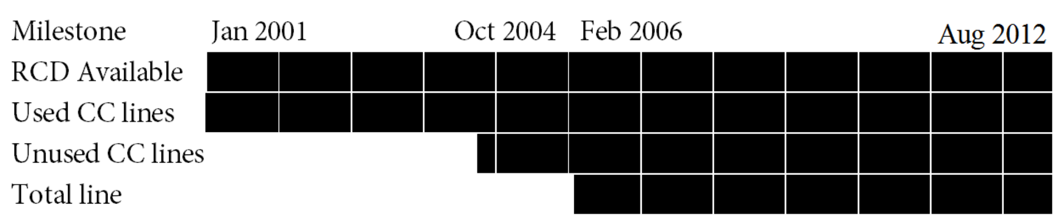


Figure Appendix D.1: Credit Card Data Availability

Source: Superintendencia de Banca, Seguros y AFP

Appendix E: Results: Intensity–Weighted Instruments

Table E1: First Stage Regression — Effect of Bank–Related Store Openings on New Credit Cards (Weighted Instrument)

Dependent Variable	New Credit Card (store–related banks)					
Type of Store Opening	Pooled Stores		Repeated vs *de Novo*		By Bank	
	(1)	(2)	(3)	(4)	(5)	(6)
entry_rucs_REL_T1			0.246***	0.224***		
			(0.009)	(0.009)		
entry_rucs_ALL_T2			0.446***	0.412***		
			(0.018)	(0.018)		
entry_rucs_REL	0.540***	0.516***				
	(0.009)	(0.009)				
entry_rucs_B2					0.703***	0.681***
					(0.011)	(0.011)
entry_rucs_B142					0.293***	0.273***
					(0.010)	(0.010)
entry_rucs_REL_T2			1.955***	1.968***		
			(0.025)	(0.026)		
entry_rucs_ALL	0.071***	0.084***			0.068***	0.080***
	(0.007)	(0.007)			(0.007)	(0.007)
entry_rucs_B73					1.338***	1.298***
					(0.023)	(0.024)
entry_rucs_ALL_T1			0.050***	0.068***		
			(0.007)	(0.007)		
District–level controls	Y	Y	Y	Y	Y	Y
District Fixed Effect	Y		Y		Y	
Individual Fixed Effect		Y		Y		Y
Time Fixed Effect	Y	Y	Y	Y	Y	Y
Time FE × initial credit cards per capita (by district, 2005)	Y	Y	Y	Y	Y	Y
Mean of Dependent Variable (×100)	1.759	1.759	1.759	1.759	1.759	1.759
Observations	117309310	117305635	117309310	117305635	117309310	117305635
Number of Clusters	2,873,208	2,869,533	2,873,208	2,869,533	2,873,208	2,869,533
F Statistic	2532.1	2507.3	2737.0	2694.3	2327.5	2282.1
R^2	0.002	0.030	0.002	0.031	0.002	0.030

Notes.— This table shows individual–level least squares regressions of bank-related retail store openings on new credit cards issued by store–related banks. The data is a monthly panel from January 2006 to August 2012 and includes all Peruvian consumers with a positive credit history (holding at least one credit card now and 12 months prior, and not in default). Due to computational limitations, the regressions randomly sample 50% of the individuals and exclude districts that never had any store openings (or neighbored districts that had). New Credit Card$_{i,t}$ counts the number of new credit cards issued by store–related banks to the individual i in month t. Store Opening$_{d(i),t-2,t+1}$ counts the number of new retail stores (supermarkets, department stores, etc.) opened in district d (where individual i lives) between months $t-2$ and $t+1$ (results are robust to the bandwidth choice). Additional district–level controls include store openings by any chain (including both bank-related and bank–unrelated stores), four measures of local economic growth (growth rates of firm registrations, construction firm registrations, independent worker registrations, and dependent worker registrations), and two measures of banking growth and activity (new bank branches, branch density). Columns 1–2 report results with all bank-related stores pooled; columns 3–4 report results separating *de novo* store openings (the first opening of a retail conglomerate in a province) with subsequent store openings; columns 5–6 report results separating by the identity of the bank-related store. Singleton groups are excluded. Coefficients and standard errors are scaled by 100. Robust standard errors in parentheses, clustered by individual. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E2

IV Regression — Direct Effects of Consumer Credit Supply Shocks on Consumer Borrowing (Weighted Instrument)

Dependent Variable	Δ log CC Credit from Store-Related Banks†				Δ log Credit Card Credit†			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
New Credit Card (store–related banks)	68.176***	64.034***	67.305***	64.354***	24.847***	21.862***	25.971***	23.438***
	(3.314)	(3.466)	(3.219)	(3.367)	(3.431)	(3.584)	(3.334)	(3.482)
entry_rucs_ALL_T1	0.066***	0.089***	0.063***	0.083***	0.087***	0.098***	0.084***	0.091***
	(0.014)	(0.015)	(0.014)	(0.015)	(0.014)	(0.015)	(0.014)	(0.015)
entry_rucs_ALL_T2	−0.199***	−0.127*	−0.190***	−0.141**	−0.008	0.044	−0.036	0.009
	(0.067)	(0.070)	(0.066)	(0.068)	(0.070)	(0.072)	(0.068)	(0.070)
District–level controls			Y	Y			Y	Y
District Fixed Effect	Y		Y		Y		Y	
Individual Fixed Effect		Y		Y		Y		Y
Time Fixed Effect	Y	Y	Y	Y	Y	Y	Y	Y
Time FE $\times$ initial credit cards per capita (by district, 2005)	Y	Y	Y	Y	Y	Y	Y	Y
Mean of Dependent Variable ($\times$100)	.2666	.267	.2666	.267	.1766	.1769	.1766	.1769
Observations	110087855	110080649	110087855	110080649	110087855	110080649	110087855	110080649
Number of Clusters	2,872,011	2,864,805	2,872,011	2,864,805	2,872,011	2,864,805	2,872,011	2,864,805
Weak identification F stat.	3008.6	2858.4	3213.8	3045.1	3008.6	2858.4	3213.8	3045.1
10% maximal IV size	19.9	19.9	19.9	19.9	19.9	19.9	19.9	19.9
Overidentification J stat.	$\chi^2(1)$=51.2	$\chi^2(1)$=46.5	$\chi^2(1)$=51.0	$\chi^2(1)$=46.9	$\chi^2(1)$=11.2	$\chi^2(1)$=8.4	$\chi^2(1)$=10.7	$\chi^2(1)$=8.5
p value	0.000	0.000	0.000	0.000	0.001	0.004	0.001	0.004

† Used $log(S/.100 + x)$ to avoid missing values

Notes.— This table shows individual–level instrumental variable regressions of new credit cards from store–related banks on credit card borrowing. Columns 1–4 report how a new credit card affects credit card borrowing from the three store–related banks, while columns 5–8 report the effects on total credit card borrowing. Store openings from store–related banks are used as instrument; see columns 3–4 of table 5. The data is a monthly panel from January 2006 to August 2012 and includes all Peruvian consumers with a positive credit history (holding at least one credit card now and 12 months prior, and not in default). Due to computational limitations, the regressions randomly sample 50% of the individuals and exclude districts that never had any store openings (or neighbored districts that had). New Credit Card$_{i,t}$ counts the number of new credit cards issued by store–related banks to the individual i in month t. Singleton groups are excluded. Coefficients and standard errors are scaled by 100. Robust standard errors in parentheses, clustered by individual. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E3

IV Regression — Substitution and Net Effects of Consumer Credit Supply Shocks on Consumer Borrowing (Weighted Instrument)

Dependent Variable	Δ log Non–CC Credit[†]				Δ log Total Credit[†]			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
New Credit Card (store–related banks)	9.078**	6.547	8.930**	6.456*	19.912***	15.313***	20.166***	16.172***
	(3.881)	(4.039)	(3.769)	(3.923)	(3.460)	(3.603)	(3.362)	(3.500)
entry_rucs_ALL_T2	−0.210***	−0.172**	−0.209***	−0.171**	−0.145**	−0.049	−0.153**	−0.068
	(0.078)	(0.080)	(0.076)	(0.078)	(0.070)	(0.072)	(0.068)	(0.070)
entry_rucs_ALL_T1	−0.046***	−0.036**	−0.041***	−0.033**	0.036**	0.053***	0.037**	0.051***
	(0.016)	(0.016)	(0.016)	(0.016)	(0.015)	(0.015)	(0.014)	(0.015)
District–level controls			Y	Y			Y	Y
District Fixed Effect	Y		Y		Y		Y	
Individual Fixed Effect		Y		Y		Y		Y
Time Fixed Effect	Y	Y	Y	Y	Y	Y	Y	Y
Time FE $\times$ initial credit cards per capita (by district, 2005)	Y	Y	Y	Y	Y	Y	Y	Y
Mean of Dependent Variable ($\times$100)	.4035	.4037	.4035	.4037	.3764	.3767	.3764	.3767
Observations	110087855	110080649	110087855	110080649	110087855	110080649	110087855	110080649
Number of Clusters	2,872,011	2,864,805	2,872,011	2,864,805	2,872,011	2,864,805	2,872,011	2,864,805
Weak identification F stat.	3008.6	2858.4	3213.8	3045.1	3008.6	2858.4	3213.8	3045.1
10% maximal IV size	19.9	19.9	19.9	19.9	19.9	19.9	19.9	19.9
Overidentification J stat.	$\chi^2(1)$=0.1	$\chi^2(1)$=0.2	$\chi^2(1)$=0.1	$\chi^2(1)$=0.2	$\chi^2(1)$=3.1	$\chi^2(1)$=3.2	$\chi^2(1)$=3.1	$\chi^2(1)$=3.5
p value	0.759	0.639	0.723	0.638	0.079	0.073	0.077	0.063

[†] Used $log(S/.100 + x)$ to avoid missing values

Notes.— This table shows individual–level instrumental variable regressions of new credit cards from store–related banks on non-credit card and total borrowing. Columns 1–4 report the effect of a new credit card on borrowing from other sources (auto loans, consumer credit, etc. but excluding mortgage loans). Columns 5–8 report the total effect on consumer borrowing. Store openings from store–related banks are used as instrument; see columns 3–4 of table 5. The data is a monthly panel from January 2006 to August 2012 and includes all Peruvian consumers with a positive credit history (holding at least one credit card now and 12 months prior, and not in default). Due to computational limitations, the regressions randomly sample 50% of the individuals and exclude districts that never had any store openings (or neighbored districts that had). New Credit Card$_{i,t}$ counts the number of new credit cards issued by store–related banks to the individual i in month t. Singleton groups are excluded. Coefficients and standard errors are scaled by 100. Robust standard errors in parentheses, clustered by individual. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E4

IV Regression — Effects of Consumer Credit Supply Shocks on Consumer Default (Weighted Instrument)

Default in…	1 quarter† (1)	2 quarters† (2)	3 quarters† (3)	4 quarters† (4)	5 quarters† (5)	6 quarters† (6)	7 quarters† (7)	8 quarters† (8)
New Credit Card (store–related banks)	−1.18	5.39	15.08***	20.83***	21.54***	21.37***	22.57***	23.36***
	(2.64)	(3.92)	(4.57)	(5.02)	(5.32)	(5.54)	(5.68)	(5.78)
entry_rucs_ALL_T1	0.01	0.00	−0.01	−0.01	−0.02	−0.02	−0.01	0.01
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)
entry_rucs_ALL_T2	0.04	−0.14	−0.36***	−0.45***	−0.43***	−0.39***	−0.36**	−0.39***
	(0.07)	(0.11)	(0.12)	(0.13)	(0.14)	(0.15)	(0.15)	(0.15)
District–level controls	Y	Y	Y	Y	Y	Y	Y	Y
Individual Fixed Effect	Y	Y	Y	Y	Y	Y	Y	Y
Time Fixed Effect	Y	Y	Y	Y	Y	Y	Y	Y
Time FE $\times$ initial credit cards per capita (by district, 2005)	Y	Y	Y	Y	Y	Y	Y	Y
Mean of Dependent Variable ($\times$100)	3.079	5.511	7.604	9.473	11.17	12.72	14.16	15.5
Relative Effect (%)	-38.3	97.7	198.3	219.9	192.8	168.0	159.4	150.7
Observations	40,544,359	40,544,359	40,544,359	40,544,359	40,544,359	40,544,359	40,544,359	40,544,359
Number of Clusters	1,194,563	1,194,563	1,194,563	1,194,563	1,194,563	1,194,563	1,194,563	1,194,563
Weak identification F stat.	1607.9	1607.9	1607.9	1607.9	1607.9	1607.9	1607.9	1607.9
10% maximal IV size	19.9	19.9	19.9	19.9	19.9	19.9	19.9	19.9
Overidentification J stat.	$\chi^2(1)$=0.1	$\chi^2(1)$=2.0	$\chi^2(1)$=1.6	$\chi^2(1)$=3.7	$\chi^2(1)$=2.2	$\chi^2(1)$=1.9	$\chi^2(1)$=0.4	$\chi^2(1)$=1.6
p value	0.811	0.162	0.199	0.055	0.138	0.165	0.504	0.205

† Default defined as 60 days past due; see appendix for details.

Notes.— This table shows individual–level instrumental variable regressions of new credit cards from store–related banks on default indicators at a varying horizon. Default is treated as an absorbing state so the frequency of default increases monotonically with the default horizon, as seen in the *mean of dependent variable* row. In turn, the *Relative Effect* row reports the normalized coefficients of interest, by dividing the coefficient for new credit cards by the average default frequency. Store openings from store–related banks are used as instrument; see columns 3–4 of table 5. Note that this is the dynamic counterpart of preceding specifications, and thus specified in levels instead of in differences. The data is a monthly panel from January 2006 to August 2012 and includes all Peruvian consumers with a positive credit history (holding at least one credit card now and 12 months prior, and not in default). Due to computational limitations, the regressions randomly sample 50% of the individuals and exclude districts that never had any store openings (or neighbored districts that had). New Credit Card$_{i,t}$ counts the number of new credit cards issued by store–related banks to the individual i in month t. Singleton groups are excluded. Coefficients and standard errors are scaled by 100. Robust standard errors in parentheses, clustered by individual. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table E5
IV Regression — Consumer Credit Supply Shocks, Credit Card Limits from Other Banks, and Net Effects (Weighted Instrument)

Dependent Variable	log Credit Card Limits, Unrelated Banks†				log Credit Card Limits, All Banks†			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Number of Credit Cards (store–related banks)	−107.4***	−85.0***	−81.5***	−60.2***	17.6***	18.1***	12.5***	17.9***
	(7.0)	(6.2)	(7.4)	(6.2)	(3.5)	(3.1)	(3.9)	(3.2)
L12.Number of Credit Cards (store–related banks)	−28.5***	−0.9	−70.1***	−34.6***	−17.4***	−11.1***	−18.6***	−13.3***
	(4.8)	(4.4)	(7.1)	(5.8)	(2.5)	(2.3)	(3.7)	(3.1)
entry_rucs_ALL_T2	0.1	1.9***	−2.3***	0.0	−1.3***	−0.7***	−1.4***	−0.7***
	(0.3)	(0.2)	(0.4)	(0.2)	(0.1)	(0.1)	(0.2)	(0.1)
entry_rucs_ALL_T1	2.0***	1.9***	1.8***	1.7***	0.5***	0.4***	0.5***	0.4***
	(0.1)	(0.1)	(0.1)	(0.1)	(0.0)	(0.0)	(0.0)	(0.0)
L12.entry_rucs_ALL_T1	1.0***	0.7***	0.7***	0.5***	0.3***	0.3***	0.4***	0.3***
	(0.1)	(0.1)	(0.1)	(0.1)	(0.1)	(0.0)	(0.0)	(0.0)
L12.entry_rucs_ALL_T2	5.9***	4.3***	6.0***	4.4***	1.3***	1.0***	1.5***	1.1***
	(0.3)	(0.3)	(0.4)	(0.3)	(0.2)	(0.1)	(0.2)	(0.2)
District–level controls (incl. lagged)			Y	Y			Y	Y
Individual Fixed Effect	Y	Y	Y	Y	Y	Y	Y	Y
Time Fixed Effect	Y	Y	Y	Y	Y	Y	Y	Y
District–level Slopes	Y		Y		Y		Y	
Individual–level Slopes		Y		Y		Y		Y
Time FE × initial credit cards per capita (by district, 2005)	Y	Y	Y	Y	Y	Y	Y	Y
Mean of Dependent Variable (×100)	733.5	733.5	733.5	733.5	860.5	860.5	860.5	860.5
Observations	85,685,251	85,685,251	85,685,251	85,685,251	85,685,251	85,685,251	85,685,251	85,685,251
Number of Clusters	2,336,927	2,336,927	2,336,927	2,336,927	2,336,927	2,336,927	2,336,927	2,336,927
Weak identification F stat.	335.8	390.8	234.2	354.1	335.8	390.8	234.2	354.1
10% maximal IV size	16.9	16.9	16.9	16.9	16.9	16.9	16.9	16.9
Overidentification J stat.	$\chi^2(2)$=18.5	$\chi^2(2)$=53.6	$\chi^2(2)$=4.8	$\chi^2(2)$=23.4	$\chi^2(2)$=10.4	$\chi^2(2)$=4.1	$\chi^2(2)$=12.5	$\chi^2(2)$=1.8
p value	0.000	0.000	0.089	0.000	0.005	0.130	0.002	0.397

† Used $log(S/.100 + x)$ to avoid missing values

Notes.— This table shows individual–level instrumental variable regressions of the number of credit cards from store–related banks on credit card limits. Effects are measured at a contemporaneous and at a twelve–month horizon. Columns 1–4 report effects on credit card limits from store–*unrelated* banks. These are intepreted as strategic responses from other banks to the increased credit supply from store–related banks. Columns 5–8 report the net effects on total credit card limits. Store openings from store–related banks are used as instrument; see appendix for the first stage tables (insert link). The data is a monthly panel from January 2006 to August 2012 and includes all Peruvian consumers with a positive credit history (holding at least one credit card now and 12 months prior, and not in default). Due to computational limitations, the regressions randomly sample 50% of the individuals and exclude districts that never had any store openings (or neighbored districts that had). Number of Credit Card$_{i,t}$ counts the number of credit cards from store–related banks available to the individual i in month t. Singleton groups are excluded. Coefficients and standard errors are scaled by 100. Robust standard errors in parentheses, clustered by individual. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix F: Including Changes in Credit Limits of Existing Cards as an Endogenous Regressor

Table F1
First Stage Regression — Effect of Store Openings on New Credit Cards Issued by Store-Related Banks.

Dependent Variable	New Credit Card (store–related banks)				S_log_lt2_pre			
Type of Store Opening	Repeated vs *de Novo*		By Bank		Repeated vs *de Novo*		By Bank	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Store Opening (repeat, bank–related)	0.175***	0.156***			0.0705***	0.0843***		
	(0.0111)	(0.0113)			(0.0215)	(0.0217)		
Store Opening (de novo, bank–related)[†]	1.983***	1.996***			−0.0240	−0.0163		
	(0.0295)	(0.0300)			(0.0459)	(0.0474)		
Store Opening (bank A)			0.737***	0.719***			0.0273	0.0495**
			(0.0132)	(0.0134)			(0.0243)	(0.0247)
Store Opening (bank B)			0.790***	0.746***			0.247***	0.267***
			(0.0283)	(0.0289)			(0.0422)	(0.0430)
Store Opening (bank C)			0.418***	0.402***			0.0178	0.0260
			(0.0115)	(0.0117)			(0.0207)	(0.0211)
Store Opening (all stores, repeat)	0.0276***	0.0452***	−0.220***	−0.205***	−0.0603***	−0.0616***	−0.0337**	−0.0341**
	(0.00888)	(0.00906)	(0.00860)	(0.00877)	(0.0179)	(0.0182)	(0.0166)	(0.0169)
Store Opening (all stores, de novo)[†]	0.257***	0.230***	1.046***	1.045***	0.142***	0.163***	0.0666**	0.0822***
	(0.0188)	(0.0193)	(0.0157)	(0.0160)	(0.0374)	(0.0383)	(0.0270)	(0.0275)
District–level controls	Y	Y	Y	Y	Y	Y	Y	Y
District Fixed Effect	Y		Y		Y		Y	
Individual Fixed Effect		Y		Y		Y		Y
Time Fixed Effect	Y	Y	Y	Y	Y	Y	Y	Y
Time FE × initial credit cards per capita (by district, 2005)	Y	Y	Y	Y	Y	Y	Y	Y
Mean of Dependent Variable ($\times 100$)								
Observations	58,659,148	58,657,318	58,659,148	58,657,318	58,659,148	58,657,318	58,659,148	58,657,318
Number of Clusters	1,436,424	1,434,594	1,436,424	1,434,594	1,436,424	1,434,594	1,436,424	1,434,594
F Statistic	1396.5	1380.8	1288.6	1259.5	41.13	65.01	39.75	61.47
R^2	0.002	0.031	0.002	0.031	0.006	0.026	0.006	0.026

[†] Denotes the first store ever opened by the conglomerate in a city.

Notes.— This is an individual-level monthly panel regression from 2006 to August 2012. The sample consists of individuals owning at least one non-new, non-defaulted credit card. Coefficients and standard errors are scaled by 100. Robust standard errors in parentheses, clustered by individual. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table F2

IV Regression — Shifts of Consumer Credit Supply through New Credit Cards: Direct Effects on Consumer Borrowing.

Dependent Variable	Δ log CC Credit from Store-Related Banks†	Δ log Credit Card Credit†	Δ log Credit Other Banks†	Δ log Non–CC Credit†	Δ log Total Credit†	Δ log Cr. Limit Other Banks†
	(1)	(2)	(3)	(4)	(5)	(6)
New Credit Card (store–related banks)	48.75***	17.03***	−2.682	1.945	12.26***	0.472
	(4.749)	(3.893)	(4.618)	(4.455)	(3.826)	(3.391)
Store Opening (all stores, repeat)	0.117***	0.0897***	0.00785	0.000301	0.0562***	0.0106
	(0.0196)	(0.0160)	(0.0189)	(0.0178)	(0.0157)	(0.0138)
Store Opening (all stores, de novo)§	−0.177	−0.0200	0.158	−0.265**	−0.0973	0.0504
	(0.113)	(0.0929)	(0.109)	(0.103)	(0.0906)	(0.0812)
S_log_lt2_pre	196.5 ***	92.83**	−114.3 **	68.48	66.36	39.29
	(54.90)	(45.08)	(53.01)	(49.60)	(43.98)	(39.20)
District–level controls	Y	Y	Y	Y	Y	Y
Individual Fixed Effect	Y	Y	Y	Y	Y	Y
Time Fixed Effect	Y	Y	Y	Y	Y	Y
Time FE $\times$ initial credit cards per capita (by district, 2005)	Y	Y	Y	Y	Y	Y
Mean of Dependent Variable ($\times$100)	.2743	.1559	-.1064	.3973	.3566	.6977
Observations	58,657,318	58,657,318	58,657,318	58,657,318	58,657,318	58,657,318
Number of Clusters	1,434,594	1,434,594	1,434,594	1,434,594	1,434,594	1,434,594
F Statistic	82.35	48.39	11.47	1.739	14.84	58.54
R^2	-0.785	-0.133	-0.469	-0.161	-0.071	-0.055

† Used $log(S/.100 + x)$ to avoid missing values

§ Denotes the first store ever opened by the conglomerate in a city.

Notes.— This is an individual-level monthly panel regression from 2006 to August 2012. The sample consists of individuals owning at least one non-new, non-defaulted credit card. Coefficients and standard errors are scaled by 100. Robust standard errors in parentheses, clustered by individual. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix G: Results: Quarterly Frequency

Table G1
Quarterly Frequency

Dependent Variable	Δ log CC Credit from Store-Related Banks†	Δ log Credit Card Credit†	Δ log Non–CC Credit†	Δ log Total Credit†
	(1)	(2)	(3)	(4)
New Credit Card (store–related banks)	1.866***	0.673***	−0.0506	0.348***
	(0.0715)	(0.0678)	(0.0744)	(0.0670)
Store Opening (all stores)	0.00305***	0.00264***	−0.000610	0.00151***
	(0.000422)	(0.000406)	(0.000435)	(0.000405)
Δ Num. Branches	0.000725***	0.000438**	0.000561***	0.000544***
	(0.000179)	(0.000175)	(0.000187)	(0.000176)
Branch Density§	0.000221***	0.000125***	0.0000232	0.0000660*
	(0.0000361)	(0.0000351)	(0.0000435)	(0.0000372)
Dependent Workers¶	−0.0182**	0.0414***	−0.0238**	0.0128
	(0.00906)	(0.00939)	(0.0119)	(0.0103)
Construction Firms¶	−0.00656***	−0.00453***	−0.00176	−0.00470***
	(0.00146)	(0.00154)	(0.00192)	(0.00165)
All Firms¶	0.0173***	0.0165***	−0.00261	0.00762
	(0.00512)	(0.00553)	(0.00696)	(0.00601)
Indep. Wkrs. & Sole Prop.¶	0.0408***	0.0467***	−0.00906	0.0137*
	(0.00698)	(0.00728)	(0.00896)	(0.00779)
Individual Fixed Effect	Y	Y	Y	Y
Time Fixed Effect	Y	Y	Y	Y
Time FE × initial credit cards per capita (by district, 2005)	Y	Y	Y	Y
Observations	31,867,674	31,867,674	31,867,674	31,867,674
Number of Clusters	2,485,860	2,485,860	2,485,860	2,485,860
F Statistic	228.7	70.67	2.642	18.10
first_SWchi2				
j	10.19	1.346	5.697	3.420
jdf	1	1	1	1
Overid. P-val (Hansen J)	0.00141	0.246	0.0170	0.0644

† Used $log(S/.100 + x)$ to avoid missing values
§ Number of bank branches per 100,000 inhabitants
¶ New registrations in the last year over total registrations in the last 5 years, at the district level.

Notes.— This is an individual-level monthly panel regression from 2006 to August 2012. The sample consists of individuals owning at least one non-new, non-defaulted credit card. Robust standard errors in parentheses, clustered by individual. Levels of significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Appendix H: A Default Externality Model

This is a static model where consumers borrow through credit cards and can default on them contingent only nonpecuniary penalties. Therefore, borrowing serves both the role of consumption smoothing and insurance. Although banks can set both interest rates and credit limits, they only compete through credit limits. In that sense, consumers first decide how much to borrow, and then decide how to allocate the borrowing with each bank using the cards at their disposal (the "share" of each bank).

Therefore, if a bank increases credit limits, there are three potential effects: i) the customer borrows more in the credit-constrained state, increasing the bank's revenue ii) as a consequence, his default probability increases for all his loans, reducing the profit margin for all banks[47], and iii) even if the customer were not to borrow more, the limit increase may cause him to switch existing borrowing to the bank's card.

The interplay of these channels in turn affects the dynamics of bank competition. If bank A were to increase its credit limits (due to e.g. a decrease in its funding costs), bank B could react in several different ways. First, it could reduce its own limits due to the increase default risk (default externalities). Second, it could increase its limits if default risks are low, in order to maintain its "share" of the customer (specially for high quality / low default risk customers).

In addition, the incentives faced by small (in terms of the credit limit given over total credit limit) and large banks are quite different. Large banks have a larger downside of providing extra credit, as they bear most of the cost of the increased default rates. On the other hand, if the cards of the large banks have a higher priority, and new cards or cards from smaller banks are only used after exhausting all others, then expected margins for small bank cards will be much lower, as they will only lend to consumers in distress.

The model abstracts away from several important features. First, it does not consider competition through other channels (interest rates, balance transfers, reward points, etc.). Second, by being static, it does not consider how banks stage changes in credit limits through the year. Lastly, the idiosyncratics of behavioral consumers are not explored in detail.

Consumer Problem

One representative consumer and two banks. The second bank only learns about the customer with probability q; otherwise they cannot contract at all[48].

Three periods. On $t = 1, 2$ the consumer will earn income and consume; he can borrow between $t = 1$ and 2[49].

[47] There is no "hard limit" on the amount of credit limit (and debt) that the customer can take. Instead, the margin decreases gradually as the risk increases.

[48] This uncertainty allows for equilibriums beyond those of e.g. Parlour & Rajan.

[49] We can exclude the possibility of savings WLOG.

Borrowing only occurs through credit cards contracted at $t = 0$, with contract terms that cannot be contingent on time 1 information. The rationale is that at $t = 1$ there will not be enough time to arrange and screen a debt contract (so it needs to be prearranged), and regulations and lack of verifiability preclude using time 1 information (such as income, card usage with other creditors, etc.)

The consumer's income will be $w_t \overset{iid}{\sim} W(w)$ with CDF $F(w)$ over $[\underline{w}, \overline{w}]$, $\underline{w} > 0$, for $t = 1, 2$. His instantaneous utility function is $u(c)$ where $u' > 0$, $u'' < 0$. He can default at $t = 2$, and will then bear a nonpecuniary penalty of γ (in utils) s.t. $0 < \gamma < u(\overline{w})$. His utility maximization problem, as viewed at $t = 1$, is thus:

$$\underset{\{c_1, c_2, b, \text{def}\}}{\text{maximize}} \quad U_1 = u(c_1) + \beta \mathbb{E}\left[u(c_2) - \gamma \times \text{def}\right] \tag{3}$$

$$\begin{aligned} \text{s.t.} \quad & c_1 = w_1 + b \\ & c_2 = w_2 - (1 - \text{def})Rb) \\ & x \geq b \geq 0 \\ & R \geq 1 \\ & \text{def} \in \{0, 1\} \end{aligned}$$

Where def is decided at time 2.

Lemma 1. *There exists an income threshold $w_2^*(Rb)$ such that the customer defaults iff his time 2 income is below it.*

Proof. The consumer will be better off defaulting if $u(w_2) - \gamma \geq u(w_2 - Rb)$, or equivalently if $Y(w_2, Rb) \equiv u(w_2) - u(w_2 - Rb) \geq \gamma$. Note that $Y(w_2, Rb) = \int_{w_2 - Rb}^{w_2} u'(w)\mathrm{d}w$, and $\frac{\partial Y}{\partial w_2} = u'(w_2) - u'(w_2 - Rb) = \int_{w_2 - Rb}^{w_2} u''(w)\mathrm{d}w < 0$ as $u'' < 0$. Since Y is decreasing in w_2, there is a point w_2^* where both sides are equal. For w_2 lower than this, $Y(w_2) < \gamma$ and the consumer will default.

Note: his indirect utility at time 2 is then $V_2(w_2, b) \equiv \max\left\{u(w_2) - \gamma, u(w_2 - Rb)\right\}$

Lemma 2. *As the amount to be repaid increases, the default threshold (time 2 income required to avoid default) increases: $\frac{\mathrm{d}w_2^*}{\mathrm{d}Rb} > 0$*

Proof. Note that $\frac{\partial Y}{\partial Rb} = -u'(w_2 - Rb)(-1) = u'(w_2 - Rb) > 0$. Now apply the implicit function theorem, so $\frac{\mathrm{d}w_2^*}{\mathrm{d}Rb} = -\frac{Y_{Rb}}{Y_{w_2^*}} > 0$.

Lemma 3. *The probability of default, given time 1 information, increases with debt: $\frac{\partial pd}{\partial Rb} > 0$*

Proof. At $t = 1$, the probability of default when taking debt is pd $\equiv \mathbb{P}\left[u(w_2) - \gamma \geq u(w_2 - Rb)\right]$ which can be rewritten as $\mathbb{P}\left[w_2 \leq w_2^*\right] = F(w_2^*)$. Then, $\frac{\partial \mathrm{pd}}{\partial Rb} = f(w_2^*)\frac{\mathrm{d}w_2^*}{\mathrm{d}Rb} > 0$.

Lemma 4. *If the solution is interior ($0 < b^* < x$), the optimal consumer borrowing $b^*(w_1, R, x)$ decreases with time 1 income: $\frac{\partial b^*}{\partial w_1} < 0$*

Proof. At $t = 1$, the consumer UMP can be written as

$$\begin{aligned}\underset{b}{\text{maximize}} \quad U &= u(w_1 + b) + \beta\mathbb{E}\left[V_2(w_2, b)\right] \\ &= u(w_1 + b) + \beta\left\{\int_{\underline{w}}^{w_2^*(Rb)} \left[u(w) - \gamma\right] f(w)\mathrm{d}w \int_{w_2^*(Rb)}^{\overline{w}} u(w - Rb) f(w)\mathrm{d}w\right\}\end{aligned} \tag{4}$$

$$\text{s.t.} \quad x \geq b \geq 0$$

We then take the FOC wrt b, simplify it, and arrive to

$$u'(w_1 + b^*) = R\beta\mathbb{E}\left[u'(w_2 - Rb^*) \times \mathbb{1}_{w_2 \geq w_2^*(Rb^*)}\right] \tag{5}$$

This equation will hold as long as $0 < b^* < x$. Given a value of w_1, if we already have an interior solution, increasing w_1 will also increase the LHS of the equality so in turn b^* needs to decrease.

Lemma 5. *If $R\beta$ is sufficiently small, there is a threshold of time 1 income such that the consumer will borrow below it: $b^* > 0 \iff w_1 < w_1^*$*

Proof. Suppose $w_1 = \underline{w}$ and we are in a corner solution where $b^* = 0$. The, the FOC doesn't hold as the marginal utility of time 1 consumption (the LHS) is smaller than that the expected marginal utility of time 2 consumption (RHS). However:

$$\begin{aligned} u'(\underline{w}) &\geq u'(w)\forall w \in [\underline{w}, \overline{w}] \\ &\geq u'(w_2 - Rb^*) \times \mathbb{1}_{w_2 \geq w_2^*(Rb^*)} \quad \text{(a debtless consumer will not default)} \\ &> \mathbb{E}\left[u'(w_2 - Rb^*) \times \mathbb{1}_{w_2 \geq w_2^*(Rb^*)}\right] \quad \text{(as long as the distribution is not collapsed)} \end{aligned}$$

If $R\beta$ is sufficiently small (close to 1), then we arrive to a contradiction ($LHS > RHS$) and our premise will not hold.

Lemma 6. *If the credit limit x is sufficiently tight (small), increasing it will increase expected borrowing: $\exists\, \overline{x} > 0$ st $\frac{\partial \mathbb{E}b}{\partial x} > 0$ if $x < \overline{x}$*

Proof. The consumer borrows in $[\underline{w}, w_1^*]$. As long as $x < b^*(\underline{w}, R, \infty) \equiv \overline{x}$, then increasing credit limits will allow a segment of low-income consumers to borrow more.

At this point, notice that if a consumer has credit cards with multiple lenders, he needs to decide how to allocate his borrowing across the several cards. If he is fully rational and the interest rates differ, he would select the lowest-rate card first, and so on. If these two assumptions do not hold, the consumer may have a different allocation rule

Bank Problem

There is at least one risk-neutral bank, which decides the credit limit x_j and interest rates R_j (this may be decided consumer-by-consumer, or at an aggregate level for a pool of consumers).

The funding costs are r_j. They depend on risk-free rates, the size of the bank's deposit base (number of agencies, etc.), its operational efficiency, and potential synergies with other business areas of the conglomerate that owns the bank (for instance, retail chains, gas stations, etc.).

All in all, the profit maximization problem is:

$$\underset{\{x_j, R_j\}}{\text{maximize}} \quad \Pi_j = \mathbb{E}_{w_1}\{b_j^* \cdot \underbrace{\left[R_j \cdot (1 - pd) - r_j\right]}_{\text{Net Margin } M_j}\} \tag{6}$$

Whenever $w_1 > w_1^*$, the consumer doesn't borrow ($b_j^* = 0$). Also, the consumer may exhaust her total credit limit $X \equiv \Sigma x$ if her time 1 income is too low. Denote this threshold by $\omega(\Sigma x) := b^{-1}(\Sigma x; R)$. Notice that $b_j^* = x_j$ in the states when $w_1 < \omega$. With this, we can rewrite the maximization problem as:

$$\begin{aligned}
\underset{\{x_j, R_j\}}{\text{maximize}} \quad \Pi_j \;&=\; \mathbb{E}_{w_1}\left[\mathbb{1}_{w_1 < \omega(\Sigma x)} \cdot x_j \cdot M_j + \mathbb{1}_{\omega(\Sigma x) w_1 < w_1^*} \cdot b_j^* \cdot M_j\right] \\
&=\; F\left[\omega(X)\right] \cdot x_j \cdot M_j\big|_{b=X} + \int_{\omega(X)}^{\overline{w}} b_j^* \cdot M_j \cdot f(w_1) \cdot \mathrm{d}w_1
\end{aligned}$$

$$\begin{aligned}
\text{where} \quad & b_j^*(w_1, \boldsymbol{R}, \boldsymbol{x}) = b^*(w_1, \Sigma x, \overline{R}) \cdot s(b^*, x_j, x_{-j}) \\
& pd = F(w_2^*()) \\
& w_2^* = w_2^*(Rb^*()) \\
& 0 \le b^* \le \Sigma x
\end{aligned}$$

Notice that $\frac{\partial M_j}{\partial x_j} = -R\frac{\partial pd}{\partial \Sigma x} = -R\frac{\partial pd}{\partial Rb}\frac{\partial Rb}{\partial \Sigma x} = -R^2 \cdot f()\frac{\partial w_2^*}{\partial Rb} \cdot \frac{\partial b^*}{\partial \Sigma x}$.

The $\textsc{foc}(x_j)$ is then:

$$\frac{\partial \Pi}{\partial x_j} = \left(M_j|_{b=X} - R \cdot x_j \frac{\partial pd|_{b=X}}{\partial b} \right) F\left[\omega(X)\right] + \underbrace{\int_{\omega(X)}^{w_1^*} b^* \cdot M_j \cdot \frac{\partial s}{\partial x_j} f(w_1) \mathrm{d}w_1}_{\mathbb{E}\left[s' b^* M_j | \omega(X) \leq w_1 \leq w_1^*\right]\left[F(w_1^*) - F(\omega(X))\right]} = 0 \quad (7)$$

The intuition of this equation can be seen in two steps. First assume there is only one bank so $s = 1$ and $s' = 0$. The FOC then becomes

$$M_j|_{b=X} = R \cdot x_j \frac{\partial pd|_{b=X}}{\partial b}$$

There is a trade-off when deciding the size of the credit limit. On one hand, a larger limit will increase proceeds proportionally to the bank's net margin M_j for those customers that are credit constrained[50]. On the other, more debt will increase the overall riskiness of these borrowers, and not just in the new tranches but in their total debt (thus, the derivative of pd is multiplied by the banks' exposure at default at time 2).

Allowing for multiple lenders introduces the term s, providing another avenue of competition: banks could benefit from increasing credit limits not just because the consumer will borrow more, but because he could switch *existing* borrowing to the bank (as long as $s' > 0$). Also, these channel applies for unconstrained borrowers, a different (ex post) group than the first channel (so each of the two channels is weighted by the probability of it ocurring)

Lemma 7 (Given certain assumptions about s). *If other banks exogenously increase their credit limit x_{-j}, then bank j's optimal response will be to increase credit limits less than proportionally, so the total credit limit will increase.*

Proof. Note that F, M and pd depend on the total credit limit X but not on its distribution across banks. From the implicit function theorem applied to the FOC, we know that

$$\frac{\partial^2 \Pi}{\partial x_j^2} \mathrm{d}x_j + \frac{\partial^2 \Pi}{\partial x_j \partial x_{-j}} \mathrm{d}x_{-j} = 0$$

Now assume that $\mathrm{d}x_{-j} = -\mathrm{d}x_j = \Delta$ so X remains unchanged. After cancelling $\Delta > 0$, the LHS of the above equation becomes

[50] Notice that these marginal customers have a higher probability of default than the unconstrained ones, so they provide a lower net margin.

$$\underbrace{F(\omega(X))R\frac{\partial pd(b^*{=}X)}{\partial b}}_{>0} \quad +$$

$$\mathbb{E}\left[\left(\frac{\partial^2 s}{\partial x_j \partial x_{-j}} - \frac{\partial^2 s}{\partial x_j^2}\right) b^* M \middle| \omega(X) \leq w_1 \leq w_1^*\right] [F(w_1^*) - F(\omega(X))]$$

So the effect will depend on the second derivatives of s.

Now, let's assume a simple functional form for $s() = x_j/X$. This implies that $\frac{\partial s}{\partial x_j} = \frac{x_{-j}}{X^2} = (1-s)/X$ and $\frac{\partial^2 s}{\partial x_j \partial x_{-j}} = \frac{\partial^2 s}{\partial x_j^2} + \frac{1}{X^2} > \frac{\partial^2 s}{\partial x_j^2}$.

Therefore, the LHS is strictly positive and a change of x_j proportional to the change in x_{-j} is excessive as the LHS of the FOC is now positive.

An implication of the above lemma is that an environment with multiple identical banks as potential players will lead to an equilibrium with a higher credit limit. To see why a potential entrant would want to establish a relationship with the consumer, notice that the potential entrant is equivalent to a participant with $x_j = 0$, so he has no downside on the FOC in terms of default risks (as his exposure at default is zero). Therefore, he would benefit from increasing his supply of credit limits.

This issue is the crux of default externalities: the downside of increasing credit limits (and credit) is not fully absorbed by the bank performing the action. Therefore, a policy implication would be to limit the number of lenders a consumer face (e.g. geographically). In practice, however, this issue is ameliorated by transaction costs associated with holding multiple credit cards: a consumer may find it too cumbersome to hold low-limit cards (and track their available balance), and banks may also have fixed costs associated with each customer (such as risk monitoring costs, costs of delivering credit card statements, etc.).

Equilibrium

Lemma 8. *If there is a continuum number of banks, the only equilibrium will be an equilibrium where banks will earn zero profit and no bank will have a finite mass of credit (i.e. an atom)*

Proof. As long as the net margin M_j of at least one bank is positive, more banks will enter. They can replicate the bank's interest rate R_j and offer a volume of credit low enough that the change in PD will not turn the net margin negative.

Thus, the equilibrium—if it exists—will involve a zero net margin for all banks, which as they share the same p and r involves the same interest rates.

Now, to prove existence, notice that if all banks set credit limits and the same interest rate such that expected margins are zero, and there is one bank that lends a positive amount of credit, then this bank can reduce supply and by doing so, reduce the probability of default. Since margins were already zero, any reduction in probability of default would lead to a positive equilibrium, so banks with positive mass of credit cannot exist in equilibrium. On the other hand, if no banks have a positive amount of credit (but together they integrate to one), then each bank cannot affect the probability of default and is thus content supplying credit at a zero profit.

Further, it can be shown that this equilibrium will entail a suboptimal (excessive) amount of lending and default.