The Difficulties of Financial Inclusion via Large Banks: Evidence from Mexico*

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Abstract

We provide evidence on why large financial institutions in developing economies have had difficulties expanding formal sector credit. Using detailed credit data and a product that accounted for 15% of all first-time formal loans, we show that borrowers new to formal credit default at high rates and generate ex-ante unpredictable revenue. Using a large country-wide experiment, we show that ex-post contract terms do little to mitigate risk, implying moral hazard is not a primary cause of default. Failing to make its flagship financial inclusion product profitable, the bank eventually discontinued it.

Keywords: Financial Inclusion, Credit cards, Default risk, Mexico.
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1 Introduction

There is a growing body of work linking financial development to improved economic outcomes and some evidence that this relationship is causal.[1] At the same time, a substantial fraction of the world's poor lack access to financial services, including formal credit.[2] Yet, the barriers to increasing credit access through large financial institutions and the experiences of such institutions with lending to poor populations remain under-studied. While micro-finance lenders have received considerable attention, much less is known about the experiences of large banks whose scale, capital and technology suggest they could play an important role in the universalization of financial access. For instance, there were approximately 2.3 million micro-finance clients in Mexico in 2009, while the single credit product we study, targeted at borrowers with non-existent or limited credit histories, taken alone had 1.3 million customers at the time.[3] While many theoretical arguments have been advanced to explain why large financial institutions find it difficult to lend to the poor (e.g. asymmetric information), there is little direct empirical evidence on the issue.

We use data from Mexico to detail some of the challenges faced by formal financial institutions in lending to poor borrowers with limited credit histories (we will often use the abbreviation NTB borrowers to denote such new to banking borrowers). Broadly speaking, we ask whether large banks, using traditional lending practices – individual liability, exclusive reliance on credit scoring and credit bureaus for borrower monitoring, and limited in-person interactions – can profitably lend to NTB borrowers. Combining a range of experimental interventions with detailed observational data we conclude that the answer is likely in the negative. We focus on credit card debt – the most common formal borrowing instrument in the country – from a specific card (henceforth the study card) targeted at NTB borrowers issued by a large, successful commercial bank (Bank A from now on). In 2010, the study card accounted for approximately 15% of all first-time formal sector loan products nationwide (Figure 1(b)).

We motivate our experimental results by establishing two facts about lending to NTB borrowers by large commercial banks. First, borrowers have high default rates – about 19% of our Bank A sample defaulted on the study card over the 26 month study period. Newer borrowers are also riskier: default is twice as high for borrowers who had been with Bank A for less than a year relative to borrowers who had been with the bank for more than two years (both measured at the start of the study period). We also document high default rates for NTB borrowers in a nation-wide

[1] For instance, Beck et al. (2007) show that about a third of the variation in poverty reduction rates across countries can be explained by variation in levels of financial development. Burgess and Pande (2005) and Bruhn and Love (2014) provide evidence that this relationship is causal (for India and Mexico respectively).

[2] Banerjee and Duflo (2010) report that only 6% of the funds borrowed by the poor (in a survey across 13 countries) come from formal sources. The World Bank estimates that 60 percent of adults in developing countries do not use *any* formal financial services and has called for Universal Financial Access by 2020 (see e.g. Demirgüç-Kunt and Klapper, 2012; World Bank, 2017). In Mexico (the country that we study), a 2011 presidential decree established the National Council for Financial Inclusion to expand financial access to underserved populations. INEGI (2015) reports that by 2015 only 57 percent all Mexican adults either had (43%) or have had (14%) an account at a financial institution and only 43 percent either had (29%) or have had (14%) a formal sector loan of any kind.

[3] The estimate of the total number of micro-finance clients comes from Pedroza (2010) and the estimates for card clients are from authors' calculations using bank data.

credit bureau sample, evidence that these default rates are a general feature of lending to NTB borrowers. Second, we use detailed customer level purchases and payments data to construct a measure of bank revenue and show that it is low and variable for newer borrowers relative to older borrowers and that revenue is difficult to predict even when using a large set of observables and a range of machine learning methods. We conclude that lending to NTB borrowers is risky and much of this risk is hard to predict at the time the card is issued. The role of ex-post (i.e. after the card is issued) contract terms in mitigating risk then assumes even greater importance.

We then ask whether default can be reduced by altering two key elements of the credit card contract – interest rates and minimum payments.[4] We estimate causal effects using a large-scale nation-wide randomized experiment carried out by Bank A, covering all 32 states (and 1360 out of 2348 municipalities). The experiment randomly selected and allocated 162,000 pre-existing study card borrowers to 8 treatment arms that varied annual interest rates (between 15%, 25%, 35% and 45%) and monthly minimum payments (between 5% and 10%) for 26 months (from March 2007 to May 2009). To our knowledge, this is the first paper examining experimental variation in both the minimum payment and interest rate in credit card contracts. Furthermore, the magnitude of experimental variation as well as the sample sizes are substantial. In addition, the sampling scheme ensures that the experimental results are representative of the bank's national population of study card customers (about 1.3 million at the start of the study).

We report five main experimental results. First, reducing the interest rate from 45% to 15% reduces default by 2.6 percentage points (on a base rate of 19 percent) over the 26 month experiment. The implied elasticity is a relatively small $+0.20$.[5] This is surprising. A positive correlation between default and interest rates (conditional on selection) is often interpreted as a measure of moral hazard. This correlation arises from both a debt or repayment burden effect (higher interest rates mechanically increase the debt burden) as well as a "pure" moral hazard or incentive effect (higher interest rates increase incentives for default independent of the debt burden effect). That the combined effect is small implies that both effects are small. Second, taking advantage of the stratified experimental design we find that default elasticities are increasing in tenure with the bank so that interest rate changes are least effective for those borrowers for whom the asymmetric information problem is likely the most acute.

Some policy-makers, worried that low minimum payments could lead to excessive borrowing and increased default with negative consequences for both borrowers and the financial system, have advocated raising minimum payments.[6] Higher minimum payments, however, could have

[4]Note that all borrowers had selected into the study card under the status quo contract terms that had a minimum payment of 4% and an annual interest rate of approximately 55%.

[5]This contrasts with other work (e.g. Adams et al., 2009) who find that interest rates are an important determinant of default for U.S. auto loans. Our default responsiveness is considerably smaller than the effects on delinquency rates documented in Karlan and Zinman (2017) although the authors do not report effects on default. It is also smaller than the elasticity implied by the Karlan and Zinman (2009) interest rate interventions in South Africa. See Table OA-22 for a comparison table.

[6]See e.g. Warren (2007); Bar-Gill (2003). In Mexico the Central Bank mandated a floor for minimum payments in 2010. In the United States policy makers have evaluated this possibility given that many minimum payments do not cover the finance charges (`https://goo.gl/X8ujTi`). Such prescriptions find some support in models of time-

two opposing effects and it is not *apriori* clear which one will dominate. On the one hand, ceteris paribus, higher minimum payments reduce debt, easing the repayment burden, thereby reducing default. On the other hand, higher minimum payments tighten short run liquidity constraints, which increases default. The latter may be particularly relevant as, at the start of the experiment, 73% of card holders' monthly payments were less than 10 percent (of the amount due). Our third finding is that doubling the minimum payment (from 5 to 10 percent of the amount due) had no effect on default over the 26 month study period.[7] This provides sobering evidence about the effectiveness of limiting default through increased minimum payments.

We attempt to disentangle the two countervailing effect of the increased minimum payments by examining default in June 2012, three years after the experiment ended. After the experiment ended, the second effect (tightened liquidity constraints) was no longer operational as all subjects were returned to a common minimum payment (4%). However, debt is lower at the end of the experiment in the higher minimum payment arms so the repayment burden is correspondingly lower. Our fourth finding is that the effect of the higher minimum payments on default was both substantively and statistically significantly stronger in June 2012 than at the end of the experiment (May 2009). During the experiment the two countervailing effects cancelled each other out, highlighting the double-edged nature of increasing minimum payments as a policy tool to limit default.

The large variation in contract terms could also have affected borrower behavior with other lenders. For instance, higher minimum payments could have led to borrowers substituting away towards other credit. We match our study sample to credit bureau data to answer this question. Our fifth finding is that the interventions had no effect on default across all other formal lenders. In addition, we find no evidence of crowd-out (or crowd-in) of borrowing, either along the extensive or intensive margin, from other lenders. This is true both during the experiment and three years after it ended.[8] We argue that this lack of response is consistent with continuing credit constraints.

Finally, we explore possible reasons for the limited default response to the changes in contract terms. Default has significant negative consequences – we find that defaulting on the study card is associated with a 80% reduction in the likelihood of a formal sector loan in the subsequent four years. Default then presumably forces borrowers towards informal credit. Using a nationally representative household survey we document that informal credit terms are significantly worse than the corresponding formal sector terms. Thus being shut out of formal credit and unattractive informal sector terms may limit moral hazard responses to interest rate changes in our context. In light of this argument, we then attempt to rationalize the high baseline default rates in our sample. We conjecture that NTB borrowers may be vulnerable to frequent, large shocks that precipitate default,

inconsistent or unaware agents (Heidhues and Kőszegi, 2010; Heidhues and Kőszegi, 2016; DellaVigna and Malmendier, 2004; Gabaix and Laibson, 2006). There is some evidence that time inconsistent preferences play a role in credit card debt accumulation (Meier and Sprenger, 2010; Laibson et al., 2003; Shui and Ausubel, 2005) and that minimum payments serve as an anchoring device (Stewart, 2009).

[7]The point estimate is a statistically insignificant reduction of half a percentage point (the implied elasticity is $+0.02$).

[8]Our results are then consistent with those documented in Karlan and Zinman (2017) and Angelucci et al. (2015) for the microlender Compartamos Banco (in Mexico).

though we are limited in our ability to test this convincingly. Using individual level social-security (IMSS) panel data linked to credit bureau data we estimate that unemployment is associated with a five percentage point increase in default a year later.

We draw two broad lessons from these results. First, it is difficult to expand credit to NTB borrowers via methods traditionally used by large financial organizations. Bank A stopped issuing the study card entirely by 2010, providing some revealed preference evidence of the difficulty of lending to poorer clients.[9] Second, ex-post contract terms do little to mitigate risk, suggesting moral hazard is not a primary cause of default. We speculate that for our population other factors (e.g. employment shocks) may be more important drivers of default. This suggests a greater emphasis on insurance relative to credit in the expansion of financial access.

This paper connects with several strands in the literature on credit markets. First, a recent literature identifies lack of access to formal financial services as a general problem in developing countries and advocates supply-side interventions aimed at increasing financial inclusion – that is the creation of broad-based access to financial services, particularly for poor and disadvantaged populations.[10] We provide a detailed empirical analysis of the difficulties involved in expanding access to credit through large commercial banks as well as causal evidence on the effectiveness of standard policy tools to mitigate credit risk. Our work also adds to an earlier literature that critiques institutional, typically state-led and agricultural, lending to the poor.[11] In this literature, limited formal private sector engagement with poor borrowers is taken as *prima facie* evidence of the inability of banks to do so profitably – our study provides detailed evidence on a private sector bank's attempt to lend to the poor, albeit in a different context.

We also contribute to the empirical literature documenting the existence and gravity of informational problems in credit markets.[12] We complement this literature (e.g. Karlan and Zinman, 2009) by including borrowers who are new to formal credit – potentially important since asymmetric information problems are more severe for this sub-population. In addition, we examine the relative importance of liquidity constraints, repayment burden and "pure" moral hazard over a long (five year) horizon. Relative to earlier work, the large, representative and stratified nature of the experiment allows us to examine responses for a range of borrowers with widely varying credit

[9]Other than microlenders, Banco Azteca (owned by Grupo Elektra, Latin America's largest retail company) is often cited as a success story in lending to under-served populations. Consistent with our findings of the limitations of traditional lending technology Ruiz (2013) attributes its success to its ability "to leverage its relationship with a large retail chain (Elektra) to reduce transaction costs, acquire effective information and enforce loan repayment." Banco Azteca also requires collateral, which invariably was in the form of household appliances (in case of default the appliance is usually resold by Elektra). In addition, the bank used screening techniques (such as home visits) and loan repayment techniques (weekly home visits from loan agents to delinquent clients) that are not generally used by large commercial banks. Ruiz writes that the low default rate at Azteca could plausibly be related to its "crude collection and repossession mechanisms."

[10]This literature has largely been descriptive, documenting, for instance, the large numbers of people world-wide who do not use formal banking services. See e.g. Demirgüç-Kunt and Klapper (2012), though Dabla-Norris et al. (2015) is a notable exception. See also Dupas et al. (2018) who provide experimental evidence (from a multi-country trial) that a focus on expanding access to bank accounts by itself may only have limited welfare impacts.

[11]See e.g Adams et al. (1984). Aleem (1990) provides detailed estimates of the substantial screening and operational costs incurred by informal lenders in such environments. In our context, Bank A has relatively limited information about borrowers. See also Ruiz (2013) who examines the expansion of Banco Azteca in Mexico.

[12]See e.g. Ausubel (1991), Edelberg (2004), Karlan and Zinman (2009), Adams et al. (2009), Einav et al. (2012).

histories. Finally, since we observe the totality of the experimental sample's formal sector credit records, we can also measure spillovers and crowding out/in of other formal lenders in response to the experiment.

The paper proceeds as follows: Section 2 outlines the various data sets we use and provides basic summary statistics. Section 3 provides relevant institutional context and establishes four facts about financial inclusion and credit in Mexico using a large representative sample of borrowers from the formal credit market and our sample from Bank A. Section 4 describes the experiment while Section 5 reports the effects of the experiment on default, cancellations and bank revenues, the primary outcomes of interest. Section 6 discusses some of the mechanisms driving the treatment effects documented in Section 5 and Section 8 concludes. Due to space constraints some robustness analyses and secondary figures and tables are reported in the Online Appendices (OA).

2 Data, Study Card and Descriptive Statistics

We use six different data sets. First, we use a large representative sample of one million consumers from the Mexican Credit Bureau from 2010 that allows us to make population level statements and comparisons. The second data set is a monthly individual level administrative panel for a sample of 162,000 cardholders from Bank A. The third data set is also from the Credit Bureau and is an annual panel matched to the sample of 162,000 study card holders. The fourth data-set is the Mexican Social Security data (IMSS) also matched to the bank and credit bureau data. The last two data sets are nationally representative surveys (ENIGH, MxFLS). We next describe each in turn.

Credit Bureau Data (Representative Cross-Section): We use a random sample of one million borrowers from the Mexican Credit Bureau (Buró de Crédito) both in 2010 and 2012 to describe the population of NTB borrowers in the country. A borrower appears in the credit bureau if she has or has had a loan with a formal financial intermediary.[13] For each borrower we observe the date of loan initiation, the source and type of loan and her delinquency and default history.[14] We also observe a limited set of demographics – age, gender, marital status and zip code. We use this information to provide a snapshot of financial inclusion – in particular we describe the characteristics of first-time and recent borrowers, their sources of credit and their repayment history. We will refer to this as the CB data.

Study Card: We use detailed data from a large commercial Mexican bank and a product (the study card) that accounted for 15% of first-time loans nation-wide in 2010. The study card is a credit card that can be used at a large set of supermarkets as well as other stores. Using the

[13]The Credit Bureau is required to maintain all records provided by reporting agencies for a fixed period of time. As of September 2004 the Credit Bureau received information from 1,021 data suppliers including banks, credit unions, non-bank leasing companies, telecommunications companies, some MFIs, retailers (e.g. department stores), SOFOLES – limited purpose financial entities specializing in consumer credit, e.g. for auto loans and mortgages – and other commercial firms (World Bank, 2005).

[14]We only have limited information on total loan amounts and no information on the interest rate and other contract terms. In addition, we do not observe credit scores.

household level data from Atkin et al. (2018) from 2011, the study card could be used at stores that accounted for 43% of all household expenditures at supermarkets and 16% of all household expenditures.[15] The card was specifically targeted at low-income borrowers with no or limited credit histories.[16] It had an initial credit limit of approximately 7,000 pesos, an annual interest rate of 55 basis points over the base rate and a monthly minimum payment of 4% of the total amount outstanding. By 2009, Bank A had approximately 1.3 million clients with this card.

Bank Data (Experimental Sample): The sample consisted of a stratified random sample of study card holders. Card holders were chosen subject to the additional constraint that they had paid at least the minimum amount due in each of the six months prior to (and including) January 2007. This left the bank with a sampling frame of about one million clients from which the study sample was drawn. The sampling frame was partitioned into nine strata based on tenure with the bank and payment behavior (each taking on three values), which the bank uses internally as predictors of default (see Section 4.1 for more details). The bank then randomly selected a sample of 18,000 clients per stratum. We use stratum weights (see Table OA-13) in all of our analysis to ensure our results are representative of the sampling frame. Within each stratum, clients were randomly assigned to one of nine study arms so there are 2,000 clients per treatment arm within a stratum. In what follows we will often restrict attention to the 8 primary study arms which gives us a total sample of 144,000 clients across the 9 strata. As noted earlier, the resulting sample is geographically widespread – covering all 31 states and the Federal District, 1,360 municipalities and 12,233 zip codes. We examine the external validity of the sample for the national population of NTB borrowers below in Table 1.

The experimental data: Bank A ran a large country-wide experiment in an attempt to better understand and mitigate default on the study card, the bank's flagship financial inclusion product. The experiment lasted from April 2007 to May 2009, and for this entire period we have monthly data on purchases, payments, debt, credit limits and default. Default is defined as three consecutive monthly payments that are less than the minimum payment. In these cases, the bank would revoke the card automatically. Therefore we will refer to default and revocation interchangeably. In addition to this detailed transaction information we also observe some basic demographic variables – age, gender, marital status and zip code of residence. Finally, we also observe default status for the study sample in June 2012, three years after the end of the experiment.

Matched Credit Bureau Data Panel: We were able to match the experimental sample to the credit bureau data once each year (from June 2007 to June 2010) and once more in June 2012. This enables us to observe other formal sector transactions by the experimental sample thereby allowing us to measure effects on non-Bank A related outcomes (e.g. overall debt or overall default). We will refer to this data as the *matched* CB data.

[15]We thank Marco Gonzalez-Navarro for kindly carrying out the calculations.

[16]Internally the bank referred to them as the C, C- and D customer segments.

Matched Social Security Data Panel: We were also able to merge our sample with the government's social security agency's records (IMSS) from November 2011 to May 2014 to obtain information on occupation and income for the 18% of the sample that worked in the formal sector and was hence covered by the IMSS.

Survey Data (ENIGH, MxFLS): We also draw upon two national surveys to supplement the data above. We use Mexico's income-expenditure survey (ENIGH 2004, 2012) to measure credit card penetration in the country and the Mexican Family Life Survey (2005 and 2008) to measure loan terms for both formal and informal loans.

2.1 Summary Statistics

The experimental sample is of broad interest since the study card accounted for 15% of all first-time formal sector loan products nation-wide. We provide summary statistics for this sample and compare it to selected sub-samples from the CB data (columns 3-5). In Column 3 we use the CB sub-sample that had at at least one active credit card in June 2010, making it a nationally representative sample of the population of borrowers with at least one credit card (in 2010). Since our experimental sample is relatively new to formal credit, we next attempt to find a comparable group in the CB data by constructing, albeit crudely, a sample whose credit history duration matches that of the experimental sample. We do this by matching the distribution of the oldest credit entry across the experimental and CB samples. This is the sub-sample for which summary statistics are reported in column 4 and we refer to it as the new (or recent) borrower sample.[17] Finally, in Column 5 we consider a sub-sample of experienced borrowers – those with a credit history of at least 8 years (the median) in the CB data.

The experimental sample is just over half male, with an average age of approximately forty, about three-fifths of whom were married at the start of the study (Panel C). Other than marriage rates (which are lower in the CB) the figures are roughly comparable to those of the three CB data sub-samples. Borrowers in the experimental sample are somewhat less well-off relative to the average CB member. For the borrowers that we could match to the IMSS, average monthly income (in 2011) in the experimental sample is 13,855 pesos compared to an average of 14,759 for recent borrowers and 22,641 for experienced borrowers.[18] Figure OA-7 shows that the distribution function for income for the experimental sample is first-order stochastically dominated by corresponding distribution for the CB sub-sample from Column 3. Since we were unable to match 82% of the experimental sample with the IMSS, we concluded that these individuals were in the informal sector with likely lower, less stable incomes.

Credit information also points towards our experimental sample being "marginal". First, the

[17]The details of the matching procedure can be found in the Online Appendix Subsection A.1.

[18]For comparison, average monthly per capita income in Mexico in 2007 was 4,984 pesos. The 25th and 75th percentiles of income for our experimental sample are 2,860 and 19,535 pesos respectively, while they are 2,580 and 6,000 pesos for the country as a whole. Our income numbers are not adjusted for family size or for other earners in the card-owner's family. These numbers are conditional on working in the formal sector. We could match 18% of our experimental sample and about 13% of the CB data to the IMSS. Well over half of Mexico's labor force is in the informal sector so is not captured in the IMSS.

mean credit score of 645 is low in absolute terms – borrowers with scores below 670 are typically ineligible for standard credit card products.[19] Second, the card issued by Bank A was the first loan product for 47 percent of the sample. Third, our study sample was, unsurprisingly, at the low-end of borrowing ability in the CB data. The credit limit for the study card was relatively low at 7,879 pesos and the overall card limit for the experimental sample (across all cards) was $15,776$ pesos in 2007, rising to $18,475$ pesos by June 2010. For comparison, in 2010 the mean card limit was $49,604$ pesos for the CB sub-sample with at least one active card, $22,082$ pesos for the CB recent borrowers sub-sample and $56,187$ pesos for the experienced sub-sample. Fourth, borrowers have high rates of default; 17% of the experimental sample as a whole defaulted on the study card over the course of the experiment (the figure was 19% for the sample we will use as our base comparison group). Based on the figures presented above, we conclude that the experimental sample was indeed drawn from a financially fragile population.

2.2 Bank Revenues per Card

Measures of bank revenues (and profits) from the study card are critical for understanding the long term viability of financial inclusion through large commercial banks. We quantify bank revenue from the study card using the detailed data on purchases, payments and debt over the 26 month experiment (under assumptions explicated below).[20] We define revenue for card i as

$$\text{Rev}_i = \text{PV(Pay - Buy)}_i - \text{Debt}_{03/07,i} + \alpha_i \text{PV}(\text{Debt}_{05/09,i}) \tag{1}$$

where PV($\cdot$) stands for the present value of the stream of payments inside parentheses that are discounted at the TIIE (the Mexican inter-bank rate). If we observed a card from inception until closure, the exercise above would reduce to subtracting the net present value of payments from the net present value of purchases. Unfortunately, we only observe cards for a 26 month window so have to account for card usage before and after the experiment. We account for pre-study behavior by subtracting the amount due from card i at the start of the experiment (March 2007). We assume that post-study, borrowers make no further purchases and default on their outstanding debt with probability ϕ_i in which case the bank recovers 10% of the amount due.[21]

Several features are worth noting. First, this measure of revenue accounts, albeit mechanically, for both default and cancellations. By the same reasoning it incorporates interest and fees.[22] Sec-

[19] See Drenik et al. (2018). Unfortunately we cannot compare scores to the other CB sub-samples (Cols 3-5) since credit scores were not provided in these CB cross-sections.

[20] The exercise is analogous to the quantification performed in Adams et al. (2009) though the on-going borrowing on the card and data censoring (outside of the study period of 26 months) are important differences. Measuring bank profits or revenues is relatively uncommon (Agarwal et al., 2015; Adams et al., 2009, are notable exceptions.) for lack of data.

[21] For each card i, ϕ_i is modeled as a function of its credit score using a non-parametric regression of default (during the 26 month window 03/07-05/09) against the credit score in June 2007 for the control group. We then assigned ϕ_i based on the estimated regression evaluated at the credit score for i in June 2009 (see figure OA-11). The 10% figure for debt collections is based on conversations with bank officials. The expected fraction (of the amount due) that would be recovered then is given by $\alpha_i \equiv \phi_i \times 0.1 + (1 - \phi_i) \times 1$. Figure OA-12 in the OA shows that our measure of revenue is not particularly sensitive to the choice of α_i.

[22] In fact because of the identity $Debt_t = Debt_{t-1} + Buy_t - Pay_t + (i/12)Debt_t + Fees_t$, an alternative representation

ond, it is not a comprehensive measure of profit since it does not include promotion costs, the cost of the physical card and maintenance or administrative expenses or any income earned by merchant discount fees or interchange fees. Nevertheless, in our estimation, it provides a useful measure of bank revenue.

Figures 2(a) and (b) plot histograms of our revenue measure. The measure shows considerable dispersion — the standard deviation in panel (c) (7,347 pesos) is considerably larger than the mean (4,197 pesos) and newer borrowers exhibit even greater dispersion. To assess its reasonableness, we examine correlations with credit scores. Revenue displays an inverted-U pattern with respect to initial credit scores. Figures 2(c) presents a kernel regression of revenue on 2007 credit scores at the borrower level for the control group, while 2(d) carries out the same exercise but for the stratum with the shortest tenure with the bank. In private conversations, Bank A officials confirmed that average revenue, its dispersion, and its relation to credit scores are reasonable. Strikingly, clients with low and high credit scores yield low revenues relative to clients with middling scores. Low score clients are more likely to default, thus yielding low revenue. On the other hand, high credit score clients generate little revenue because they accrue lower interest charges and fees (e.g. by paying off the amount outstanding each month). This inverted-U shaped relationship between bank revenues has been documented in other markets which gives us further confidence in our construct.[23]

3 Financial Inclusion with Credit in Mexico

3.1 Traditional Banks Expand Credit to Underserved Populations

Formal credit penetration is low in Mexico,[24] but has been growing, primarily by large banks using credit cards to reach previously under-served populations. Figure 1(b) shows that more than 70 percent of first time loans are through a credit card. The number of credit cards nationwide grew from 10 million in the first quarter of 2004 to 24.6 million in the last quarter of 2011 with a substantial part of the growth being concentrated among lower income individuals (see Figure 1(a)).[25] Our study card played an important role in this growth (see Figure 1(c)).

This pursuit of low-income clients by large banks appears to have been, in part, inspired by the success of Banco Compartamos and Banco Azteca.[26] However, both Compartamos and Azteca pursue markedly different strategies than those used by Bank A. Compartamos uses joint liability

of equation (1) is $\sum_{t=1}^{T}(1+r)^{-t}\left[(i/12)Debt_t + Fees_t\right]$. We have information on late payment fees and overdraft fees, but do not directly observe merchant discount fees. The merchant discount fee is charged by the acquiring bank (i.e. the merchant's bank) to the merchant and is 1.7% of purchases in our case.

[23] Fig. II.E in Agarwal et al. (2015) plots a inverted-U relationship using credit card data for the United States.

[24] The ratio of private credit to GDP was 23 percent in 2010, while in the same year the figure was 52, 98, 43 and 40 percent for Brazil, Chile, Colombia, and Latin America and the Caribbean respectively. The percentage of adults with at least one credit card was 17 percent (in 2014) compared to about 70% – 80% for the US. See US: https://goo.gl/bVWnaS and https://goo.gl/UG6pgn. For Mexico, see the "Reporte de Inclusion Financiera" (2016) (https://goo.gl/kYy4ae), Graph 1.12.

[25] See e.g. Banco de México (2016).

[26] See e.g. https://goo.gl/7HufqG; https://goo.gl/vi2EYK; https://goo.gl/sjgoAn.

(via group lending) while Azteca requires collateral (typically household durables). In addition, both lenders expend considerable resources on face-to-face interactions and home visits. This approach is costly – both Compartamos and Azteca have higher operating expenses (relative to assets, see Fig OA-27) than Bank A – potentially limits the scale of inclusion and is quite foreign to large banks. An important question therefore is: can the lending model followed by large banks – non-collateralized credit, individual liability, limited debt collection efforts,[27] credit bureau scoring, and limited in-person interactions – work in lending to NTB borrowers? In this section we provide detailed evidence on why the answer may be no by highlighting some of the difficulties faced by Bank A's study card operation.

3.2 Stylized Facts

We document four facts about traditional large banks' experience of lending to NTB borrowers: (a) NTB borrowers default at high rates and default decreases with formal credit sector tenure, (b) revenue per borrower is low, variable, and (c) difficult to predict. Finally, we find suggestive evidence for a first-lender externality, whereby better NTB borrowers are more likely to cancel their cards, and quantify its impact on bank revenues.

A. High Default Rates

During the 26 month study approximately 19 percent of the control group defaulted on their card (compared to an average cumulative 26-month default rate of 12 percent in the credit bureau). NTB borrowers for the study card are thus quite risky, even though the sample is presumably positively selected since (a) it comprises successful applicants approved by Bank A using best-practice methods for large banks and (b) all borrowers had made at least the minimum payment in the six months prior to January 2007.

Default risk, however, is not homogeneous. Figure 3(a) shows that newer borrowers are riskier: default rates for borrowers who had been with the bank for less than a year (the "6-11m" stratum) when the experiment began are 36 percent during the experiment, whereas the corresponding figure for the oldest borrowers (those who had been with the bank for more than 2 years when the experiment began) are half that rate at 18 percent.[28] The finding that newer borrowers are riskier also holds for a representative sample of NTB borrowrers in the credit bureau – mimicking the experiment, Figure 3(b) examines all borrowers in the CB who had an active credit card as of January 2007, for whom this was the first card and who had not been delinquent in the previous 6 months. We plot default in the next 26 months for this group (through May 2009) as a function of card tenure. The study card has much higher default rates than other cards (even those with Bank A) consistent with it being a "financial inclusion" product aimed at NTB borrowers. Bank A overall (across all cards) has default rates similar to those at other banks, suggesting our partner

[27] Mexican banks typically do not pursue default amounts less that 50,000 pesos.

[28] These differing rates may be driven by at least three forces: positive selection, as riskier borrowers leave the market; experience, as borrowers learn how to manage their cards; and lower moral hazard, as non-defaulting borrowers exert more effort to avoid default to protect their reputation. We do not attempt to separate these.

bank is at least as sophisticated as the others in the market. Finally, the negative slope shows that, across all banks, newer borrowers are more likely to default – for instance, borrowers with a card tenure of less than 3 months are twice as risky as borrowers with a card tenure of 3 years.

B. Low and Highly Variable Revenue for Newer Borrowers

In spite of higher default rates, newer borrowers could in principle be more profitable (e.g. if they incur higher fees and/or take on more debt). This is not the case; Figure 3(a) shows that revenues were about 50 percent higher for older borrowers than for those in the newest borrower stratum. In addition, the standard deviation of revenue is 7,198 for those in the 24+ months strata, and 8,738 in the 6-11 month strata. In other words, newer borrowers are a less attractive business proposition than older borrowers.

C. Revenue is Difficult to Predict

Large banks' lending procedures rely heavily on credit scoring and predictive modelling. In other contexts such methods have led to significant credit expansions.[29] It is unclear whether such an approach is suitable for NTB borrowers who by definition have little or no credit information to estimate such models. The performance of such methods with sparse information is ultimately, however, an empirical question.

We find that predicting revenue is extremely difficult in our NTB sample (see Table OA-9 and Section B.3) using CB information available at the time of application and modern machine-learning predictive methods.[30] We find an out-of-sample correlation (ρ) between predicted and realized revenue of 0.04 for OLS models, and 0.28 for the best machine learning model (random forests), while the root mean square error was 7,200 pesos (much higher than the 4,197 peso mean revenue). Using CB information at the time the experiment began does not increase predictive power, although adding information on ex-post behavior with the study card does increase the out-of-sample correlation to 0.41. Part of the difficulty in predicting revenue stems from the difficulty of predicting default (ρ=0.45) and voluntary cancellations by clients (ρ=0.15) and predicting which cards will pay interest (ρ=0.44).

D. The First Lender Externality

A new borrower's repayment behavior on their first loan product generates valuable information about the borrower's creditworthiness. To the extent that this information is public – via the credit bureau – there is an externality since other potential lenders can now condition their lending

[29] See e.g. Einav et al. (2013).

[30] We note that our sample consists of successful applications that are, presumably, positively selected for the outcomes examined. The high prevalence of adverse outcomes (e.g default) even for such a population is indicative of the magnitude of the bank's selection problem.

on this information. Previous work has recognized the public good nature of this initial interaction in theory but there is little empirical evidence on its existence or magnitude.[31] We provide some evidence for this externality and attempt to quantify its importance (within Bank A, this phenomenon was widely recognized and referred to as "poaching").

An implication of the externality argument is that new borrowers with larger improvements in their credit scores should be more likely to obtain cards from other banks. We examine this possibility in Figure 4 and find exactly this pattern. To focus attention on new borrowers we restrict our sample to borrowers for whom the study card was the first card and who had been with the bank for less than a year as of January 2007. These are precisely the set of clients for whom the generated credit history should be most critical, as they did not have a credit score prior to obtaining the study card.

First, in Figure 4(a) we plot a non-parametric regression of voluntary client cancellation between June 2008 and May 2009 against the change in credit scores in the preceding twelve months. Increases in credit scores are associated with higher borrower initiated cancellations. Virtually none of the borrowers that experience a decrease of 100 points in the credit score cancel, whereas more than 8 percent of those that experience a 50 point increase cancel the study card. Second, in Figure 4(b) we plot non-parametric regressions of new card acquisitions between June 2008 and May 2009 against the same regressor as in Figure 4(a). We see that increases in credit scores are associated with increased card acquisition by NTB borrowers – 22% of borrowers with a 50 point increase in credit scores acquire at least one more card in the following year. The results support the idea that subsequent lenders use histories generated by the first lender to screen borrowers.[32]

A natural question then is how much revenue the first lender loses when a borrower is "poached." To assess this we need a counterfactual – the earnings foregone by Bank A when a borrower is poached. Note that a poached borrower need not cancel the study card but merely open another card with another lender. This will weakly reduce the first bank's revenues (as long as the second card substitutes for the first for some purchases) and may also increase the likelihood of default.[33] To simplify the calculation, however, we focus on borrowers who cancel their initial card when they leave Bank A for another lender ("switchers").

We construct the counterfactual in a transparent way by matching switchers to non-switchers. In the interest of space, we relegate details to the notes in Table OA-11 which also displays the different measures of revenue lost and placebos based on alternative assumptions. Our preferred estimates (Row 3, Column 1 in Table OA-11) suggest that the average revenue foregone by the bank for each switcher is 4,324 pesos, approximately the same as our revenue measure per card, a

[31] For instance, Stiglitz (1993) writes "The observation that another lender is willing to supply funds $\cdots$ confers an externality, the benefit of which is not taken into account when the first lender undertakes his or her lending activity." Petersen and Rajan (1995) conjecture that this problem is aggravated in more competitive markets, and indeed find that newer firms (in the U.S.) in concentrated markets receive *more* financing than do similar firms in more competitive markets. In their survey piece, Banerjee and Duflo (2010) also note this problem and point out that the externality is particularly acute in a pure adverse selection model.

[32] Interestingly, we note that good borrowers are also more likely to receive additional cards from Bank A itself (perhaps partly as a retention strategy).

[33] See e.g. Drenik et al. (2018).

substantial revenue loss.[34] This further attenuates the bank's revenue gains from NTB borrowers, reducing its incentives to pursue financial inclusion.

4 Using Contract Terms to Change Behavior

The previous section documented high rates of card exit and variable revenues as well as the difficulty of predicting these outcomes. This limited ability to screen borrowers ex-ante increases the importance of ex-post measures such as contract term adjustments – the most important being the interest rate on debt, the credit limit and the minimum payment required – in limiting default and maximizing profits. For instance interest rate reductions may attenuate moral hazard. Similarly, minimum payment increases may limit indebtedness (as argued by policy makers) and subsequent default.

Whether and to what extent such variation in contract terms can mitigate default and its implications for bank profits is an open empirical question. We were fortunate to observe a large-scale experiment conducted by Bank A that induced large experimental variation in interest rates and minimum payments (the bank did not experimentally vary credit limits).[35,36] We use this experiment to transparently answer the question of the extent to which contract terms mitigate default for NTB borrowers. In addition, we use our revenue measure to discuss the effects of the contract term variations on bank revenue.

4.1 Experiment Description

4.1.1 Sample Selection

As outlined in Section 2, the sample frame consisted of all study card borrowers who had paid at least the minimum amount due in each of the six months prior to January 2007. The bank divided this sample frame of more than one million study card clients into nine different strata based on two pre-intervention characteristics which were used internally as default predictors: the length of tenure with the bank, and repayment history over the past 12 months (both measured

[34]One may ask why Bank A – which presumably has more information about cancellers than other lenders – is unable to retain what appear (from the above calculation) to be highly profitable clients. Bank A could potentially limit departures by improving terms (e.g. lowering interest rates) for profitable potential switchers. There are, however, at least two limitations of such an approach. First, predicting cancellation may be a difficult exercise. Table OA-10 in the appendix predicts voluntary cancellations using a battery of machine learning methods and finds AUCs in the 0.6 – 0.7 range. This may help explain why researchers (see e.g. Ponce et al., 2017; Ioannidou and Ongena, 2010) have documented relatively limited price discrimination in credit cards and loans (in Mexico and Bolivia respectively). Given this, the bank faces a trade off between extracting rents from borrowers today at the risk of increasing the likelihood of their subsequent departure (This trade off is modeled explicitly in (Taylor, 2003)). Second, after cancellation and obtaining a new card, it is not clear that the bank would wish to tempt the former client back since establishment of the second card could change Bank A's profitability and risk calculations.

[35]We found out ex-post about the existence the experiment and were surprised by its size and by the magnitude of the changes in interest rates and minimum payments. The experiment was designed by the bank's statisticians, and in conversations with bank officials it appears that the experiment was motivated by a discussion between Bank A and the Central Bank about the causes of high card default rates.

[36]Aydin (2018) finds that experimental changes in credit limits have no effect on card default (at least over a nine month horizon).

in January 2007).[37] Each borrower was classified into one of three categories of tenure with the bank: (a) a long term customer who had been with the bank for more than 2 years, (b) a medium term customer who had been with the bank for more than one but less than two years, and (c) a new customer who had been with the bank for more than six months but less than a year. Each borrower was also classified into one of three categories based on her repayment behavior over the past 12 months: (a) a "full payer" who had paid her bill in full in each of the previous 12 months and hence accrued no debt, (b) a "partial payer" whose average payment over the past 12 months was greater than 1.5 times the average of the minimum payments required from her during this time, and (c) a "poor payer" whose average payment over the past 12 months was less than 1.5 times the average of the minimum payments required from her during this time. The product of both categories defined 9 strata, and 18,000 borrowers were randomly selected from each of these strata. We use sampling weights in our analysis to account for unequal stratum sizes and can thus make valid statements about the entire sampling frame.

4.1.2 Experimental Design

Within each stratum, the bank randomly allocated 2,000 members each to each of 8 intervention arms and one control arm. Each treatment arm is a combination of two contract characteristics: (a) a required minimum monthly payment which is expressed as a fraction of amount outstanding (debt) on the card, and (b) the interest rate on the amount outstanding. The minimum payment was set at either 5% or 10%. The interest rate could take on one of four values: $15\%, 25\%, 35\%$ or 45%. The interest rate for the study card prior to the study was approximately about 55% so all the experimental interest rates are reductions relative to the status quo. The two different minimum payments and four different interest rates yield 8 unique contract terms. The experimental design thus identifies for each outcome and for each month 8 treatment effects within each of 9 different strata. In addition 2,000 customers within each stratum also served as a control group whose contract terms did not change during the period of the experiment. The minimum payment for the control arm was 4% but the interest rate varied across clients and, unfortunately, we do not observe this rate in our data. Consequently, we do not use the control group as a contrast in most of the analysis below and are explicit in the sequel about which arm serves as the reference or comparison group. In most cases we use the 5% minimum payment and the 45% interest rate group (abbreviated to $(45, 5)$) as the comparison group and we often refer to it as the base arm or base group. Panel A of Table OA-14 in the Online Appendix tests the randomization procedure and shows that treatment assignment is uncorrelated with baseline observables.[38]

Figure OA-14 shows the time-line of the experiment as well as measurement dates. Each study client was sent a letter in March 2007 stating the new set of contract terms that would be in force starting in April 2007. Clients were not informed about the study or of any time-line for when the new contract terms would change. The measurement of experimental outcomes began in March

[37] For borrowers with less than 12 months the full available history was used for stratification.

[38] Panel B shows that the sample of non-attriters across treatment arms is also balanced along observables at the end of the experiment.

2007 and lasted until May 2009. During this period the interest rate and the minimum payment were kept fixed at their experimentally assigned levels. The experimental terms were not revealed to the risk department (in charge of deciding credit limits).[39] The experiment ended in May 2009 at which point all treatment arm participants received a letter setting out their new contract terms. These terms were the standard conditions with an interest rate of approximately 55% and a minimum payment of 4%.

We take advantage of the stratified randomization scheme to estimate simple regressions of the form:

$$Y_i = \beta_0 + \sum_{j=1}^{7} \beta_j T_{ji} + \delta_s + \epsilon_i. \tag{2}$$

where Y_i the outcome of interest (default, cancellation or bank revenues) for borrower i and $\{T_{ji}\}_{j=1}^{7}$ are a set of treatment indicators – the excluded group is the (45,5) treatment arm. The strata fixed effects, δ_s, are included in a way that allows us to interpret β_0 as the base group mean.[40] We use stratum weights (see Table OA-13) in all regressions.

5 Experimental Effects on Default, Cancellations and Revenues

We estimate the causal effects of interest rate and minimum payment variation on three primary outcomes – default, cancellations and revenue. Defaults are important for policy makers since they can be a source of financial instability and may be particularly worrisome in poor populations. In fact, the experiment described in this paper was driven in part by the Mexican Central Bank's concern over default rates among NTB borrowers. Defaults are clearly important for banks as well since they directly affect profits. In addition, the literature on credit market imperfections has focused on default in testing for moral hazard. We also examine cancellations – when the borrower pays down her debt and cancels the study card. Comparing cancellations provides some evidence on the effect of contract term changes on the relative attractiveness of the study card. Finally, revenues are our measure of the bank's bottom line and the effect of contract term changes on revenues is informative about the commercial feasibility of maintaining the altered contract terms.

5.1 Default

A. Interest Rate Changes Have Small Effects on Default

Default is a binary variable equal to one if borrower i defaults at some point during the 26 month experiment and zero otherwise. Column (1) in Table 2 shows a substantial part (19%) of the base group (i.e. the $(45\%, 5\%)$ arm) defaulted over the course of the experiment. By comparison, the effects of the interventions were quite modest. Reducing the interest rate to a third of the base

[39] We cannot reject the null of no differences in credit limits across treatment arms at baseline and end-line (Table OA-15 and Figure OA-16).

[40] This is equivalent to estimating Eq. (2) with a full set of strata dummies and the additional constraint that $\sum_s \delta_s = 0$. Also, note that treatment assignment was done within each stratum and that the treatment assignment probabilities do not vary across strata.

group rate (i.e. from 45% to 15%) reduced default by approximately two and a half percentage points over 26 months. The implied elasticity of default with respect to the interest rate is $+0.20$. This is considerably lower than the delinquency elasticity of 1.8 in Karlan and Zinman (2017) and also lower than the default elasticity of 0.27 in Karlan and Zinman (2009) (see Table OA-22 for a comparison to other effects documented in the literature).

The treatment effects for the other intermediate treatment arms are also similarly small. The reduction in default in the $(25, 5)$ arm relative to the base arm is essentially the same as for the $(15, 5)$ arm (so that the corresponding elasticity is somewhat higher at $+0.27$) while the treatment effect for the $(35, 5)$ arm is estimated to be zero. The results for the comparisons between the $(45, 10)$ and the $(r, 10)$ arms are even more stark with none of the estimated treatment effects being statistically different from zero (and the implied elasticities are all less than 0.1) – the higher payments attenuated the effect of the interest rate declines essentially to zero.

Interestingly, these effects are smaller for newer borrowers. For borrowers who had been with Bank A for less than one year (as of January 2007) the elasticity is $+0.08$ while the corresponding elasticity for borrowers who had been with the bank for more than two years (as of January 2007) is $+0.30$, nearly four times as large. Thus, borrowers for whom the asymmetric information problem is most acute are precisely those for whom the contract terms variations are least effective in limiting default.

Figure 5 examines the evolution of default over the entire 26 month period by plotting the regression coefficients from estimating equation (2) month-by-month. The default measure at time t is a cumulative measure: i.e. $Y_{it} = 1$ if i has defaulted at any point up to t. Default was essentially zero for six months, and we see only small declines in the last months of the intervention. To summarize, the consistent finding across all experimental contrasts and over all 26 months of the experiment is that the interest rate decreases have negligible short-term and modest long-term negative effects on default.

The large experimental variation in interest rates (from 45% to 15%) permits a clean test for the presence of moral hazard, while the large sample sizes and stratified randomization ensure that tests are well-powered for different sub-populations (e.g the most recent NTB borrowers).[41] Moral hazard comprises a "pure" incentive or strategic (i.e. choice driven) effect and a repayment burden effect, both of which should respond in the same direction to interest rate changes. Viewed in this light, the results above imply that both effects are small since their cumulative effect is small.[42] Since debt decreases over time, the flat time-path of initial default response (for the first six months) is consistent with the repayment burden effect being more important than the strategic effect.

Thus, even though baseline default rates are high, it appears that moral hazard is not an important determinant of default at least for the range and nature of interest rate variations considered here. The treatment and strata variables together explain only about one tenth of one percent of

[41] Recall that all borrowers were pre-existing clients and had selected in under the same terms.

[42] As Einav and Finkelstein (2011) note, however, the magnitude of the correlation test does not necessarily map monotonically into the welfare loss from moral hazard. We do not attempt to quantify welfare losses here.

the variance in default suggesting that default is driven in large part by forces not observed in our data (we examine this issue at greater length in Section 7).

B. Minimum Payment Increases Do Not Change Default

In contrast to the literature on interest rates and default, there is relatively limited empirical work on the relationship between minimum payments and default. However, minimum payments have received substantial attention in policy circles as a regulatory tool to protect consumers from default driven by over-indebtedness.[43] We view minimum payments as having two primary effects: (a) a tightening of liquidity constraints and (b) a reduction in debt over the longer term. These will be a useful lens through which to interpret our findings.

The experiment doubled the minimum payment from 5% to 10%. This was a large and significant change since 73% of borrowers paid less than 10% of the amount due before the experiment began (see figure OA-15). Table 2 shows that, perhaps surprisingly in the light of this figure, the minimum payment increase had no effect on default – the point estimate is a statistically insignificant increase of 0.5 percentage points on a base default rate of 19% for an elasticity of $+0.02$.[44] The effects in the other arms are all broadly comparable, with the estimated elasticities ranging from -0.01 to $+0.08$. As with interest rates, the elasticities are smaller for newer borrowers (a statistically insignificant $+0.03$) relative to older borrowers for whom the corresponding elasticity is substantially larger ($+0.10$) and also statistically significant. This reinforces the point that it is precisely those borrowers for whom the asymmetric information problem is most dire that are most unresponsive to contract term changes. Next, examining the evolution of the treatment response in Figure 5 we see that the minimum payment increase had minimal effects for the first six months, following which the default rate rose slightly and then stayed relatively stable thereafter.

The overall effects are small, particularly relative to the policy attention paid to increasing minimum payments as a means of limiting default. A key, albeit implicit, component of the policy argument is that increasing minimum payments should decrease debt which in turn should reduce default. As we show in Appendix C.4.5, the first part of the argument is perhaps true – debt does decline, though it is imprecisely estimated (in part because of attrition via default and cancellation). However, any such reductions did not translate into lower default. In Section 6.1 we present some

[43]In Mexico, concerned over the size of minimum payments and its link to indebtedness and default (`https://goo.gl/MkYbV0`), the central bank mandated a floor for minimum payments in 2010. In the United States, a Congress commissioned study found that minimum payment requirements had decreased markedly over time – declining from 5% of outstanding balance in the mid-seventies to 2% by 2000 (Smale, 2005). In January 2003, US federal regulators issued inter-agency guidance on credit card lending that criticized minimum payments for being too low, noting that some did not even cover the finance charges and bills accrued in a billing cycle (`https://goo.gl/X8ujTi`). The Credit Card Act of 2009 mandated disclosures of how long it would take to pay off debt if clients only made the minimum payment.

[44]The results are lower than those for delinquency in Keys and Wang (2016) but of the same order of magnitude as those for default documented by d'Astous and Shore (2017). Both studies employ a quasi-experimental design to estimate causal effects using observational data from the United States. The latter document that an increase in minimum payments of 2% on average over a base-rate of 3% increased default rates by 4% over two years (which implies an elasticity of .06). To our knowledge this is the first experimentally estimated effect of minimum payments on default. See Table OA-22 for a comparison table.

evidence that the null effect is due to two countervailing forces (lower debt repayment burden versus tighter liquidity constraints) off-setting each other.

Our results on the unresponsiveness of new borrower default are important for at least two reasons. First, they show that the standard tools used by large banks have smaller effects on NTB borrower behavior than typically assumed in policy discussions. Second, the inability to control default and cancellations affects the profitability of new borrowers, something we discuss at greater length below.

5.2 Card Cancellations

Cancellation is a binary variable that equals one if borrower i voluntarily cancels her card at some point during the 26 month experiment. Cancelling a card is an active decision which the client can take after repaying the debt outstanding on the card. Cancellations are of direct interest because they provide evidence on the (change in) the study card's attractiveness as a result of contract term changes and because they typically reduce bank revenues, thereby making financial inclusion harder.

A. Lower Interest Rates Reduce Cancellations

Table 2 shows that cancellations in the base arm $(45, 5)$ were 13.4% over the 26 month period of the experiment, and that reducing the interest rate to 15% decreases cancellations by a statistically significant 3.5 percentage points, for an implied elasticity of $+0.39$. The reduction in interest rates made the study card unambiguously more attractive and it is perhaps not surprising then that fewer borrowers chose to cancel. This behavior is consistent with consumers engaging in some search or at least being open to outside options.[45] Treatment effects from the other arms provide broadly comparable results. We next chart treatment effects using monthly data in Figure 5 which presents the regression coefficients from estimating equation (2) on a monthly basis. Cancellations begin to decline after about six months with the rate of decline remains roughly constant through the end of the experiment. To summarize – the results from all the experimental contrasts and over the entire duration of the experiment show that the interest rate had a robust moderate effect on card cancellations.[46]

That interest rate changes have larger effects on cancellations than on default is consistent with the idea that non-preference factors play a larger role in the latter. The larger response also allows us to rule out inattention as a cause for the limited effects on default. Finally, from the bank's perspective, the benefits from these decreased cancellations need to be compared against the revenue losses from lowering interest rates, which we discuss below in Section 5.3.

[45] Section 3.2 presented evidence that cancellations are followed by account openings elsewhere for a selected sample of "good" borrowers.

[46] It is, though, a bit unclear how to benchmark this finding. If we map cancellations to repeat borrowing for microfinance borrowers, Karlan and Zinman (2017) find no effect of a interest rate reduction on the probability of repeat borrowing (p.18) by Compartamos borrowers over a 29 month period.

B. Higher Minimum Payments Increase Cancellations

Doubling the minimum payment increased cancellations (over the entire experiment) by a statistically significant 1.7 percentage points, for an implied elasticity of $+0.12$. The estimated treatment effects for the other arms are all roughly comparable with elasticities ranging from $+0.12$ to $+0.24$. As noted previously, the increase in minimum payments tightened short run liquidity constraints which is consistent with increased cancellations.

Just as with interest rates, the minimum payment increase had a much larger effect on cancellations relative to default. One might reasonably have expected that this decline in attractiveness also finds expression in higher default rates. That this is not the case suggests that default may be less strategic than cancellations.

5.3 Effects on Bank Revenues

Since we are concerned with studying financial inclusion via commercial institutions, revenues are a critical benchmark for evaluating the effects of the intervention. In this section we examine the effects of the interventions on revenue (we note that our measure of revenue is net-of-default in the sense that it incorporates default as detailed in Section 2.1). We find that departures from the $(45, 5)$ arm reduced bank revenues. This is consistent with Bank A's standard choice of minimum payments and interest rates being profit maximizing (at least relative to the alternatives considered in the experiment).

A. Lower Interest Rates Reduce Revenues

Table 2 shows that revenues are monotonically increasing in the interest rate. Taken literally, the point estimates imply that reducing interest rates from 45% to 15% over the 26 month period of the experiment reduced bank revenues (per borrower) by a substantial $2,859$ pesos (approximately half of the mean revenue measure) for an estimated elasticity of 1.54, with similar elasticities for the other interest rate arms.

In the Appendix (sections C.4.4, C.4.6, and C.4.8) we explore the effects of the intervention on three proximate determinants of revenues – purchases, payments and debt – and establish three facts. First, interest rate declines have relatively small (although imprecisely estimated) effects on purchases – the Lee bounds for the elasticity are $[-0.38, +0.25]$.[47] Second, monthly payments declined modestly in response to the interest rate decreases (the Lee bounds are $[+0.04, +0.39]$).

Third, debt *declined* in response to the interest rate reductions and the Lee bounds are $[+0.34, +0, 74]$. The small effects on behavior (purchases and payments) suggest that NTB borrowers had limited substitution possibilities, and the fact that debt declined (and this decline was larger than implied by the changes in payments and purchases) suggests that interest rate compounding is the dominant component of debt changes.

Extrapolating from the experiment suggests that increasing interest rates (relative to the experimental choices) may be a profit maximizing strategy for the bank, even after accounting for

[47] We use Lee bounds Lee (2009), to account for the attrition caused by default and cancellation.

default and cancellations – since our measure of bank revenue accounts for both. That the bank's business-as-usual interest rate was higher than 55% is consistent with this hypothesis.

B. Higher Minimum Payments Reduce Revenues

Perhaps more surprisingly, higher minimum payments also reduced bank revenues which are 469 pesos lower in the $(45, 10)$ arm compared to the $(45, 5)$ arm, with an implied elasticity is -0.16 (the other contrasts are similar). In the appendix (C.4.7, C.4.9 and C.4.5) we find that the minimum payment increase led to modest increases in purchases and payments (the Lee bounds are $[+0.18, +0.85]$ and $[+.01, +.48]$ respectively). The point estimate on effect on debt shows a decrease of about one-third though the the Lee bounds for the elasticity are quite wide – $[-0.46, +0.15]$. That lowering minimum payments increases revenue is also consistent with the bank's standard minimum payment being 4% (lower than either of the experimental alternatives).

The revenue findings emphasizes the problems with using higher minimum payments as a policy lever – higher minimum payments reduced bank revenues (lowering bank profits), increased cancellations (thereby perhaps lowering borrower welfare) and had no effects on default during the experiment.

5.4 Spillovers and Long-Run Effects

We use the credit bureau data to examine whether the experimental changes in the study card's contract terms affected borrowing with other formal lenders. For instance, lower interest rates might lead to increased cancellations of other – presumably more expensive – cards or clients might change their overall borrowing or default behavior on other loans. Similarly, higher minimum payments might lead borrowers to look elsewhere for more attractive terms.

Table 3 shows that this is not the case. Neither default, cancellations nor new borrowing, (all measured in June 2009) with all other formal lenders respond to the changes in minimum payments and interest rates. The results are unambiguous and demonstrate that even significant improvements in credit terms, through lower interest rates, did not change borrowing patterns with other lenders. Next, although borrowers are more likely to cancel the study card in response to higher minimum payments, there is no substitution towards other formal credit sources. We read this as evidence for continued credit constraints in that borrowers in the higher minimum payment arms while being more likely to cancel their cards were unable to replace the study card credit with other formal sector credit.

We were also able to obtain information from the credit bureau in June 2012, three years after the experiment ended. Columns 1 and 2 of Table 4 show the extensive margin treatment effects on other formal sector borrowing at that time. Consistent with the previous findings, we find that being exposed to lower interest rates or higher minimum payments on the study card did not (at a five year horizon) lead to a greater number of loans or interactions with a larger number of lenders.[48]

[48] Karlan and Zinman (2017) also find no crowd-in or crowd-out among Compartamos borrowers.

5.5 Bank A Discontinues the Study Card

In addition to the the limitations of ex-ante screening NTB borrowers documented in Section 3.2, the previous results document that even substantial changes in contract terms were relatively ineffective at reducing default or increasing revenue (among pre-existing borrowers). Viewed in this light, it is perhaps unsurprising that Bank A subsequently reduced its interactions with NTB borrowers. Figure 1(c) shows both the current stock and new issues of the study card over time. After issuing the study card in substantial numbers for several years, the bank stopped issuing it completely in 2009 and by 2013 there were no borrowers with the study card in the CB. Thus, financial inclusion using the traditional large bank lending model – even by one of Mexico's most sophisticated banks – turned out to be a fraught proposition.

6 What Explains The Limited Default Response?

In this section we explore possible reasons for the limited response of default to the experimental changes in contract terms. For the case of minimum payments, we argue that the null default effect arises from the "cancelling" out of two opposing forces and we identify the debt repayment burden as a potentially important channel for explaining default. Second, we document that NTB borrowers are likely credit constrained with limited outside options, which might limit moral hazard and default.

6.1 Offsetting Effects of the Debt Burden and Liquidity Constraints

As mentioned earlier, increasing minimum payments sets in motion two counteracting forces. First, short-run liquidity constraints increase which could increase default. Second, over time increased minimum payments decrease the debt repayment burden, which should reduce default. The estimated null effect is consistent with these two opposing effects negating each other.

We can assess the reasonableness of this argument by studying default when one of the two effects is no longer operational. We were able to obtain data on default for the experimental sample for June 2012 (three years after the experiment ended). After May 2009, all study borrowers were returned to the same set of contract terms. The minimum payment was set at the pre-experiment level of 4% and the interest rates were likewise returned to their pre-experiment levels (we confirm this in Table 4, column (3) and in Figure OA-25). This means that for those previously in the 10% minimum payment arms, the tightening of short-run liquidity constraints effect was no longer operational. On the other hand, since the higher minimum payments reduced debt (see C.4.5), clients in the 10% minimum payment arm faced a lower debt repayment burden so the second effect was still present after the experiment ended.[49] Thus, of the two countervailing forces described above, only the debt burden effect was operational after the experiment ended. The hypothesis is then that default should decline in the 10% minimum payment arms after the end of the experiment.

[49]For instance, the debt in the in the $(45, 10)$ arm was lower than in the $(45, 5)$ arm in May 2009 – although the Lee bounds are wide and suggest reductions in the range of 760 pesos to no change in debt are both consistent with the data. For the selected sample that survived through the experiment, debt reduced substantially, by about 30%.

Table 4 shows that this is the case. Higher minimum payments (between April 2007 and May 2009) decreased default through June 2012 by a statistically significant 4.6 percentage points. The implied elasticity of -0.11 is much larger and opposite in sign from the statistically insignificant elasticity of $+0.02$ at the end of the experiment in May 2009.[50] The results are thus consistent with the hypothesis that during the experiment tightened short-run liquidity constraints counteracted the effect of a lower debt repayment burden yielding a zero net effect on default.

We can also directly examine the effect of the debt burden on default by using an instrumental variables strategy. The LATE of debt (measured in thousands of pesos in May 2009) on default (between June 2009 and June 2012) using the 10% minimum payment arm as an instrument is 0.06, i.e. every 1000 peso reduction in debt due to increased minimum payments leads to a six percentage point reduction in default rates.[51] Finally, the (Spearman) rank correlation between the 7 ITT estimates of default and the reduction in debt in May 2009 across the 7 arms is 0.96 so that that the largest contrasts were those with the largest reductions in debt at the end of the experiment.

6.1.1 The Debt Burden Versus the "pure" Moral Hazard Effect

The previous section highlighted the importance of the debt repayment burden effect in determining default using the minimum payment intervention. In this section we attempt to further quantify this effect by disentangling it from the "pure" moral hazard effect using the interest rate intervention.

We again use outcomes three years after the experiment to learn about the relative magnitudes of the two effects. During the experiment both effects should be operational. Since they both increase default and we found small default responses, we concluded that both effects were likely small. After the experiment, however, no differential "pure" moral hazard incentive effect was operational since borrowers across arms faced the same interest rates (see col. (3) of Table 4). However, Table OA-16 shows that the debt burden did vary systematically across the interest rate arms – the Lee bounds for the debt elasticity were $[+0.34, +0.74]$, a robust effect.[52] Consistent with the debt burden hypothesis we find that default rates were lower in the lower interest rate arms three years after the experiment ended. Table 4 finds that five-year default rates in the $(15, 5)$ arm were 4 percentage points lower relative to the base arm – the implied elasticity is $+0.15$.[53]

[50] In the interest of clarity we focus on ITT estimates using cumulative default over the entire five year period from March 2007 through June 2012. This has the advantage of avoiding the selection problems that would need to be dealt with if we restricted attention to the non-attriters as of May 2009. On the other hand default now includes default both during the experiment as well as after the experiment. We note, however, that at the end of the experiment (May 2009) of the seven treatment arms, only two (the $(15, 5)$ and the $(25, 5)$) arm had default rates that were substantively or statistically different from zero. Four of the remaining arms had default rates that were (typically much) less than 1% away from the $(45, 5)$ arm and none were statistically significant (see Table 2 for details).

[51] The exclusion restriction is violated if the 10% payment arm affected post-experiment borrower behavior through channels other than debt. For instance if two years of exposure to higher minimum payments changed borrower purchase and payment behavior, perhaps through habit formation. In Section Appendix D. we explore this hypothesis and do not find much support for it. In addition, we only use those treatment arms with non-differential attrition in May 2009.

[52] The Lee bounds are for the $(15, 5)$ arm relative to the base arm.

[53] This is approximately the same as the $+0.20$ elasticity at the end of the experiment which is estimated off a reduction in default of 2.6 percentage points over the two year experiment. Note also that the 4% figure includes individuals who defaulted during the experiment. We do not limit attention to post-experiment defaults only to avoid sample selection

This finding further emphasizes the importance of the debt-burden effect in our sample of NTB borrowers.

This discussion needs to be qualified since the post-treatment effects may also be driven by other non-debt effects across treatment arms (e.g. borrowers in the lower interest rate arms may default less after the experiment if they expect favorable terms from the bank in the future). In the absence of relevant data, we cannot rule out such non-debt channels. However, to the extent that lower interest rates reduced default among more marginal borrowers (i.e. those who would have defaulted absent the interest rate reduction), the post-experiment sample is likely not positively selected, which would mean higher (not lower) default in the lower interest rate arms after the interest rate returns to 55%.

To conclude, the evidence on default particularly from the minimum payment arms suggests that the reduction in the debt repayment burden was an important channel for reducing default three years after the end of the experiment.

6.2 Severe Consequences of Default May Limit Moral Hazard

Default has serious consequences for NTB borrowers, which may explain why default responses were limited. Section 5 was indirectly informative about outside options. Recall that debt and interest rates are positively correlated, suggesting that clients had limited substitution alternatives. Here we provide evidence for three claims: first, NTB borrowers are credit constrained in the formal sector and therefore presumably value formal sector credit. Second, default sharply reduces access to subsequent formal sector credit. Third, informal credit terms are substantially worse than formal credit terms. These claims together suggest that the range of variation in contract terms, even though substantial, was "infra-marginal" in the light of these concerns and so borrower responses were limited.

A. NTB Borrowers are Credit Constrained

Using the methodology proposed in Gross and Souleles (2002), Section B.1 finds that our NTB sample is credit constrained: a credit limit increase of 100 pesos on the study card translates into 32 pesos of subsequent additional debt. For comparison, this propensity to consume out of increases in the credit limit is about thrice as large as the figure for the United States.

B. Default Reduces Access to Formal Credit

Default is associated with large declines in subsequent formal sector borrowing. Using the experimental sample we estimate a cross-sectional regression where the primary explanatory variable is an indicator if borrower i defaulted on the study card in the six months after the start of the experiment (i.e. between March and September 2007) and the dependent variable is an indicators

issues, but note that such concerns might in fact lead us to *under*-estimate the debt-burden hypothesis if it is the case that the lowered interest rates led to more financially vulnerable borrowers surviving in the lower interest rate group post-experiment the $(15, 5)$ arm had a 1.43% lower default rate than the base arm.

for i obtaining a new loan or card six,twelve, or forty eight months after September 2007. We include age, gender, and zip code indicators as controls, and restrict attention to the sub-sample for whom the study card was the first formal sector loan product and who had been with the bank for between 6 to 11 months at the start of the experiment. Panel A of Table 5 shows the results for all types of loans, while Panel B focuses only on credit cards. We further group columns by lender type (any lender, all lenders except Bank A, and Bank A).

Default on the study card is associated with a substantial 26 percentage point decrease in the likelihood of obtaining any new formal sector loans in the next 6 months (relative to a mean of 29 percent for non-defaulters). The negative consequences of default are also long-lived – we continue to find substantial effects four years after default. Since default is reported to the Credit Bureau, we might expect the negative correlation to show up not only in Bank A but in all banks, and indeed this is what columns (4)–(6) reveal. Panel B restricts attention to credit cards and finds, if anything, even starker results – default on the study card is associated with an absence of any subsequent credit cards up to four years later. Lenders appear to adopt harsher stances towards default on uncollateralized debt.

One concern with the regression above is that omitted variables may drive both default and future loan demand. We attempt to address this by adding borrower and time fixed effects. This increase in flexibility forces us to restrict attention to delinquency as the primary outcome rather than default.[54] We continue to find a negative relationship between delinquency and subsequent borrowing. The rate at which borrowers get loans from any bank is 7 percentage points per month before being delinquent for the first time, but only 5 percentage points after the first delinquency. Borrowers cease to obtain any subsequent additional credit from Bank A following the first delinquency.

We take the evidence above as being primarily suggestive, though the conclusion is unsurprising.[55] Decreased access to formal lending is precisely what one would expect from default on a formal loan given the Credit Bureau, though the magnitudes are striking. Defaulters are then forced to rely on informal lenders and this is not an enticing prospect.

C. Informal Terms are Worse Than Formal Terms

We use the Mexican Family Life Survey (MxFLS) to compare interest rates, loan amounts, and loan duration for formal and informal loans.[56] We find that informal loan terms are significantly worse than formal loan terms. Table 6 shows the results from regressing contract terms on an indicator for a formal loan and controls. The first striking fact is that the average annual interest

[54]The problem with using default in an event study of this kind is that default is preceded formally by three events that are reported to the credit bureau (three consecutive delinquencies over three billing cycles) so that the deterioration in credit access precedes actual default. As a result we focus on the first delinquency for eventual defaulters.

[55]Bos et al. (2018); Dobbie et al. (2018) document similar magnitudes using more persuasive empirical designs.

[56]We define a loan as formal if the lender is a bank and informal otherwise. Informal loan sources comprise: Cooperatives (13%), money-lenders (8%), Relatives (38%), Acquaintances (20%), Work (11%), pawn-shops (5%), and others (5%). Consistent with the evidence from a range of developing countries (See e.g. Banerjee and Duflo (2010)) only 6% of borrowers have any formal loans and 91% of borrowers have only informal loans. Note that we do not observe any informal sector loans in our bank data.

rate for informal loans is 291% while the corresponding rate for formal loans is 94 points lower (col. 1). The average loan amount is 3658 pesos for informal loans and 9842 pesos for formal ones (col. 4), and the term of the loan is 0.52 years for informal loans and 1.07 years for formal loans (col. 9). Figure OA-26 shows that the distribution of interest rates for informal loans stochastically dominates the distribution for formal loan rates while the opposite is true for loan terms and loan amounts. These results are robust to controlling for income and wealth proxies (columns 2,4 and 7). The results on loan terms and duration also survive the addition of household fixed effects.[57] Based on these results we conclude that it is costly to be excluded from the formal loan market. To conclude, default responded only in a muted fashion to even relatively large changes in contract terms perhaps because of the dire outside options outlined above.

7 What explains the high baseline default rates?

If default is indeed as costly as documented above, why are default rates so high? We speculate that the answer is partly that NTB borrowers are vulnerable to shocks and such shocks precipitate default. The importance of the debt repayment burden documented above is consistent with this hypothesis.

We provide some evidence that shocks indeed precipitate default by estimating the effect of losing a formal job on default. We observe employment spells for the the subset of our CB sample employed in the formal sector by matching the CB data with Mexican social security data (the IMSS). The matching yields a panel of $86,363$ individuals with information on employment history in the formal sector as well as their formal credit records.[58]

We estimate the following regressions using OLS for individual i living in state s in month t:

$$\text{default}_{it}^{j} = \alpha_i^j + \gamma_{s,t}^j + \sum_{k\geq 1} \beta_k^j \times \mathbb{1}(\text{ months unemployed}_{it} = k) + \varepsilon_{it}^j \tag{3}$$

where α_i is an individual fixed effect and $\gamma_{s,t}$ controls for trends at the state month level. The independent variables are a set of dummies $1(\text{ months unemployed}_{it} = k)$ that are equal to 1 if individual i in month t has been unemployed for k months. For individuals who are employed $1(\text{ months unemployed}_{it} = k)$ is equal to zero for all k. The dependent variable, $default_{it}^j$ is equal to one if individual i at month t has a 'default code' of j months, meaning that she has at least one loan in delinquency for j or more months. Figure 6 plots $\hat{\beta}_k^j$ for different values of k and j. The likelihood of default is increasing in the length of the unemployment spell so that for instance, being unemployed for 10 months is associated with a 5 percentage point higher likelihood

[57]Only about 3 percent of households hold both formal and informal sector loans so that the identifying variation in the fixed effects model arises from a small (and likely selected sample).

[58]The matching proceeds as follows: Of the 1m borrowers in our 2014 CB data, $542,959$ had both a tax identifier as well as a bank loan at some point between January 2011 and May 2014. We used the tax identifier to match borrowers to the IMSS monthly data from October 2011 to May 2014. We observe employment for at least one month for 86,363 individuals. Since the IMSS is a census of all formal sector workers, a match indicates employment in the formal sector and we assume that a lack of a match indicates no employment in the formal sector. Since we do not observe employment in the informal sector, we cannot construct a more comprehensive indicator of employment.

of being delinquent on at least one loan – the unconditional mean is is 12 pp, so this is a 41% increase.[59] These results demonstrate the severe effect of one particular shock (unemployment) on default (even after controlling for individual fixed effects). This is consistent with the view that large negative shocks, such as prolonged unemployment (or health shocks), could increase default markedly. At the same time, these results are only partial since unemployment in the formal sector is likely only one possible shock affecting NTB borrowers.[60]

8 Conclusion

Expanding financial access to under-served populations is a central part of the development agenda. While the role of innovative organizations and approaches, such as micro-finance, has received considerable attention, much less is known about the experiences of large financial institutions whose scale suggests an important role in the expansion of financial access. In this paper we examine a large Mexican bank's efforts at expanding financial access with a credit card specifically targeted towards borrowers with limited credit histories but that otherwise utilized standard large commercial bank approaches to individual lending – exclusive reliance on credit scoring and credit bureaus for monitoring and limited client interactions. The card was available nationally starting in 2002 and by 2010 accounted for 15% of all first time formal sector loan products.

The bank's experience was particularly difficult. We construct a measure of bank revenue per borrower and show that it is low, variable and unpredictable. In addition, the bank loses revenue as new borrowers with positive credit histories were more likely to leave the bank (for other lenders). Moreover, newer borrowers defaulted at much higher rates that borrowers with longer credit histories. For these reasons, we conclude that ex-ante screening is likely a difficult task and that ex-post contract terms assume even greater importance.

We find that adjusting contract terms ex-post does not reduce default. We use a large-scale randomized experiment and show that even substantial changes in interest rates and minimum payments have small effects on default. In the case of minimum payments, we show that the null effect may be explained by the offsetting effects of increased short-run liquidity constraints and a decreased debt repayment burden – highlighting the double-edged nature of higher minimum payments as a policy tool for limiting default. The limited response to interest rate changes implies a minor role for moral hazard even among our population of recent borrowers, and we provide some speculative evidence that other features of the economic environment (e.g. unemployment shocks) may be more important barriers to financial inclusion. The difficulties of lending to this population is captured most starkly in the bank's abandonment of its flagship financial inclusion product and a declining engagement with borrowers with limited credit histories. Taken together, these findings highlight the difficulties of expanding credit access to under-served populations via financial organizations using traditional large-bank lending methods.

[59] The unconditional mean for the dependent variables are 18 pp. (>1m), 16 pp. (>2m), 15 pp. (>3m) and 12 pp. (>6m).

[60] Only about 20% of our experimental sample is employed in the formal sector.

Tables

Table 1: Summary statistics and baseline characteristics

	Experimental sample	Experimental sample	Credit bureau sample		
			≥ 1 Card Holders	New borrowers (matched)	Experienced borrowers
	(1)	(2)	(3)	(4)	(5)
Panel A. Information from the experimental sample dataset					
Month of measurement	March 2007	May 2009			
Payments	711	908	-	-	-
	(1,473)	(1,811)			
Purchases	338	786	-	-	-
	(1,023)	(2,064)			
Debt	1,198	5,940	-	-	-
	(3,521)	(6,160)			
Credit limit	7,879	12,376	-	-	-
	(6,117)	(9,934)			
Card revenue†	4,197	-	-	-	-
	(7,347)				
Credit score	645	-	-	-	-
	(52)				
(%) Consumers for whom experiment is their first card	57	-	-	-	-
(%) Consumers who default between Mar/07 - May/09	17	-	-	-	-
(%) Consumers who cancel between Mar/07 - May/09	10	-	-	-	-
Panel B. Information from the credit bureau dataset					
Month of measurement	June 2007	June 2010	June 2010	June 2010	June 2010
Mean card limit (all cards)	15,776	18,475	49,604	22,082	56,187
	(15,776)	(17,557)	(32,596)	(28,710)	(43,032)
Total credit line (all loans)	53,652	64,804	53,718	49,348	139,804
	(70,292)	(79,994)	(103,503)	(87,855)	(162,568)
Tenure in months of oldest credit	68	100	79	68	206
	(54)	(51)	(87)	(57)	(85)
Panel C. Demographic information					
Month of measurement	June 2007	June 2010	June 2010	June 2010	June 2010
(%) Male	52	-	47	47	53
(%) Married	62	-	50	48	47
Age (in years)	39	42	45	44	58
	(6)	(6)	(19)	(18)	(22)
Monthly income (10/11)‡	13,855	-	14,391	14,759	22,641
	(11,244)		(12,949)	(12,885)	(15,928)
Observations	164,000	-	221,151	57,450	55,120

Notes: This table presents means and standard deviations for selected variables from the experimental sample and three different credit bureau sub-samples. Column 1 shows statistics for the experimental sample at the beginning of the experiment – March 2007 (Panel A) and June 2007 (Panels B and C). Column 2 (Panel A) shows statistics for the experimental sample at the end of the experiment (May 2009) and June 2010 (Panels B and C). Column 3 presents summary statistics for the credit bureau sub-sample restricted to borrowers with at least one credit card in June 2010. Column 4 selects a sub-sample from the Column 3 sample that mimics the distribution of card tenure for the experimental sample (see the Appendix A.1 for details). Column 5 restricts the sample from Column 3 to individuals with at least eight years of credit history with the bureau. (†) The card revenue measure is constructed using monthly data on purchases, payments and debt and the procedure is described in Section 2.2. (‡) Income is obtained by matching our data with social security data (IMSS) from October 2011. The IMSS contains firm reports of employee earnings. Approximately 18% and 13% of the experimental sample and the CB sub-samples were matched with the IMSS.

Table 2: Treatment Effects on Default, Cancellations, and Revenue
(Outcomes measured in May 2009)

	Default		Cancellations		Revenue	
Strata:	All	6-11M	All	6-11M	All	6-11M
	(1)	(2)	(3)	(4)	(5)	(6)
r = 15, MP = 5	-0.026	-0.019	-0.035	-0.033	-2,859	-3,139
	(0.008)	(0.002)	(0.004)	(0.007)	(212)	(353)
r = 15, MP = 10	-0.015	0.012	-0.011	-0.027	-2,642	-2,893
	(0.010)	(0.004)	(0.003)	(0.003)	(178)	(314)
r = 25, MP = 5	-0.023	-0.014	-0.024	-0.026	-1,889	-1,956
	(0.007)	(0.002)	(0.004)	(0.003)	(140)	(177)
r = 25, MP = 10	-0.008	-0.004	-0.003	-0.024	-1,893	-2,108
	(0.006)	(0.002)	(0.004)	(0.005)	(135)	(294)
r = 35, MP = 5	-0.000	0.004	-0.018	-0.022	-964	-1,008
	(0.003)	(0.002)	(0.005)	(0.005)	(72)	(132)
r = 35, MP = 10	-0.002	-0.010	-0.004	-0.003	-1,167	-1,062
	(0.006)	(0.004)	(0.002)	(0.003)	(114)	(128)
r = 45, MP = 10	0.005	0.015	0.017	0.004	-469	-394
	(0.007)	(0.005)	(0.005)	(0.003)	(41)	(118)
Constant (r = 45, MP = 5)	0.193	0.315	0.134	0.099	2,768	1,615
	(0.006)	(0.001)	(0.002)	(0.004)	(110)	(189)
Observations	143,916	47,959	143,916	47,959	143,916	47,959
R-squared	0.001	0.001	0.002	0.002	0.035	0.025

Notes: All regressions include strata dummies and use sample weights. The dependent variable for Columns (1) and (2) is default (bank-initiated revocations). The dependent variable for Columns (3) and (4) are (client-initiated) cancellations. The dependent variable for Column (5) and (6) is our measure of bank revenue from the study card. All outcomes measure the dependent variables at the end of the experiment (26 months). Columns (1), (3) and (5) include all cardholders in the experiment. Columns (2), and (4) and (6) restrict only the cardholders in the 6-11 months strata.

Table 3: Treatment Effects on Other Loans: Default, Cancellations and New Loans
(Existing loans by March 2007, Outcomes measured in June 2009, Any Loan Type)

	Default			Cancellations			New loan		
	Any Bank (1)	Same Bank (2)	Other Bank (3)	Any Bank (4)	Same Bank (5)	Other Bank (6)	Any Bank (7)	Same Bank (8)	Other Bank (9)
r = 15, MP = 5	0.006	-0.004	0.002	0.017	0.005	0.013	0.011	0.011	0.009
	(0.004)	(0.002)	(0.002)	(0.007)	(0.003)	(0.005)	(0.005)	(0.005)	(0.004)
r = 15, MP = 10	-0.006	-0.017	-0.001	0.016	0.006	0.011	0.018	0.009	0.011
	(0.003)	(0.007)	(0.003)	(0.010)	(0.003)	(0.007)	(0.011)	(0.004)	(0.007)
r = 25, MP = 5	-0.002	-0.014	0.003	0.005	0.002	0.003	0.009	0.003	0.003
	(0.002)	(0.006)	(0.003)	(0.005)	(0.003)	(0.002)	(0.007)	(0.005)	(0.005)
r = 25, MP = 10	-0.001	-0.008	0.002	0.003	0.006	-0.002	0.013	0.004	0.006
	(0.004)	(0.007)	(0.003)	(0.006)	(0.004)	(0.004)	(0.010)	(0.004)	(0.006)
r = 35, MP = 5	-0.002	-0.003	0.001	0.004	0.003	0.002	0.010	0.003	0.005
	(0.002)	(0.004)	(0.002)	(0.002)	(0.001)	(0.002)	(0.003)	(0.001)	(0.004)
r = 35, MP = 10	0.007	0.003	-0.001	0.006	0.002	0.005	-0.003	0.002	-0.007
	(0.002)	(0.003)	(0.006)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)
r = 45, MP = 10	-0.014	-0.010	-0.010	0.009	0.000	0.009	-0.004	-0.008	0.001
	(0.006)	(0.006)	(0.006)	(0.005)	(0.002)	(0.004)	(0.003)	(0.003)	(0.004)
Constant (r = 45, MP = 5)	0.560	0.336	0.449	0.137	0.021	0.121	0.541	0.165	0.490
	(0.002)	(0.004)	(0.002)	(0.005)	(0.002)	(0.003)	(0.004)	(0.002)	(0.003)
Observations	143,916	143,916	143,916	143,916	143,916	143,916	143,916	143,916	143,916
R-squared	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Notes: All regressions include strata dummies and use sample weights. The regressions include all loan types (mortgage, auto loan, credit card, etc). The dependent variable for Columns (1) to (3) is default (bank-initiated revocations). The dependent variable for Columns (4) to (6) are (client-initiated) cancellations. The dependent variable for Columns (7) to (9) are new loan originations after March 2007. All columns exclude the experimental card. Columns (1), (4) and (7) refer to loans issued by any bank. Columns (2), (5) and (8) refer to loans issued by the same bank as the experimental card (i.e. Bank A). Columns (3), (6) and (9) refer to loans issued by any bank except for Bank A. All dependent variables restrict to loans that were issued on or before by March 2007 that remained active by March 2007. All outcomes are measured in June 2009, one month after the experiment ended.

Table 4: Long-term Treatment Effects
(Outcomes measured in June 2012)

	New Loan (any bank) Jun 09 - Jun 12 (1)	# Banks active Jun 12 (2)	Int Rate (Study Card) Jun 12 (3)	Cumulative Default Jun 12 (4)	Cumulative Cancellations Jun 12 (5)
r = 15, MP = 5	0.003	0.012	0.002	-0.039	0.008
	(0.002)	(0.006)	(0.002)	(0.013)	(0.009)
r = 15, MP = 10	0.011	0.017	0.003	-0.066	0.027
	(0.006)	(0.008)	(0.001)	(0.021)	(0.013)
r = 25, MP = 5	-0.001	0.008	-0.001	-0.040	0.017
	(0.004)	(0.008)	(0.001)	(0.011)	(0.007)
r = 25, MP = 10	-0.006	0.008	0.001	-0.048	0.012
	(0.001)	(0.004)	(0.002)	(0.014)	(0.012)
r = 35, MP = 5	0.006	0.016	-0.003	-0.004	-0.005
	(0.005)	(0.008)	(0.003)	(0.004)	(0.002)
r = 35, MP = 10	0.015	0.002	0.005	-0.038	0.009
	(0.010)	(0.004)	(0.001)	(0.007)	(0.006)
r = 45, MP = 10	0.012	0.016	0.002	-0.043	0.021
	(0.005)	(0.011)	(0.002)	(0.014)	(0.007)
Constant (r = 45, MP = 5)	0.342	1.620	0.530	0.405	0.428
	(0.004)	(0.005)	(0.001)	(0.010)	(0.007)
Observations	139,197	139,197	42,463	139,197	139,197
R-squared	0.000	0.000	0.001	0.002	0.000

Notes: All regressions include strata dummies and use sample weights. Each column is a separate regression. The dependent variable in column (1) is a binary variable equal to 1 if client i took out at least one loan between June 2009 and June 2012. The dependent variable in column (2) is the number of banks with whom client i had an "active relationship" as of June 2012. An active relationship means having at least one active loan with a lender. Column (3) uses as dependent variable the interest rate (between 0 and 1) on the study card in June 2012 for those cards that remained open. Columns (4) and (4) use as dependent variables binary variables that are equal to 1 if the client defaulted, or cancelled, respectively, at any point between March 2007 and June 2012.

Table 5: Probability of getting a new loan or card against default

	Any bank			Any bank except Bank A			Bank A		
	September 07 up to			September 07 up to			September 07 up to		
	Feb/08	Aug/08	Aug/11	Feb/08	Aug/08	Aug/11	Feb/08	Aug/08	Aug/11
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Any loan									
Default in Mar/07 - Aug/07	-0.26	-0.33	-0.44	-0.21	-0.27	-0.37	-0.10	-0.15	-0.22
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.01)	(0.02)	(0.02)
mean dep. var non-defaulters	0.29	0.39	0.55	0.25	0.33	0.49	0.08	0.12	0.19
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	22,813	22,813	22,813	22,813	22,813	22,813	22,813	22,813	22,813
R-squared	0.363	0.366	0.370	0.363	0.361	0.369	0.346	0.359	0.365
Panel B. Credit cards only									
Default in Mar/07 - Aug/07	-0.24	-0.31	-0.43	-0.18	-0.24	-0.34	-0.09	-0.14	-0.21
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)
mean dep. var non-defaulters	0.23	0.30	0.42	0.19	0.25	0.35	0.07	0.11	0.18
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Observations	22,813	22,813	22,813	22,813	22,813	22,813	22,813	22,813	22,813
R-squared	0.354	0.356	0.364	0.356	0.354	0.359	0.349	0.360	0.366

Notes: This table regresses measures of subsequent new card ownership against previous default on the study card. The sample consists of the set of borrowers with (a) the experimental card, that (b) belong to the 6-11 months strata, and (c) for whom the experimental card was their first formal loan. The observations are at the level of the card holder. Each column within each panel is a different regression. For all regressions the independent variable is equal to 1 if cardholder i defaulted in the experimental card between the start of the experimental period and 6 months after the experiment started (March 2007 to August 2007). The dependent variable varies by column. For columns (1), (2) and (3) in Panel A, the dependent variable is an indicator variable equal to 1 if a borrower obtains a new loan (any kind of loan: mortgage, auto loan, credit card, etc) in any bank between the periods September 2007 and February 2007, August 2008, and August 2011 (6, 12, and 48 months). Columns (4), (5) and (6) repeat the exercise but restricting to loans with banks that are not Bank A, whereas Columns (7), (8) and (9) restrict to Bank A, exclusively. All regressions include postal code fixed effects, age, a male dummy, and a married dummy. Standard errors are shown in parentheses.

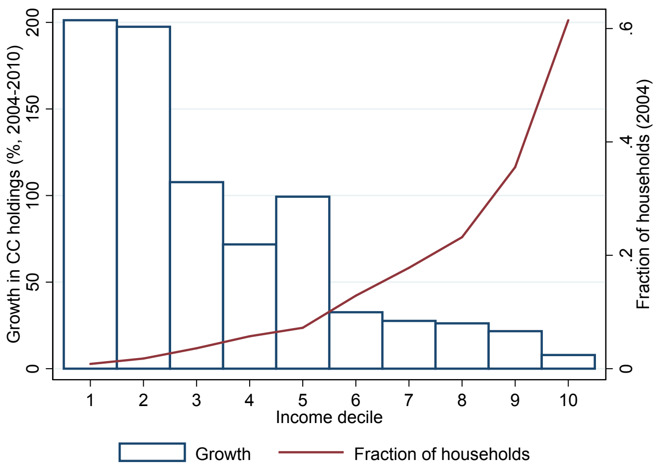
Table 6: Formal vs Informal Loan Terms

	Interest rate			Loan amount			Loan duration in years		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Formal credit	-94	-108	-7.08	6,184.3	4,926	3,934	0.554	0.544	0.491
	(31)	(48)	(38)	(288)	(484.3)	(659.3)	(0.034)	(0.058)	(0.104)
Education dummies	No	Yes	No	No	Yes	No	No	Yes	No
Sample dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Household controls	No	Yes	No	No	Yes	No	No	Yes	No
Household FE	No	No	Yes	No	No	Yes	No	No	Yes
Dependent variable mean	254	254	231	5022	5022	5061	0.732	0.732	0.732
Dependent variable SD	503	503	423	6,938	6,938	7,023	0.757	0.757	0.757
Observations	2,427	880	202	8,810	2,992	423	4,257	1,522	301
R-squared	0.006	0.036	0.860	0.063	0.171	0.661	0.083	0.119	0.646

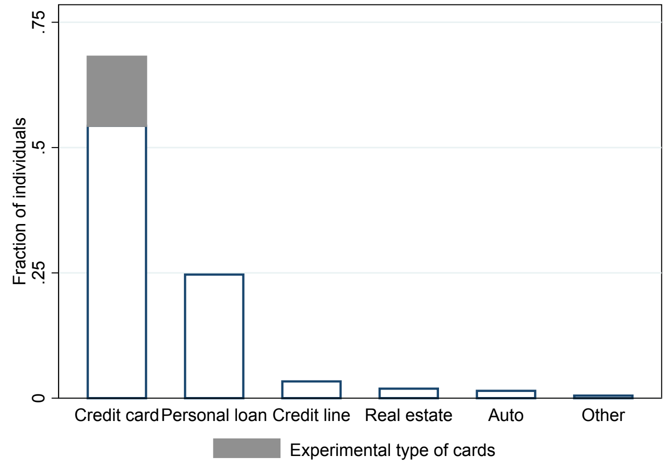
Notes: Data from National Survey of Household Living Standards (Rubalcava and Teruel, 2006) is used to construct the table. The table shows the difference between formal and informal interest rates (Columns (1)–(3)), peso loan amounts (Columns (4)–(6)) and the loan duration (Columns (7)–(9)). We consider a loan to be from a formal entity which we define as a banking institution and informal otherwise. The household controls include age, monthly expenditures, and dummy variables for car ownership, washing machines, and other household appliances. Standard errors are shown in parentheses.

Figures

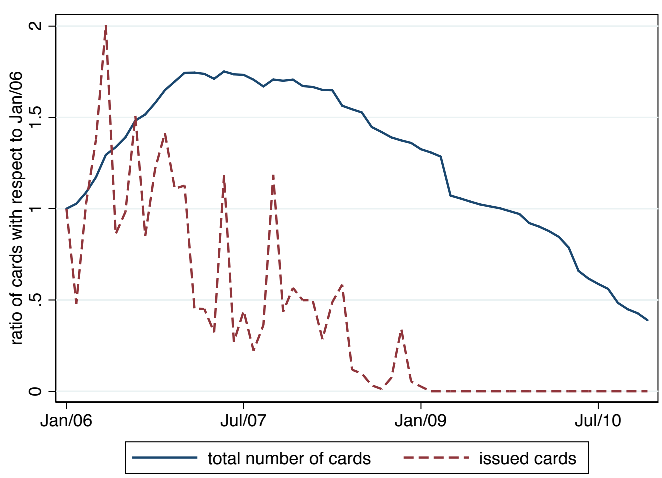
Figure 1: Credit Card Growth, Study Card and First Time Loans and Study Card Stocks and Flows



(a) CC Growth and share of HH with CC's



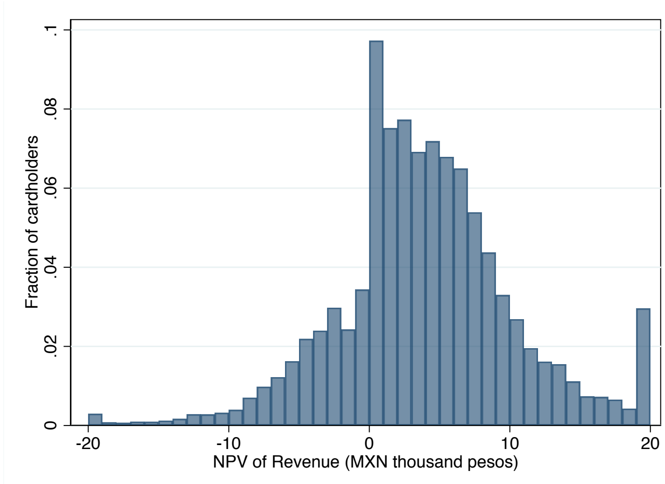
(b) First Time Loan, by type



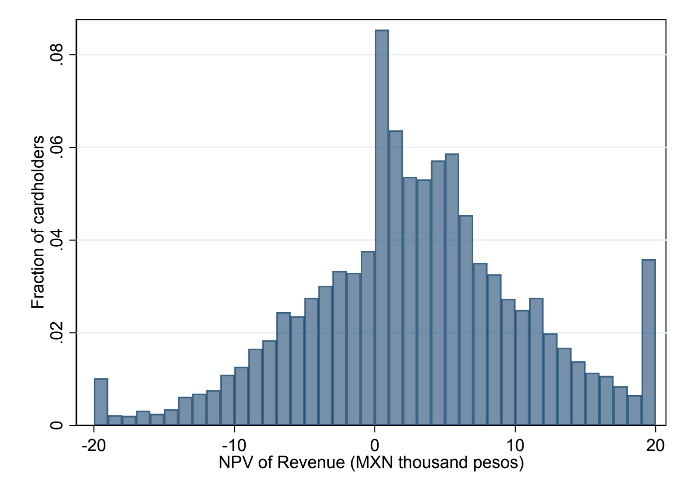
(c) Study Card Stocks and Flows

Notes: Panel (a) is constructed using data from the 2004 National Income Expenditure Survey (ENIGH). The X-axis represents (household) income deciles. The left Y-axis – corresponding to the hollow bars– shows the percentage growth in the number of households that have at least one credit card from 2004 to 2010. The right Y-axis – associated with the red line – plots the fraction of households in each income decile that have at least one card in 2004. Panel (b) is constructed using a representative sample of the 2010 credit bureau population (i.e those with formal sector loans). For each individual, we identify the oldest loan and record its type (e.g. auto loans, credit card, real estate loans). We then plot the fraction of first loans by type. The gray area represents the study card described in Section 2. Panel (c) is constructed using credit bureau data from 2012 on Card A. For confidentiality purposes we normalize the January 2006 values for both the total number of study cards and the number of issued study cards to 1. The solid blue line represents the total number of study cards in a given month. The red dashed line represents the flow of study cards: the total number of new study cards were issued in a given month.

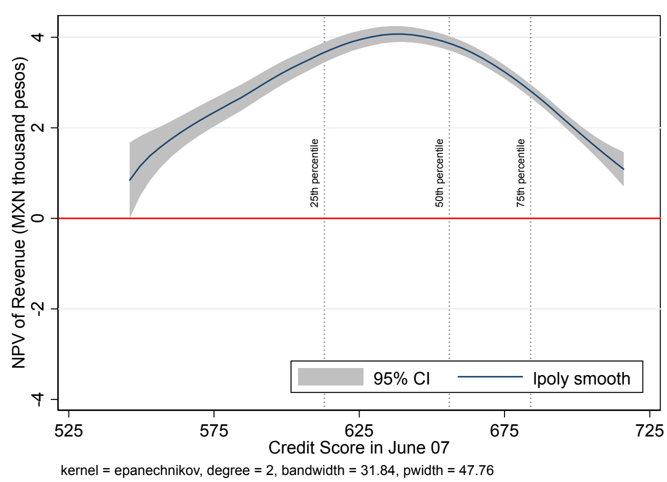
Figure 2: Distribution of Measured of Revenue per Card and relation to Credit Score



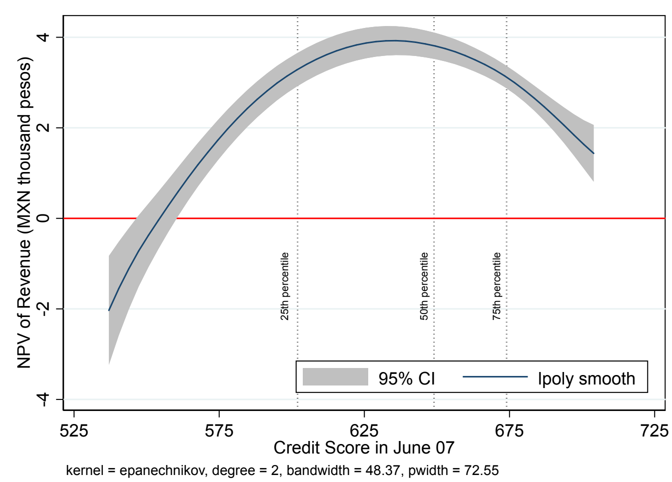
(a) Net Present Value of Revenue ('000 Pesos)
(control group borrowers)



(b) Net Present Value of Revenue ('000 Pesos)
(control group borrowers with 6-11M)



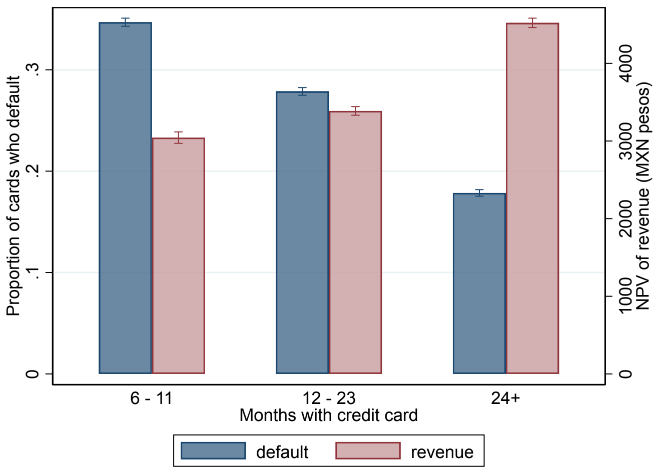
(c) Mean revenue by Credit Score
(control group borrowers)



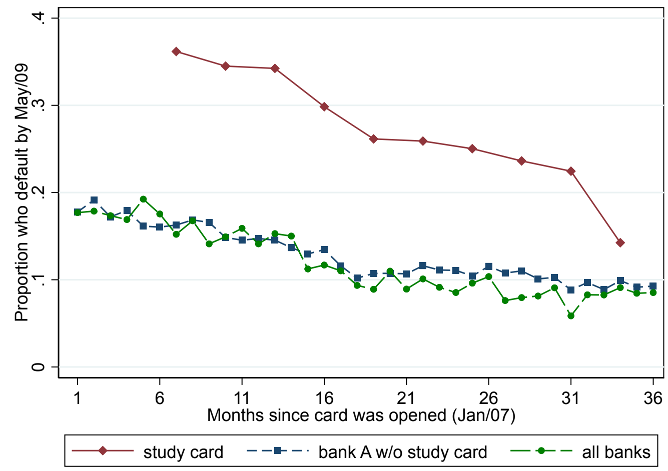
(d) Mean revenue by Credit Score
(control group borrowers with 6-11M)

Notes: Panel (a) represents the distribution of our revenue measure for the control group (using sampling weights). Panel (b) represents the analogous graph for the new to banking strata. For clarity, the histograms are censored at $\pm 20,000$ 2007 Pesos. Panel (c) displays a local polynomial kernel regression of our revenue measure against credit scores in June 2007 done at the individual card level (for the control group). The grey shaded area denotes point-wise 95% confidence intervals. The x-axis ranges from 5th to the 95th percentiles of the credit score distribution. Panel (d) presents the analogous graph for the new to banking strata.

Figure 3: Default and revenue by months with the credit card



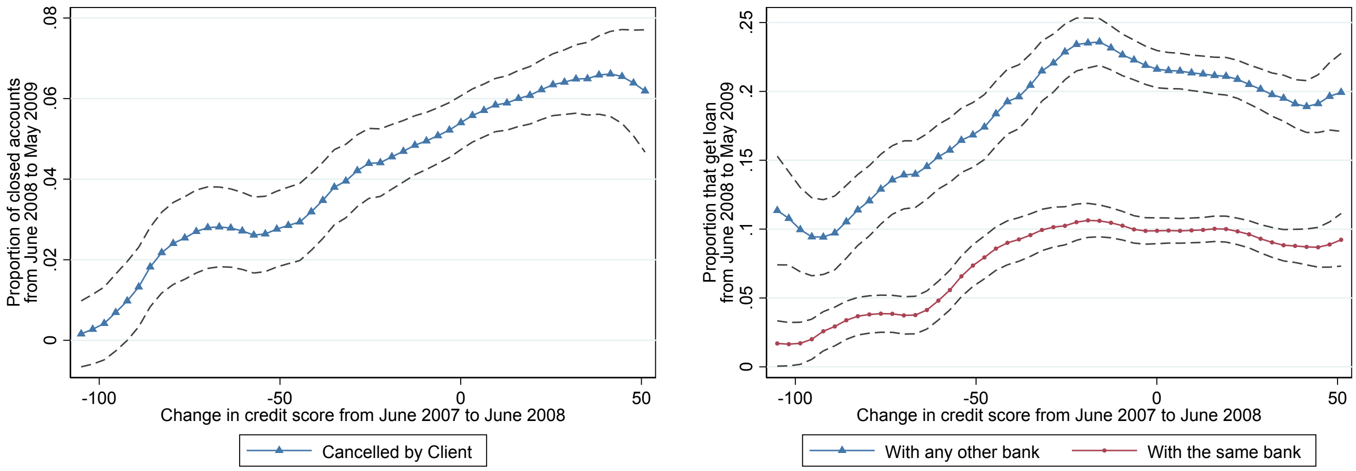
(a) Default and revenue by months with the credit card



(b) Tenure at CB and future default, CB sample

Notes: Panel (a) plots default (in dark blue) and revenue (in light red) during the experiment on the study card for three different strata defined by the length of time the borrower had had the study card prior to the start of the experiment (tenure). We restrict to cardholders from the control group of the experimental sample. The blue bars represent the proportion of cardholders who defaulted at any point in time between the beginning and end of the experimental period (March 2007 to May 2009). The red bars represent the mean NPV of revenue by card from March 2007 to May 2009. We use stratum weights and the error bars show the confidence interval for the estimate of the mean. Panel (b) uses the control group of the experimental sample and the 1 million representative sample from the Credit Bureau. The red diamonds show, for the control group, the proportion of cardholders that default by the months since the card was opened (binned into quarters because of the sample size). The blue squares and green circles repeat the sampling exercise in the credit bureau data. The green circles use all cards, whereas the blue squares restrict attention to Bank A cards that are not the same type as the study card (identified by the first 6 digits of the card). To mimic the sampling frame from the experimental sample, we restrict attention to credit cards that were opened on or before Jan 2007 that had been non-delinquent in the last 6 months prior to Jan 2007. For each of these cards, we plot the probability that the card is on default on before May 2009 (y-axis) against card tenure as of January 2007 (x-axis). The resulting graph shows that newer borrower in the formal credit market are more likely to default.

Figure 4: Client "Poaching" and the First Lender Externality

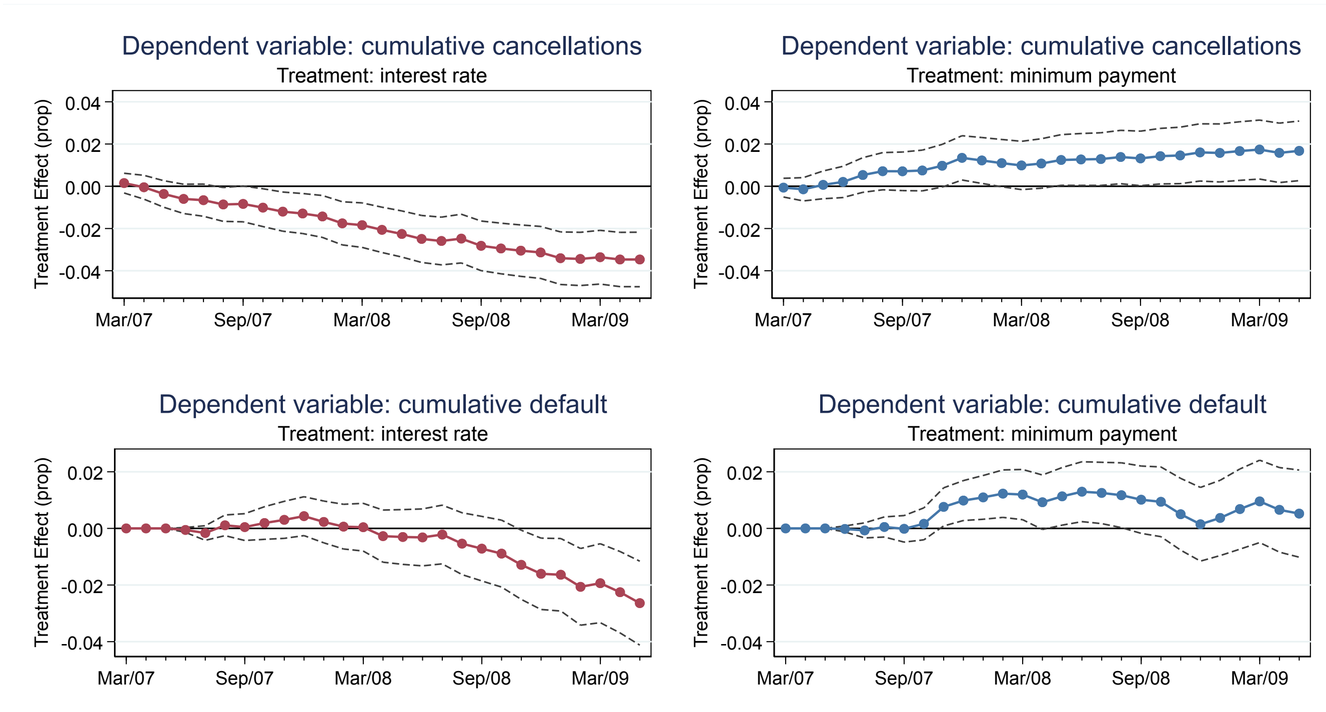


(a) Card Closings and Changes in Credit Scores

(b) New Loans and Changes in Credit Scores

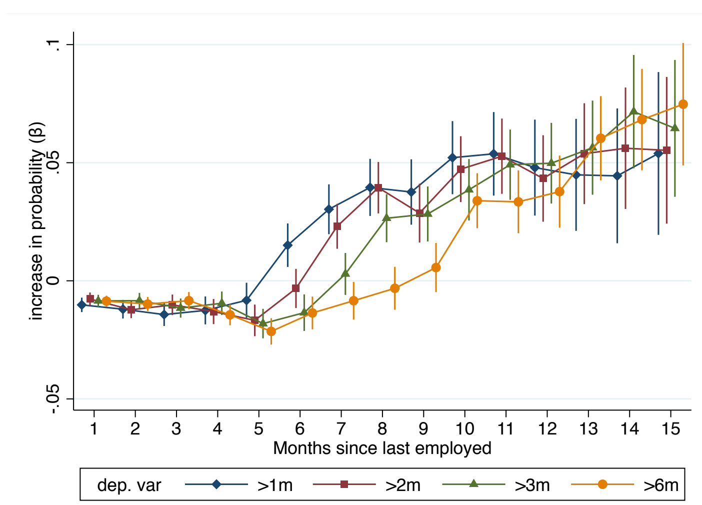
Notes: These figures use data from borrowers in the experiment who had been with the bank between six and eleven months (as of January 2007) and for whom the study card was the first card of any kind (24,146 individuals satisfy these criteria). Panel (a) shows a local polynomial kernel regression where the explanatory variable is the *change* in the credit score from June 2007 to June 2008. The dependent variable is an indicator for whether a borrower cancelled the study card between June 2008 and May 2009. Panel (b) shows two local polynomial kernel regressions where the explanatory variable in each case is the same as in panel (a). The dependent variables are binary variables for whether a borrower obtained a new card (between June 2008 and May 2009) either from Bank A (red dotted line) or from any other bank (blue line with triangles). Dashed lines represent point-wise 95% confidence intervals.

Figure 5: Default/Revocation and Client Initiated Cancellations



Notes: Figures plot monthly treatment effects. For each month we regress the outcome (default or cancellation) on all treatment and stratum indicators using sampling weights. For clarity, we only display treatment effects for a subset of treatments. Each dot corresponds to the coefficient on the treatment indicator for that month along with the point-wise 95% confidence interval. That is, each point represents the difference between the means of the plotted treatment and comparison group. In all sub-figures, the comparison group is the $(45\%, 5\%)$ group. For the graphs on the left – examining the interest rate changes and colored red– the treatment group is the $(15\%, 5\%)$ arm. For the graphs on the right – examining the minimum payment treatment and colored blue – the treatment group is the the $(45\%, 10\%)$ arm. In all sub-figures the dependent variable is either (a) cumulative cancellations (top row) or (b) cumulative default (bottom row).

Figure 6: The "Effect" of Unemployment Spells on Delinquency



Notes: The figure presents $\widehat{\beta}_k^j$ estimates from (3) estimated using OLS. The data is matched CB-IMSS data at the borrower-month level. We observe employment for 86,363 borrowers from 10/11 to 05/14 (unbalanced panel). Each line corresponds to a regression with a different measure of delinquency – delinquency is defined as j-months past due where $j \in \{1, 2, 3, 6\}$. The β coefficients are intended to capture the associational effect of unemployment spells (by duration of unemployment) on delinquency . For instance $\hat{\beta}_6^3$ is the correlation between having been unemployed for 6 consecutive months (relative to being employed during that time) and being 3 months delinquent in the current month.

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For Online Publication

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Contents

Appendix A. Data

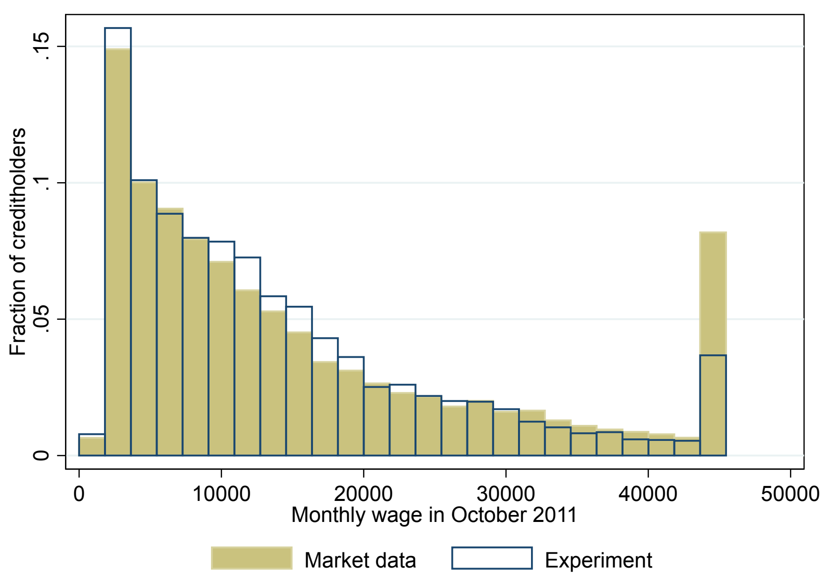
A.1 Construction of "matched" sample for summary statistics

This subsection describes how we constructed the sample from Column 4 in Table 1. First, note that, for the experimental sample in March 2007 (Column 1), Panel B shows that the mean tenure is 68 months with a standard deviation of 54 months. Using the individuals from the experimental sample in (described in Section 2) and focusing in March 2007, we construct 50-quintiles for the tenure in months of the oldest credit. Doing so gives us values $r_1, \ldots, r_{49}$ where those cardholders whose loan tenure falls between $[r_i, r_{i+1})$ are in the $(i+1)-$th quintile, and we can define r_0 and r_{50} as the min and max values for the tenure to have the first and last 50-quintile groups defined. By construction, we have the same amount of cardholders in each $[r_i, r_{i+1})$ region.

Next, we restrict to individuals in the credit bureau who had at least one credit card open in June 2010 (i.e. those shown in Column 3). We then drop any individual whose tenure in months of the oldest credit falls outside of r_0 and r_{50}. Then, for each $i = 1, \ldots, 50$ we define q_i as the number of individuals whose loan tenure in June 2010 falls in $[r_{i-1}, r_i)$, and define by $q^* = \min_i q_i$ as the region where we observe the smallest amount of individuals. In our data $q^* = 1,149$. Finally, for each $i = 1, \ldots, 50$ we randomly select (without replacement) q^* individuals whose loan tenure falls between $[r_{i-1}, r_i)$. This leaves us with a sample of 57,450 individuals shown in Column 4.

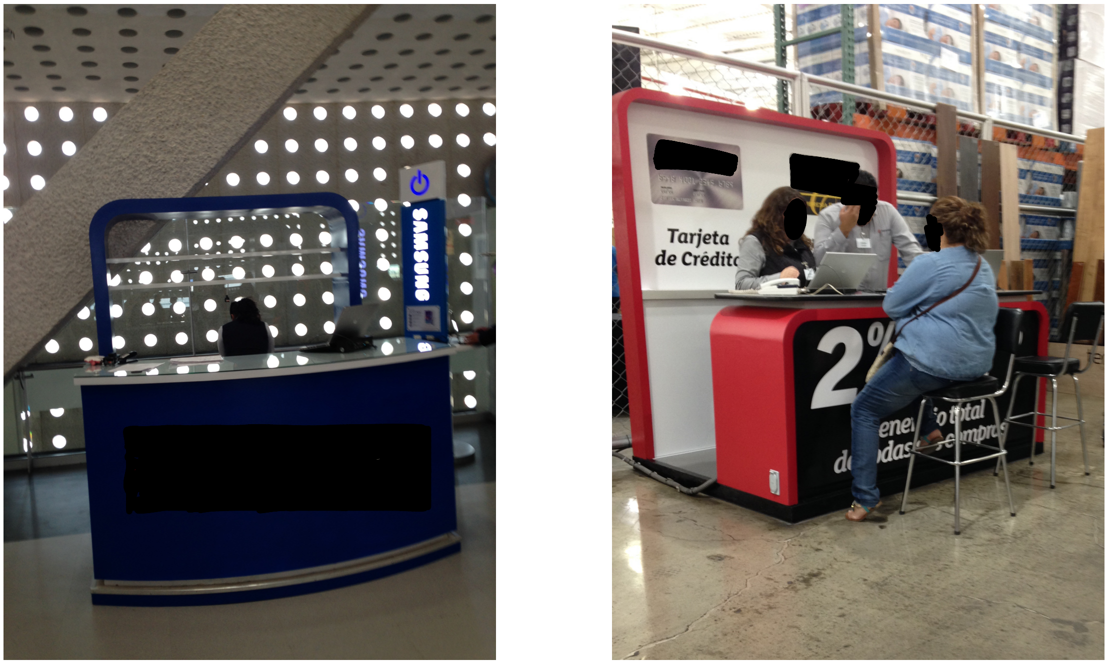
A.2 Background data

Figure OA-7: Creditholders by income in October 2011



Notes: The histogram in dark bars is the income distribution of a random sample of consumers in the credit bureau with at least one credit card. The light bars shows the corresponding distribution for the experimental sample (using sampling weights). Both histograms are censored at 45,000 pesos. Income data is from the IMSS and we were able to match 18 and 13 percent of the experimental and credit bureau random sample datasets to the IMSS.

Figure OA-8: Example of Promotional Kiosks



A.3 Data Check

We argue the following relation holds in our data:

$$\text{amount due}_{i,t} = \text{amount due}_{i,t-1} + \text{purchases}_{i,t} - \text{payments}_{i,t} + \text{fees}_{i,t} + \text{debt}_{i,t} \times \text{interest rate}_i \tag{4}$$

To test such an equation in our data we use observations with positive debt (as the coefficient on the interaction between debt and interest rate is not identified in the case when debt is zero). The following Table OA-7 summarizes our results. We find that that inferred interest rates match closely with experimental interest rates. This suggests that the debt transition equation (4) above is a good approximation to reality and that the data on purchases, debt, payments, and fees is consistent. The R^2=1 means that the formula is virtually an identity in the data.

Table OA-7: Data check

	(1)
Amount Due$_{i,t-1}$	0.996
	(0.000248)
Payments$_{i,t}$	-1.000
	(0.000363)
Purchases$_{i,t}$	1.008
	(0.00102)
15% x Debt$_{i,t}$	0.179
	(0.00343)
25% x Debt$_{i,t}$	0.279
	(0.00356)
35% x Debt$_{i,t}$	0.380
	(0.00370)
45% x Debt$_{i,t}$	0.476
	(0.00474)
Fees$_{i,t}$	0.495
	(0.00178)
R-squared	1.000
Observations	483536

Notes: This table estimates equation (4) by OLS on months with positive debt. That is we estimate the β's in the following equation: $Amount\ due_{it} = \beta_0 + \beta_1 Amount\ due_{it-1} + \beta_2 Payments_{it} + \beta_3 Purchases_{it} + \sum_k \gamma_k\ Debt_{it} \times I(r = k) + \beta_5 Fees_{it} + \epsilon_{it}$, where $k \in \{15, 25, 35, 45\}$ The coefficients are unconstrained, so a coefficient of payments =-1 for instance is a result and not an imposed constraint. The same is true of interest rates: the coefficient on $I(r = 25\%)$, i.e. γ_{25} =0.27 being close to 0.25 is a result as well.

Appendix B. Credit and Financial Inclusion

B.1 Are NTB Borrowers Credit Constrained?

Recent and limited participation in the formal credit sector raises the possibility that NTB clients continue to be credit constrained. Evidence of continuing credit constraints will provide the context for understanding the experimental treatment effects in the sequel. We test for the existence of credit constraints by examining debt responses (in the experimental sample) to increases in credit limits for the study card. If borrowers are not liquidity or credit constrained, their debt should not respond to exogenous increases in credit limits.[61] Conversely, one can view debt (or more generally consumption) responses to changes in credit limits as evidence of credit constraints.[62] Note, however, increases in borrowing following credit limit expansions for a particular card could also be consistent with the *lack* of credit constraints if borrowers replace costlier debt with cheaper debt. We can partly address this problem by examining *all* (formal sector) debt responses (using the CB data) to credit limit changes. However, since we do not observe informal borrowing, we cannot rule out the possibility of substitution away from informal loans as a response to changing formal sector credit limits.

First, we use monthly data on debt and credit limits (using the bank data for the experimental sample) to regress one month changes in debt on 12 lagged one month changes in credit limits.[63] Let Debt_{it} be the amount of debt held by card i at the end of month t, let Limit_{it} denote the credit limit for account i at the beginning of month t and X_{it} denotes a set of controls. Following the main specification in Gross and Souleles (2002) we estimate

$$\Delta\text{Debt}_{i,t} = \delta_t + \sum_{j=0}^{T} \beta_j \Delta\text{Limit}_{i,t-j} + \gamma' X_{i,t} + \epsilon_{i,t} \tag{5}$$

where Δ is the first-difference operator and β_j represents the incremental increase in debt between month $t-1$ and t associated with a one peso change in credit limit in period $t-j$. The scalar parameter $\theta \equiv \sum_{j=0}^{T} \beta_j$ then provides us with a summary measure of the long-run (T month) total effect of credit limit on debt; we report $\hat{\theta} \equiv \sum_{j=0}^{T} \hat{\beta}_j$ for each regression.[64] Because the bank evaluates a card for credit-limit changes using predetermined durations, cards that had received a credit limit change further back in the past will have a higher present probability of a credit limit change than otherwise identical cards that received a credit limit increase relatively recently. To address concerns that credit-limits change endogenously, we can therefore instrument limit changes by the time since the last limit increase, while controlling for the total number of increases in the sample period.[65]

The results are presented in Table OA-8. In all tables, we adopt the convention of three asterisks denoting significance at the .1% level, two asterisks at the 1% significance level and one asterisk at the 5% significance level. Panel A uses debit and limit data for just the study card while Panel B uses (changes in) total credit card debt (from the CB data) as the dependent variable.[66] For Panel B, since we only have annual data, we modify equation (5) and regress one year changes in debt on one year changes in credit limits (i.e $T = 2$). Column (1) presents results for the entire experimental sample while the subsequent columns estimate the model on the 9

[61] Assuming no wealth effects of the increased limits.

[62] See e.g. Deaton (1991), Carroll (1992), Gross and Souleles (2002).

[63] Covariates include time dummies, demographics, credit score in June 2007, as well as indicators for the number of credit changes during the experiment. Results were robust to including card level fixed effects.

[64] Standard errors were computed using the delta method.

[65] See Gross and Souleles (2002) for the same approach.

[66] Adding non-revolving loans would induce a mechanical effect as debt is equal to the limit for these.

different strata.

First, focusing on the entire sample we find that after 12 months a credit limit increase of 100 pesos for the study card translates into 32 pesos of additional debt (Row 1). This number remains essentially unchanged when we add controls (not reported) while the IV estimate is substantially larger (73 pesos). This propensity to consume out of increases in the credit limit is about thrice as large as the figure for the US and suggests that these Mexican borrowers are credit constrained and significantly more so than their US counterparts.[67]

This conclusion finds further support in the stratum-specific results where we document two main findings. First, longer tenure with the bank (controlling for baseline payment behavior) corresponds to lower estimated responses – for instance borrowers who have had the card for more than two years are on average less than half as responsive to changes in credit limits relative to those who have been with the bank for less than a year. Second, controlling for bank tenure, borrowers with worse baseline repayment behavior are more responsive to credit limit changes relative to borrowers with good baseline repayment behavior. For instance, borrowers who have historically paid close to the minimum amount each period are about three times (or more) as responsive to changes in credit limits relative to borrowers who have historically paid off their entire balance each month. These results suggest that a shorter tenure with the bank and poor repayment behavior are in part at least reflective of greater credit constraints.

Finally, in Panel B we estimate equation (5) for the experimental sample using (annual) credit bureau data (with $T = 0$ — i.e. we only include once lagged credit limit changes) and debt and credit limits are now *total* debt and *total* credit limit summed across all of the borrower's formal credit history. This allows us to partly address the issue of credit substitution raised earlier. The results largely confirm the previous panel although the point estimates are now, on average, smaller than earlier. Our overall conclusion from the preceding exercise is that the experimental sample's response to changes in credit limits are consistent with the existence of credit constraints and these credit constraints appear to be stronger for borrowers with shorter bank tenure and poorer repayment histories.

[67] Gross and Souleles (2002) find estimates in the range of $0.11 - 0.15$ relative to our baseline estimate of 0.32. Our estimates are also higher than those obtained by Aydin (2018) who induces experimental variation in credit card limits (in an unnamed European country) and estimates a response of 0.20 (with $T = 9$).

Table OA-8: Suggestive Evidence for Credit Constraints: Cumulative Effect of Credit Limit Changes on Debt

		6-11 months			12-23 months			24+ months		
	All	Minimum	Two +	Full	Minimum	Two +	Full	Minimum	Two +	Full
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel A. Bank A's debt (dependent variable) and Card A's credit limit (independent variable)										
Baseline estimate	0.32	0.69	0.41	0.23	0.56	0.47	0.13	0.33	0.13	0.03
	(0.04)	(0.06)	(0.04)	(0.03)	(0.05)	(0.05)	(0.02)	(0.06)	(0.03)	(0.01)
IV estimate	0.73	2.14	1.24	0.47	1.60	1.06	0.09	0.62	0.52	-0.08
	(0.14)	(0.32)	(0.28)	(0.37)	(0.28)	(0.39)	(0.09)	(0.19)	(0.27)	(0.14)
Observations	1,366,035	118,687	143,397	170,791	125,859	145,077	174,305	14,6291	155,290	186,338
Mean dependent variable	70	184	102	59	100	55	23	95	43	23
	(2292)	(3631)	(2771)	(1756)	(2639)	(2092)	(1163)	(2863)	(2174)	(1272)
Mean changes in limit	-104	-141	-115	-105	-97	-90	-77	-100	-97	-120
	(1460)	(1532)	(1452)	(1486)	(1149)	(1129)	(1177)	(1446)	(1487)	(1956)
Mean utilization	.52	.72	.59	.39	.68	.58	.4	.64	.53	.3
	(2.96)	(.34)	(3.07)	(.33)	(3)	(3.56)	(4.81)	(.35)	(3.6)	(2.82)
Median utilization	.5	.81	.58	.33	.78	.58	.3	.71	.51	.2
Panel B. Total debt across all cards (dependent variable) and total credit limit across all cards (independent variable)										
Baseline estimate	0.29	0.37	0.40	0.32	0.42	0.35	0.19	0.29	0.24	0.15
	(0.01)	(0.03)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)
IV estimate	0.45	1.17	0.76	0.51	0.84	0.45	0.37	0.38	0.34	0.24
	(0.05)	(0.12)	(0.07)	(0.04)	(0.09)	(0.06)	(0.04)	(0.07)	(0.06)	(0.04)
Observations	210,886	24,249	23,473	22,932	23,103	22,560	22,250	23,959	23,789	24,571
Mean dependent variable	598	1440	889	549	808	453	258	577	360	198
	(4402)	(7023)	(5220)	(3342)	(5045)	(3886)	(2140)	(5095)	(3769)	(2257)
Mean changes in limit	657	485	558	722	564	584	744	730	711	770
	(2228)	(2058)	(2163)	(2438)	(1726)	(1807)	(2131)	(2246)	(2285)	(2820)
Mean utilization	.45	.67	.5	.33	.62	.47	.28	.54	.42	.22
	(.38)	(.42)	(.38)	(.31)	(.39)	(.37)	(.28)	(.37)	(.35)	(.24)
Median utilization	.38	.65	.45	.24	.59	.41	.2	.51	.35	.14

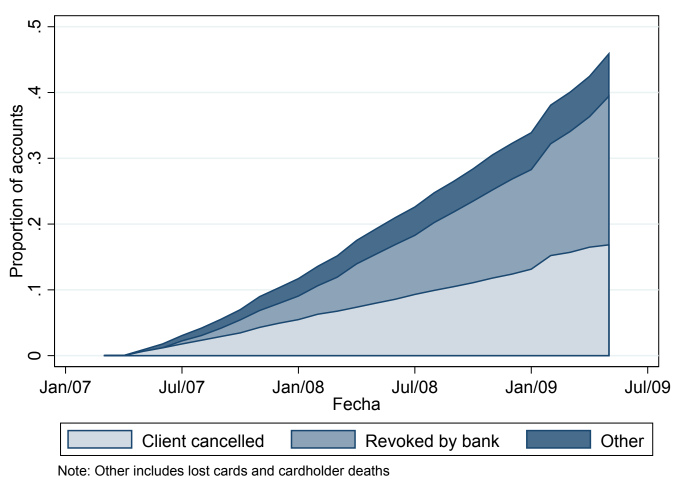
Notes: Each cell represents a separate regression and displays estimates of $\hat{\theta} \equiv \sum_{j=0}^{T} \hat{\beta}_j$ from Equation 5; all regressions include month dummies. The first row ("Baseline") in each panel displays estimates from regressions of current debt on past changes in credit limits (equation 5) estimated using OLS. The second row in each panel ("IV") displays results from estimating the equation using (dummies for the) months since the last credit limit change as instrumental variables. For the IV specification equation 5 controls directly for the total number of credit limit increases and decreases as well. Column (1) estimates include probability weights based on the size of each of the strata in the population. Columns (2)-(8) present stratum specific estimates. Both panels use the experimental sample albeit at different frequencies. Panel A presents results from estimating (5) at the monthly level with $T = 12$. The dependent variable is the total debt on the *study card* and the independent variable of interest is the credit limit for the study card. The dependent variable for Panel B is the total debt across *all cards* in the credit bureau for the experimental sample and the main independent variable is the total limit among across *all* cards. Since we only observe data at the annual level for the credit bureau, Panel B has $T = 2$. The instrument for both panels is months since last credit limit change in the study card only. Standard errors are shown in parentheses and are clustered at the individual level.

B.1.1 Variation in Credit Constraints Across Strata

A direct test of whether the strata vary systematically in terms of credit constraints is to estimate equation (5) separately for each stratum and compare the magnitudes of the estimates of θ across strata. The results are presented in Table (OA-8) and show that by this metric the stratum with the newest borrowers and the poorest repayment history (i.e. the "6-11 Month ,Min Payer" stratum) is the most credit constrained and the stratum containing the oldest borrowers with the best ex-ante repayment history (the "24+Month, Full Payer" stratum) is the least constrained. For the former stratum, a 100 peso increase in the credit limit leads to debt increase of 69 pesos twelve months later, while the corresponding figure for the latter stratum is only 3 pesos (Panel A Row 1).[68] This pattern is confirmed across the remaining seven strata: controlling for tenure with the bank, poorer repayment histories are correlated with higher estimates of θ and correspondingly, controlling for baseline repayment history, increased tenure with the bank is correlated with lower debt responses to credit limit changes.

B.2 Risks and Revenues from Financial Inclusion

Figure OA-9: Card Exits: Experimental Data
Card Closings in Experiment, by Type of Closing

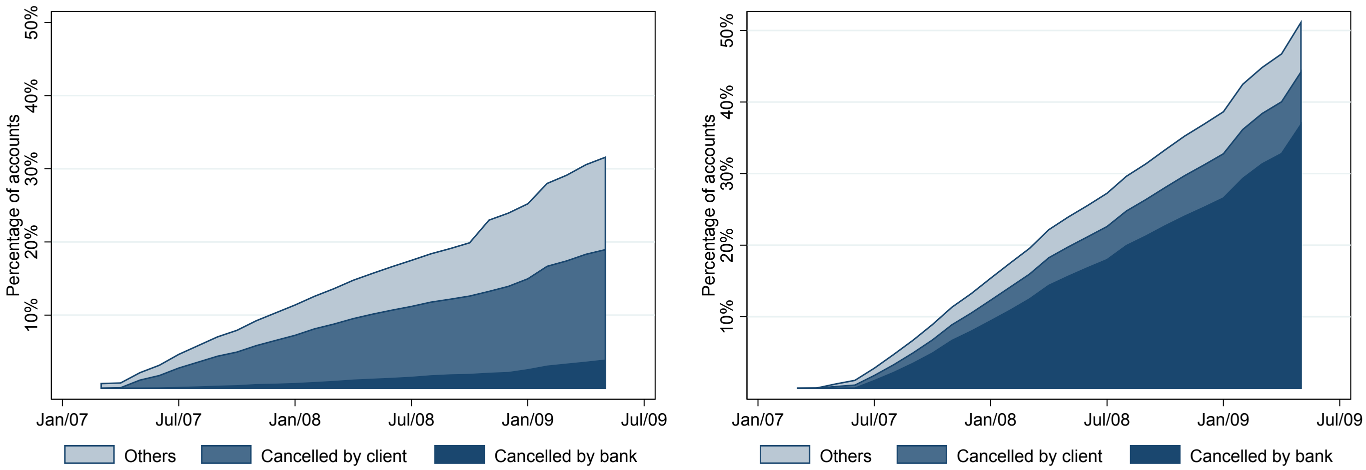


Notes: This figure plots card closing rates over the course of the experiment for the control group. Card closings are subdivided into (a) bank initiated revocations (i.e. default), (b) (borrower initiated) cancellations, and (c) other reasons (e.g. death of owner). For comparison, Figure OA-10 in the online appendix plots the analogous graphs for this figure for two different strata.

[68] The IV estimates are substantially larger for the most constrained stratum – a 214 peso increase in debt – but unchanged for the least constrained stratum.

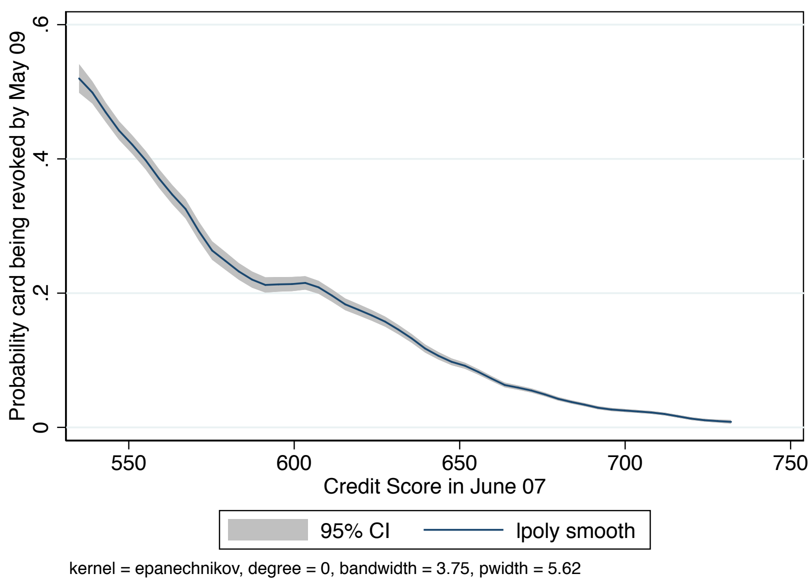
Figure OA-10: Card Exit during the Experiment: Selected Strata

(a) Full payers with 24+ months with the credit card **(b)** Min. payment payers w/ 6-11 months with the card



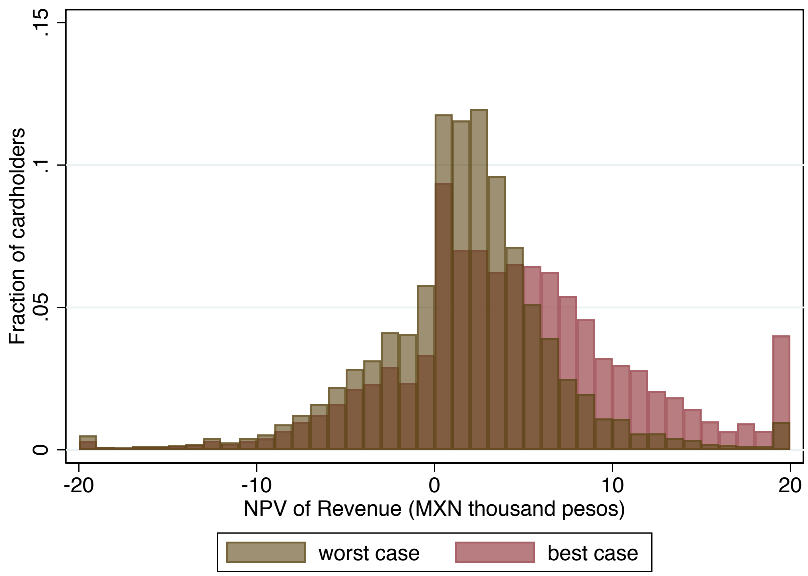
Notes: These figures plot card exit rates over the course of the experiment for the control group and for two different strata. Card closure is subdivided into whether it was a (a) default/revocation, (b) cancellation or (c) closure for some other reason (primarily lost cards or death). The aggregate exit rates (for the entire sample) are in Figure OA-9 in the main paper.

Figure OA-11: Default and Credit Scores



Notes: This figure plots a kernel regression of default (in May 2009) against credit scores (in June 2007). This forms basis of our estimate of the likelihood of default used in constructing our bank revenue measure (see Section 2.2).

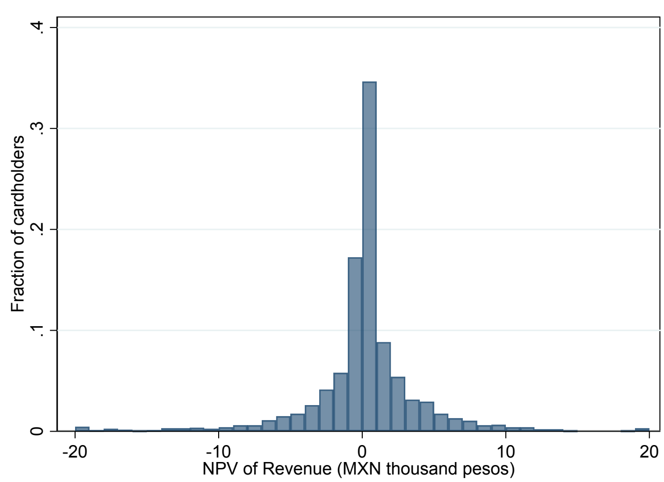
Figure OA-12: Robustness: Best and Worse Case Bounds (for α_i)



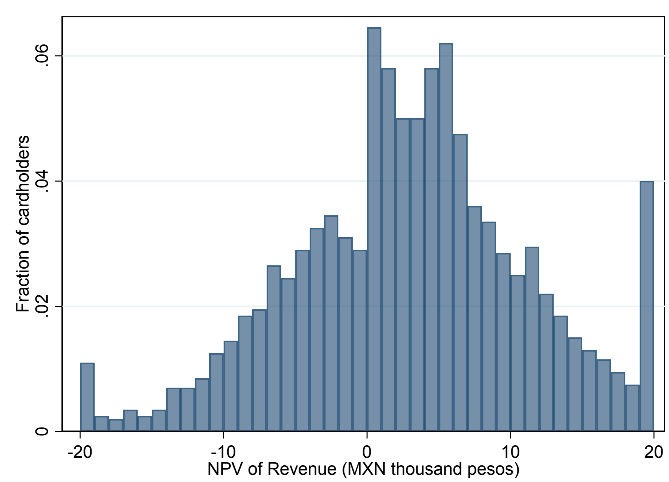
Notes: Histograms of the revenue measure computed with $\alpha_i = .05$ (low recovery rates) for all borrowers (in brown) and the best case scenario with $\alpha_i = 0.95$ (in pink). The resulting histograms do not vary significantly from the one constructed using estimated values of α_i for each borrower.

Figure OA-13: Revenue and Credit Scores: By Strata

(a) Distribution of Revenue
Full payers with 24+ months with the study card ("Full,24+M")



(b) Distribution of Revenue
Min. payment payers with 6-11 months with the study card ("Min,6-11M")



Notes: Figures (a) and (c) represent the distribution of the revenue measure for two different strata (the graph is censored at ±20,000 pesos). Figures (b) and (d) display kernel regressions of the revenue measure on credit scores (in June 2007) at the borrower level. Each kernel regression is censored at the 5th and 95th percentile of the corresponding credit score distribution in the given strata.

B.3 Predicting Default, Revenue, Cancellations and Paid Interest

We show the results from predicting revenues, default, cancellations, and which consumers end up paying interest at least once on Tables OA-9 and OA-10. The results in both Tables focus on the benchmark model, OLS and Random Forests. We use cross validation to fine tune the depth of each tree and the number of minimum samples within each leaf in the Random Forest model. For each model we predict the dependent variables (one at a time) using different sets of covariates. Each panel uses a different information set starting with a minimal set (most closely corresponding to the bank's information set when it issued the card) to progressively larger ones. Panel A includes variables measured at the time of application while Panel B uses the same variables but as observed in March 2007 (i.e. after the card was awarded) as well as the credit score in June 2007.[69] Panel C adds purchases, payments and total debt in March 2007 yielding the richest set of covariates. The most successful model, the Random Forest, has an out-of-sample R^2 of 0.06 in Panel A and 0.17 in Panel C. For revenues, the out-of-sample root MSE is 7,204 pesos for Panel A and 4,474 for Panel C, which are about the same as the intercept-only model. Performance improves somewhat in Panel C so that interactions with the bank (measured here in terms of payment, purchase and debt history) are useful indicators of revenue. We note, however, that even the best performing ML tool does not significantly out-perform the simplest intercept-only model on all measures. We also attempted to predict default and cancellations using the same covariates and strategy.

The general message from the differing information sets and methods is the same – it is quite difficult to predict which NTB borrowers will generate revenues for the bank and that adding a range of subsequent information (unavailable to the bank at the time of application such as payments, purchases and debt) does improve prediction, but only modestly.[70] A caveat is in order. We only observe successful applicants (rather than the entire applicant pool) and the prediction exercise is carried out on this (presumably positively) selected sample. This is clearly a limitation, but even this screened sample is by no means homogeneous or risk free and as we show above this risk is hard to predict. Even though the bank presumably screened as best it could, the result appears unsatisfactory – that the bank decided to shut down the study card provides further evidence of this.

[69] The variables include zip code, marital status, sex, date of birth, number of prior loans, number of prior credit cards, number of payments in the credit bureau, number of banks interacted with, payments in arrears, date of previous default and tenure with the credit bureau.

[70] However, see e.g. Björkegren and Grissen (2017) that also uses machine learning methods to predict loan default with more promising results (using borrowers' mobile phone usage patterns).

Table OA-9: Predicting Revenue and Default with Different Information Sets

	Revenue			Default		
	Benchmark	Linear Regression	Random Forest	Benchmark	Linear Regression	Random Forest
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Public information available at the moment of application						
ρ(predicted,realized)	0.00	0.04	0.28	0.00	0.45	0.45
Out of sample root MSE	7452	7444	7204	0.43	0.38	0.38
Out of sample MAE	5198	5136	4954	0.32	0.29	0.28
Out of sample R-squared	0.00	0.00	0.06	0.00	0.19	0.20
AUC - ROC Curve	-	-	-	0.50	0.79	0.79
Panel B. March 2007 public information						
ρ(predicted,realized)	0.00	0.05	0.28	0.00	0.45	0.45
Out of sample root MSE	7399	7389	7149	0.43	0.38	0.38
Out of sample MAE	5161	5096	4914	0.32	0.29	0.28
Out of sample R-squared	0.00	0.00	0.06	0.00	0.19	0.20
AUC - ROC Curve	-	-	-	0.50	0.79	0.79
Panel C. March 2007 public and private information						
ρ(predicted,realized)	0.00	0.33	0.41	0.00	0.46	0.49
Out of sample root MSE	7409	7023	6765	0.43	0.38	0.37
Out of sample MAE	5169	4695	4474	0.32	0.28	0.26
Out of sample R-squared	0.00	0.11	0.17	0.00	0.21	0.24
AUC - ROC Curve	-	-	-	0.50	0.81	0.81

Note: *MODELS:* We predict revenues and default using a range of standard machine learning methods including Support Vector Machines, Neural Networks, Boosting, and Random Forests. Model parameters are tuned using out-of-sample (OoS) cross validation. The table shows results for the Random Forest in columns (3) and (6) since it achieved the smallest out-of-sample mean squared error across all the methods mentioned above. Columns (1) and (2) present results for a constant only model and a linear regression model to provide benchmarks. *INPUTS:* The Table contains three panels, which differ in the input variables. Panel A uses variables measured *at the moment of application*. These include the state, applicant/borrower zip code, marital status, gender, date of birth, number of prior loans, number of prior credit cards, number of payments in the credit bureau, number of banks interacted with, number of payments in arrears, number of payments in arrears specifically for credit cards, length of presence (in months) in the credit bureau, the date of the last time the borrower was in arrears, and the date of the last time the borrower was in arrears for any credit card. Panel B uses all variables from Panel A, but *measured in March 2007*, i.e. after our experimental cards were awarded. We are thus easing the lender's prediction problem by including information unavailable to the lender at the time of application. In addition, we also include a the credit score (measured in June 2007) – this is our earliest credit score measure). Panel C adds further information (that was likewise unavailable to lender at the time of application): beside using all variables in Panel B, it adds purchases, payments, debt, and amount due from the study card, all measured in March 2007. *GOODNESS OF FIT:* We partition the control group into a training sample composed by cardholders who have had the experimental card for more than one year (i.e. those that belong to the 12-23M and 24+M strata and all payment behaviors) and a test sample composed by individuals who have had the experimental card for more than 6 months but less than a year (i.e. those that belong to the 6-11M strata and all payment behaviors). We estimate the 3 models (for each panel) using the training sample, and then evaluate each model by comparing its predicted predicted outcome to the true observed outcome in the test (holdout) sample. The cells above show different goodness-of-fit measures for each model and set of inputs. The first row in each panel represents the correlation between the predicted value (in the case of discrete variables we use predicted probabilities) and the realized value in the test sample. The second row presents the mean squared error, the third shows the mean absolute error, the fourth displays the "R-squared" (defined as 1 minus the ratio of the variance of the prediction errors relative to the variance of the dependent variable), and the fifth row shows the area under the ROC curve, used for indicator outcomes.

Regarding AUCs for default, They are lower than those documented in several studies and somewhat higher than those found in for credit cards in the US[71], but lower than those from loans in Australia, Japan, and Poland;[72] lower than those in the housing market in the US;[73] lower than those for credit default swaps in the US;[74]; higher than those to predict repayment using cellphone data in an unnamed South American country;[75] and higher than those for a micro-finance lender in Bosnia Herzegovina.[76]

[71] Khandani et al. (2010) shows AUCs between 0.89 to 0.95 for credit cards in the US in a similar time period to our paper

[72] Ala'Raj and Abbod (2016) reports AUCs of 0.80, 0.94, 0.93, 0.77 and 0.84 for loan data from Germany, Australia, Japan, Iran, and Poland, respectively. Abellán and Mantas (2014) reports AUCs of 0.93, 0.93 and 0.78 for loan data from Japan, Australia, and Germany, respectively.

[73] Fuster et al. (2017) reports an AUC of 0.86 for US mortgage data from 2009 to 2014.

[74] Luo et al. (2017) reports AUCs around 0.92 for credit default swaps on 2016.

[75] Björkegren and Grissen (2017) reports AUCs between 0.61 and 0.76.

[76] Van Gool et al. (2012) reports an AUC of 0.71 for a mid-sized Bosnian microlender.

Table OA-10: Predicting cancellations and paid interest

	Cancellations			Paid interest		
	Benchmark	Linear Regression	Random Forest	Benchmark	Linear Regression	Random Forest
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Public information available at the moment of application						
ρ(predicted,realized)	0.00	0.14	0.15	0.00	0.42	0.44
Out of sample root MSE	0.35	0.35	0.34	0.50	0.45	0.45
Out of sample MAE	0.27	0.26	0.26	0.50	0.42	0.42
Out of sample R-squared	0.00	0.02	0.02	0.00	0.17	0.18
AUC - ROC Curve	0.50	0.62	0.62	0.50	0.75	0.75
Panel B. March 2007 public information						
ρ(predicted,realized)	0.00	0.14	0.15	0.00	0.41	0.44
Out of sample root MSE	0.35	0.35	0.35	0.50	0.45	0.45
Out of sample MAE	0.27	0.26	0.26	0.50	0.42	0.42
Out of sample R-squared	0.00	0.02	0.02	0.00	0.17	0.18
AUC - ROC Curve	0.50	0.61	0.62	0.50	0.75	0.75
Panel C. March 2007 public and private information						
ρ(predicted,realized)	0.00	0.26	0.31	0.00	0.45	0.52
Out of sample root MSE	0.35	0.34	0.33	0.50	0.44	0.42
Out of sample MAE	0.27	0.24	0.23	0.50	0.41	0.38
Out of sample R-squared	0.00	0.07	0.10	0.00	0.20	0.27
AUC - ROC Curve	0.50	0.66	0.70	0.50	0.78	0.80

Notes: MODELS: We predict cancellations using a range of standard machine learning methods including Support Vector Machines, Neural Networks, Boosting, and Random Forests. Model parameters are tuned using out-of-sample cross validation. The table shows results for the Random Forest in column (3) since it achieved the smallest out-of-sample mean squared error across all the methods mentioned above. Columns (1) and (2) present results for a constant only model and a linear regression model as benchmarks. *INPUTS:* The Table contains three panels, which differ in the input variables. Panel A uses variables measured *at the moment of application*. These include the state, applicant/borrower zip code, marital status, gender, date of birth, number of prior loans, number of prior credit cards, number of payments in the credit bureau, number of banks interacted with, number of payments in arrears, number of payments in arrears specifically for credit cards, length of presence (in months) in the credit bureau, the date of the last time the borrower was in arrears, and the date of the last time the borrower was in arrears for any credit card. Panel B uses all variables from Panel A, but *measured in March 2007*, i.e. after our experimental cards were awarded. We are thus easing the lender's prediction problem by including information unavailable to the lender at the time of application. In addition, we also include a the credit score (measured in June 2007) – this is our earliest credit score measure). Panel C adds further information (that was likewise unavailable to lender at the time of application): beside using all variables in Panel B, it adds purchases, payments, debt, and amount due from the study card, all measured in March 2007. *GOODNESS OF FIT:* We randomly partition the control group into two samples: a training sample composed by cardholders who have had the experimental card for more than one year (i.e. those that belong to the 12-23M and 24+M strata and all payment behaviors) and a test sample composed by individuals who have had the experimental card for more than 6 months but less than a year (i.e. those that belong to the 6-11M strata and all payment behaviors). We estimate the 3 models (for each panel) using the training sample, and then evaluate each model by comparing its predicted predicted outcome to the true observed outcome in the test sample. The cells above show different goodness-of-fit measures for each model and set of inputs. The first row in each panel represents the correlation between the predicted value and the realized value in the test sample. The second row presents the mean squared error, the third shows the mean absolute error, the fourth displays the "R-squared" (defined as 1 minus the ratio of the variance of the prediction errors relative to the variance of the dependent variable), and the fifth row shows the area under the ROC curve, used for indicator outcomes.

B.4 The cost of business stealing

Table OA-11: Quantifying First Lender Loss

	counterfactual revenue estimated			placebo estimation on non attriters		
				predicted revenue	real revenue	bias
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A. Switcher Definition						
Closed exp. card and opened w/ other bank in +- 6m	yes	yes	yes	-	-	-
No cards opened between May/03 and Oct/06	no	yes	yes	-	-	-
Experimental card was first card	no	no	yes	-	-	-
Panel B. Estimation Results						
Individuals in switcher definition	924	365	178	200	200	200
Potential controls	17,076	17,365	17,882	9,945	9,945	9,945
Mean loss by account	4,324	3,783	4,141	6,837	6,945	-44
Confidence interval for mean loss	(4044, 4785)	(3513, 4335)	(3639, 5008)	[6126, 7522]	[6065, 7918]	[-852,519]

Notes: This table estimates the cost (to Bank A) from losing NTB clients to other banks using only the control group (18,000 borrowers). We focus on borrowers who satisfy 3 conditions. (a) They cancelled the study card during the 26 month study; (b) They opened a card with another bank within a twelve month period ($\pm$ 6 months) of cancellation; (c) They did not open any other cards between May 2003 and October 2006 (i.e. until 6 months before the experiment started); and (d) the study card was their first credit card. Columns (1) to (3) use a subset of these conditions as detailed in Panel A. The number of individuals who jointly satisfy these criteria are defined as switchers and are detailed in Panel B. For each of these individuals, we compute how much revenue they would have provided Bank A had they not switched. This counterfactual is calculated using a matching estimator (defined in Section 3.2) that pairs the switcher i that closed the study card at time t with 10 "control" clients $(j_1, \ldots, j_{10})$ from the pool of non-switchers with an active study card at t. The matching is done using the Mahalanobis distance (on a vector of observables detailed next) from the switching client i in period $t-1$. We require an exact match on stratum so we are using borrowers from the same stratum to serve as counterfactuals. The remaining matching variables are credit limit in $t-1$, purchases in $t-1$, payments in $t-1$, debt in $t-1$, and revenue from March 2007 through period $t-1$. Having construnted the counterfactuals $(j_1, \ldots, j_{10})$, we then impute as i's foregone revenue the average revenue generated by $(j_1, \ldots, j_{10})$ from t through May 2009. If any of the counterfactuals exits, their subsequent revenue is zero (following equation 1). We carry out this exercise for every switcher and present the average foregone revenue (and associated standard errors computed using sub-sampling in parentheses) of the mean in Panel B, columns (1)-(3). We use sub-sampling (Politis et al. (1999)) since the bootstrap is inconsistent for matching based es timators (see Abadie and Imbens, 2008). As detailed Panel A, the columns differ in the definition of a switcher. Columns (4) to (6) are a placebo estimation exercise to assess the validity of our estimation results. We take the 10,145 individuals from the control group who do not exit during the experiment and randomly assign 200 of them to be switchers with an artificial cancellation date randomly assigned between March 2007 and May 2009. Since we observe the true revenue for these 200 "switchers", we can use this exercise to compare the revenue from our estimation to the actual revenue for these borrowers. We repeat the placebo exercise 100 times.Column (4) shows the average predicted revenue "foregone" for those in the artificial switchers. Column (5) shows the real revenue "foregone" from the data. Column (6) shows the difference between the predicted and the true revenue. The numbers in squared brackets report the 5th and 95th percentiles out of the 100 repetitions.

Appendix C. Experiment

C.1 Experiment Details and Randomization Check

Table OA-12: Experimental Design

Panel A: Stratification

	Full-balance payer	Minimum payer	Part-balance payer	Total
6 to 11 months	18,000	18,000	18,000	54,000
12 to 23 months	18,000	18,000	18,000	54,000
24+ months	18,000	18,000	18,000	54,000
Total	54,000	54,000	54,000	162,000

Panel B: Sample Sizes for Arms Within Strata

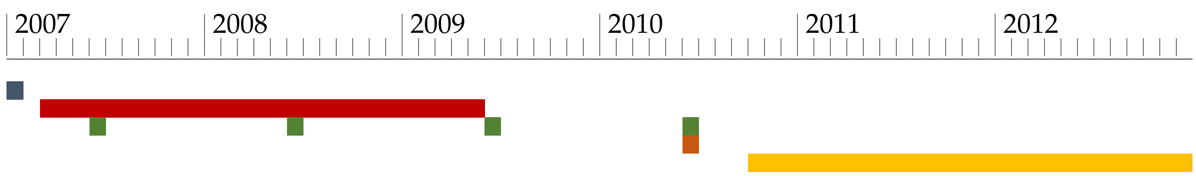
Interest Rate	Minimum payment	
	10%	*5%*
15%	2000	2000
25%	2000	2000
35%	2000	2000
45%	2000	2000
Control	2,000	

Table OA-13: Sampling weights

	Cardholder's payment behavior			Total
	Minimum payer (1)	Part-balance payer (2)	Full-balance payer (3)	(4)
Months of credit card use				
6 to 11 months	9.8	1.6	0.6	12
12 to 23 months	10.7	1.7	0.7	13
24+ months	61.5	9.8	3.8	75
Total	82	13	5	100

Figure OA-14: Timeline for the Experiment

0. **Strata information:**
   - Strata variables recorded.
1. **Bank data:**
   - Monthly card level data from 03/07 to 05/09.
2. **Credit Bureau data:**
   - Loan level data matched to experimental sample for 06/07 to 06/10, annually.
   - Loan-level data for 06/10 representative of the entire credit bureau population.
3. **Social security data:**
   - Individual-level, monthly information from 10/10 to 05/14.



Notes: This figure presents a timeline for the experiment. The data for the 9 experimental strata was recorded in January 2007. Data from the experiment is provided monthly for each card from March 2007 to May 2009. We use CB information for the experimental sample, which is provided to us in 4 snapshots: June 2007-2010. The full description of the experiment is in Section 4.1.

Table OA-14: Randomization Check - Baseline statistics for March 2007

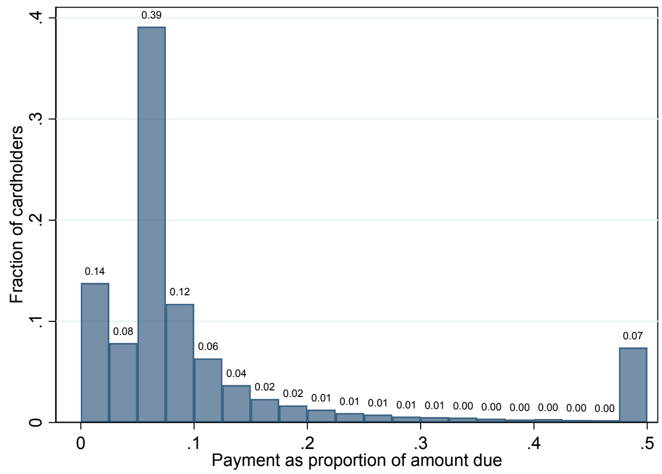
	CTR	r = 15 %		r = 25 %		r = 35 %		r = 45 %		Total	P-value	Observations
		mp = 5 %	mp = 10 %	mp = 5 %	mp = 10 %	mp = 5 %	mp = 10 %	mp = 5 %	mp = 10 %			
	(1)	(2)	(3)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Panel A. All observations												
Age	39	39	39	39	39	39	39	39	39	39	0.70	160,935
	(6)	(6)	(6)	(6)	(6)	(6)	(6)	(6)	(6)	(6)		
Female (%)	47	47	46	48	47	48	48	47	47	47	0.63	161,878
	(50)	(50)	(50)	(50)	(50)	(50)	(50)	(50)	(50)	(50)		
Married (%)	64	65	64	65	65	65	65	64	65	65	0.86	157,822
	(48)	(48)	(48)	(48)	(48)	(48)	(48)	(48)	(48)	(48)		
Debt	1,191	1,195	1,184	1,259	1,202	1,299	1,111	1,136	1,208	1,198	0.22	161,590
	(3,368)	(3,468)	(3,402)	(3,744)	(3,559)	(3,742)	(3,245)	(3,457)	(3,669)	(3,521)		
Purchases	333	332	352	344	329	352	328	351	324	338	0.43	161,590
	(1,041)	(975)	(1,145)	(1,069)	(964)	(1,016)	(1,014)	(1,056)	(909)	(1,023)		
Payments	708	694	762	722	704	704	704	698	703	711	0.77	161,590
	(1,457)	(1,292)	(1,878)	(1,541)	(1,391)	(1,359)	(1,587)	(1,302)	(1,352)	(1,473)		
Credit limit	7,814	7,867	7,937	7,853	7,927	7,999	7,739	7,925	7,848	7,879	0.61	161,590
	(6,064)	(6,003)	(6,279)	(5,948)	(6,226)	(6,269)	(5,632)	(6,403)	(6,186)	(6,117)		
Delinquent (%)	1.4	1.8	1.6	1.9	1.4	1.7	1.8	1.5	1.5	1.6	0.37	161,590
	(11.9)	(13.2)	(12.7)	(13.5)	(11.7)	(13.0)	(13.3)	(12.1)	(12.1)	(12.6)		
Panel B. Excluding attriters												
Age	39	39	39	39	39	39	39	39	39	39	0.35	96,928
	(6)	(6)	(6)	(6)	(6)	(6)	(6)	(6)	(6)	(6)		
Female (%)	46	48	47	47	48	49	49	46	47	47	0.32	97,163
	(50)	(50)	(50)	(50)	(50)	(50)	(50)	(50)	(50)	(50)		
Married (%)	65	65	66	64	65	66	66	65	66	65	0.78	94,835
	(48)	(48)	(48)	(48)	(48)	(47)	(47)	(48)	(47)	(48)		
Debt	805	728	747	811	844	871	680	713	828	780	0.13	97,248
	(2,693)	(2,764)	(2,775)	(3,099)	(3,133)	(3,027)	(2,533)	(2,591)	(3,225)	(2,882)		
Purchases	386	379	412	395	376	395	367	386	358	384	0.46	97,248
	(1,045)	(1,051)	(1,237)	(1,163)	(1,037)	(1,092)	(1,092)	(1,152)	(982)	(1,099)		
Payments	752	715	769	727	711	717	690	686	733	722	0.33	97,248
	(1,417)	(1,264)	(1,701)	(1,342)	(1,227)	(1,291)	(1,390)	(1,234)	(1,345)	(1,363)		
Credit limit	7,865	7,897	7,916	7,932	7,933	7,941	7,688	7,782	7,757	7,859	0.71	97,248
	(6,291)	(5,977)	(6,319)	(6,021)	(6,189)	(6,291)	(5,430)	(5,930)	(6,147)	(6,070)		
Delinquent (%)	0.2	0.2	0.4	0.2	0.1	0.2	0.2	0.2	0.2	0.2	0.11	97,248
	(3.9)	(4.9)	(6.2)	(4.5)	(2.9)	(5.0)	(4.6)	(4.3)	(4.9)	(4.7)		

Notes: Columns (1) to (10) tabulate the mean (standard deviation in parentheses) for the various treatment arms in the experiment. The standard error for the mean estimates can be computed by dividing the standard deviation by the (square root of the) number of individuals in each treatment arm. Time-varying variables are measured here at the beginning of the experiment. Panel A includes all individuals, whereas Panel B excludes those individuals who exit the experiment at any point. Column (11) shows the mean and standard deviations of the complete sample. Column (12) shows the p-value of a test of the null hypothesis that all means from (1)–(10) are equal.

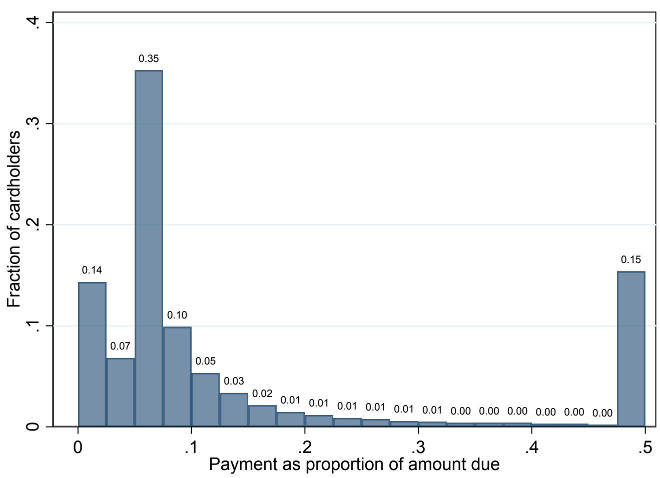
C.2 Minimum Payments Bind for a Substantial Fraction of Borrowers

Figure OA-15: Payment as a fraction of debt before the experiment

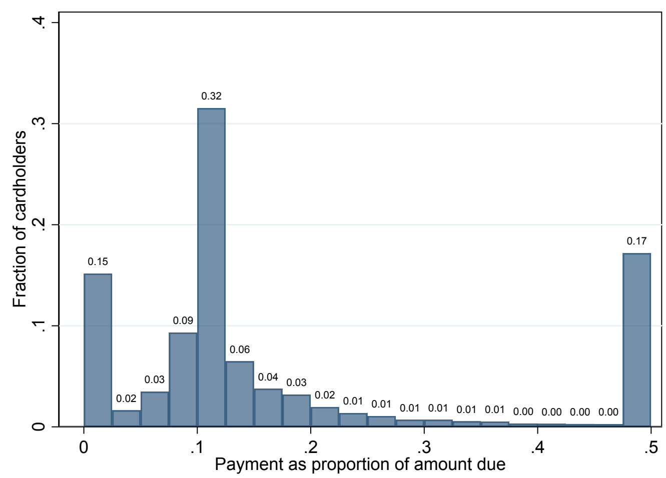
(a) Mar/07 - all treatment arms



(b) Oct/07 - treatment arms with mp = 5%



(c) Oct/07 - treatment arms with mp = 10%



Notes: We plot monthly payment divided by the amount due. In Figure (a) this is the ratio of monthly payments in April 2007 and the amount due in the March 2007 statement. In Panels (b) and (c) we examine the ratio of monthly payments in October 2007 to the amount due in the September 2007 statement. We right-censor all figures at 0.5, so the rightmost bin for each panel includes those whose payment ratio is 0.5 or higher. The leftmost bin starts at 0, and all bins have a width of 0.25. The number above each bin represents the fraction of cardholders in the given bin. The variable in the x-axis is only an approximation to the minimum payment since the minimum payment may include some fees or discounts that we do not observe.

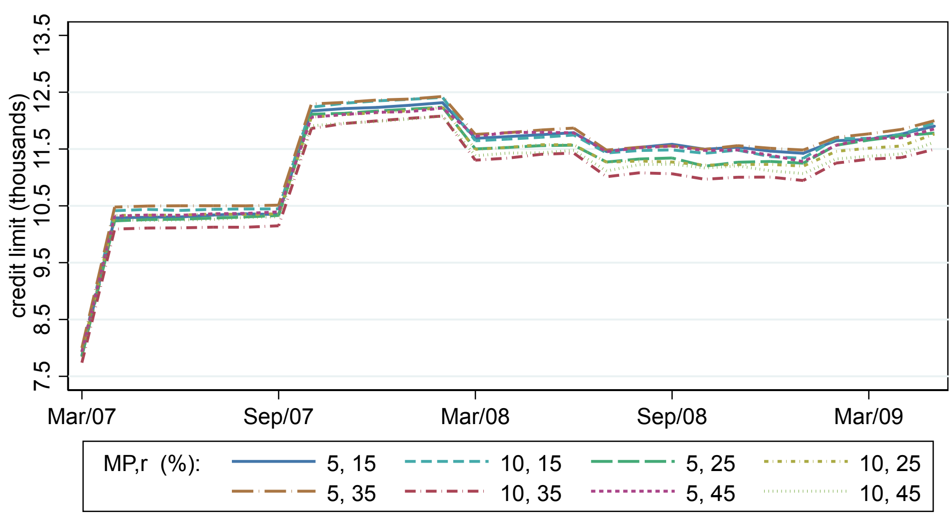
C.3 Credit Limits Are Orthogonal to Randomization

Table OA-15: Credit Limits and Treatment Arms

	Card Limit (1)	(2)
I:15% P:5%	44.791	37.083
	(210.287)	(210.174)
I:15% P:10%	41.241	43.153
	(217.952)	(217.839)
I:25% P:5%	-83.622	-89.419
	(209.235)	(209.124)
I:25% P:10%	-108.242	-102.967
	(210.609)	(210.506)
I:35% P:5%	119.108	115.921
	(220.234)	(220.135)
I:35% P:10%	-312.358	-305.073
	(208.315)	(208.206)
I:45% P:10%	-226.953	-216.079
	(208.907)	(208.802)
Constant	11778.035	11779.590
	(157.032)	(156.951)
Time fixed effects	No	Yes
Observations	3,201,085	3,201,085
p-value Treatments	0.438	0.486
p-value Strata	0.000	0.000
R-squared	0.021	0.030
Dependent Variable Mean	11157	11157

Notes: Each column represents a different regression. The dependent variable is credit limit in month t for individual i. Independent variables comprise treatment and strata indicators. Column (2) adds month fixed effects.

Figure OA-16: Credit Limits by Month by Treatment Arms

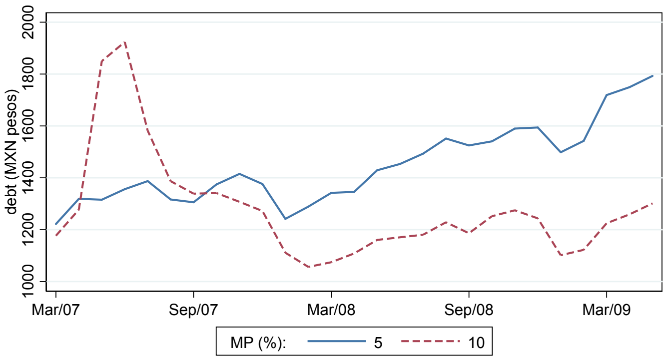


C.4 Experimental Results: Other Outcomes

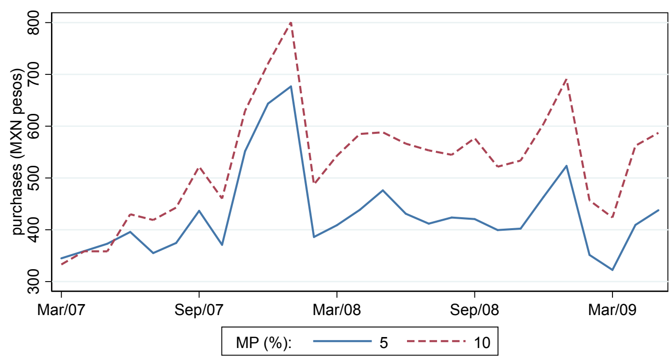
C.4.1 Experiment: Raw Data

Figure OA-17: Effect of Minimum Payment Variations

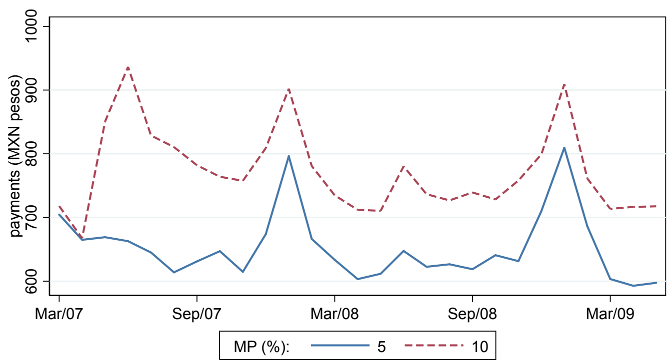
(a) Interest paying debt



(b) Purchases



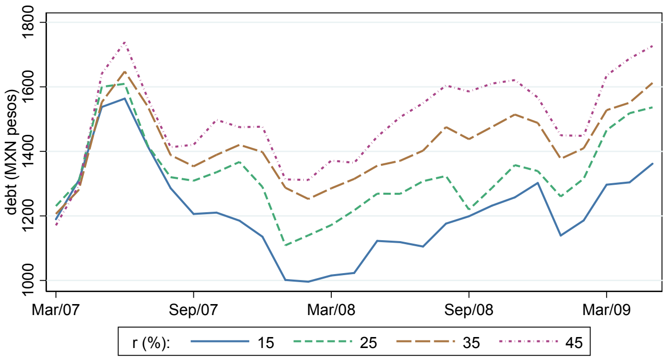
(c) Payments



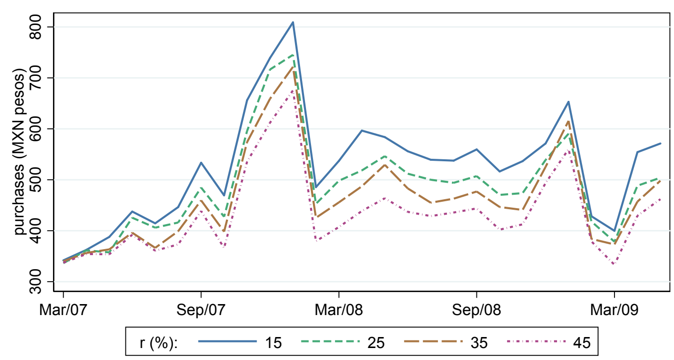
Note: The figures plot different outcomes over time separately for borrowers in the 5% and 10% minimum payment arms (pooling over the interest rate arms).

Figure OA-18: Effect of Interest Rate Variation

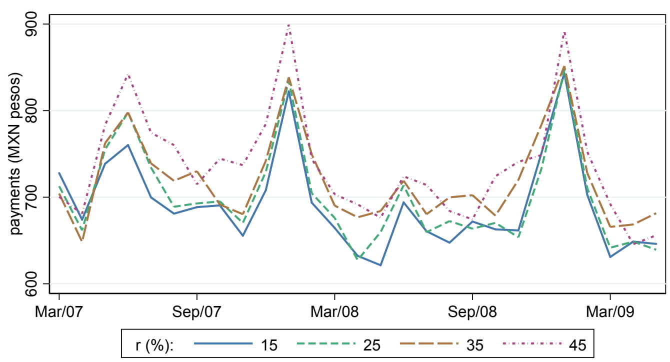
(a) Interest paying debt



(b) Purchases



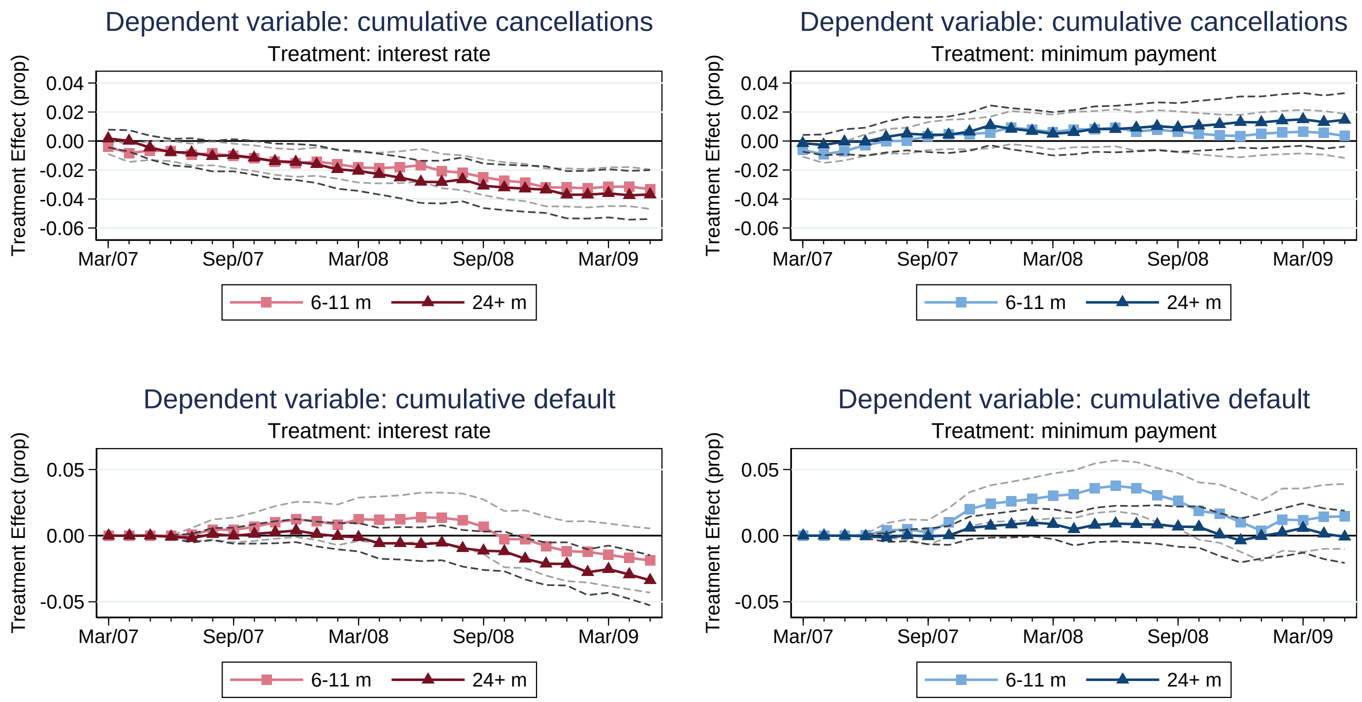
(c) Payments



Note: The figures plot different outcomes over time separately for borrowers in each of the four interest rate arms (pooling over the minimum payment arms).

C.4.2 Default heterogeneous effects

Figure OA-19: Default/Revocation and Client Initiated Cancellations: by Strata



Notes: This Figure is analogous to Figure 5 but estimated separately for two strata. The dark triangles correspond to the "24m+" strata and the light diamonds correspond to the '6-11m" strata.

C.4.3 Debt, Purchases and Payments: Methodology

In Section 3.2 card exit was an outcome of interest in itself; here we view card exit as a threat to the internal validity. Specifically, we wish to account for card exit as we examine the effect of the experimental interventions on debt, purchases and payments. We attempt to address attrition in a number of ways: First, we implement Lee (2009) and present upper and lower bounds on treatment effects that account for attrition. These bounds are generally wide but for the most part still informative. Second, we present month-by-month treatment effects and because card-exit is low in the initial months, our short-term estimates are much less affected by attrition bias. Finally, in some cases (i.e. for card cancellations) it seems plausible to impute a value of zero to outcomes in the periods after card exit. Such a strategy is useful when we are interested in the effects of the treatment on the outcome without distinguishing between the extensive and intensive margins.

We present both short-term (at the six month horizon) as well as long-term effects (after 26 months at the end of the experiment). We also present month-by-month treatment effects for each of the 26 months of the experiment.[77] In addition, when useful, we also examine treatment effect heterogeneity by presenting stratum-specific treatment effects for three strata – (a) the "Full, 24M+" stratum comprising borrowers who had been with the bank for at least 24 months before January 2007 and had always paid their bills in full (4% of the population) (b) the "Min, 6-11M" stratum consisting of borrowers who had been with the bank for less than a year before January 2007 and had the poorest repayment history[78] (10% of the population) and(c) the

[77]These are currently presented in graphical form. Tables available upon request.
[78]viz. their average payments prior to January 2007 were less than 1.5 times the average minimum payments during this period.

"Min, 24M+" stratum comprising the longest term borrowers in the poorest repayment category (62% of the population and the largest stratum).

For each estimand we present point estimates and account for attrition using bounds. We view attrition in two distinct ways and thus provide two sets of bounds – first, we consider all card exits regardless of reason (i.e. cancellations, revocations and the other category) as attrition. Second, we set all post-exit outcomes for card cancellers to zero and only consider the defaulters and other category of card exits to be attriters. The latter strategy is arguably justified if we are willing to conflate treatment effects on the extensive and intensive margins. Further, since card cancellers have chosen to set purchases, payments and debt to zero by exiting the system one can plausibly set those outcomes to zero for cancellers rather than missing.[79]

We estimate the full set of treatment effects in the tables but to simplify exposition we focus on only two contrasts in the discussion here: (a) The effect of an interest rate decrease from 45% to 15% for borrowers with a minimum payment of 5% (the $(45\%, 5\%)$ arm vs the $(15\%, 5\%)$ arm). (b) The effect of a minimum payment increase from 5% to 10% for borrowers who faced an APR of 45% (the $(45\%, 5\%)$ arm vs the $(45\%, 10\%)$ arm).

Treatment effects for other arms are provided in some cases and the full set of results are available on request. For both the short- and long-run results we estimate regressions of the form

$$Y_i = \sum_{j=1}^{7} \beta_j T_{ji} + \sum_{s=1}^{9} \delta_s S_{ji} + \epsilon_i \tag{6}$$

where Y_i is the outcome measured either six months after the experiment began or in the last month of the experiment. The $\{T_{ji}\}_{j=1}^{7}$ are treatment dummies for each of 7 intervention arms. The omitted arm is the $(MP = 5\%, r = 45\%)$ arm since it is the group with terms closest to the status quo and we do not use the control group.[80] We include strata dummies $\{S_{ji}\}_{j=1}^{9}$ and probability weights in all specifications.[81]

We also estimate month-by-month treatment effects throughout the experiment. In the interest of brevity we restrict discussion to the two main contrasts above. In particular, we estimate separately for $t = 1 \dots 26$

$$Y_{it} = \alpha_{1t} + \beta_{1t} T_i^{(15\%,5\%)} + \nu_{1it} \tag{7}$$

$$Y_{it} = \alpha_{2t} + \beta_{2t} T_i^{(45\%,10\%)} + \nu_{2it} \tag{8}$$

and in both cases the excluded arm is the $(45\%, 5\%)$ arm.[82] We then graph the estimates of β_{1t} and β_{2t} against time along with the corresponding Lee bounds in Figure OA-20. This is a parsimonious way of presenting the numerous treatment effects as well as allowing the reader to trace the evolution of the treatments over time. In most of the graphs, the bounds are typically tight for the first 6 months – reflecting limited attrition – and the point estimates at six months are of the same sign and typically the same order of magnitude as the long term (26 month) effects. Having described the general methodology we next turn to describing the effects of the interventions – first on debt and then on purchases, payments and fees.

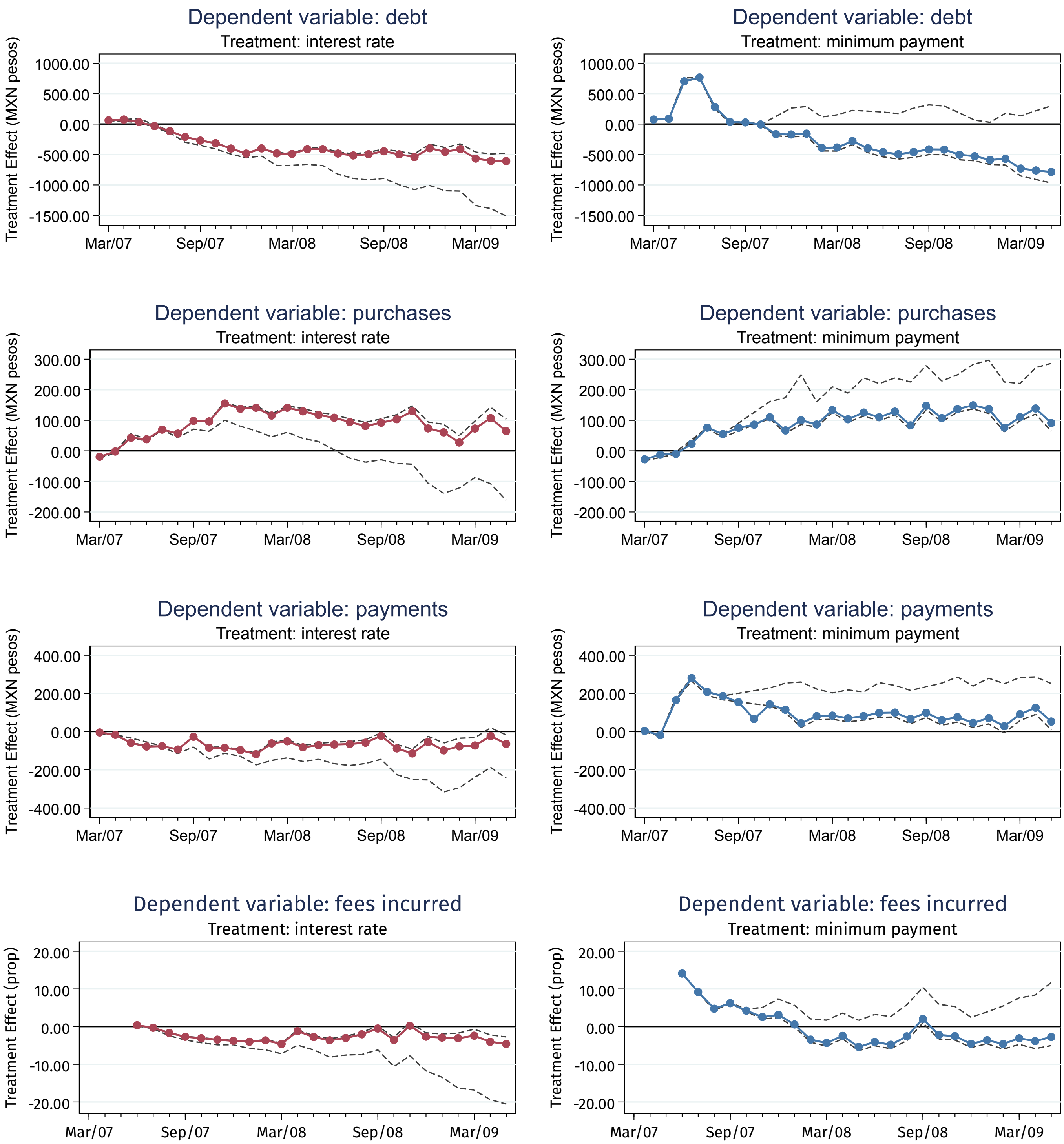
[79] A similar argument is harder to justify for defaulters.

[80] As mentioned earlier, the issue with the control arm is that we do not observe the different interest rates faced by borrowers in the arm.

[81] Alternatively we estimate treatment effects stratum-by-stratum and use the stratum weights to arrive at the treatment effect. This is equivalent to a regression of the outcome on the treatment indicator using probability weighting. The results from this exercise were very similar to those presented here and are omitted.

[82] We do not include stratum fixed effects in these regressions in order to present the corresponding Lee bounds in a straightforward manner. In the appendix we construct Lee bounds conditional on strata and use stratum weights to arrive at unconditional bounds. The results are qualitatively similar and so we focus discussion on the simpler estimator.

Figure OA-20: Treatment effect estimates



Notes: The left side of the panel shows the effect of increasing the minimum payment to 10% relative to the 5% group. The right side of the panel shows the effect of decreasing the interest rate from 45% to 15%. For each month t in the experiment, we run $y_{it} = \alpha_t + \beta_t T_i + \delta_s + \epsilon_{it}$ with treatment being either (45% IR, 10% MP – left side) or (15% IR, 5% MP – right-side) compared to the (45% IR, 5% MP) arm. Dependent variables are – total debt, monthly purchases, monthly payments, and fees. We also plot Lee bounds (Lee, 2009) for debt, purchases, and payments (though for computational reasons we do could not include strata dummies δ_s for the bounds).

C.4.4 Debt: Effect of Interest Rate Decrease

Debt responses to the interest rate changes follow an interesting and, at first-glance, a somewhat counter-intuitive pattern. Figure OA-20 show that interest rate increases result in a steady, gradual *decline* in debt (even after accounting for attrition). At the six-month mark, with relatively limited attrition, the implied elasticity bounds are relatively tight at $[0.28, 0.42]$.[83] The bounds begin to widen after the first year but remain consistently negative and even the upper bounds suggest reasonable sized treatment effects. At endline, the upper bound is a decline of 474 pesos and the lower bound is a decline of 1576 pesos. These final bounds imply a strictly positive elasticity ranging from $+0.34$ to $+1.12$ respectively. Replacing missing values with zeros for card cancellers provides similar results though the upper bound is now tighter at $+0.74$. These results suggest a robust, negative effect of interest rate reductions on total debt.[84]

The treatment effects for the other intermediate treatment arms are in line with these results. We compare debt for the $(45, 5)$ group to the $(r, 5)$ group where $r \in \{25, 35\}$ and debt in the $(45, 10)$ group to the $(r, 10)$ group where $r \in \{15, 25, 35\}$. The five ITT estimates are all comparable to the estimate above.[85] The implied elasticities of debt with respect to the interest rate from the five other ITT estimates thus are also in line with the elasticities from the primary contrast.[86]

The negative effect of interest rate declines on debt seems counter-intuitive since borrowers appear to respond to price (interest rate) declines by decreasing quantities (debt). We explore this further by examining the effect of interest rates on purchases, payments and fees which together mechanically determine debt. In Appendix C.4.6 and C.4.8 we establish three facts about these outcomes. First, interest rate declines have inconclusive effects on purchases with the Lee bounds for the long-term effect being a relatively wide $[-0.38, +0.25]$.[87] Second, monthly payments declined modestly in response to the interest rate decreases with the long-term bounds estimated to be $[+0.04, +0.39]$.[88] Third, interest rate declines have a modest negative effect on fees (the Lee bounds for the implied elasticity are $[+0.15, +1.22]$).

Jointly, these facts suggest that the relatively large negative debt response to interest rate declines arises from the fact that lower interest rates result in debt outstanding being compounded at a correspondingly lower rate.[89] This decline more than offsets any increase in purchases as well as the decline in monthly payments observed earlier. To summarize, there is a fairly robust, though moderate, decline in total debt outstanding as a result of the interest rate decrease.

C.4.5 Debt: Effect of Minimum Payments

Debt response to the minimum payment increase follows an interesting pattern. Figure OA-20 show that debt increases markedly in the third and fourth month of the experiment, increasing by almost 750 pesos by June 2007. However, there is an similarly precipitous decline soon after with the increase being wiped out by

[83] Recall that the interest rate manipulation envisaged here is a decline from 45% to 15% so a resultant decrease in debt will result in a positive elasticity.

[84] Other papers examining examining debt responses to interest rate variation are Karlan and Zinman (2017), Attanasio et al. (2008) and Dehejia et al. (2012) who estimate debt elasticities in Mexico, the United States, and Bangladesh respectively. In all these papers declines in interest rates are associated with increases in debt though the magnitudes vary considerably. Attanasio et al. (2008) cannot reject that the elasticity is zero while the three-year elasticity for Karlan and Zinman (2017) is -2.9; Dehejia et al. (2012) provide estimates in the range of $[-0.73. - 1.04]$.

[85] For the $(45, 5)$ vs the $(15, 5)$ arm.

[86] Figure OA-21 shows the variation in the treatment effects across strata. Debt for the stratum ex-ante least likely to be liquidity constrained – the "Full,24M+" borrowers– does not respond at all to the changes in interest rates while the effects are strongest for the stratum ex-ante most likely to be liquidity constrained – the "Min,12M–" borrowers.

[87] The short-term effects have tighter bounds of $[-0.38, -0.18]$ that suggest modest increases in purchases. More details are in Table OA-17.

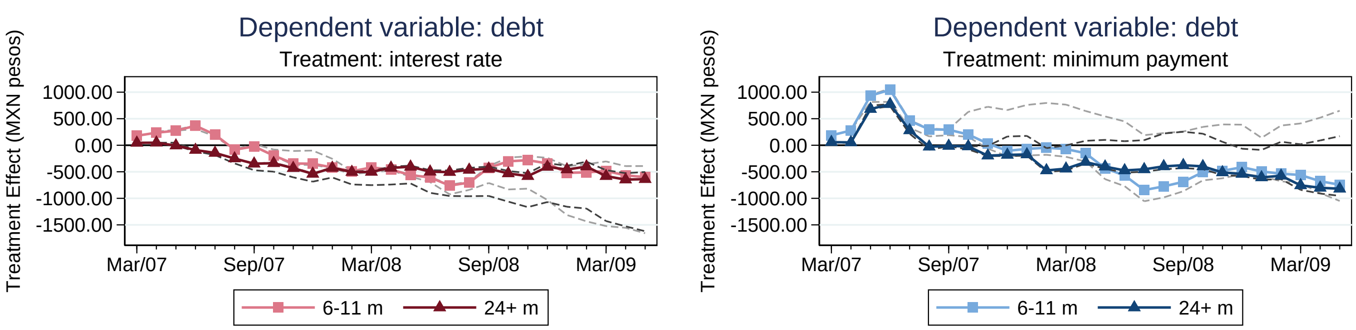
[88] Bounds for the short-term are qualitatively similar at $[+0.06, +0.24]$. See Table OA-18 for more details.

[89] By large we mean relative to the purchases, payments and fees responses.

September so that the six-month effects are very small – the bounds for the implied elasticities are quite small at $[0.02, 0.08]$.

Part of the increase in debt in the first months of the experiment appears to arise from late payment fees.[90] Following that, debt decreases gradually for the rest of the experiment though the Lee bounds become increasingly wide so that by the end of the experiment we cannot rule out declines (971 pesos or an elasticity of -0.46) or increases (326 pesos or an elasticity of +0.15). In the case of debt, imputing a value of zero for all cancellers is a particularly reasonable approach if policy makers are interested in the overall effect of minimum payments on debt, not distinguishing between borrowers who remain with the card and accumulate (or decumulate) debt or borrowers who cancel their card and cannot by definition accumulate any more debt with the card. This approach yields qualitatively similar results and the bounds for the implied elasticity tighten on the upper end so that the new bounds are $[-0.44, -0.01]$. These results suggest that doubling the minimum payment had a statistically significant, modest effect on overall debt.

Figure OA-21: Effect on Debt: Heterogeneity Across Strata and Time



Notes: For each stratum, for each month of the experiment we regress debt on a treatment indicator. Each point (triangle or diamond) corresponds to the coefficient on treatment for that month along with point-wise Lee bounds. For simplicity the comparison group here is the $(45\%, 5\%)$ arm and the comparison group for the interest rate change is the $(15\%, 5\%)$ arm; the comparison arm for the minimum payment inrease is the $(45\%, 10\%)$ arm. Each line corresponds to a different stratum. The dark triangles (red or blue) correspond to the "24+ months" stratum and the light diamonds (red or blue) correspond to the "6-11 months" stratum.

Examining heterogeneity in the treatment effects by strata (see Figure OA-21) yields similar results as above and we omit the discussion here. To conclude, doubling the minimum payment led to a long-term decrease in debt though the elasticities are probably smaller than those anticipated by policy-makers.

[90]The late payment fee is 350 pesos for any payment less than the minimum required payment. We summarize the long term effects of fees in Table OA-19 and note that most of the increases in fees occurred in in the first few months of the experiment. Unfortunately, we do not have information on fees for the first three months of the experiment.

Table OA-16: Treatment Effects on Debt with Bank A

	Standard Outcome			Deflated by Amount Due in $t-1$		Selected Strata (May/09)		
	Sep/07 (1)	May/09 (2)	May/09 w/zeros (3)	Sep/07 (4)	May/09 (5)	Min.Pay,6-11M (6)	Full Pay,24+M (7)	Min.Pay, 24+ M (8)
r = 15, MP = 5	-271.306	-602.941	-419.136	-0.016	-0.021	-632.527	-59.693	-684.244
	(82.179)	(62.698)	(40.367)	(0.006)	(0.006)	(254.239)	(80.647)	(189.418)
r = 15, MP = 10	-131.825	-908.277	-726.710	0.004	-0.008	-1.3e+03	-98.833	-968.263
	(37.038)	(74.059)	(59.372)	(0.003)	(0.002)	(246.008)	(78.507)	(181.610)
r = 25, MP = 5	-123.728	-318.241	-199.647	-0.002	-0.012	-160.146	-33.820	-326.770
	(9.845)	(26.464)	(26.283)	(0.001)	(0.003)	(271.326)	(91.550)	(212.753)
r = 25, MP = 10	-76.255	-860.327	-704.486	0.008	-0.001	-1.1e+03	-179.102	-924.682
	(13.474)	(60.496)	(49.118)	(0.003)	(0.002)	(251.472)	(71.428)	(181.226)
r = 35, MP = 5	-14.085	-332.818	-228.272	0.010	-0.011	-98.670	-70.836	-444.117
	(19.275)	(85.630)	(61.025)	(0.005)	(0.005)	(269.839)	(88.442)	(196.150)
r = 35, MP = 10	-95.723	-680.189	-556.369	0.004	-0.009	-1.0e+03	52.546	-723.580
	(23.358)	(57.504)	(44.952)	(0.002)	(0.003)	(256.232)	(97.176)	(191.343)
r = 45, MP = 10	24.243	-804.015	-699.266	0.007	-0.018	-750.309	-204.448	-908.631
	(46.438)	(78.336)	(63.856)	(0.003)	(0.006)	(263.820)	(66.603)	(183.892)
Constant (r = 45, MP = 5)	1408.794	2117.133	1735.354	0.091	0.091	3432.694	413.443	2174.629
	(218.089)	(165.882)	(139.343)	(0.012)	(0.006)	(198.896)	(58.580)	(151.527)
Observations	134,385	87,093	105,180	120,189	76,082	7,820	10,948	9,839
R-squared	0.001	0.005	0.004	0.001	0.001	0.008	0.001	0.005
Lee Bounds IR	[-397.281, -266.049]	[-1.6e+03, -473.775]	[-851.598, -388.340]	[-0.021, -0.016]	[-0.091, -0.014]	[-1.8e+03, -385.538]	[-379.885, -45.816]	[-1.8e+03, -529.896]
Lee Bounds MP	[21.827, 106.293]	[-971.173, 326.368]	[-766.284, -0.595]	[0.005, 0.027]	[-0.023, 0.027]	[-1.1e+03, 894.767]	[-205.675, -135.592]	[-1.1e+03, 244.803]
ϵ Lee Bounds IR	[0.28, 0.42]	[0.34, 1.12]	[0.34, 0.74]	[0.26, 0.35]	[0.22, 1.50]	[0.17, 0.79]	[0.17, 1.38]	[0.37, 1.23]
ϵ Lee Bounds MP	[0.02, 0.08]	[-0.46, 0.15]	[-0.44, -0.00]	[0.06, 0.29]	[-0.25, 0.29]	[-0.33, 0.26]	[-0.50, -0.33]	[-0.49, 0.11]

Columns (1) and (4) are estimated for debt 6 months after the start of the intervention and the remainder are for monthly purchases at the end of the experiment (26 months). Columns (2) and (5) present OLS results on the non-attriters and account for attrition by presenting Lee bounds (bottom 4 rows). The Lee bounds compare the (r=15, MP=5) and (r=45,MP=10) arms against the (r=45, MP=5) arm. Columns (3) and (6) redo the analysis by assigning a zero to card cancellers post exit. Columns (7),(8) and (9) estimate the endline regressions for three different strata – (a) "Min, 6-11M" borrowers who were with the bank for less than a year in January 2007 and were in the lowest payment category ;(b) "Full,24M+" who had been with the bank for more than 2 years by January 2007 and had were in the highest payment category; (c) "Min,24M+" borrowers who had been with the bank for more than 2 years by January 2007 and were in the lowest payment category. Standard errors are shown in parentheses.

C.4.6 Purchases: Effect of Interest Rates

We begin by examining the effect of the experimental variation in interest rates on purchases in Figure OA-20 and Table OA-17. Figure OA-20 shows monthly treatment effects over the course of the experiment and Table OA-17 presents short- and long-term regression results accounting for attrition. We see in Figure OA-20 that purchases in the 15% arm grew gradually (relative to the 45% arm) over the first year or so of the experiment. The Lee bounds during the first six months of the intervention are quite tight and the bounds for the implied short-term elasticity (bottom of Table OA-17 col (1)) are $[-0.38, -0.18]$ indicating modest effectsc. The long-term results, however, are inconclusive. Attrition starts to widen the bounds particularly after the first year and by the end of the experiment we cannot rule out increases in monthly purchases of 104 pesos or *declines* of 192 pesos. These imply correspondingly wide bounds on the elasticity ranging from -0.38 to +0.69 respectively (bottom of Table OA-17 col (2)). Imputing zeros to purchases for all card cancellers reduces the upper bound, but it remains positive (bottom of Table OA-17 col (3)).

The long-term elasticity bounds are wide but even at the lower bound they are substantially smaller (in absolute value) than those found in other developing country studies that examine the effect of interest rate changes on total loan quantity.[91] For instance, Karlan and Zinman (2017) compute a two year elasticity of -2.9 of loan quantity with respect to interest rate in an experiment in Mexico with *Compartemos*. Gross and Souleles (2002) estimate a still high elasticity of -1.3 for credit-card holders in the United States using observational data. Dehejia et al. (2012) use plausibly exogenous geographic variation in interest rates to estimate slightly lower but still significant elasticities in the range of $(-1.04, -0.73)$ for micro-credit borrowers in Bangladesh. Our long-term lower-bound is close to the elasticity of -0.32 documented by Karlan and Zinman (2008) for short-term individual loans in South Africa and also the approximately zero elasticity for auto-loans documented in Attanasio et al. (2008).

[91]The total quantity of loans demanded might perhaps be thought to correspond to total debt in our context. As we see below, however, debt responds *negatively* to interest rate reductions in our experiment. Therefore we benchmark our *purchase* responses to interest rate changes instead.

Table OA-17: Treatment Effects on Monthly Purchases

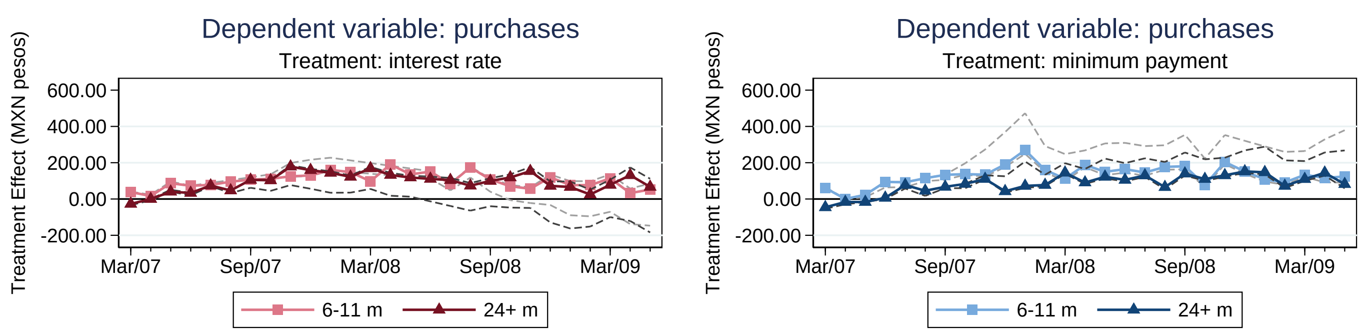
	Standard Outcome			Deflated by Amount Due in $t-1$		Selected Strata (May/09)		
	Sep/07 (1)	May/09 (2)	May/09 w/zeros (3)	Sep/07 (4)	May/09 (5)	Min.Pay,6-11M (6)	Full Pay,24+M (7)	Min.Pay, 24+ M (8)
r = 15, MP = 5	98.225	63.111	75.424	0.018	0.007	42.462	12.398	73.787
	(14.889)	(8.567)	(5.671)	(0.002)	(0.001)	(49.821)	(101.033)	(38.935)
r = 15, MP = 10	167.396	255.839	219.567	0.033	0.041	208.526	-14.717	295.633
	(16.508)	(32.453)	(24.176)	(0.003)	(0.003)	(55.631)	(100.848)	(44.391)
r = 25, MP = 5	30.758	7.482	20.499	0.008	0.003	-15.715	-5.049	9.415
	(11.336)	(5.573)	(3.705)	(0.001)	(0.001)	(50.215)	(105.771)	(33.370)
r = 25, MP = 10	136.848	177.373	145.717	0.027	0.032	134.955	-93.924	208.505
	(13.409)	(23.958)	(17.270)	(0.002)	(0.004)	(56.325)	(94.405)	(37.990)
r = 35, MP = 5	13.945	17.590	25.207	0.003	-0.001	-47.703	63.801	28.540
	(3.739)	(12.914)	(10.339)	(0.001)	(0.002)	(55.706)	(106.178)	(37.506)
r = 35, MP = 10	102.988	151.069	124.285	0.021	0.024	117.853	199.461	151.997
	(9.793)	(8.819)	(6.292)	(0.002)	(0.003)	(53.269)	(161.724)	(39.998)
r = 45, MP = 10	75.533	97.397	64.141	0.019	0.022	125.441	61.158	86.869
	(9.333)	(11.186)	(6.560)	(0.002)	(0.001)	(55.897)	(118.180)	(38.139)
Constant (r = 45, MP = 5)	401.196	414.738	339.949	0.058	0.060	353.705	1340.796	335.934
	(66.354)	(74.101)	(61.634)	(0.010)	(0.008)	(42.659)	(72.779)	(24.768)
Observations	134,385	87,093	105,180	118,732	78,735	7,820	10,948	9,839
R-squared	0.002	0.004	0.003	0.006	0.010	0.006	0.001	0.009
Lee Bounds IR	[49.029, 100.533]	[-191.779, 103.874]	[-56.132, 85.142]	[0.018, 0.019]	[-0.030, 0.013]	[-154.772, 77.406]	[-383.802, 65.484]	[-157.850, 116.218]
Lee Bounds MP	[74.845, 106.839]	[64.652, 351.981]	[51.012, 231.490]	[0.018, 0.031]	[0.017, 0.059]	[85.467, 401.779]	[57.176, 124.061]	[61.097, 284.369]
ϵ Lee Bounds IR	[-0.38, -0.18]	[-0.38, 0.69]	[-0.38, 0.25]	[-0.48, -0.47]	[-0.32, 0.76]	[-0.33, 0.66]	[-0.07, 0.43]	[-0.52, 0.70]
ϵ Lee Bounds MP	[0.19, 0.27]	[0.16, 0.85]	[0.15, 0.68]	[0.30, 0.53]	[0.29, 0.99]	[0.24, 1.14]	[0.04, 0.09]	[0.18, 0.85]

Columns (1) and (4) are estimated for monthly purchases 6 months after the start of the intervention and the remainder are for monthly payments at the end of the experiment (26 months). Columns (2),(4)–(8) drop all card exits and the Lee Bounds are more informative than the point-estimates for these columns. The Lee bounds compare the (r=15, MP=5) and (r=45,MP=10) arms against the (r=45, MP=5) arm. Column (3) assigns a zero for all outcomes for card cancellers and the resulting Lee bounds are tighter than in Column (2). Columns (6)-(8) estimate endline regressions for three different strata: (a) "Min Payers, 6-11M" borrowers who were with the bank for more than six months but less than a year in January 2007 and were in the lowest payment category ;(b) "Full Payers,24M+" borrowers who had been with the bank for more than 2 years by January 2007 and were in the highest payment category; (c) "Min Payers,24M+" borrowers who had been with the bank for more than 2 years by January 2007 and were in the lowest payment category at baseline. Standard errors are shown in parentheses.

We also examined treatment effects after normalizing purchases by the amount due each month and obtained relatively sharp results in the short-run but the effect is even weaker in the long-run. At the six-month mark, the bounds on fraction purchased is fairly tight around .018 (relative to a comparison group fraction of .06) while in the long run the bounds include zero and and are consistent with both small increases and significant declines in purchases.

In summary, the effect of interest rate reductions on purchases appears to be relatively small relative to the previous literature.

Figure OA-22: Effect on Purchases: Heterogeneity Across Strata and Time



For each stratum, for each month in the experiment we regress purchases on a treatment dummy. Each dot corresponds to the coefficient on the treatment dummy for that month along with point-wise Lee bounds. For simplicity the comparison group here is the $(45\%, 5\%)$ ground and the treatment group for the interest rate change is the $(15\%, 5\%)$ ground and the $(45\%, 10\%)$ group for the minimum payment intervention. Each line corresponds to a different stratum. The dark triangles correspond to the "24+ months" stratum and the light diamonds correspond to the "6-11 months" stratum.

C.4.7 Purchases: Effects of Minimum Payments

Doubling the minimum payment led to an *increase* in monthly purchases. Figure OA-20 shows that purchases increase gradually over the first six months of the experiment after which there appears to be no systematic increase. The short-term effect of the raise in payment requirements increased purchases by about 75 pesos per month, with the Lee bounds being relatively tight at$[75, 107]$, and the corresponding elasticity bounds are similarly tight at $[.19, .27]$ suggesting a modest positive effect.

This point estimate remains more or less stable over the remainder of the experiment even though attrition increases and the bounds start to widen. The lower Lee bound at the end of the experiment is 65 pesos and the upper Lee bound is 352 pesos – implying lower and upper bounds on the elasticities of 0.16 and 0.85 respectively. We obtain broadly similar results if we impute zeros to all cancellations with the only significant change being that the upper Lee bound reduces to 0.68.

The increase in purchases is somewhat unexpected. In principle, it could arise from higher payments easing borrowers credit lines. However, this is not the case since the point estimates and bounds are very similar when we restrict attention to borrowers who are at less than 50% of their credit limit.[92] Alternatively, since higher minimum payments imply, ceteris paribus, a decrease in debt, the increase in purchases may reflect changes in borrower behavior as a result of reduced debt. This argument implies that the effect of minimum payments on purchases should be higher for borrowers who see larger reductions in debt. We explore this implication by

[92]Results available upon request.

examining the changes in purchases across the various interest rate arms keeping the required payment fixed at 10%.

Finally, as expected, the "Full,24M+" stratum is largely unaffected by the minimum payment increase throughout the intervention while the effect is stronger for the "Min,12M–" stratum and the bounds for the implied elasticities are consistent with both modest (0.24) and substantive (1.14) effects. Finally, we also normalized monthly purchases by expressing purchases as a fraction of amount due (cols (4) and (5) of Table OA-17) and the results were similar to the ones described above so we omit a discussion. To summarize, monthly purchases rose modestly but persistently and (statistically) significantly for borrowers who were in the higher minimum payment arm.

C.4.8 Payments: Effect of Interest Rates

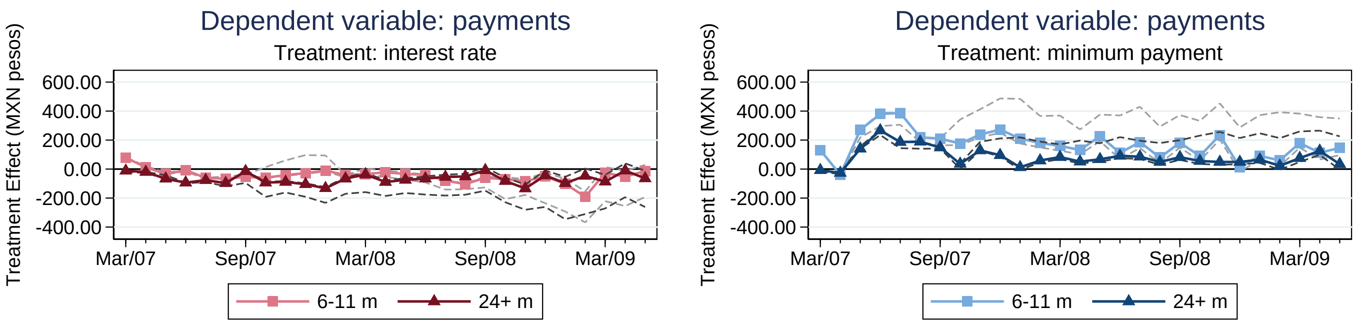
Figure OA-20 presents the Lee bounds along with the point estimates from equation (8) for each month in the experiment. We see that there is a gradual decline in monthly payments during the first six months and the bounds at the six-month mark are $[-103, -24]$ pesos with implied elasticity bounds of $[.06, .24]$ suggesting relatively modest *declines* in payments.

The upper bound remains relatively stable over the remainder of experiment but the lower bound begins to widen in the last months of 2007 and by the end of the experiment the data is consistent with both small (17 pesos) and substantial (267 pesos) declines in monthly payments. These final bounds imply elasticities of monthly payments with respect to interest rates ranging from 0.04 to 0.64 respectively. Estimating the long-term effects after setting monthly payments to zero for cancelled cards tightens the upper bound for the elasticity so that the new bounds are $[0.04, 0.39]$.

The evidence then suggests that declines in interest rates led to modest, yet discernible, declines in monthly payments. The fact that monthly payments actually decreased when interest rates fell suggests that the primary channel through which the interest rate effects function is via reducing the rate at which outstanding debt is compounded.

We also explored treatment effects on payments by examining two other outcome variables – (a) a binary variable equal to 1 if the borrower paid at least 5% of the amount outstanding each month and (b) the payment expressed as a fraction of the amount outstanding each month. The results for both are consistent with the previous results and we omit the discussion.

Figure OA-23: Effect on Payments: Heterogeneity Across Strata and Time



Notes: For each stratum, for each month in the experiment we regress payments in that month on a treatment dummy. Each dot corresponds to the coefficient on the treatment dummy for that month along with point-wise Lee bounds. For simplicity the comparison group here is the $(45\%, 5\%)$ control group and the treatment group for the interest rate change is the $(15\%, 5\%)$ group and the $(45\%, 10\%)$ group for the minimum payment intervention. Each line corresponds to a different stratum. The dark red triangles correspond to the "24+ months" stratum and the light red diamonds correspond to the "6-11 months" stratum.

C.4.9 Payments: Effect of Minimum Payment

It is reasonable to expect that the most direct effect of the minimum payment intervention would be on monthly payments. Figure OA-20 documents a sharp increase in monthly payments in the treatment group in the third month of the experiment[93] (May 2007) and after a small increase in the next month there is a steady decline over the remainder of the experiment. The six month treatments effects are precisely estimated and the Lee Bounds for the implied elasticity are very tight at $[.24, .29]$ suggesting small, though robust, effects of the increase in required payments. The bounds then begin to widen considerably starting in the last months of 2007 and remain relatively wide throughout the remainder of the experiment. By the end of the experiment attrition widens the bounds considerably and the bounds for the implied elasticity, while still positive, range from 0.01 to 0.48. Imputing zero values to card cancellations provides qualitatively similar results with the upper bound tightened to 0.37. These bounds indicate that the implied effects, even at the upper bound, are relatively small in substantive terms. We also consider the effect of the treatment on monthly payments measured as a fraction of the amount due in each month. The results suggest are broadly similar to the previous analysis with the short term bounds on the elasticity being $[0.24, 0.35]$ and the long-term bounds are somewhat wider at $[.16, .58]$. The patterns of heterogeneity in treatment effects are as expected with no effects on the "Full, 24M+" stratum and larger effects for the other strata particularly the "Min,12M–" stratum though even in that case the effects are not particularly large.

[93] Initial borrower inattention is a plausible explanation for the lack of response in the first two months. In particular, we see a corresponding increase in delinquencies in the first two months of the intervention followed by a decline. Further, we see a corresponding increase in late fees as well in the first two months of the intervention.

Table OA-18: Treatment Effects on Monthly Payments

	Standard dependent variable			Deflated by amount due in $t-1$		Selected strata in May/09		
	Sep/07 (1)	May/09 (2)	May/09 w/zeros (3)	Sep/07 (4)	May/09 (5)	Min.Pay,6-11M (6)	Full Pay,24+M (7)	Min.Pay, 24+ M (8)
r = 15, MP = 5	-27.319	-65.235	-25.593	-0.003	-0.012	-13.831	-101.747	-68.754
	(11.696)	(8.418)	(7.922)	(0.001)	(0.002)	(46.611)	(101.912)	(36.571)
r = 15, MP = 10	128.597	107.577	98.989	0.031	0.028	124.880	-13.804	134.640
	(16.417)	(20.897)	(15.594)	(0.003)	(0.004)	(48.917)	(109.685)	(43.850)
r = 25, MP = 5	-23.121	-62.865	-32.318	-0.002	-0.007	-23.186	-102.743	-65.769
	(10.161)	(8.319)	(8.132)	(0.002)	(0.000)	(48.735)	(110.701)	(36.160)
r = 25, MP = 10	133.639	92.100	75.892	0.030	0.029	99.053	-74.694	100.199
	(9.086)	(9.075)	(6.674)	(0.003)	(0.003)	(49.672)	(102.718)	(38.464)
r = 35, MP = 5	23.434	10.494	24.524	0.001	-0.002	-32.696	19.624	27.748
	(5.526)	(12.718)	(13.363)	(0.001)	(0.001)	(43.325)	(111.886)	(43.193)
r = 35, MP = 10	160.415	99.379	82.046	0.034	0.026	144.575	95.171	108.133
	(19.472)	(8.184)	(8.585)	(0.004)	(0.002)	(48.355)	(161.454)	(47.500)
r = 45, MP = 10	154.539	58.212	26.703	0.029	0.026	162.784	-23.413	32.274
	(12.554)	(20.692)	(15.828)	(0.001)	(0.001)	(57.049)	(108.380)	(38.970)
Constant (r = 45, MP = 5)	637.643	627.486	514.333	0.115	0.105	530.369	1402.374	575.204
	(45.857)	(53.950)	(45.427)	(0.016)	(0.010)	(33.248)	(86.455)	(29.021)
Observations	134,385	87,093	105,180	125,152	79,612	7,820	10,948	9,839
R-squared	0.003	0.003	0.002	0.008	0.013	0.005	0.000	0.005
Lee Bounds IR	[-102.895, -24.498]	[-266.502, -17.273]	[-134.228, -14.158]	[-0.005, -0.002]	[-0.043, -0.003]	[-196.583, 31.730]	[-400.173, -50.724]	[-247.656, -16.305]
Lee Bounds MP	[153.445, 184.136]	[8.669, 301.360]	[6.840, 192.554]	[0.028, 0.040]	[0.017, 0.061]	[102.845, 375.104]	[-27.578, 84.002]	[-11.854, 236.821]
ϵ Lee Bounds IR	[0.06, 0.24]	[0.04, 0.64]	[0.04, 0.39]	[0.03, 0.07]	[0.04, 0.62]	[-0.09, 0.56]	[0.05, 0.43]	[0.04, 0.65]
ϵ Lee Bounds MP	[0.24, 0.29]	[0.01, 0.48]	[0.01, 0.37]	[0.24, 0.35]	[0.16, 0.58]	[0.19, 0.71]	[-0.02, 0.06]	[-0.02, 0.41]

Columns (1) and (4) are estimated for monthly payments 6 months after the start of the intervention and the remainder are for monthly payments at the end of the experiment (26 months). Columns (2),(5)–(8) drop all card exits and the Lee Bounds are more informative than the point-estimates for these columns. The Lee bounds compare the (r=15, MP=5) and (r=45,MP=10) arms against the (r=45, MP=5) arm. Column (3) assigns a zero for all outcomes for card cancellers and the resulting Lee bounds are tighter than in Column (2). Columns (7)-(9) estimate endline regressions for three different strata: (a) "Min Payers, 6-11M" borrowers who were with the bank for more than six months but less than a year in January 2007 and were in the lowest payment category ;(b) "Full Payers,24M+" borrowers who had been with the bank for more than 2 years by January 2007 and were in the highest payment category; (c) "Min Payers,24M+" borrowers who had been with the bank for more than 2 years by January 2007 and were in the lowest payment category at baseline. Standard errors are shown in parentheses.

Finally, we also examine two other outcome variables – (a) a binary variable equal to 1 if the borrower paid at least 5% of the amount outstanding each month and (b) the payment expressed as a fraction of the amount outstanding each month. The results for both are consistent with the previous results and we omit the discussion.

Our overall conclusion from the results above is that a doubling of the minimum payment had a long-term positive, albeit modest, effect on monthly payments.

C.4.10 Effect on Fees

The effect of the interventions on card fees are summarized in Table OA-19 and Figure OA-24. Monthly fees averages about 28 pesos in the base group and this amount remained more or less unchanged through the 26 month study period (fees were about 4% of monthly payments).[94]

The interest rate decline has a modest negative long-term effect on average fees although the bounds are quite wide ranging from $+0.15$ to $+1.22$. In contrast, the effect of the minimum payment increase is only very imprecisely estimated with the Lee bounds covering zero and ranging from -0.19 to $+0.47$.

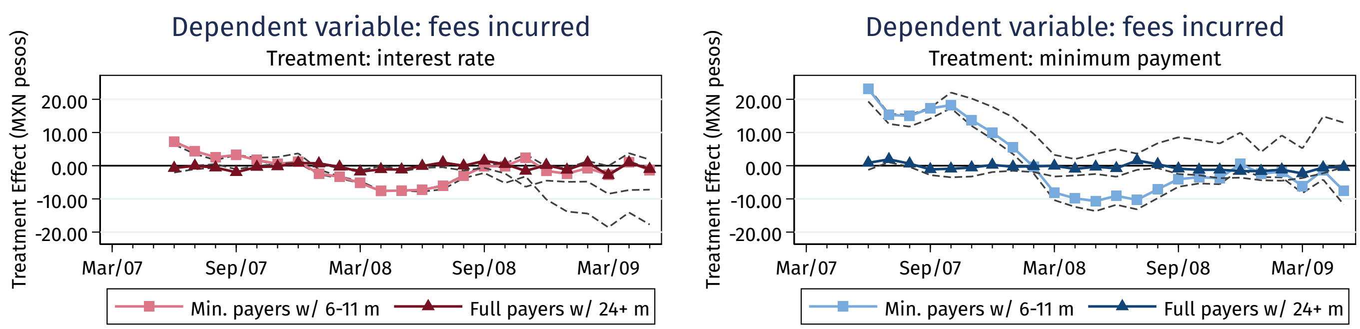
Table OA-19: Treatment Effects on Fees

	Standard dependent variable		Selected strata in May/09		
	Sep/07	May/09	Min.Pay, 6-11M	Full Pay,24+M	Min.Pay,24+M
	(1)	(2)	(3)	(4)	(5)
r = 15, MP = 5	-2.68	-4.58	-1.33	-1.01	-6.62
	(1.42)	(1.52)	(3.50)	(1.41)	(2.65)
r = 15, MP = 10	4.12	-4.12	-7.71	-0.20	-4.71
	(0.73)	(0.63)	(3.45)	(1.46)	(2.76)
r = 25, MP = 5	-2.39	-4.19	-1.80	-0.94	-5.50
	(0.99)	(1.00)	(3.51)	(1.44)	(2.68)
r = 25, MP = 10	4.68	-3.93	-4.18	-2.21	-4.30
	(0.75)	(0.32)	(3.53)	(1.35)	(2.79)
r = 35, MP = 5	-0.29	-1.60	0.65	-1.70	-3.45
	(0.53)	(1.45)	(3.56)	(1.38)	(2.76)
r = 35, MP = 10	4.25	-1.58	-3.14	2.36	-1.65
	(1.18)	(0.46)	(3.55)	(1.66)	(2.89)
r = 45, MP = 10	6.22	-2.74	-7.58	-0.41	-2.76
	(1.29)	(0.46)	(3.49)	(1.47)	(2.90)
Constant (r = 45, MP = 5)	27.96	26.44	37.04	7.22	27.14
	(0.71)	(0.63)	(2.54)	(1.04)	(2.03)
Observations	134,306	87,027	7,804	10,948	9,828
R-squared	0.002	0.001	0.001	0.001	0.001
Lee bounds r	[-3.54, -2.59]	[-21.45, -2.73]	[-17.72, 1.85]	[-7.22, -0.77]	[-25.95, -4.50]
Lee bounds MP	[6.15, 6.33]	[-5.02, 12.35]	[-11.71, 12.98]	[-0.43, 0.22]	[-4.84, 12.06]
Lee bounds ε r	[0.14, 0.19]	[0.15, 1.22]	[-0.07, 0.72]	[0.16, 1.50]	[0.25, 1.43]
Lee bounds ε MP	[0.21, 0.21]	[-0.19, 0.47]	[-0.32, 0.35]	[-0.06, 0.03]	[-0.18, 0.44]

Columns (1) is estimated for monthly fees 6 months after the start of the intervention and the remainder are for monthly fees at the end of the experiment (27 months). Columns (2)-(5) drop all card exits (so the Lee Bounds are most relevant). The Lee bounds compare (r=15, MP=5) and (r=45,MP=10) arms against the (r=45, MP=5) arm. Columns (3)-(5) estimate the endline regressions for three different strata – (a) "Min Payers, 6-11M" borrowers who were with the bank for more than six months but less than a year in January 2007 and were in the lowest payment category ;(b) "Full Payers,24M+" borrowers who had been with the bank for more than 2 years by January 2007 and were in the highest payment category; (c) "Min Payers,24M+" borrowers who had been with the bank for more than 2 years by January 2007 and were in the lowest payment category at baseline.

[94]Unfortunately, we do not have information on fees for the first three months of the experiment.

Figure OA-24: Effect on Fees: Heterogeneity Across Strata and Time



For each stratum, for each month in the experiment we regress fees on a treatment dummy. Each dot corresponds to the coefficient on the treatment dummy for that month along with point-wise Lee bounds. For simplicity the comparison group here is the $(45\%, 5\%)$ ground and the treatment group for the interest rate change is the $(15\%, 5\%)$ ground and the $(45\%, 10\%)$ group for the minimum payment intervention. Each line corresponds to a different stratum. The dark triangles correspond to the "24+ months" strata and the light diamonds correspond to the "6-11 months" strata.

C.4.11 Effect on other loans

Table OA-20: Treatment Effects on Other **Cards**: Default, Cancellations and New Loans
Existing loans by March 2007, Outcomes measured in June 2009.

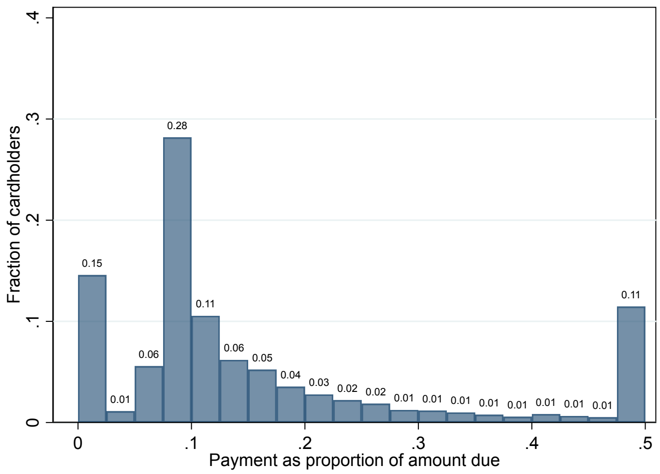
	Default			Cancellations			New card		
	Any Bank (1)	Same Bank (2)	Other Bank (3)	Any Bank (4)	Same Bank (5)	Other Bank (6)	Any Bank (7)	Same Bank (8)	Other Bank (9)
r = 15, MP = 5	0.004	0.007	-0.001	0.011	0.002	0.010	0.011	0.008	0.012
	(0.003)	(0.003)	(0.002)	(0.008)	(0.001)	(0.007)	(0.006)	(0.002)	(0.007)
r = 15, MP = 10	-0.009	-0.012	-0.002	0.016	0.005	0.011	0.011	0.009	0.010
	(0.003)	(0.005)	(0.003)	(0.009)	(0.002)	(0.007)	(0.007)	(0.003)	(0.006)
r = 25, MP = 5	0.001	-0.002	0.002	0.004	0.001	0.002	0.003	0.001	0.003
	(0.004)	(0.001)	(0.003)	(0.004)	(0.002)	(0.003)	(0.005)	(0.003)	(0.005)
r = 25, MP = 10	-0.006	-0.002	-0.006	0.003	0.003	-0.000	0.003	0.001	0.005
	(0.005)	(0.003)	(0.004)	(0.005)	(0.003)	(0.004)	(0.005)	(0.001)	(0.004)
r = 35, MP = 5	-0.003	0.000	-0.002	0.002	0.000	0.002	0.006	0.001	0.005
	(0.002)	(0.003)	(0.002)	(0.003)	(0.001)	(0.002)	(0.004)	(0.001)	(0.005)
r = 35, MP = 10	-0.001	0.001	-0.004	0.006	0.000	0.006	-0.005	-0.001	-0.002
	(0.007)	(0.004)	(0.008)	(0.004)	(0.001)	(0.003)	(0.002)	(0.003)	(0.001)
r = 45, MP = 10	-0.026	-0.016	-0.017	0.007	-0.000	0.006	-0.017	-0.008	-0.008
	(0.010)	(0.007)	(0.008)	(0.004)	(0.001)	(0.004)	(0.007)	(0.005)	(0.003)
Constant (r = 45, MP = 5)	0.498	0.222	0.428	0.096	0.018	0.082	0.424	0.137	0.366
	(0.003)	(0.003)	(0.003)	(0.005)	(0.001)	(0.003)	(0.002)	(0.001)	(0.003)
Observations	143,916	143,916	143,916	143,916	143,916	143,916	143,916	143,916	143,916
R-squared	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000

Notes: All regressions include strata dummies and use sample weights. This is the analogous table to Table 3 but restricted exclusively to credit cards. The dependent variable for Columns (1) to (3) is default (bank-initiated revocations). The dependent variable for Columns (4) to (6) are (client-initiated) cancellations. The dependent variable for Columns (7) to (9) are new loan originations. All columns exclude the experimental card. Columns (1), (4) and (7) refer to loans issued by any bank. Columns (2), (5) and (8) refer to loans issued by the same bank as the experimental card (i.e. Bank A). Columns (3), (6) and (9) refer to loans issued by any bank except for Bank A. All dependent variables restrict to loans that were issued on or before by March 2007 that remained active by March 2007. All outcomes are measured in June 2009, one month after the experiment ended.

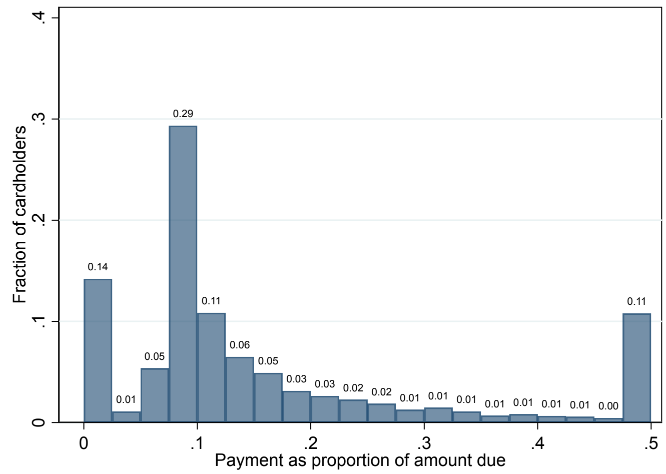
C.5 Comparison of min. payment across treatment arms 3 years after the experiment ended

Figure OA-25: Payment as a fraction of debt 3 years after the experiment

(a) Jun/12 - treatment arms with mp = 5%



(b) Jun/12 - treatment arms with mp = 10%



Notes: We plot monthly payment divided by the amount due. In Panels (a) and (b) we examine the ratio of monthly payments in June 2012 to the amount due in the May 2012 statement. We examine separately cardholders by the minimum payment group (pooling across interest rates groups) during the experimental period (Mar/07-May/09). We right-censor all figures at .5, so the rightmost bin for each panel includes those whose payment ratio is 0.5 or higher. The leftmost bin starts at 0, and all bins have a width of 0.25. The number above each bin represents the fraction of cardholders in the given bin. The variable in the x-axis is only an approximation to the minimum payment since the minimum payment may include some fees or discounts that we do not observe.

Appendix D. Habit formation

Table OA-21: Habit formation regressions

	No controls		Months with CC strata		Months + Current Terms	
	First stage	Second stage	First stage	Second stage	First stage	Second stage
	(1)	(2)	(3)	(4)	(5)	(6)
r = 15	618		616		295	
	(150)		(150)		(110)	
MP = 10	5.1	7.3	4.7	7.5	44	3.8
	(138)	(28)	(138)	(28)	(86)	(28)
Min. payer	1383	-475	1383	-478	224	-433
	(158)	(59)	(157)	(59)	(108)	(34)
MP = 10 × Min. payer	-159	32	-160	32	-26	28
	(233)	(40)	(233)	(40)	(157)	(39)
Amount due		0.097		0.097		0.14
		(0.035)		(0.036)		(0.075)
Strata FE	no	no	yes	yes	yes	yes
Current card terms	no	no	no	no	yes	yes
Dependent variable mean	6680	748	6680	748	6680	748
Observations	33,206	33,206	33,206	33,206	33,206	33,206
R-squared	0.0084	0.1683	0.0118	0.1689	0.5109	0.1780

Notes: Robust standard errors are shown in parenthesis. The sample is those cards that (i) participated in the experiment (ii) remained opened by 2010, and (iii) were assigned to either the highest or lowest interest rate groups (eg. [r = 15, MP = 5], [r = 15, MP = 10], [r = 45, MP = 5], and [r = 15, MP = 10]). Each column represents a different regression. Columns (2), (4) and (6) have as a dependent variable the amount paid on June 2010, as a function of the minimum payment that was assigned during the experiment and debt. Since debt can be endogenous, we instrument for debt using the interest rate group cardholders were assigned to. We also allow for a differential treatment effect for those in the "minimum-payment" strata. The dependent variable of Columns (1), (3) and (5) is the amount due on June 2010. Columns (1) and (2) show the regression equations without additional controls. Columns (3) and (4) add the months with credit cards strata dummies. Columns (5) and (6) add both the months with credit cards strata dummies as well as current contract terms, namely the interest rate and the required minimum payment in pesos in June 2010.

Appendix E. Comparisons

Table OA-22: Comparisons with the Literature

Paper	Outcome	Table (page)	Point Estimate	Elasticity
Karlan and Zinman (2007)	Account in Collection	3 (p.40)	-1.6	+0.27
Karlan and Zinman (2017)	Delinquency	5 (p.42)	-0.0196	+1.80
Adams et al. (2009)	Default Hazard	4 (p.28)	1.022	+2.2
Keys and Wang (2016)	Delinquency	p.4	.01	+0.20
d'Astous and Shore (2017)	Default	p.3	.04	+0.06

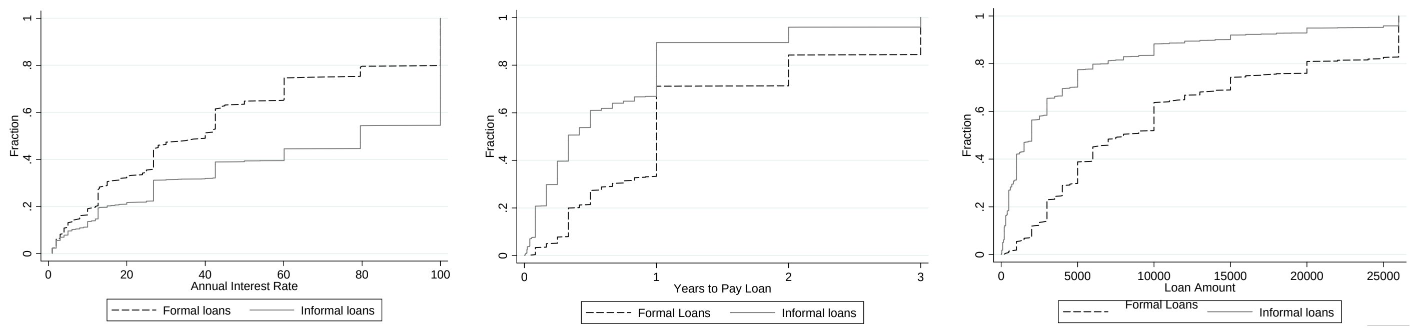
Notes: We use the working paper version of Karlan and Zinman (2009). Table 3 cols (4) and (5) for the "repayment burden effect." The table reports a decline from 13.9 to 12.3 in the percentage of accounts in collection status over a four month period. The difference between the high and the low interest rate was on average 350 basis points. We use the high risk category upper bound for the interest rate of 11.75 percent as the base rate and convert the monthly interest rates to APR to facilitate comparisons (the calculation is $(-1.6/13.9)(279/-120) = .27$). For Karlan and Zinman (2017) we use the results from Table 5 (col (4), Panel B) that delinquencies decline by 1.96 percentage points off of a control baseline of 10.5%. Low rate regions faced APRs of 80% while high rate regions faced APRs of 90%. The implied elasticity is $(-2/10)/(80-90/90) = 1.8$. Adams et al. (2009) estimate a hazard model and the hazard rate suggests that a one percent increase in the APR leads to a 2.2 percent increase in the hazard rate of default. Keys and Wang (2016) and d'Astous and Shore (2017) study changes in minimum payments while the remaining papers examine interest rate variation.

Appendix F. Mechanisms

F.1 Consequences of default

Figure OA-26: Comparison formal and informal loan market in Mexico

(a) Interest rate **(b)** Loan duration **(c)** Loan amount



Notes: The above figures compare the formal and informal credit market in Mexico using the annual interest rate (a), the loan tenure in years (b) and the loan amount in pesos (c). This data comes from ENNVIH survey reported by the INEGI on years 2002, 2005, and 2009. The lines represent the cumulative distribution of the three variables; divided between formal and informal.

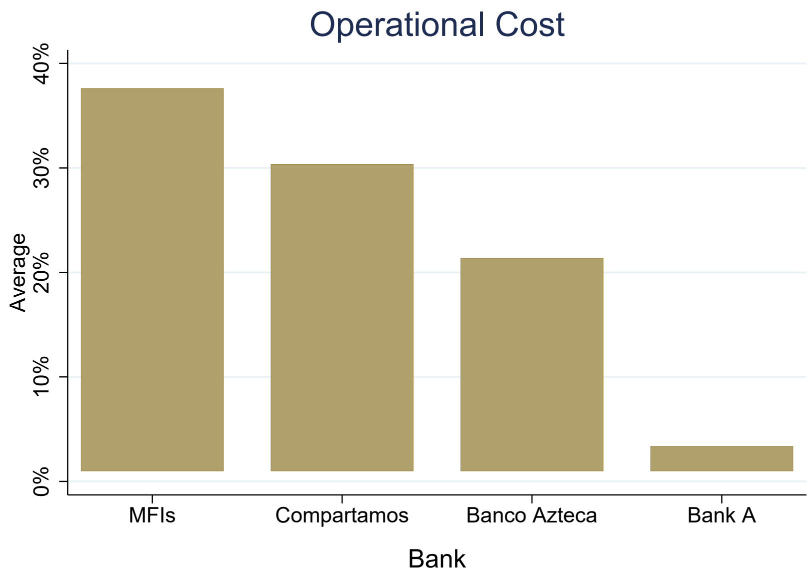
Table OA-23: Access to loans after the first delinquency

	any new loan with any bank b/se (1)	any new loan with other banks b/se (2)	any new loan with bank A b/se (3)
after first delinquency	-0.02 (0.00)	-0.02 (0.00)	-0.01 (0.00)
mean dep. var before default	0.070	0.057	0.015
Observations	354,255	354,255	354,255
R-squared	0.023	0.016	0.012

Notes: This table focuses on the sample of borrowers on the experimental sub-sample for whom the study card was the first formal sector loan product and who had been with Bank A between 6 to 11 months at the start of the experiment. We observe 55 months of data, from March/07 to Sept/11. We further restrict the sample to borrowers who defaulted in this period. This leaves us with 6,441 borrowers. For each of those borrowers we locate the first month they were delinquent (i.e. 30 days past due) on the experimental card, and create an indicator for any time period after this first delinquency I(After 1st Del for $i)_{it}$. We estimate by OLS the regression $y_{it} = \alpha_i + \gamma_t + \beta$ I(After 1st Del for $i)_{it} + \epsilon_{it}$, where y_{it} is an indicator for borrower i getting a new loan (any kind of loan) in period t with any bank (column 1), non-Bank A (column 2), or Bank A (column 3). The table reports estimated β's, as well as the mean of the dependent variable in the periods before default; β's estimates the within borrower difference of the likelihood of get new loans in periods after delinquency compared to the likelihood of getting new loans before being delinquent, for the same borrower.

Appendix G. Comparison with Compartamos and Azteca

Figure OA-27: Operational Costs (relative to Assets)



Notes: The cost ratio is defined as the ratio of administrative and promotion spending to total assets. Data is taken from the Mexican Banking Comission (CNBV) at https://portafoliodeinformacion.cnbv.gob.mx/bm1/Paginas/infosituacion.aspx (under 040-5Z-R6, indicadores financieros). We average annual figures from 2007-2009 to be consistent with the study period.