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ABSTRACT

This paper provides evidence on the difficulty of expanding access to credit through large institutions. We use detailed observational data and a large-scale countrywide experiment to examine a large bank's experience with a credit card that accounted for approximately 15% of all first-time formal sector borrowing in Mexico in 2010. Borrowers have limited credit histories and high exit-risk – a third of all study cards are defaulted on or canceled during the 26 month sample period. We use a large-scale randomized experiment on a representative sample of the bank's marginal borrowers to test whether contract terms affect default. We find that large experimental changes in interest rates and minimum payments do little to mitigate default risk. We also use detailed data on purchases and payments to construct a measure of bank revenue per card and find it is generally low and difficult to predict (using machine learning methods), perhaps explaining the bank's eventual discontinuation of the product. Finally, we show that borrowers generating a favorable credit history are much more likely to switch banks providing suggestive evidence of a lending externality. Taken together these facts highlight the difficulty of increasing financial access using large formal sector financial organizations.

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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] Perhaps unsurprisingly, developing country governments and international development institutions have made financial inclusion a key policy priority. 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.[2] In Mexico, a 2011 presidential decree established the National Council for Financial Inclusion to expand financial access to underserved populations.[3]

While the role of new and innovative organizations in expanding financial access in developing countries (e.g. micro-finance lenders) has received considerable attention,[4] much less is known about the corresponding experiences of large formal financial institutions whose scale suggests 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 financial product we study in this paper (targeted at borrowers with no or limited credit histories) had a customer base of 1.3 million alone at that time.[5] In this paper, we provide some evidence on the issues around expanding financial access via large formal financial institutions by examining a large Mexican bank's (Bank A from now on) experience of lending to borrowers with limited credit histories. We focus on uncollateralized credit card debt – the most common formal borrowing instrument in the country – from a specific card (henceforth the study card) targeted at new borrowers. In 2010, the study card accounted for approximately 15% of all first-time formal sector loan products nationwide.[6]

We begin by using a range of data to document some market facts that are relevant for understanding the challenge of financial inclusion in Mexico. First, we show that "new to banking borrowers" (NTB borrowers henceforth) appear to be credit constrained by showing that debt responds sharply to increases in credit limits using a representative sample of NTB borrowers from Bank A. Next, focusing on the study card, we show that NTB borrowers exit at high rates – about a third of our sample of NTB borrowers defaulted on or canceled their cards over the 26 month study period. We use detailed payment and purchase data to construct a measure of bank revenue per card and show that it is low and highly variable. Using a large set of observables (including those observed by the bank at the time of card issue) and machine learning methods, we can only weakly predict card revenue. These findings lead us to conclude that lending to NTB borrowers is risky

[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] See e.g. Demirgüç-Kunt and Klapper (2012), World Bank (2017).

[3] 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.

[4] See e.g. the book-length treatment in Aghion and Morduch (2005) or the more recent overview in Banerjee and Duflo (2010).

[5] 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.

[6] Authors' calculations. See also Figure 1(b).

and much of this risk is hard to predict at the time the card is issued. The bank stopped issuing the study card entirely by 2010, providing some revealed preference evidence of the importance of these issues in lending to NTB clients.

In addition, we also find evidence for an externality based barrier to financial inclusion. A lender's decision to issue a card to an NTB client and the consequent public history of card repayment behavior (via the credit bureau) confers a positive externality to subsequent potential lenders, the benefits of which are not internalized by the first lender.[7] In particular, other lenders can use the credit history with the first lender to condition their own credit decisions (indeed, the phenomenon is common enough that it is known as "poaching" internally by the bank). We find evidence consistent with this hypothesis – 28% of NTB borrowers who do not default on their first card in the first year obtain a second card with a different bank in the subsequent year whereas the corresponding figure for clients who default in the first year is 2%. This in turn reduces the first lender's incentives to lend to new potential clients since it knows that better borrowers may be subsequently "poached" away. We carry out a rough calculation to quantify this externality in terms of lost revenue for the first lender and find that it is approximately the same as the average revenue measure per card.

If screening risky borrowers is difficult ex-ante (as suggested by the foregoing), a natural next question is whether banks can use contract terms to mitigate risk ex-post. Specifically, we test whether default is mitigated by lower interest rates and higher minimum payments – key components of the card contract – using a large randomized experiment carried out by Bank A. The experiment was representative of the entire population of the bank's study card clients across the country and allocated 162,000 NTB clients to 8 treatment arms that varied annual interest rates (between {15%, 25%, 35%, 45%}) and monthly minimum payments (between 5% and 10%) for 26 months. 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 is considerable. In addition, the sampling scheme ensures that our experimental results are representative of the bank's national population of customers who held this particular card (about 1.3 million at the start of the study).

We report three findings. First, reducing interest rates by a factor of three reduces card default by 2.6 percentage points (on a base rate of 19 percent) over the 26 month experiment. The implied elasticity is $+0.20$, suggesting a limited response to even relatively large interest rate reductions.[8] Second, we find that lower interest rates reduce debt modestly – our preferred bounds for the implied elasticity ranges from $+0.3$ to $+0.7$.[9] Our conclusion is that even relatively large changes in annual interest rates have limited effects on card default and debt for NTB borrowers. These results are somewhat surprising. A positive correlation between default and interest rates (or loan

[7]This is the second of seven financial market failures enumerated in Stiglitz (1993).

[8]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 also much 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.

[9]We discuss why interest rate reductions decrease (rather than increase) debt in more detail below.

size) is often interpreted as a measure of moral hazard. In our context this suggests low levels of moral hazard in our NTB population for which the information asymmetry problem is particularly severe.

Several researchers and policy makers have argued that contract terms such as low minimum payments could lead to excessive borrowing and consequently an increased likelihood of default with negative concequences for both borrowers and the financial system.[10] Higher minimum payments could lead to lower defaults in the long-term both through the incentive effect of a lower debt overhang and also through a selection effect, as weaker borrowers exit (though the latter may generate higher default in the short term). Conversely, higher minimum payments may be welfare-reducing for borrowers whose other sources of borrowing are more expensive than the card. 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.[11] This provides some sobering evidence about the effectiveness of limiting default through increased minimum payments.[12] The experimental results as a whole then suggest that contract terms have only limited effects on reducing default risk.

We then explore, albeit speculatively, the causes and consequences of default from the borrowers perspective. We conjecture that NTB borrowers may be vulnerable to frequent, large shocks that precipitate default, though we largely lack the detailed individual level data and the identifying variation required to test this convincingly. We find that default has significant negative consequences – 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 to rely on informal credit – and we document that informal credit terms are significantly worse than the corresponding formal sector terms. Using a nationally representative sample, we find that relative to informal loans, formal loans are on average one-third cheaper, more than one and a half times larger and have a repayment period that is twice as long. Taken together, these results highlight the difficulty of expanding financial access to credit using large formal financial organizations.

In addition to the papers mentioned above, this paper connects with several strands in the liter-

[10] See e.g. Warren (2007); Bar-Gill (2003). In Mexico, concerned over the size of minimum payments and its link to indebtedness (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 interagency guidance on credit card lending that criticized minimum payments for being too low, particularly when they did even not cover the finance charges and bills accrued in a billing cycle (https://goo.gl/X8ujTi). Such prescriptions find some support in models of time-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 mimimum payments serve as an anchoring device (Stewart, 2009).

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

[12] A key, albeit implicit, component of the policy argument appears to be that increasing minimum payments should decrease debt which in turn should reduce default. We find that doubling the minimum payment had a small negative effect on debt – our preferred bounds for the implied elasticity range from -0.4 to -0.01 – but as noted above, no effect on default.

ature on credit markets. First, there is a substantial empirical literature documenting the existence of liquidity and credit constraints[13] and some evidence that these arise from limited borrowing ability.[14] An extensive theoretical literature attributes this inability to borrow to information failures in the credit market and a smaller empirical literature documents the existence and gravity of such informational problems.[15] In our setting the experimental variation in interest rates allows us to test for moral hazard as well as estimate the elasticity of debt with respect to the interest rate, an object of direct policy and academic interest.[16]

Second, a more recent, policy-motivated, 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. This literature has largely been descriptive, documenting, for instance, the large numbers of people world-wide who do not use formal banking services.[17] The experience of our NTB borrowers with a large private bank highlights the challenges associated with expanding financial access through large formal institutions. Our work is also complementary to an earlier literature that critiques institutional (typically state-led and agricultural) lending to the poor.[18] 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 concrete evidence in a much different context. A final strand of literature is concerned with consumer protection, showing for instance that the availability of payday loans causes financial hardship, that borrowers have cognitive limitations, and that regulation limiting quantities or prices may increase consumer welfare.[19] The effects documented in these papers suggest that increasing minimum payments may have ambiguous welfare impacts in our context and we use the experimental variation in minimum payments to explore this further.

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 describes some facts about financial inclusion and credit in Mexico using a large representative sample of borrowers from the formal credit market. In particular, we attempt to document (a) that NTB borrowers are credit constrained, (b) that they are risky prospects for formal sector lenders, and (c) the existence of a lending externality that has important implications for financial inclusion. Section 4 describes the experiment while Section 5 reports the effects of the experiment on borrower exit (default and cancellations) and bank revenues, the two primary outcomes of interest. We use the stratified

[13]See e.g. Parker (1999), Gross and Souleles (2002), Johnson et al. (2006).

[14]See e.g. Zeldes (1989), Deaton (1991).

[15]Ausubel (1991), Edelberg (2004), Karlan and Zinman (2009), Adams et al. (2009), Einav et al. (2012).

[16]See Karlan and Zinman (2017), Attanasio et al. (2008) and Dehejia et al. (2012) for similar exercises in Mexico, the United States, and Bangladesh respectively.

[17]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.

[18]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.

[19]See Melzer (2011), Bertrand and Morse (2011) and Agarwal et al. (2015) respectively.

nature of the experiment to examine treatment effect heterogeneity and link it to variations in credit constraints and earnings across strata. Section 6 discusses some more distal mechanisms that affect overall default in our NTB sample and Section 7 concludes. Due to space constraints some robustness analyses and secondary figures and tables are reported in the Online Appendices (OA).

2 Data

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

2.1 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.[20] A borrower appears in the credit bureau if she has or has had a loan with a formal financial intermediary.[21] For each borrower we observe the date of loan initiation, the name of the lender, the type of loan, total debt outstanding, amount in arrears and her delinquency and default history.[22] We also observe a limited set of demographics – age, gender, marital status and place of residence. 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.

[20] Credit bureaus have long been proposed as a means of reducing asymmetric information in the credit market. The Mexican Credit Bureau began operations in 1996 and currently has approximately 57 million registered borrowers (see World Bank (2005) for a brief overview of the history of credit bureaus in Mexico). See e.g De Janvry et al. (2010) for an evaluation of the introduction of credit bureaus (in Guatemala) and Giné et al. (2012) for an evaluation of a technology that improved lenders' abilities to identify borrowers (in Malawi).

[21] The CB is required to maintain all records provided by reporting agencies for 84 months. 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).

[22] 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 for the 2010 cross-section. We do observe credit-scores (only) for a one million cross-section sample in 2016 that we use to compare credit score distributions with the study sample.

2.2 Credit Bureau Data: Panel for Experimental Sample

We were also able to match the experimental sample to the credit bureau data once each year (from June 2007 to June 2010). 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.

2.3 Bank Data: Experimental Sample

We use data from a 26 month randomized experiment with 162,000 clients of Bank A to examine the effect of contract term variation on borrower behavior. The sample consisted of a subset of Bank A's borrowers who held the study card — a particular type of store credit card. In private conversations, bank officials noted that the study card targeted low-income populations with limited credit histories.[23] The card 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.[24] By 2009, Bank A had approximately 1.3 million clients with this store card. In 2010 the card accounted for approximately 15% of all first-time formal sector loan products.

Consumers in the experimental sample 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 more than one million clients from which the study sample was drawn. The sampling design thus ensures the study sample is representative of this population; we examine the external validity of the sample for the national population of NTB borrowers below in Table 1.

The sampling frame was partitioned into nine strata based on two pre-intervention characteristics that the bank uses internally as predictors of default and are detailed in Section 4.1. The bank then randomly selected a sample of 18,000 clients per stratum. We use stratum weights (see Table OA-13) in all regressions to ensure our results are representative of the population described above. Within each stratum, clients were randomly assigned to one of nine study arms so that we have 2000 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. The resulting sample is geographically widespread – covering all 31 states and the Federal District, 1,360 municipalities (out of 2,348), and 12,233 zip codes (out of 32,378).

The experiment lasted from April 2007 to May 2009, and for this entire period we have monthly data on purchases, payments, debt, credit limits, delinquencies and default. A client is deemed delinquent if s/he pays less than the minimum payment in a billing cycle (which is one month). Three consecutive months of delinquency result in the bank revoking the card which is also referred to as default. In addition to this detailed transaction information we also observe some basic demographic variables – age, gender, marital status and place of residence.

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

[24] The base rate is the inter-bank rate also known as the TIIE in Mexico.

2.4 Matched Social Security Data (IMSS)

We were also able to merge our sample with the government's social security 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.

2.5 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. We use the 2005 and 2008 Mexican Family Life Survey (MxFLS) to measure loan terms for both formal and informal loans.[25]

2.6 Summary Statistics

Table 1 reports summary statistics for the 162,000 borrowers in our experimental sample (columns 1 and 2) and compares them to selected sub-samples of from the larger 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. 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 length 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 computed in column 4 and we refer to it as the new (or recent) borrower sample.[26] Finally, in Column 5 we consider a sub-sample of experienced borrowers – those with a credit history of at least 8 years in the CB data.

2.6.1 Basic Demographics and Income

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. Unfortunately, we do not observe income for the entire experimental sample – we only observe income for those individuals who also have records in the social security database (i.e. those currently employed in the formal sector) which is approximately 18% in our experimental sample and about 13% in the CB data.[27] Average monthly income in the experimental sample is 13,855 pesos compared to an average of 14,759 for recent borrowers and 22,641 for experienced

[25] See Rubalcava and Teruel (2006, 2008).

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

[27] Well over half of Mexico's labor force is in the informal sector so is not captured in the IMSS. We cannot also discount the possibility that matching errors account for part of the low match rate.

borrowers.[28] Our experimental sample is thus somewhat less well-off relative to the average CB member at least when looking at those who work in the formal sector. In fact, Figure OA-9 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. From these admittedly rough comparisons we conclude that our study sample is likely reasonably representative of the population of all marginal borrowers in Mexico.

2.6.2 Credit Information

We first observe credit scores for our experimental sample in June 2007 (Panel A). The mean credit score is 645 which is low – a borrower with a score below 670 is typically ineligible for standard credit card products.[29] Unfortunately we cannot compare this to the other CB sub-samples (Cols 3-5) since the CB did not provide us with credit scores for these sub-samples.

We next study the credit history of the experimental sample using the matched data from the CB. The card issued by Bank A was the first card for 57 percent of the sample, though by the start of the experiment most borrowers had more than one card – on average borrowers had 2.75 cards at the start of the experiment including the study card. Figure OA-11 in the OA shows a steady increase in card coverage for the experimental sample over the two years immediately preceding the experiment. By the start of the experiment, almost 80% of the sample had an additional card (an increase from 50% at the start of 2005). This is consistent with the substantial expansion in credit cards in Mexico during this period. Other (i.e. non-card) forms of formal borrowing remained relatively rare by comparison.

Finally, we summarize pre-experiment credit card usage for the study card in Panel A. Average debt as of March 2007 was 1,198 pesos. The credit limit for Bank A's card was relatively low at 7,879 pesos and the overall card limit for the experimental sample (summing across all cards) was $15,776$ pesos in 2007 and rose 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. These figures suggest that our study sample was, unsurprisingly, at the low-end of borrowing ability in the CB data. Turning next to borrower default behavior with Bank A, 17% of the experimental sample defaulted over the course of the experiment while 10 percent of clients canceled their cards. These figures highlight the high turnover in this segment of the credit card market.

[28]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-owning family.

[29]The credit score of the Mexican CB was developed by Trans Union and Fair Isaac and takes values from 400 to 800. It does not appear, however, to be directly comparable to the credit score ranges in the United States.

2.7 Quantifying Bank Revenues per Card

We next attempt to quantify the bank's revenue from the study card using the detailed data on purchases, payments and debt we obtained over the 26 month experiment. 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. Constructing the revenue measure requires making strong assumptions about borrower purchases, payments and default which we explicate below. These assumptions, while clearly simplistic and somewhat arbitrary, have the benefit of being transparent and easy to interpret and enable us to get a basic handle on the revenue a card generates for the bank through the end of the study period.

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).[30] 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. We account for pre-study behavior by subtracting the amount due from card i at the start of the experiment (March 2007).

We account for post-study behavior by making some assumptions about the likelihood of default and the amount recovered in case of default. In particular, we assume no further purchases and then estimate for each card i the probability of default (ϕ_i) as a function of its credit score.[31] We next define the expected fraction (of the amount due) that would be recovered at card exit as $\alpha_i \equiv \phi_i \times 0.1 + (1 - \phi_i) \times 1$, where we assume (based on conversations with bank officials) that in the case of default the bank only recovers 10 centavos on every peso lent (Figure OA-14 in the OA shows that our measure of revenue is not particularly sensitive to the choice of α_i). 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.[32] Second, 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.[33]

[30] $PV(X)_i = \sum_{t=t_0}^{T_i} (1+r)^{-t} X_{it}$ where time is measured in months, t_0 is March 2007 (03/07) and T_i is either May 2009 (05/09) or the month in which the card exited the study (if this happened before 05/09.

[31] This was done 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-13).

[32] In fact because of the identity $Debt_t = Debt_{t-1} + Buy_t - Pay_t + (i/12)Debt_t + Fees_t$, an alternative representation of equation (1) is $\sum_{t=1}^{T} \beta^t [(i/12)Debt_t + Fees_t]$. 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.

[33] Informal conversations with bank officials suggest that the promotion cost of a card is about 80 pesos, the cost of issuing the card and the plastic is close to 100 pesos, and the management cost is about 20 pesos per month.

Figure 2(a) plots the histogram of the constructed revenue measure. The measure shows considerable dispersion — the standard deviation (7347 pesos) is considerably larger than the mean (4197 pesos). To assess its reasonableness, we examine correlations of our constructed measure with credit scores. First, revenue displays an inverted-U pattern with respect to initial credit scores. Figure 2(b) presents results from a kernel regression of revenue on 2007 credit scores at the borrower level. In private conversations, Bank A officials confirmed that average revenue, its dispersion and its relation to credit scores are reasonable. Clients with low scores yield low revenues since they are more likely to default. On the other hand, clients with high credit scores generate low revenues because they accrue lower interest charges and fees that generate revenue for the bank (e.g. by paying off the amount outstanding each month). This inverted U shape relationship between bank revenues has also been documented in other contexts which gives us further confidence in our construct.[34]

For our purposes, the important facts we have sought to establish in this section and which are relevant going forward are that our study sample comprises relatively low-income, NTB borrowers with poor credit histories and that our measure of bank revenue from such borrowers displays wide dispersion and an inverted-U relationship with credit scores.

3 Credit and Financial Inclusion in Mexico

Formal credit penetration remain low in Mexico. The ratio of private credit to GDP was 23 percent in 2010, low even by Latin American standards.[35] Credit card penetration is likewise low – the percentage of adults with at least one credit card was 17% (in 2014) compared to about 70% – 80% for the US.[36] The credit card market is also highly concentrated, with the five largest banks jointly controlling approximately 90% of the market for revolving debt,[37] with average APRs of 24%.

Credit cards are the primary instrument used by large financial organizations to expand financial access in Mexico – they are typically the first loan product offered to NTB consumers. Figure 1(b) shows that more than 70 percent of first time loans are through credit card balances. Interviews with bank staff suggested that expanding financial access using credit cards is, however, a relatively recent phenomenon. 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.[38] A substantial part of the growth in card holders was concentrated among lower income individuals. Figure 1(a) shows that from 2004 to 2012 the growth rate of credit card ownership for the lowest two deciles of the income distribution was 200%. In spite of this growth, conversations with bank officials suggest card application

[34] See fig. II.E in Agarwal et al. (2015) which plots a inverted U relationship between realized profits (as a fraction of daily balances) and credit scores for the United States.

[35] According to the World Bank (https://goo.gl/6BNrK6) Brazil (52%), Chile (98%), Colombia (43%), Latin America and the Caribbean (40%) all had higher ratios in the same year.

[36] 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.

[37] See the Mexican Central Bank's "Indicadores Basicos de Tarjeta de Credito" (2012) at https://goo.gl/nGQJC. In the US the corresponding figure is is 64% (https://goo.gl/y39Xy8)

[38] Banco de México (2016).

rejection rates average about 50 percent with the figure rising to about 70 percent for NTB clients.

3.1 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.[39] Conversely, one can view debt (or more generally consumption) responses to changes in credit limits as evidence of credit constraints.[40] 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.[41] 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

$$\Delta\text{Debt}_{i,t} = \delta_t + \sum_{j=0}^{T} \beta_j \Delta\text{Limit}_{i,t-j} + \gamma' X_{i,t} + \epsilon_{i,t} \tag{2}$$

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.[42] Because the bank evaluates a card for credit-limit changes using pre-determined 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.[43]

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

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

[41] 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.

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

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

The results are presented in Table 2. 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.[44] For Panel B, since we only have annual data, we modify equation (2) 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 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.[45]

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 (2) 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.

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

[45] 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 $T = 9$).

3.2 The Perils of Financial Inclusion

While there is a substantial literature documenting the effects of credit on borrowers, we know much less about the determinants of and the barriers to expanding the supply of credit particularly in a low income context. In this section we provide some basic evidence on the difficulty of expanding formal credit to NTB borrowers.

The literature has proposed three broad categories of explanation for this difficulty. The first is that lenders, particularly large financial institutions such as Bank A, typically have limited information on NTB borrowers – it is hard to identify new creditworthy borrowers. Second, NTB borrowers typically demand small loans which makes it difficult for lenders to recoup fixed costs (i.e. costs incurred irrespective of loan size). Third, subsequent lenders can use the (publicly available) credit history established by NTB borrowers with their first lender to condition their own lending decisions. This generates a negative externality for the first lender so that overall NTB lending may be lower than it would be in the absence of this externality.

We present evidence for the first and third of these explanations. First, we document that NTB borrowers default at high rates, generate variable and unpredictable revenues for the bank and that such behavior is hard to predict ex-ante (Section 3.2.1) or alter subsequently using contract terms (Section 5). Finally, we provide some evidence of the externality wherein NTB clients who have established a good credit history with Bank A leave for another bank (Section 3.2.2).

3.2.1 High Exit and Unpredictable Revenues

Profit considerations are, clearly, central to bank decisions about extending credit cards to NTB borrowers. We examine the link between profits and NTB borrowers using two complementary pieces of evidence. First, since card exits typically decrease profits, we examine exit rates. Second, we use our revenue measure as a proxy for profits and examine the extent to which Bank A can predict revenue from NTB borrowers (we carry out a similar exercise for card exit).

During the 26 month study approximately 44 percent of the control group accounts exited the bank; 19 percent defaulted, 16 percent canceled their cards while another 9 percent exited for other reasons.[46] Figure 3(b) shows that these different causes of card exit evolved smoothly over time during the experiment. Such high exit rates appear to be typical of NTB borrowers in general. We document this by examining exit rates as a function of credit limits – since small credit limits are a reasonable proxy for being NTB. Figure 3(a) uses the CB data to plot a kernel regression of (an indicator for) card closure within 26 months of opening on the initial credit limit for the card. We see strikingly similar exit rates in the CB data for cards that like the experiment have a initial credit limit close to 7000 pesos.[47]

[46]The bank charges no fees for the card so there are no direct costs to the borrower. However, we conjecture that there are other reasons for canceling a card. Ponce et al. (2017) shows that about 10% of card-holders in Mexico report fraudulent card activity and about 6% had their cards stolen so that fear of theft or fraud on a card that borrowers may not need could prompt cancellations.

[47]Figure 3(a) also shows that most new cards start at low credit limits.

We next examine the extent to which Bank A can predict exit as well as, more relevantly, profits (proxied by our measure of bank revenues) from NTB borrowers. We show below that predicting exit and revenue is difficult for our NTB sample using a range of information sets – starting with information typically available to the bank at the time of application and subsequently adding information based on observed borrower behavior with the bank.[48]

We conducted the exercise using a variety of methods – Random Forests, K-Nearest Neighbors, Boosting, Neural Networks and Support Vector Machines, in addition to OLS and a benchmark intercept only model. We estimate each model in a training sample, and predict outcomes in a hold-out sample. To separate between the training and validation samples, we will make use of our strata variables defined in Section 4.1.1. Our training sample corresponds to all cards in the control group of the experimental sample that, by January 2007, had been with the bank for more than a year and our validation sample corresponds to those cards in the control group of the experimental sample that, by January 2007, had been with the bank for less than a year but more than 6 months. The results in Table 3 focus on the benchmark model, OLS and Random Forests since the last one dominated the other ML methods. 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 default or revenue (defined on p.10) using different sets of variables. 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.[49] 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. The out-of-sample root MSE is 7,204 pesos for Panel A and 4474 for Panel C, which are about the same as the intercept-only model. The analogous numbers for default are 0.2 and 0.24 (for the random forest) and 0.00 and 0.43 (for the intercept-only model). Finally, we see that 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 default and revenue.[50]

Finally, we use the area under the receiver-operating characteristic (ROC) curve in order to compare our default estimates against others who use the same measure. The area under the ROC curve (AUC) is frequently used in machine learning as a 'threshold-free' measure of predictive performance, where a higher AUC denotes a higher predictive power. Our AUC estimates range from 0.79 (Panel A) to 0.81 (Panel C). These AUCs are lower than those found for credit cards in

[48]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.

[49]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 at the credit bureau.

[50]We note, however, that the Random Forest does not significantly out-perform the simplest intercept-only model on all measures particularly for revenues.

the US;[51] lower than those from loans in Australia, Japan, and Poland;[52] lower than those in the housing market in the US;[53] lower than those for credit default swaps in the US;[54]; higher than those to predict repayment using cellphone data in an unnamed South American country;[55] and higher than those for a micro-finance lender in Bosnia Herzegovina.[56]

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.[57] 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 using ex-post information unavailable to the bank at the time of application). Even though the bank presumably screened as best it could, the result appears unsatisfactory – that the bank decided to shut down the study card provided further evidence of this.

3.2.2 Client "Poaching" and the First Lender Externality

The previous section documents the difficulty of predicting borrower quality using ex-ante information and the improvements, albeit modest, in prediction using borrower behavior post-selection. In this sense we can view the first loan to an NTB borrower as a trial balloon that provides the lender valuable information about borrower profitability. To the extent that this information is public – via the credit bureau – there is a potential externality to other potential lenders.

Previous work has recognized the public good nature of this initial interaction. In an influential piece, 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". There is, however, little empirical evidence on the existence and extent of this problem.[58]

In this section we provide supporting evidence by documenting two facts. First, we show that

[51] 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.

[52] 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.

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

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

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

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

[57] 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).

[58] 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.

for a NTB client, building a good (respectively, bad) public credit history with the first ("inside") bank is a good predictor of getting (not getting) a second card with a different ("outside") bank. This is suggestive evidence that outside lenders do condition their lending decisions on CB data. From our Credit Bureau data in 2010, we find that for individuals for whom the study card is the first credit card, about 15% of the borrowers get a new loan with a different bank twelve months after they opened the experimental card, and 7% have zero active loans with Bank A after one year. We then use our experimental data to show that those those who leave Bank A are likely to be "good" borrowers. Second, we show that such departures substantially reduce Bank A revenues. This in turn should reduce the number of unbanked clients the inside bank is willing to take on. However, in the absence of extensive margin data and a credible design we cannot document this final step in the argument.

In order to focus attention on new borrowers, we restrict our sample to borrowers for whom the study card was the first card and who have been with the bank for less than a year as of January 2007.[59] These are precisely the set of clients in our data for whom the generated credit history should be most critical, as these individuals did not have a credit score prior to obtaining the study card.

An implication of the Stiglitz argument is that new borrowers with larger improvements in their credit scores should be more likely to receive cards from other banks. We examine this possibility in Figure 4(b) below and find exactly this pattern. We plot non-parametric regressions of two binary indicators of card acquisition between June 2008 and May 2009 against the changes in credit scores over the previous year. The results are consistent with the idea that subsequent lenders are using credit histories generated by the first lender to screen borrowers. Interestingly, we note that good borrowers are also more likely to receive additional cards from Bank A itself (perhaps partly as a strategy to retain them as customers).

In Figure 4(a) we plot non-parametric regressions of voluntary client cancellation and bank revocations between June 2008 and May 2009 on the study card against the same x-axis variable as in the previous figure. Decreases in credit scores are positively correlated with subsequent bank initiated revocations, and conversely larger increases in the credit scores are associated with higher borrower initiated voluntary 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.

A natural question then is how much revenue the first bank loses when a borrower is "poached." To assess this we need to estimate a counterfactual – the earnings foregone by Bank A when a borrower is poached. Note that in our context a poached borrower need not leave the initial lender 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. To simplify the calculation, however, we focus on borrowers who cancel their initial card when they leave Bank A for another lender.

[59] Note, however, that we only observe borrowers who have been with the bank for at least six months.

In this case, we can estimate the counterfactual in a transparent (albeit admittedly ad-hoc) fashion. We restrict the sample to the control group and define as a switcher a borrower who satisfied the following conditions: (a) she cancelled the study card during the 26 month study, (b) opened a card with another bank within a twelve month period ($\pm$ 6 months) of cancellation, (c) did not obtain any other cards between May 2003 and October 2006 (i.e. until six months before the experiment began) and (d) the study card was her first credit card. For each switcher, we calculate the revenue Bank A would have earned had the switcher not switched using a matching estimator that pairs switcher i (who cancelled at time t) with ten "control" clients $(j_1, \cdots, j_{10})$ from the pool of non-switchers with an active study card at t. The matching is done using the Mahalanobis distance from the switcher in $t-1$ for a vector of observables.[60] We then define foregone revenue from i to be the average revenue generated by the matches $(j_1, \cdots, j_{10})$ through May 2009. If any of the matches exits, their subsequent revenue is zero. We carry out this exercise for every switcher and present the average foregone revenue and associated standard errors (computed using subsampling) in Panel B of Table 4 (columns 1-3).[61] We calculate that average revenue foregone for each switcher is 4324 pesos per account, which is approximately the same as our revenue measure per card, a substantial revenue loss. We also carry out a placebo exercise (details in Cols (4)–(6)) and find that we can reproduce the matching estimates quite closely. See the notes below Table 4 for more details.

Lastly, we 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-11 in the appendix predicts voluntary cancellations using a battery of machine learning methods and finds AUCs in the 0.6 – 0.7 range.[62] 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 .[63] 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.

[60]We require an exact match on stratum so we use 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 $t-1$.

[61]The columns differ in the definition of a switcher and the differences are described in Panel A. We use sub-sampling (Politis et al. (1999)) since the bootstrap is inconsistent for matching based estimators (see Abadie and Imbens, 2008).

[62]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).

[63]This trade off is modeled explicitly in (Taylor, 2003)

4 Using Contract Terms to Change Behavior

The previous section documented high rates of card exit and variable bank revenue per borrower. Further, we showed that these variables are difficult to predict. This limited ability to screen borrowers ex-ante leads banks to rely more on ex-post measures like contract term adjustments – the most important being the interest rate on debt, the credit limit and the minimum payment required – to limit default and maximize profits. For instance reductions in the interest rate may be used to reduce default occurring for moral hazard reasons. Similarly, increases in the minimum required payment can be used to limit indebtedness and consequent default or to simply select out borrowers who cannot meet the more stringent requirements.

Whether and to what extent such variation in contract terms can mitigate default and its implications for bank profits is an open empirical question. This is both because (a) contract terms are endogenous to expected default, and (b) actual variation of contract terms particularly for NTB borrowers is quite limited. We were fortunate to observe a large-scale experiment conducted by one of Mexico's largest banks that induced large experimental variation in interest rates and minimum payments (the bank did not experimentally vary credit limits).[64,65] 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 2.3, the bank divided its sample 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. These were (a) the length of time a borrower had been with the bank and (b) the borrower's repayment history over the past 12 months.[66] 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

[64]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. Banks in Mexico run randomized experiments to test products as a matter of course and the current experiment appears to be one of many run by the bank during this period.

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

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

payments required from him/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 him during this time. From each stratum 18000 card-holders were randomly selected for the study. 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 2000 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 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 minimum payment prior to the study was 4% and about 70% of our study sample paid less than 10% of their amount outstanding (debt) in March 2007 (See figure OA-16). The interest rate could take on one of four values: $15\%, 25\%, 35\%$ or 45%. The interest rate for the product in the period 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 2000 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 conversations with Bank A we learned that while the majority of borrowers faced an APR of 55%, some borrowers had an APR of 60% but this information was not included in the data we were provided. 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 $(45\%, 5\%)$ or $(45, 5)$) as the comparison group and we often refer to it as the **base arm** or **base group**.

Figure 5 shows the timeline of the experiment, as well as measurement dates. The 9 strata were defined in January 2007. 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 told they were part of a study or any time-line for when the new contract terms would change. The measurement of experimental outcomes with bank administrative information began in March 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).[67] The experiment ended in May 2009 at which point the study participants received a letter setting out their new contract terms. These terms were

[67] We cannot reject the null of no differences in credit limits across treatment arms at baseline and endline (Table OA-15 and Figure OA-17).

the standard conditions with an interest rate of approximately 55% and a minimum payment of 4%. Finally, Panel A of Table OA-14 in the Online Appendix tests the randomization procedure and shows that treatment assignment is uncorrelated with baseline observables (as of March 2007).[68]

5 Experimental Effects on Default and Revenues

We begin by exploring the effect of the contract term interventions on the primary outcome of interest, card exit. Card exits – through either bank initiated revocation (default) or borrower initiated cancellations – are natural outcomes of interest for both the bank and policy-makers. For the bank, card exit is a direct determinant of revenues and ultimately profits. For policy-makers, defaults are an important concern since they can be a source of financial instability. In fact, the experiment described in this paper was driven in part by the Mexican Central Bank's interest in learning about the responsiveness of debt and default to changes in Bank A's contract terms.

For researchers, default is a natural outcome in the literature on credit market imperfections. In addition to default, we also examine client initiated cancellations – the situation where the borrower pays down her debt on the card and cancels the card. For our purposes the important conceptual distinction between the two is that cancellations are entirely borrower initiated whereas defaults involve bank action since the bank revokes the borrower's card (regardless of the borrower's preferences). Cancellations thus provide some (albeit tentative) revealed preference evidence on the attractiveness of the study card and state of the credit market.

We estimate regressions of the form

$$Y_i = \sum_{j=1}^{8} \beta_j T_{ji} + \sum_{s=1}^{9} \delta_s S_{si} + \epsilon_i. \tag{3}$$

where Y_i the outcome of interest (default, cancellation or bank revenues). Default is a binary variable equal to one if borrower i defaults at some point during the experiment. Our measure of cancellation, likewise, is a binary variable that equals one if borrower i voluntarily cancels her card at some point during the 26 month experiment. The right hand side variables are a set of full treatment and stratum dummies.

5.1 Default

5.1.1 Effect of Interest Rate Changes on Default

Turning to default, column (2) in Table 5 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 group rate

[68]Panel B shows that the sample of non-attriters across treatment arms is also balanced along observables at the end of the experiment. This further reinforces the point earlier that attrition (or exit) is difficult to predict using bank observables.

(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 a relatively low $+0.20$.[69]

The treatment effects for the other intermediate treatment arms are also similarly weak. The reduction in default in the $(25, 5)$ arm relative to the base arm is essentially the same as the treatment effect 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). Finally, note that the treatment variables (as well as the stratum dummies) together explain about one tenth of one percent of 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 6.3).

Finally, we examine the evolution of default over the entire 26 month period using monthly data in Figure 6 which presents the regression coefficients from estimating equation (3) month-by-month.[70] The figure shows that default from the interest rate decrease was was essentially zero for six months, and we only see statistically significant 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.

Many models of asymmetric information imply a positive correlation between risk (as measured by default) and prices (interest rates).[71] This could be due to (a) adverse selection wherein borrowers of a riskier "type" are more likely to be attracted by higher interest rates and/or (b) moral hazard wherein (holding type constant) higher interest rates induce borrowers to take actions that make them more likely to default. A common summary statistic for asymmetric information is therefore the correlation between interest rates and default. The large, experimental, variation in interest rates (from 45% to 15%) permits a clean test for the presence of moral hazard in our sample of NTB borrowers. The relatively small reductions in default despite large decreases in interest rates suggest weak moral hazard on average in this high risk population across the range of interest rates studied.[72]

5.1.2 Effect of Minimum Payment Increase on Default

Turning to the minimum payment intervention in Table 5, doubling the minimum payment from 5% to 10% had no effect on defaults – the point estimate is a statistically insignificant increase of 0.5 percentage points on a base default rate of 19.3% or an elasticity of $+0.02$.[73] To our knowledge

[69]For comparison, this is considerably lower than the delinquency elasticity of 1.8 implied by Karlan and Zinman (2017) and also lower than the default elasticity of 0.39 implied by the interventions in Karlan and Zinman (2009).

[70]The default measure at time t is a cumulative measure: i.e. $Y_{it} = 1$ if i has defaulted at any point upto t.

[71]See e.g. Stiglitz and Weiss (1981); Chiappori and Salanie (2000); Einav and Finkelstein (2011).

[72]Einav and Finkelstein (2011) note, however, that the magnitude of the correlation test does not necessarily map monotonically into the welfare loss from moral hazard.

[73]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 esti-

this is the first experimentally estimated effect of minimum payments on default. The effects in the other arms are all quite small and broadly comparable with the estimated elasticities ranging from -0.01 to $+0.08$. For some perspective, we note that monthly payments for 51% of borrowers were between 5% and 10% of the amount due in the March 2007.

Examining the evolution of the treatment response in Figure 6 we see that doubling the minimum payment had minimal effects for the first six months following which the default rate rose approximately one percentage point and then stayed relatively stable thereafter. These effects appear to be quite small, particularly relative to the policy attention paid to increasing minimum payments for poorer households as a means of limiting default.

A key, albeit implicit, component of the policy argument appears to be that increasing minimum payments should decrease debt which in turn should reduce defaults. As we show in Appendix C.4.5, the first part of the argument is true – increasing minimum payments does decrease debt. However, the evidence above shows that minimum payments have minimal effects on default.[74] Therefore, to the extent reducing default is a key policy objective (as opposed to merely limiting debt), then increasing minimum payments does not appear to be a promising avenue.

To summarize, the short- and long-term evidence shows that even substantial changes in interest rates and minimum payments have very limited effects on default, a sobering conclusion relative to the weight placed on these levers within policy circles. In Section 6 we will use the heterogeneity in the stratum specific treatment effects to better understand the mechanisms underlying the muted treatment effects documented in this section.

5.2 Card Cancellations

Cancellations are initiated by clients after repaying all card debt so the bank does not need to recover or write off any amount outstanding. However, the loss of a borrower will reduce revenues (if only from transaction fees) and so is a direct object of interest for the bank. For policymakers and researchers, cancellations are interesting because they provide some evidence on the degree to which borrowers may be competed away by other lenders (as documented earlier) and also provide a better understanding of borrower behavior (as we discuss below).

Section 3.2.2 showed that cancellations are common and that there appears to be competition for good borrowers. Cancellations reveal that clients preferred to no longer hold on to the study card, perhaps because they had access to a better outside option.

5.2.1 Effect of Interest Rate Changes on Cancellations

In Table 6 we see that cancellations in the base comparison arm $(45, 5)$ were 13.4% over the 26 month period of the experiment, and reducing the interest rate to 15% decreases cancellations by

mate 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).

[74] In fact, the only statistically significant effects of minimum payments on default in Table 5 are **positive**.

(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 relative to other cards and it is perhaps not surprising that fewer individuals chose to cancel. From the bank's perspective, the benefits from these decreased cancellations need to be compared against the revenue losses from lowering prices and we defer this to Section 5.3 where we examine revenue effects at length. Treatment effects from the other arms provide broadly comparable results.[75]

We next chart cancellations using monthly data in Figure 6 which presents the regression coefficients from estimating equation (3) monthly. Cancellations begin to decline after about six months of the intervention and the rate of decline remains roughly constant from then onwards through the end of the experiment. To conclude – 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.[76]

The robust decline in cancellations in response to interest rate declines provides evidence that the lower interest rate card is attractive to borrowers. Such a preference might lead one to reasonably expect similar declines in default as well (on the low interest rate card). However, the effects on default are much smaller than on cancellation – the elasticity of default is +0.20 compared to to +0.39 for cancellations. This disparity is consistent with the claim that borrowers have much less control over default than over cancellations and that non-preference factors may play a larger role. In the sequel we explore some of these possible underlying factors. Finally, we note that the significant and substantive effect of the interest rate declines on cancellations also allows us to rule out inattention as a cause for the limited effects of the interest rate declines on default.

5.2.2 Effect of Minimum Payment Increase on Cancellations

Turning to the minimum payment intervention, doubling the minimum payment led to a (statistically significant) long-term (26 month) increase of 1.7 percentage points in cancellations 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 $+0.24$.[77] Examining the evolution of treatment response in Figure 6 we see that cancellations remain roughly flat for the first six to seven months after which they rise, stabilize by the end of the first year and remain roughly constant until the end of the experiment. Just as with interest rates, the minimum payment increase had a much larger effect on cancellations relative to default (indeed we cannot reject the null that the minimum payment increases had no effect on default). The rise in cancellation rates suggest that increasing minimum payment requirements decreased the study card's attractiveness. One might reasonably have expected that this decline in attractiveness also finds expression in higher default rates. That

[75] As before, we compare cancellations for the $(45, 5)$ group to the $(r, 5)$ group where $r \in \{25, 35\}$ and cancellations in the $(45, 10)$ group to the $(r, 10)$ group where $r \in \{15, 25, 35\}$. The results are in Table 6 and the implied elasticities are in the the $\{+0.30, +0.60\}$ range and are all significant at conventional levels.

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

[77] As before, we compare cancellations for the $(r, 10)$ group to the $(r, 5)$ group where $r \in \{15, 25, 35\}$.

this is not the case suggests (as with the interest rate intervention) that borrowers may have less control over default (or that default may be less strategic) than cancellations.

In summary, there was substantial secular default during the experiment relative to which the effect of both the interest rate and the minimum payment interventions was quite modest. The relatively small magnitudes of the treatment effects is disappointing for arguments advocating improved contract terms to limit credit card default among NTB borrowers. The modest effects are also somewhat surprising given the context – viz. large variations in the interest rate and minimum payments among a low-income NTB sample.

5.3 Effect on Bank Revenues

Revenues are a critical benchmark for evaluating the effects of the intervention on Bank A. In this section we examine the effect of the interventions on the revenue measure constructed in Section 2.7 (with the usual caveats about its reliability as a measure of profit). The results are in Table 7 and show that in general any departures from the $(45, 5)$ arm reduced bank revenues. This is consistent with the notion that Bank A's standard choice of minimum payments and interest rates yields higher profits than the choices considered in the experiment.[78]

5.3.1 Effect of Interest Rate Changes on Revenues

The estimated treatment effects reveal that revenue is monotonically increasing in the interest rate. Taken literally, the point estimates suggest that reducing interest rates from 45% to 15% over the 26 month period of the experiment reduced our bank revenue measure per borrower by about $2,859$ pesos (for the 5% minimum payment group) for an estimated elasticity of 1.54 which is quite similar to the estimated elasticities for the other contrasts.[79]

In sections C.4.7–C.4.11 of the online appendix we explore the effects of the intervention on three proximate determinants of revenues – purchases, payments and fees – and establish three facts. First, interest rate declines have inconclusive effects on purchases with the Lee bounds for the long-term effect being a relatively wide $[-0.38, +0.25]$.[80] Second, monthly payments declined modestly in response to the interest rate decreases with the long-term bounds estimated to be $[+0.04, +0.39]$.[81] Third, fees decline moderately in response to the interest rate declines . These three results are then consistent with the observed decline in revenues as a result of the interest rate decline.

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[78]Note that the $(45, 5)$ was the treatment arm closest in terms to the business as usual arm which had a minimum payment of 4% and an individually varying interest rate with an average rate of 55%. As pointed out earlier, we do not use the business as usual arm in the experimental comparisons since we do not observe individual interest rates. We also note that the choice of minimum payment and interest rate in the business as usual scenario is consistent with what our treatment effects suggest about profit maximization (i.e. increasing interest rates and lowering minimum payments).

[79]The corresponding elasticities were 1.54 and 1.56 for the $(25, 5)$ and $(35, 5)$ arms (relative to the base arm of $(45, 5)$ respectively).

[80]The short-term effects have tighter bounds of $[-0.38, -0.18]$ that suggest modest increases in purchases. More details are in Table OA-19.

[81]Bounds for the short-term are qualitatively similar at $[+0.06, +0.24]$. See Table OA-20 for more details.

Extrapolating from these points suggests that increasing interest rates may well be a profit maximizing strategy for the bank even after accounting for default and cancellations (at least for the range of interest rates considered) – since our measure of bank revenue accounts (albeit in a highly stylized fashion) for card exits.

5.3.2 Effect of Minimum Payment Changes on Revenues

Table 7 shows that, perhaps surprisingly, the increase in minimum payment requirements reduced bank revenues. Bank revenues from borrowers in the $(45, 10)$ arm are 469 pesos lower than the $(45, 5)$ arm. The implied elasticity is -0.16 and is comparable to the implied elasticities from the other arms.[82]

We next explore the effects of the intervention on purchases, payments and fees. In Appendix C.4.8 we find that the minimum payment increase led to a modest but robust **increase** in purchases with the long term Lee bounds for the elasticity being $[+0.18, +0.85]$.[83] In Appendix C.4.10 we show that monthly payments also go up modestly in response to the increase in minimum payments with the long-term Lee bounds (for the elasticity) being $[+.01, +.48]$. On net there is a modest decline in the difference between payments and purchases so that it is not surprising that overall bank revenues respond negatively to the increased minimum payment requirement.

This finding reinforces the difficulties and limitations of using higher minimum payments as a policy lever – on the one hand higher minimum payments increased default (and so perhaps lowered welfare) despite lowering debt levels while on the other hand they reduced bank revenues. More generally, the experiment provides sobering evidence on the difficulty of using contract terms to alter consumer behavior.

Our results thus far suggest that long-term financial inclusion for NTB borrowers is a fraught proposition – NTB borrowers default at high rates, ex-ante screening is difficult and borrowers are unresponsive to changes in contract terms. Therefore, it is perhaps unsurprising that the bank stopped subsequently reduced its dealings with NTB borrowers. Since our measure of revenue has limitations, this decision to stop serving NTB clients provides separate corroborating evidence. Figure 1(c) shows the stock and the new issues of cards –of the same type as the experiment's card– by Bank A. After issuing them in substantial numbers for several years, the bank stopped issuing them completely in January 2009 and by 2013 the CB data shows no borrowers with the study card.

5.4 Heterogeneity and Implications for Inclusion

In this section we explore treatment effect heterogeneity across strata to infer how variation in borrowers underlying circumstances affect their treatment responses. In particular, we focus on strata that vary in the extent of their credit constraints and examine differences in treatment effects across such strata.

[82] The elasticity of revenue with respect to minimum payments for the $(35, 10)$ vs the $(35, 5)$ arm was -0.11 and -0.0004 for the $(25, 10)$ vs the $(25, 5)$ arm.

[83] We discuss is unexpected result further in Appendix C.4.8.

A direct test of whether the strata vary systematically in terms of credit constraints is to estimate equation (2) separately for each stratum and compare the magnitudes of the estimates of θ across strata. The results are presented in Table (2) 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).[84] 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.

These results suggest that variation in treatment effects across strata can be understood in part as a reflection of underlying credit constraints. Such findings may have implications for the prospects of financial inclusion via such credit products as examined here. Methodologically, the stratified experimental design ensures that the estimated treatment effects retain internal credibility (as opposed to ex-post stratification not based on explicit stratified randomization).

We make three main points from examining the stratum specific effects in Table 5. First, while the newest borrowers with the poorest baseline repayment rates had the highest default rates in the study (34.6%), they were completely unresponsive to the interventions. Second, the oldest borrowers with the best baseline repayment rates had the lowest default rates in the study (at 4%) and were also completely unresponsive to the interventions. The unresponsiveness of the latter is unsurprising – their bills were paid in full each month, incurring no debt so it is reasonable that both minimum payment and interest rate changes should not affect their behavior. For this group, the study card appears to offer mainly convenience since they do not carry any debt on it (they could also be using it to improve their credit scores). The unresponsiveness of the former group is somewhat surprising – they are relatively poor and credit constrained and one might expect that reductions in the interest rate for such groups should reduce default. This was clearly not the case. On the other hand, the interest rate reductions did reduce cancellations substantively for this group (see col. (4) in Table 6 and Figure 7) suggesting that to the extent possible the group did respond to improved terms and that the lack of default responsiveness to the improved terms was perhaps a result of large uninsurable negative shocks that swamped the effects of the interest rate reductions.[85]

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

[85] A simple model wherein defaults occur if income falls below a certain threshold and in which the improvements in interest rates do not move the threshold sufficiently for the poorest borrowers would generate this type of behavior.

6 Mechanisms

The previous sections document that (a) the NTB population is characterized by high default rates and that (b) even large changes in interest rates and minimum payments have muted effects on default. In this section we seek to better understand the high underlying default rates during the study. We document that (a) default has significant negative consequences for borrowing from the formal sector, (b) that the borrowing terms (specifically interest rates and duration) in the informal sector are significantly worse than in the formal sector and finally that (c) the high default rates in our sample can in part be explained by large negative shocks experienced by the NTB population.

6.1 Consequence of Default

A primary rationale for the establishment of credit bureaus is to provide information about borrower behavior – including default – to lenders in the market. Prospective lenders use this information to make loan decisions; this in turn provides an ex-ante incentive for the borrower to limit default.[86] In order for this incentive to be effective, the consequences of default must be sufficiently dire. Here we provide correlational evidence on the question. In particular, we test whether default is associated with subsequent declines in formal sector borrowing.

We restrict attention to the experimental sub-sample for whom the study card was the first formal sector loan product and who are in the newest borrower stratum.[87] 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 indicator for i obtaining a new loan or card six, twelve, or forty eight months after September 2007. We include age and gender and zip code dummies as additional controls. Panel A of Table 8 shows the results for all types of (formal sector) loans, while Panel B focuses only on credit cards. We further group columns by lender (any lender, all lenders except Bank A and Bank A).

We find that default on the study card is associated with a substantial 26 percentage point decrease in the likelihood of getting any formal sector loans within the next 6 months (relative to a mean of 29 percent for non-defaulters). The negative consequences of default are long lived – we find substantial effects four years out. Since default is reported to the Credit Bureau, we might expect the negative correlation shows up not only in Bank A but in all banks and indeed this is exactly 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 for up to four subseuquent years with any bank.[88]

One concern with the regression above is that omitted variables may drive both default and

[86] Note that the theoretical arguments for credit bureaus is strongest for environments not characterized purely by adverse selection, which we assume in the statement above.

[87] That is, those who had been with the bank for between 6 to 11 months at the start of the experiment.

[88] The starker results for credit cards is consistent with lenders adopting harsher stances towards uncollateralized debt.

future demand for loans. We attempt to address this by adding borrower and time fixed effects. However, this increase in flexibility means that we must restrict attention to delinquency as the primary outcome rather than default.[89] 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.[90] Note however that the result is not surprising. Decreased access to formal lending is what one would expect from default on a formal loan given the Credit Bureau. Default thus forces borrowers to rely on informal lenders. As we document in the next section this is not an enticing prospect.

6.2 Informal Loan Terms

We now briefly describe contract terms for informal loans and document that they are typically worse on multiple dimensions relative to formal loans. We rely on survey data as informal loans do not appear in the CB data. Fortunately the nationally representative Mexican Family Life Survey (MxFLS) has data on interest rates, loan amounts, and loan terms for formal and informal loans (in the 2005-2006 and 2009-2012 rounds). We define a loan as formal if the lender is a bank and informal otherwise.[91] Consistent with the evidence from a range of developing countries[92] only 6% of borrowers have any formal loans and 91% of borrowers have only informal loans.

Informal loans have significantly worse terms than formal loans. Figure OA-25 shows that the distribution of interest rates for informal loans stochastically dominates the distribution for informal loan interest rates while the opposite is true for loan terms and loan amounts. Table 9 shows the results from regressing contract terms on a formal loan dummy and other controls. The first striking fact is that informal loans have on average a yearly interest rate of 291% while formal loans have a rate that is 94 points lower (column 1). Loan amounts are 3658 pesos for informal loans and 6184 pesos higher for formal ones (column 4), and the term of the loan is 0.52 years for informal loans and 0.55 extra years for formal loans (column 9). These results are robust to controlling for income and wealth proxies in columns 2,4 and 7.[93] The results on loan terms and duration also survive the addition of household fixed effects.[94] Based on these results we conclude that it is costly to be excluded from the formal loan market.

[89] 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 in the first delinquency for eventual defaulters.

[90] Other authors (Bos et al., 2018; Dobbie et al., 2018) document similar magnitudes using more persuasive empirical designs.

[91] Informal loan sources comprise: Co-operatives (13%), money-lenders (8%), Relatives (38%), Acquaintances (20%), Work (11%), pawn-shops (5%), and others (5%).

[92] See e.g. Banerjee and Duflo (2010).

[93] Unfortunately the MxFLS has missing values for a number of covariates resulting in reduced sample size.

[94] 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).

6.3 Drivers of Default

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 frequent, large shocks that precipitate default. We largely lack the individual level data required to test this conjecture convincingly. However, we can observe employment spells for the a subset of our CB sample that is 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.[95]

Given the matched data, for individual i living in state s at month t we estimate the following regressions using OLS:

$$\text{default}^j_{it} = \alpha^j_i + \gamma^j_{s,t} + \sum_{k\geq 1} \beta^j_k \times \mathbb{1}(\text{ months unemployed}_{it} = k) + \varepsilon^j_{it} \tag{4}$$

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. For individuals who are never employed this dummy is undefined. The dependent variable, $default^j_{it}$ 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 who has been delinquent for j months or more. Figure 8 plots our regression results for β^j_k 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 of having at least one loan with more than a month in arrears. The unconditional mean is is 12 pp, so that the associated increase is a 41% increase.[96] These results demonstrate the severe effect of unemployment on default (controlling for individual fixed effects) and are thus consistent with the view that large negative shocks, such as prolonged unemployment, increase default markedly. At the same time, these results are only suggestive since first, only about 20% of our experimental sample is employed in the formal sector and second, the unconditional unemployment rate for the estimation sample is about three percent so that unemployment alone likely cannot explain the high levels of observed default.

[95] The matching proceeds as follows: Of the 1m borrowers in our 2014 CB data, $542,959$ had both a tax identifier (RFC) as well as a bank loan at some point between January 2011 and May 2014. We used the RFC to match borrowers to the IMSS monthly data from October 2011 to May 2014. We observe employment (or more accurately earnings, but the two are synonymous in the IMSS) for at least one month for 86,363 individuals. Since the IMSS is a census of all formal sector workers, a positive match indicates employment in the formal sector and we assume that a lack of a match indicates unemployment in the formal sector. Since we do not observe employment in the informal sector, we cannot construct a more comprehensive indicator of employment.

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

7 Conclusion

Expanding financial access to under-served populations is by now 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 formal sector 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. The card was available nationally starting in 2002 and by 2010 accounted for 15% of all first time formal sector loan products.

We provide evidence that the targeted borrowers were in fact credit constrained in the formal credit market and that informal credit market terms were markedly more onerous than formal sector terms. Despite this, we document high card exit rates with about one-third of our sample either defaulting or cancelling their card over the 26 month study period. We next use machine learning methods and document that screening borrowers using ex-ante information has limited predictability. We then use a large national level randomized experiment and find that even large variation in contract terms (minimum payments and interest rates) had limited effects on default.

We also provide additional evidence on the difficulties of financial inclusion by the study bank. We construct a measure of bank revenue using detailed individual level monthly data on purchases, payments and fees and find that revenue per borrower is low on average and highly variable. These bleak results find confirmation in the the fact that the bank discontinued the card and moved away from borrowers with limited formal sector experience. Finally, we documented suggestive evidence of a first-lender externality in that new to banking borrowers who generate good credit histories with their first lender are more likely to obtain other cards and leave the bank than borrowers who do not generate comparable histories. Taken together, these findings highlight the difficulties of expanding credit access to new borrowers via large financial organizations such as our study bank.

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*	4197	-	-	-	-
	(7347)				
Credit score	645	-	-	-	-
	(52)				
(%) Consumers delinquent (in month)	1	6.8	-	-	-
(%) Consumers for whom experiment is their first card	57	-	-	-	-
(%) Consumers who default between Mar/07 - May/09	17	-	-	-	-
(%) Consumers delinquent between Mar/07 - May/09	27	-	-	-	-
(%) 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)
Number of credit cards	2.75	1.15	1.94	2.04	2.69
	(1.90)	(1.65)	(1.60)	(2.04)	(2.11)
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)
Number of banks interacted with†	2.63	2.66	1.44	1.49	1.80
	(1.25)	(1.29)	(0.80)	(1.49)	(1.00)
Tenure in months of oldest credit	68	100	79	68	206
	(54)	(51)	(87)	(57)	(85)
Total amount in arrears given that it is positive	9,738	54,401	20,349	20,682	56,266
	(49,604)	(100,267)	(52,759)	(48,263)	(96,039)
Pct. of accounts with positive amount in arrears	22	47	24	27.57	24
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
(%) Consumers with information in the social security database	18	-	13	14	13
Age	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 subsamples. 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 online 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 2.7.† The number of banks interacted with represents the average number of financial institutions with whom each consumer has had at least one loan prior to the month of measurement. ‡ 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: 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
Panel A	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Panel A. Bank A's debt and limit									
Baseline	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	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	1366035	118687	143397	170791	125859	145077	174305	146291	155290	186338
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

		6-11 months			12-23 months			24+ months		
	All	Minimum	Two +	Full	Minimum	Two +	Full	Minimum	Two +	Full
Panel B	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Panel B. Total limit and total debt									
Baseline	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	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	210886	24249	23473	22932	23103	22560	22250	23959	23789	24571
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: The first row ("Baseline") in each panel displays estimates from regressions of current debt on past changes in credit limits (equation 2) 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 2 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. Each cell represents a separate regression and displays estimates of $\hat{\theta} \equiv \sum_{j=0}^{T} \hat{\beta}_j$; all regressions include month dummies. Both panels use the experimental sample albeit at different frequencies. Panel A presents results from estimating (2) 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 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. * denotes statistical significance at the 5% level, ** at the 1% level and *** at the 0.1% level.

Table 3: 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:* e 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 (ie. 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 (ie. 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 (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.

Table 4: 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) opened a card with another bank within a twelve month period ($\pm$ 6 months) of cancellation; (c) did not open any other cards between May 2003 and October 2006 (i.e. until 6 months before the experiment started); and (c) 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 made Bank A had they not switched. This counterfactual is calculated using a matching estimator (defined in Section 3.2.2) that pairs the switcher i that closed the study card A at time t with 10 "control" clients $(j_1, \ldots, j_{10})$ in the pool of non-switchers that still have an active study card at t and have the smallest 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 found 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 parenthesis) of the mean in Panel B, columns (1)-(3). 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.

Table 5: Treatment Effects on Default

	Standard dependent variable		Selected strata in May/09		
	Sep/07 (1)	May/09 (2)	Min.Pay, 6-11M (3)	Full Pay,24+M (4)	Min.Pay,24+M (5)
r = 15, MP = 5	0.000	-0.026*	-0.018	-0.001	-0.037**
	(0.001)	(0.008)	(0.015)	(0.006)	(0.012)
r = 15, MP = 10	-0.001*	-0.015	0.015	-0.002	-0.028*
	(0.001)	(0.010)	(0.015)	(0.006)	(0.012)
r = 25, MP = 5	0.002**	-0.023*	-0.015	-0.010	-0.032**
	(0.001)	(0.007)	(0.015)	(0.006)	(0.012)
r = 25, MP = 10	-0.003	-0.008	-0.005	-0.003	-0.016
	(0.002)	(0.006)	(0.015)	(0.006)	(0.012)
r = 35, MP = 5	0.003*	-0.000	0.006	-0.007	-0.003
	(0.001)	(0.003)	(0.015)	(0.006)	(0.012)
r = 35, MP = 10	-0.000	-0.002	-0.013	-0.000	-0.008
	(0.001)	(0.006)	(0.015)	(0.006)	(0.012)
r = 45, MP = 10	-0.000	0.005	0.018	0.001	-0.004
	(0.000)	(0.007)	(0.015)	(0.006)	(0.012)
Constant (r = 45, MP = 5)	0.016***	0.193***	0.346***	0.040***	0.182***
	(0.000)	(0.006)	(0.011)	(0.004)	(0.009)
Observations	143,916	143,916	15,978	16,000	15,987
R-squared	0.000	0.001	0.001	0.000	0.001

Notes: All regressions contain strata dummies and use sample weights. Column (1) is estimated for default (bank-initiated revocations) 6 months after the start of the intervention while the remainder use default at the end of the experiment (26 months). Columns (3),(4) and (5) estimate the endline regressions for three different strata – (a) "Min Payers,6-11M" borrowers who were with the bank for less than a year (but more than six months) in January 2007 and were in the lowest payment category (as of January 2007) ;(b) "Full Payers,24+M"borrowers who had been with the bank for more than 2 years by January 2007 and were in the highest payment category (as of January 2007); (c) "Min Payers,24+M" borrowers who had been with the bank for more than 2 years by January 2007 and were in the lowest payment category (as of January 2007). * denotes statistical significance at the 5% level, ** at the 1% level and *** at the 0.1% level.

Table 6: Treatment Effects on Client-Initiated Cancellations

	Standard dependent variable		Selected strata in May/09		
	Sep/07 (1)	May/09 (2)	Min.Pay, 6-11M (3)	Full Pay,24+M (4)	Min.Pay,24+M (5)
r = 15, MP = 5	-0.008**	-0.035***	-0.039***	-0.011	-0.040***
	(0.002)	(0.004)	(0.008)	(0.011)	(0.010)
r = 15, MP = 10	0.001	-0.011**	-0.030***	-0.008	-0.008
	(0.002)	(0.003)	(0.009)	(0.011)	(0.011)
r = 25, MP = 5	-0.005*	-0.024***	-0.029***	0.011	-0.028**
	(0.002)	(0.004)	(0.009)	(0.011)	(0.011)
r = 25, MP = 10	0.008	-0.003	-0.028**	0.008	0.001
	(0.005)	(0.004)	(0.009)	(0.011)	(0.011)
r = 35, MP = 5	-0.003	-0.018**	-0.026**	0.002	-0.024*
	(0.001)	(0.005)	(0.009)	(0.011)	(0.011)
r = 35, MP = 10	0.004*	-0.004	-0.006	0.019	-0.006
	(0.002)	(0.002)	(0.009)	(0.012)	(0.011)
r = 45, MP = 10	0.007	0.017**	0.002	0.022	0.017
	(0.003)	(0.005)	(0.009)	(0.012)	(0.011)
Constant (r = 45, MP = 5)	0.051***	0.134***	0.095***	0.150***	0.142***
	(0.002)	(0.002)	(0.007)	(0.008)	(0.008)
Observations	143,916	143,916	15,978	16,000	15,987
R-squared	0.001	0.002	0.003	0.001	0.003

Notes: All regressions contain strata dummies and use sample weights. Column (1) is estimated for cancellations 6 months after the start of the intervention while the remainder use cancellations at the end of the experiment (26 months). Columns (3),(4) and (5) estimate the endline regressions for three different strata – (a) "Min Payers,6-11M" borrowers who were with the bank for less than a year (but more than six months) in January 2007 and were in the lowest payment category (as of January 2007) ;(b) "Full Payers,24+M"borrowers who had been with the bank for more than 2 years by January 2007 and were in the highest payment category (as of January 2007); (c) "Min Payers,24+M" borrowers who had been with the bank for more than 2 years by January 2007 and were in the lowest payment category (as of January 2007). * denotes statistical significance at the 5% level, ** at the 1% level and *** at the 0.1% level.

Table 7: Treatment Effects on Bank Revenues

	Standard dependent variable	Selected strata in May/09		
	May/09	Min.Pay, 6-11M	Full Pay,24+M	Min.Pay,24+M
	(1)	(2)	(3)	(4)
r = 15, MP = 5	-2,859***	-3,426***	-514***	-3,113***
	(212)	(222)	(123)	(164)
r = 15, MP = 10	-2,642***	-3,148***	-701***	-2,854***
	(178)	(222)	(144)	(164)
r = 25, MP = 5	-1,889***	-2,098***	-328*	-2,067***
	(140)	(229)	(129)	(169)
r = 25, MP = 10	-1,893***	-2,346***	-258*	-2,049***
	(135)	(223)	(124)	(165)
r = 35, MP = 5	-964***	-1,115***	-329*	-1,058***
	(72)	(239)	(134)	(184)
r = 35, MP = 10	-1,167***	-1,165***	-208	-1,324***
	(114)	(233)	(127)	(167)
r = 45, MP = 10	-469***	-488*	-23	-522**
	(41)	(245)	(130)	(176)
Constant (r = 45, MP = 5)	2,768***	1,708***	-185	3,291***
	(110)	(172)	(96)	(133)
Observations	143,916	15,978	16,000	15,987
R-squared	0.035	0.027	0.003	0.042

Notes: All regressions contain strata dummies and use sample weights. The dependent variable is our measure of bank revenue from a study card. Column (1) is estimated for all clients while Columns (2),(3) and (4) estimate the endline regressions for three different strata – (a) "Min Payers,6-11M" borrowers who were with the bank for less than a year (but more than six months) in January 2007 and were in the lowest payment category (as of January 2007) ;(b) "Full Payers,24+M"borrowers who had been with the bank for more than 2 years by January 2007 and were in the highest payment category (as of January 2007); (c) "Min Payers,24+M" borrowers who had been with the bank for more than 2 years by January 2007 and were in the lowest payment category (as of January 2007). * denotes statistical significance at the 5% level, ** at the 1% level and *** at the 0.1% level.

Table 8: 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, autoloan, 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. * denotes statistical significance at the 5% level, ** at the 1% level and *** at the 0.1% level.

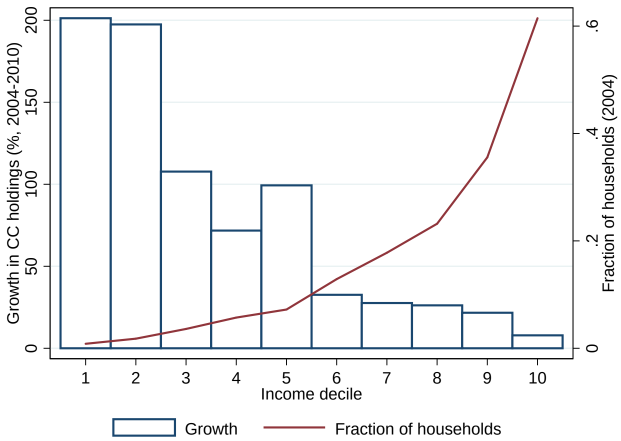
Table 9: Formal vs Informal Loan Terms

	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)
Age		-0.483			97.86***		0.005***		
		(1.45)			(10.73)		(0.002)		
Monthly expenditure		0.014*			0.382***		0.000		
		(0.007)			(0.060)		(0.000)		
Car		-26			-760***		-0.059***		
		(16)			(130)		(0.020)		
Washing machine		-43			110		0.007		
		(36)			(226)		(0.040)		
Appliances		28			-364*		-0.023		
		(31)			(198)		(0.034)		
Constant	291***	336***	152***	3,658***	564	4699***	0.520***	0.333**	0.436***
	(19)	(125)	(41)	(134)	(960)	(762)	(0.021)	(0.149)	(0.122)
Education dummies	No	Yes	No	No	Yes	No	No	Yes	No
Sample dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
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

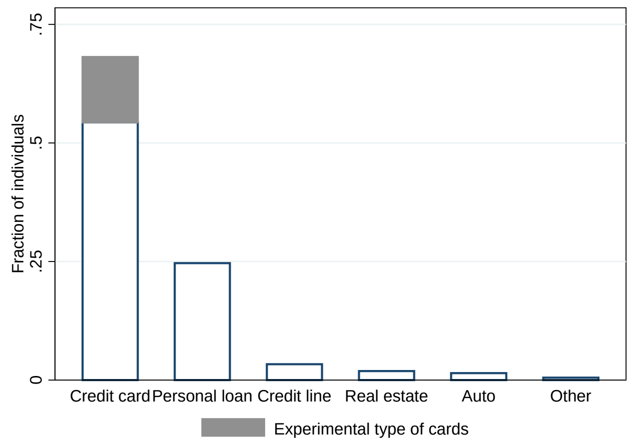
Notes: Data from National Survey of Household Living Standards (Rubalcava and Teruel, 2006) is used to construct the table. The table shows the difference between formal and informal interest rates (Columns (1)–(3)), peso loan amounts (Columns (4)–(6)) and the loan duration (Columns (7)–(9)). We consider a loan to be from a formal entity which we define as a banking institution and informal otherwise. Standard errors are shown in parentheses. ***, ** and * show statistical significance at the 1, 5 and 10 percent, respectively.

Figures

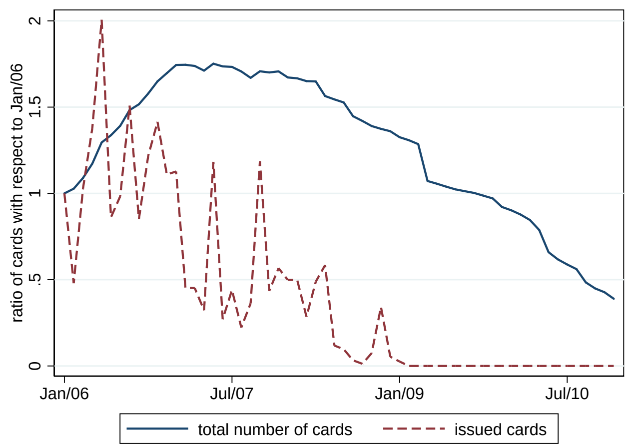
Figure 1: Credit Card Growth, Study Card and First Time Loans and Study Card Stocks and Flows



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



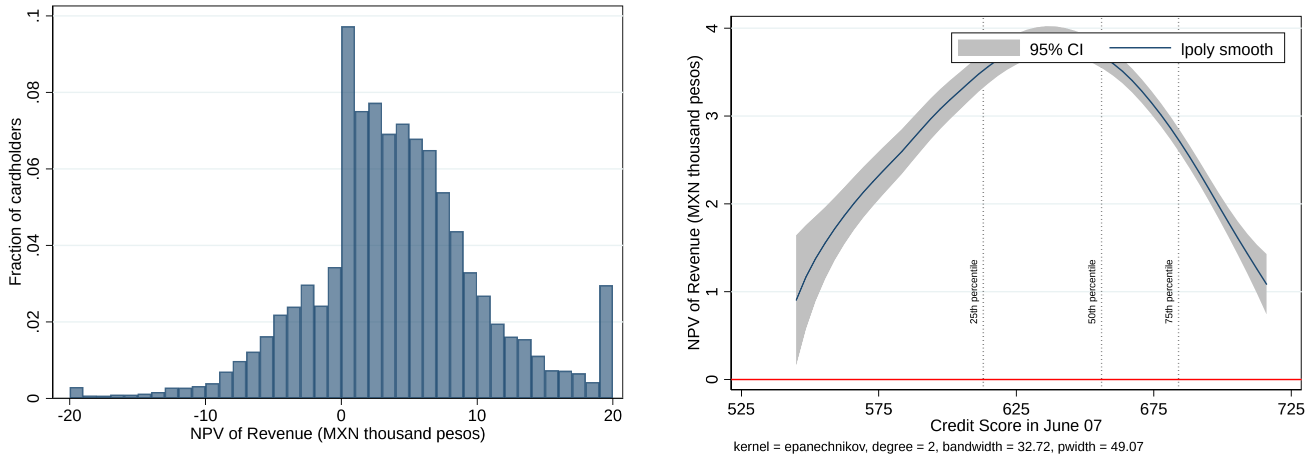
(b) First Time Loan, by type



(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 ("Card A"). 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.

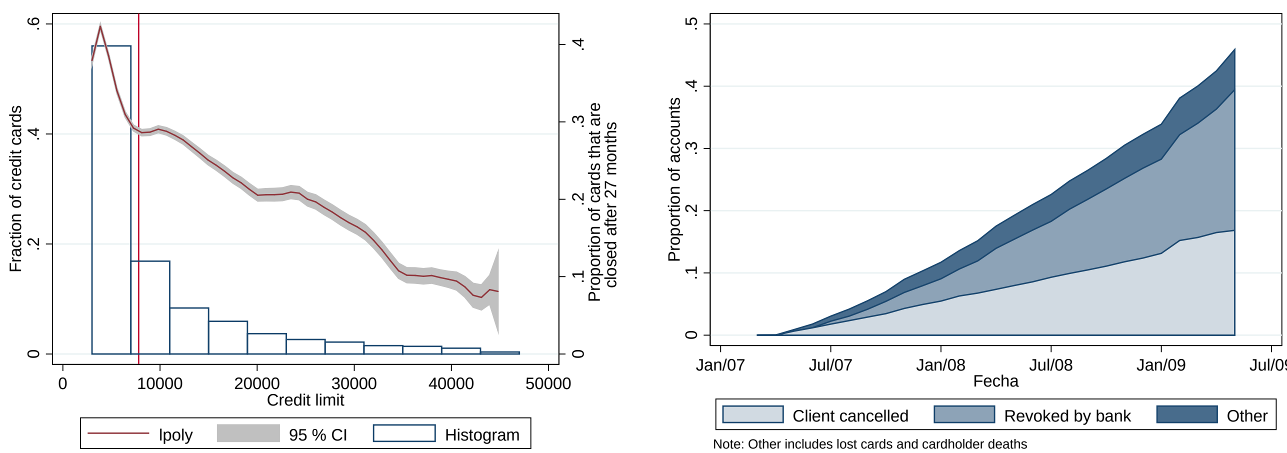


(a) Net Present Value of Revenue ('000 Pesos) Control Group

(b) Mean revenue by Credit Score

Notes: Panel (a) represents the distribution of our revenue measure for the control group (using sampling weights). For clarity, the histogram is censored at $\pm 20,000$ 2007 Pesos. Panel (b) 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. The analogous graphs for different strata are in Figure OA-15.

Figure 3: Card Exits: Experimental Data vs Population

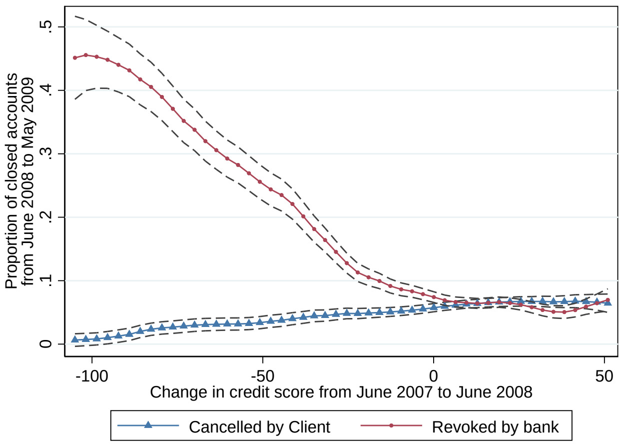


(a) Card Closings in Representative CB Sample

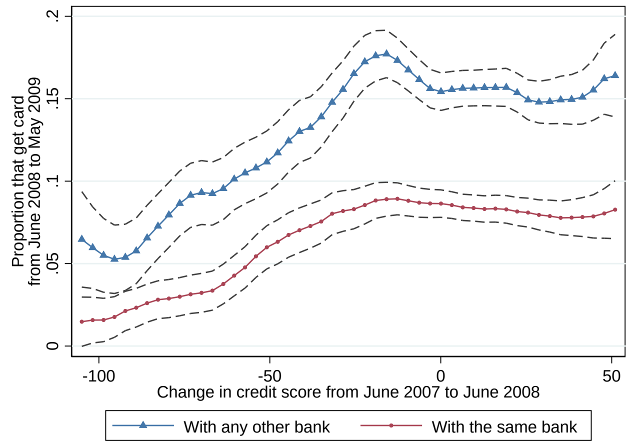
(b) Card Closings in Experiment, by Type of Closing

Notes: Panel (a) uses credit card information from the CB 2010 data. The X-axis represents credit limits. The left Y-axis – corresponding to the hollow bars – represents the fraction of cards that fall in the respective credit limit bin. The right Y-axis – corresponding to the unbroken red line – marks the fraction of cards closed. The unbroken red line represents the results of a local polynomial kernel regression of card closing (an indicator equal to 1 if a credit card is closed within 26 months of being opened) against the card credit limit at origination. The grey shaded area denotes point-wise 95% confidence intervals. The vertical red line shows the mean initial credit limit for the study card in the experiment. Panel (b) 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-12 in the online appendix plots the analogous graphs for figure (b) for two different strata.

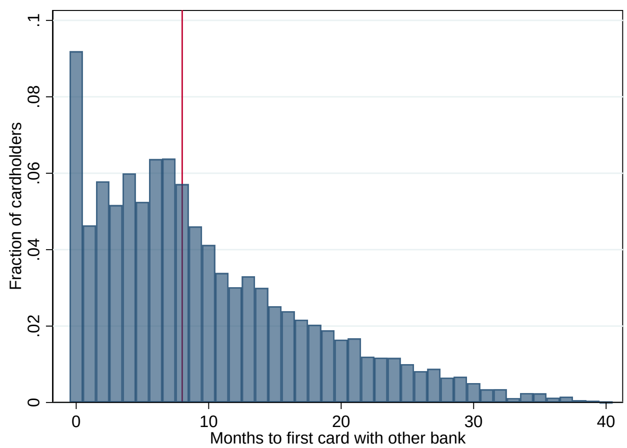
Figure 4: Client "Poaching" and the First Lender Externality



(a) Card Closings and Changes in Credit Scores



(b) New Cards and Changes in Credit Scores



(c) Time between getting 1st and 2nd card in Mexico

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 local polynomial kernel regressions where the explanatory variable in each case is the *change* in the credit score from June 2007 to June 2008. The dependent variable is either an indicator variable for default from June 2008 to May 2009 (the red line), or an indicator variable for cancellation between June 2008 - May 2009 (the blue line). We exclude borrowers who cancel or default prior to June 2008. The dashed lines denote point-wise 95% confidence intervals. Panel (b) examines new card origination using two kernel regressions. The X-axis remains the same as in Panel (a). The Y-axis represents the fraction of cardholders that obtain new cards between June 2008 and May 2009. The dependent variable for the first regression – represented by the red line – is an indicator variable equal to one if a borrower obtains a card from Bank A between June 2008 to May 2009. The dependent variable for the second regression – represented by a blue line – is an indicator variable equal to one if the borrower obtains a card from *any other bank* during that same period. The broken lines represent point-wise 95% confidence intervals. Panel (c) displays the distribution of the number of months from when a borrower obtains their first card (i.e the study card) to when they obtain their second card at a *different* bank. The vertical line shows the median of the distribution.

Figure 5: Timeline for the Experiment

0. **Strata information:**

   Strata variables recorded.

1. **Bank data:**

   Monthly card level data from 03/07 to 05/09.

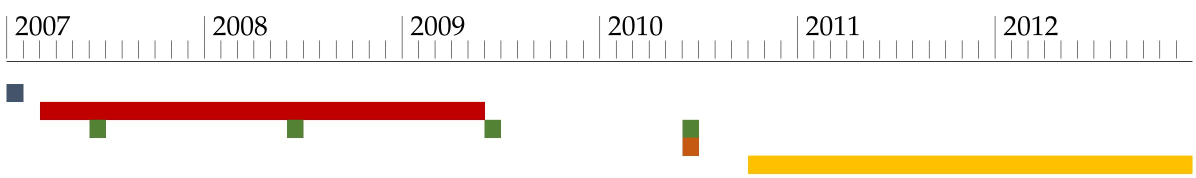
2. **Credit Bureau data:**

   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.

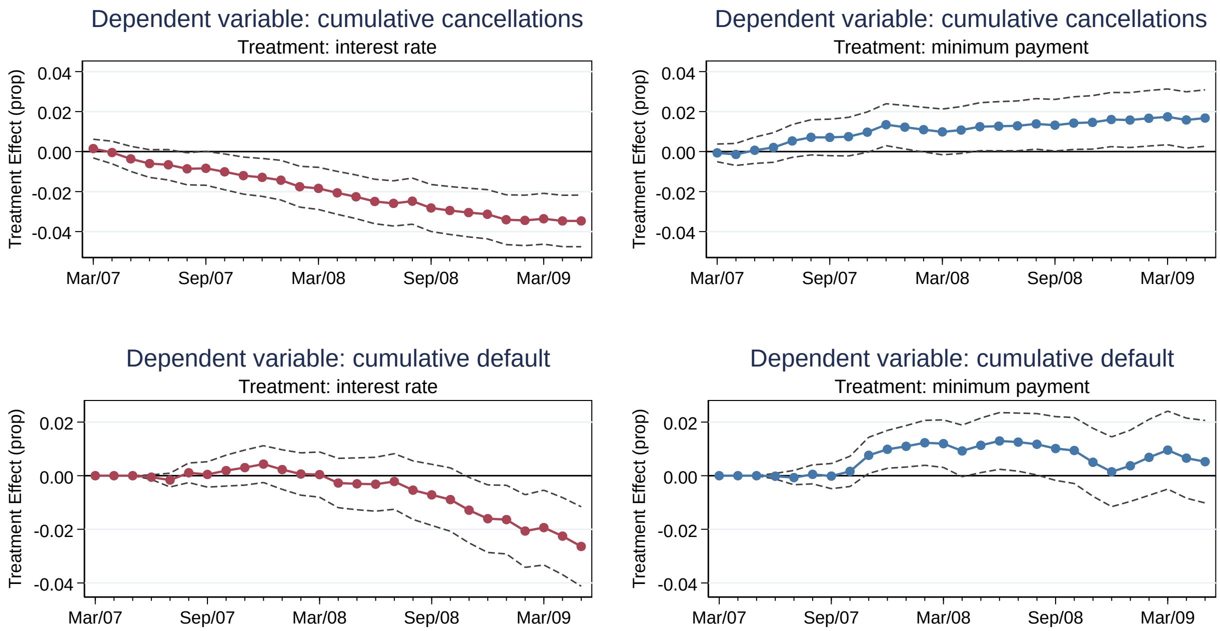
3. **Social security data:**

   Individual-level, monthly information from 10/10 to 05/14.



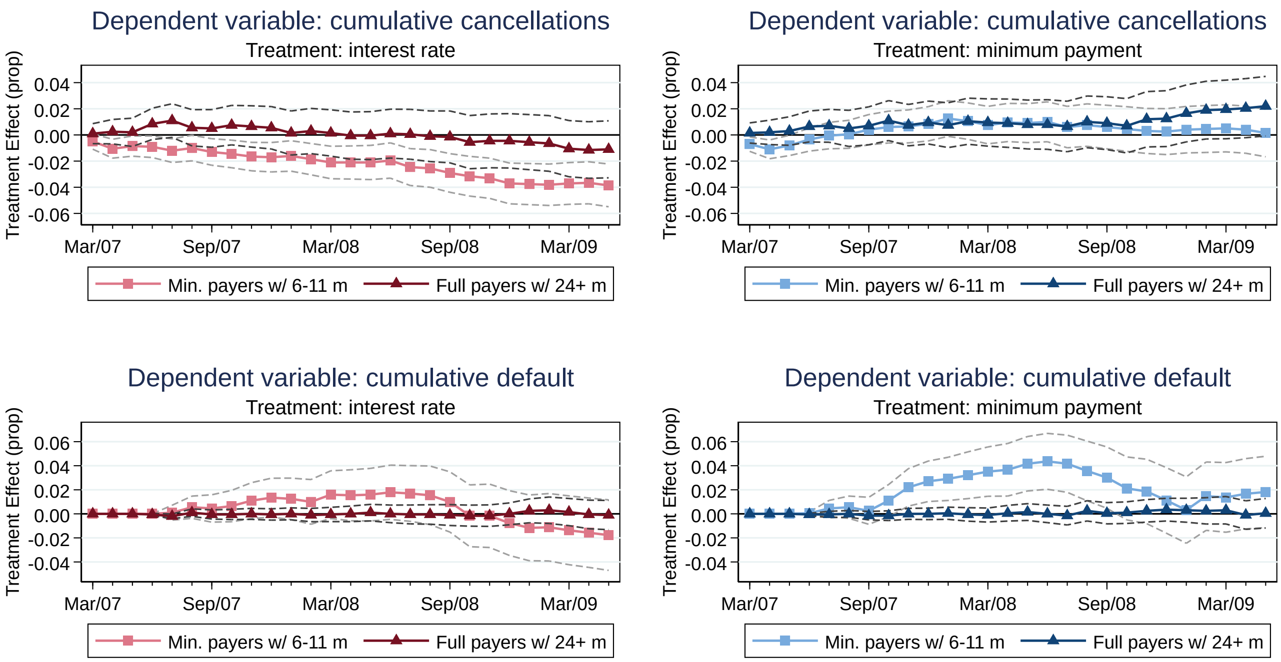
Notes: This figure presents a timeline for the experiment. The data for the 9 experimental strata was recorded in January 2007. Data from the experiment is provided monthly for each card from March 2007 to May 2009. We use CB information for the experimental sample, which is provided to us in 4 snapshots: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

Figure 6: Default/Revocation and Client Initiated Cancellations



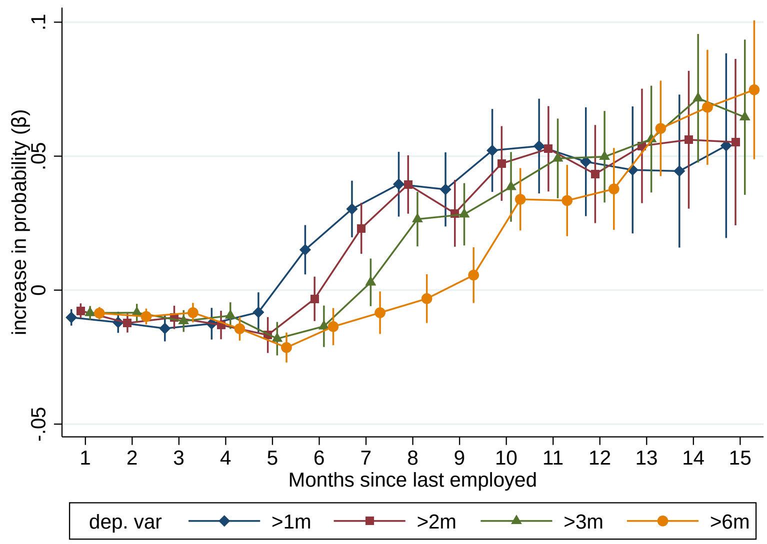
Notes: These sub-figures plot month-by-month treatment effects. For each month (between March 2007 and May 2009 inclusive) we regress the outcome (default or cancellation) on all treatment and stratum indicators using sampling weights. For clarity, we only display treatment effects for a subset of treatments. Each dot corresponds to the coefficient on the treatment indicator for that month along with 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) from March 2007 until the respective month. The last coefficients, therefore, coincide with those from the treatment effect tables for May 2009.

Figure 7: Default/Revocation and Client Initiated Cancellations: by Strata



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

Figure 8: The "Effect" of Unemployment Spells on Delinquency



Notes: The figure presents $\widehat{\beta}_k^j$ estimates from the regression specification (4) estimated using OLS. The data is matched CB - Social Security Employment (IMSS) data and an observation is a borrower-month. We observe binary employment status for 86,363 borrowers from October 2011 to May 2014 (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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Financial Inclusion and Contract Terms:
Experimental Evidence from Mexico

Sara G. Castellanos, Diego Jiménez-Hernández, Aprajit Mahajan, Enrique Seira

Online Appendix

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 2.3) 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.

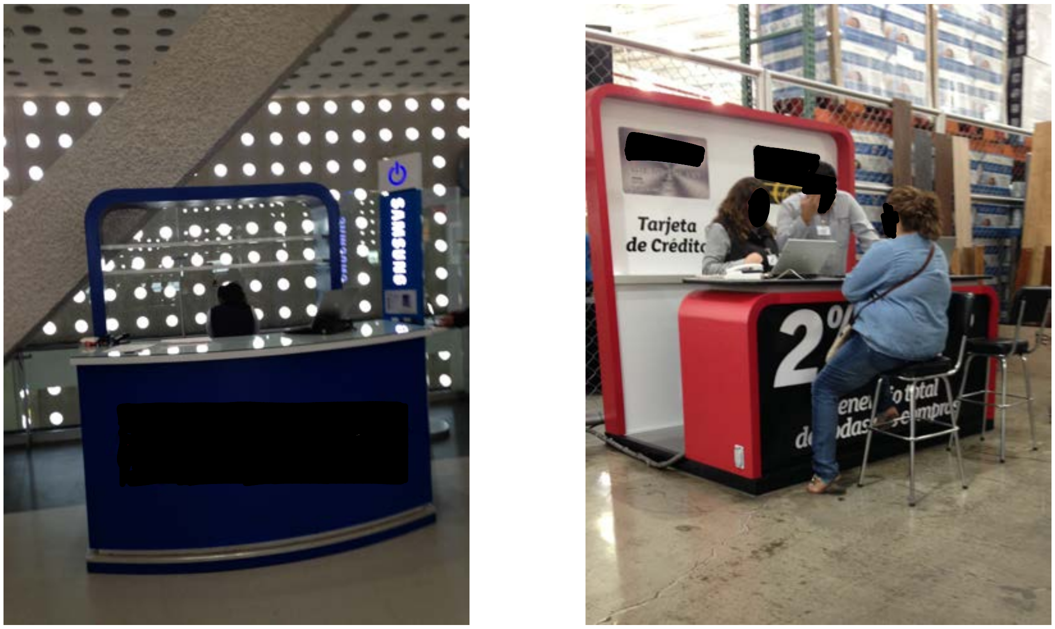
Next, we restrict to individuals in the credit bureau who had at least one credit card open in June 2010 (ie. those shown in Column 3). We then drop any individual whose tenure in months of the oldest credit falls outside of r_0 and r_{50}. Then, for each $i = 1, \ldots, 50$ we define q_i as the number 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-9: 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-10: Example of Promotional Kiosks



A.3 Data Check

We argue the following relation holds in our data:

$$\text{amount due}_{i,t} = \text{amount due}_{i,t-1} + \text{purchases}_{i,t} - \text{payments}_{i,t} + \text{fees}_{i,t} + \text{debt}_{i,t} \times \text{interest rate}_i \quad (5)$$

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-10 summarizes our results. We find that that inferred interest rates match closely with experimental interest rates. This suggests that the debt transition equation (5) 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-10: 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

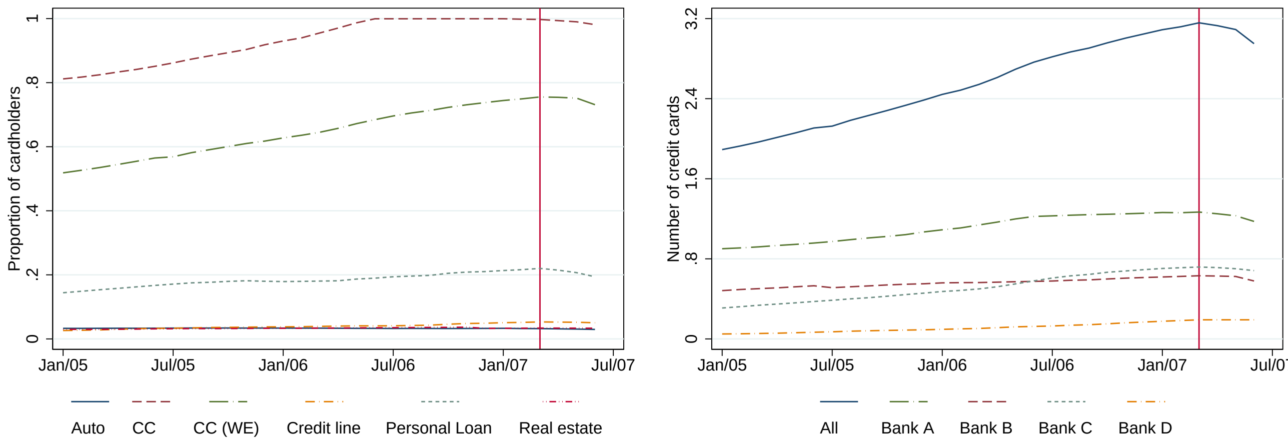
Notes: This table estimates equation (5) by OLS on months with positive debt. That is we estimate the β's in the following equation: $Amount\,due_{it} = \beta_0 + \beta_1 Amount\,due_{it-1} + \beta_2 Payments_{it} + \beta_3 Purchases_{it} + \sum_k \gamma_k Debt_{it} \times I(r = k) + \beta_5 Fees_{it} + \epsilon_{it}$, 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 Evolution of Financial Inclusion

Figure OA-11: Prior loans for the experimental sample

(a) Prior loans **(b)** Number of credit cards

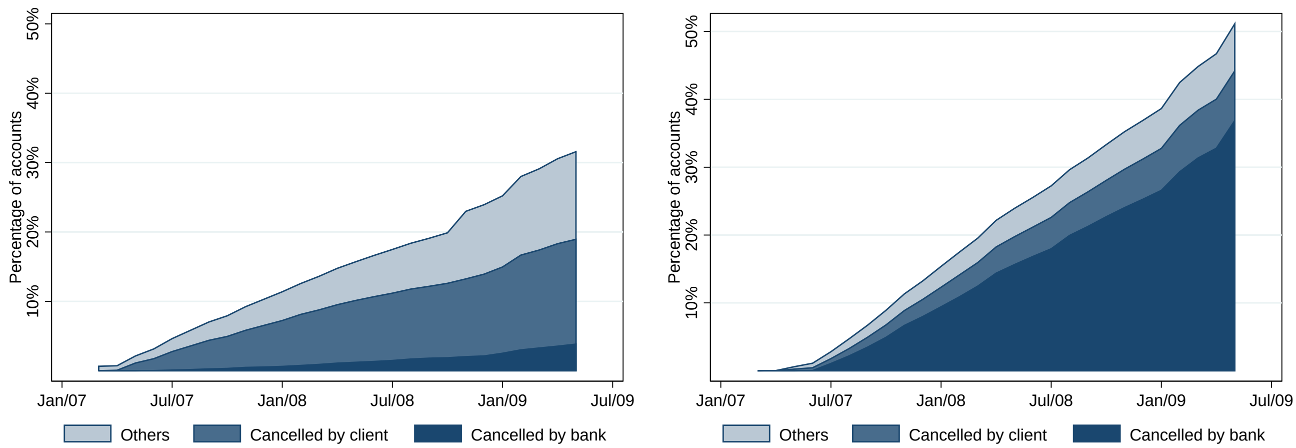


Notes: The graphs plot loans and credit cards held by the experimental sample from January 2005 through the beginning of the experiment (the vertical line marks the start of the experiment). Panel (a) shows the proportion of cardholders that, in any given month t, have the given type of loan, plotted separately for the five most common loan categories. CC stands for credit cards and CC(WE) stands for credit cards excluding the study card. We exclude a small number of other loans loans (loans for furniture, car lease loans, home equity loans, guaranteed cards, and unsecured loans) that together, represent less than the 0.02 percent of the dataset. Panel (b) shows the number of credit cards the experimental sample has in each period of time broken up by originating bank (Bank A is our study bank). We exclude five banks that together represent 0.1 percent of all cards in the credit bureau at the beginning of the experiment.

B.2 Risks and Revenues from Financial Inclusion

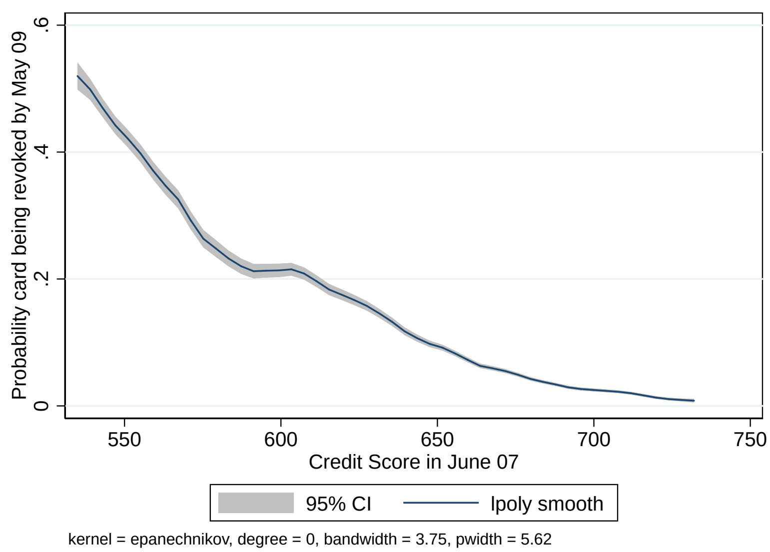
Figure OA-12: Card Exit during the Experiment: Selected Strata

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



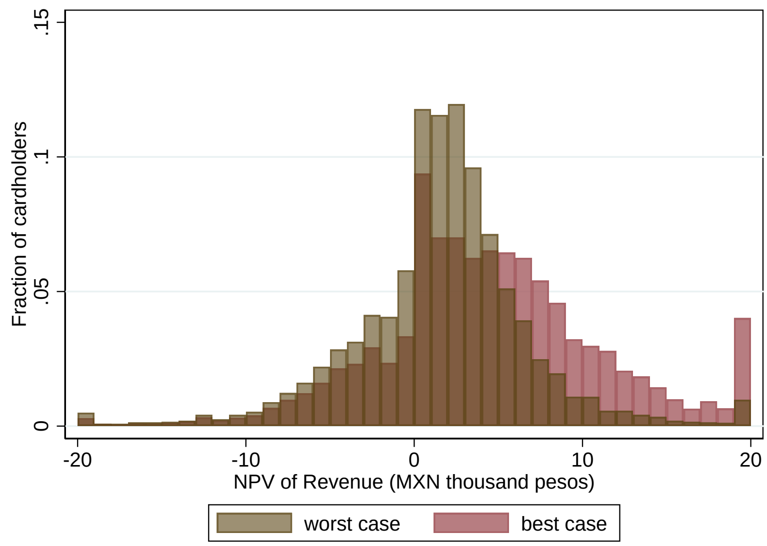
Notes: These figures plot card exit rates over the course of the experiment for the control group and for two different strata. Card closure is subdivided into whether it was a (a) default/revocation, (b) cancellation or (c) closure for some other reason (primarily lost cards or death). The aggregate exit rates (for the entire sample) are in Figure 3b in the main paper.

Figure OA-13: 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 2.7).

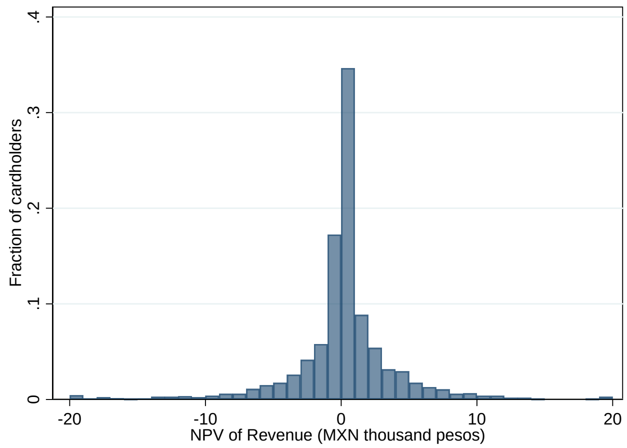
Figure OA-14: Robustness: Best and Worse Case Bounds (for α_i)



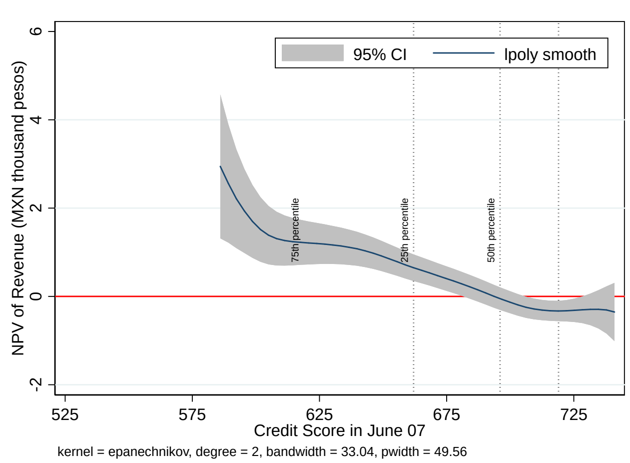
Notes: Histograms of the revenue measure computed with $\alpha_i = .05$ (low recovery rates) for all borrowers (in brown) and the best case scenario with $\alpha_i = 0.95$ (in pink). The resulting histograms do not vary significantly from the one constructed using estimated values of α_i for each borrower.

Figure OA-15: Revenue and Credit Scores: By Strata

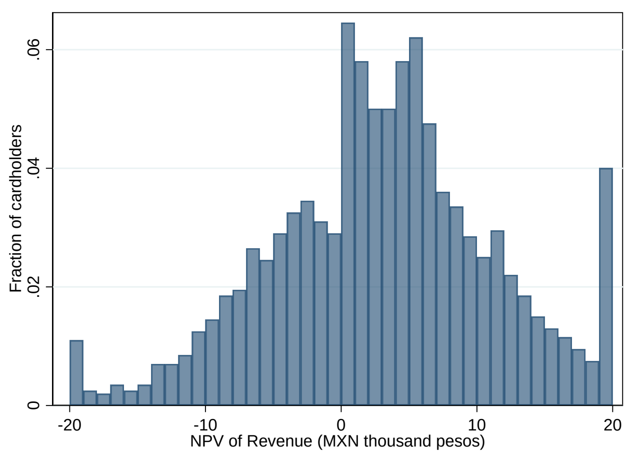
(a) Distribution of Revenue
Full payers with 24+ months with the study card
("Full,24+M")



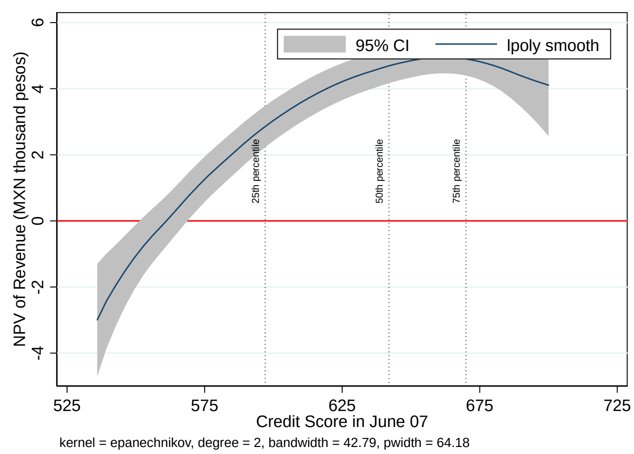
(b) Revenue and Credit Score
Full payers with 24+ months with the study card
("Full,24+M")



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



(d) Revenue and Credit Score
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 Cancellations

Table OA-11: Predicting cancellations

	Cancellations		
	Benchmark	Linear Regression	Random Forest
	(1)	(2)	(3)
Panel A. Public information available at the moment of application			
ρ(predicted,realized)	0.00	0.14	0.15
Out of sample root MSE	0.35	0.35	0.34
Out of sample MAE	0.27	0.26	0.26
Out of sample R-squared	0.00	0.02	0.02
AUC - ROC Curve	0.50	0.62	0.62
Panel B. March 2007 public information			
ρ(predicted,realized)	0.00	0.14	0.15
Out of sample root MSE	0.35	0.35	0.35
Out of sample MAE	0.27	0.26	0.26
Out of sample R-squared	0.00	0.02	0.02
AUC - ROC Curve	0.50	0.61	0.62
Panel C. March 2007 public and private information			
ρ(predicted,realized)	0.00	0.26	0.31
Out of sample root MSE	0.35	0.34	0.33
Out of sample MAE	0.27	0.24	0.23
Out of sample R-squared	0.00	0.07	0.10
AUC - ROC Curve	0.50	0.66	0.70

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 (ie. 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 (ie. 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.

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	Part-balance payer	Full-balance payer	
	(1)	(2)	(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

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

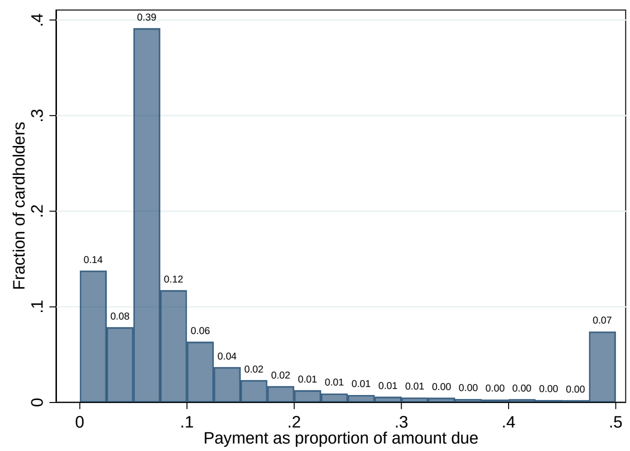
	CTR	r = 15 %		r = 25 %		r = 35 %		r = 45 %		Total	P-value	Observations
		mp = 5 %	mp = 10 %	mp = 5 %	mp = 10 %	mp = 5 %	mp = 10 %	mp = 5 %	mp = 10 %			
	(1)	(2)	(3)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
Panel A. All observations												
Age	39	39	39	39	39	39	39	39	39	39	0.70	160,935
	(6)	(6)	(6)	(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.

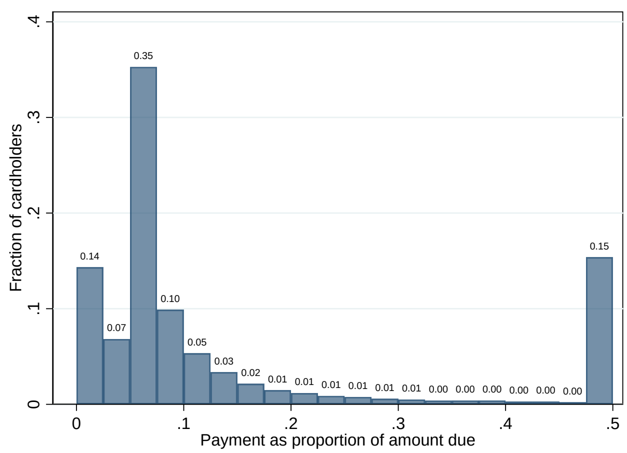
C.2 Minimum Payments Bind for a Substantial Fraction of Borrowers

Figure OA-16: Payment as a fraction of debt before the experiment

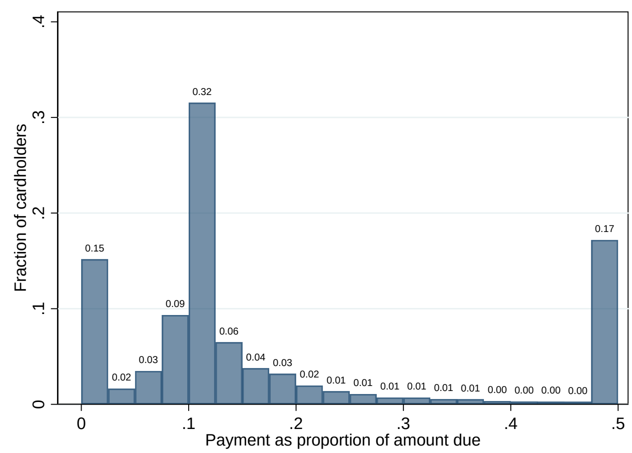
(a) Mar/07 - all treatment arms



(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 $.5$, so the rightmost bin for each panel includes those whose payment ratio is $.5$ 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.

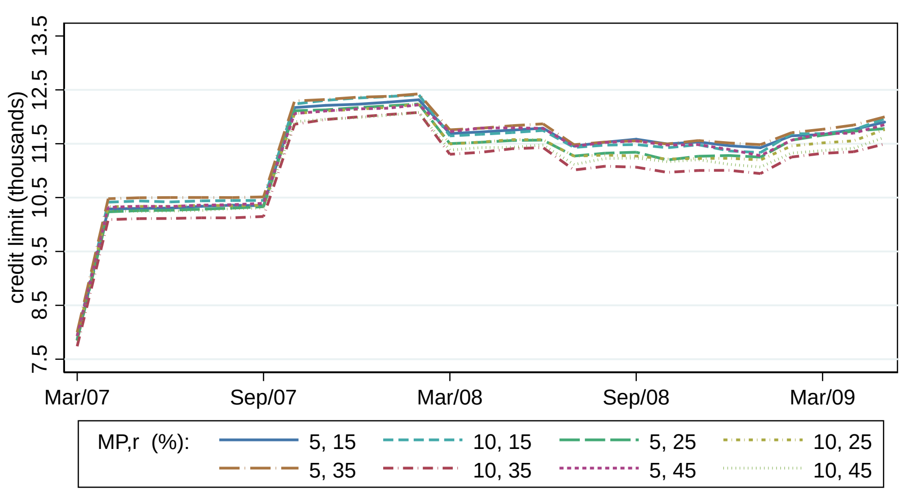
C.3 Credit Limits Are Orthogonal to Randomization

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.

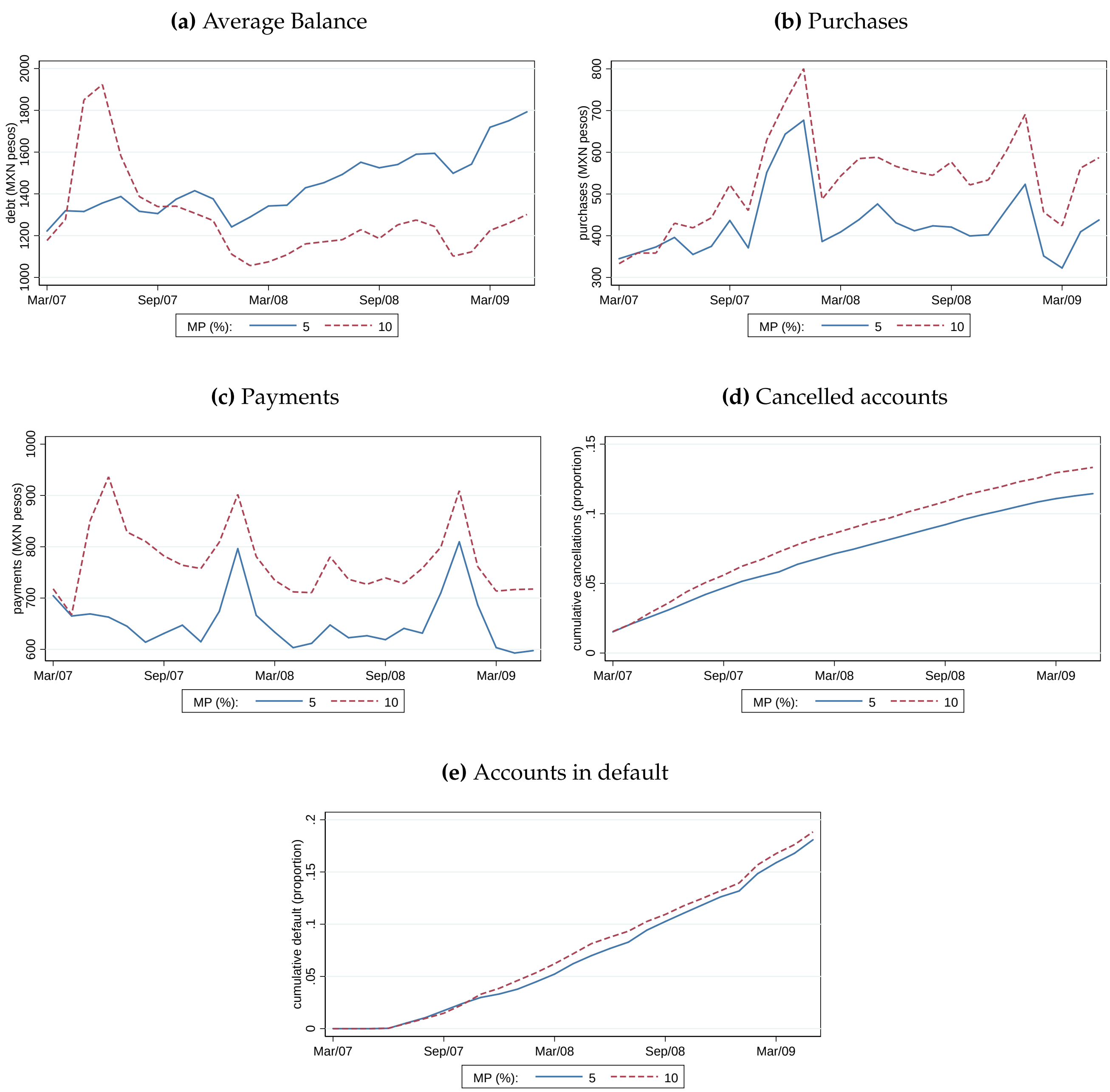
Figure OA-17: Credit Limits by Month by Treatment Arms



C.4 Experimental Results: Other Outcomes

C.4.1 Experiment: Raw Data

Figure OA-18: Effect of Minimum Payment Variations



(a) Average Balance

(b) Purchases

(c) Payments

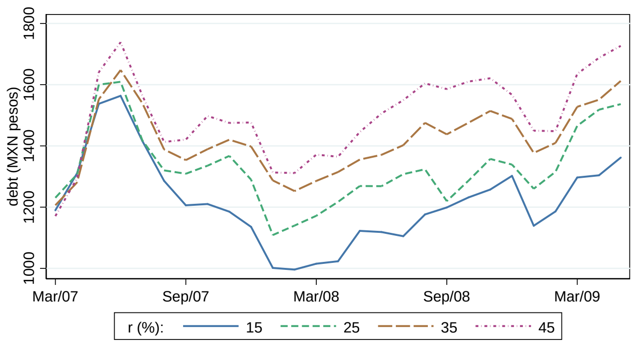
(d) Cancelled accounts

(e) Accounts in default

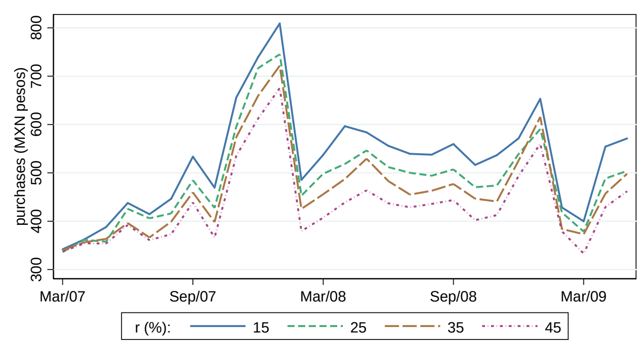
Note: The figures plot five different outcomes over time separately for borrowers in the 5% and 10% minimum payment arms (pooling over the interest rate arms). Figure OA-18d graphs the cumulative cancelled cards and Figure OA-18e graphs cumulative defaults.

Figure OA-19: Effect of Interest Rate Variation

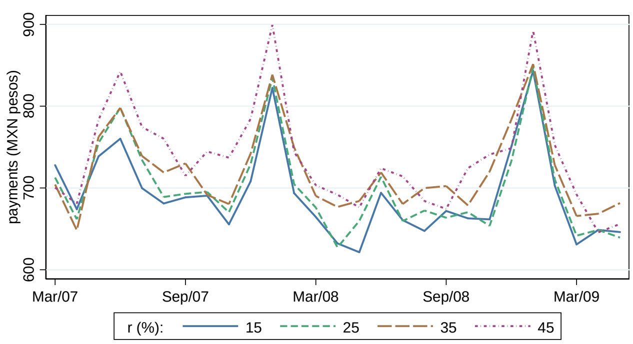
(a) Average Balance



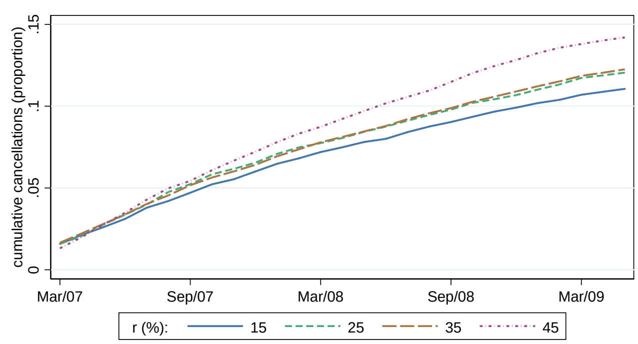
(b) Purchases



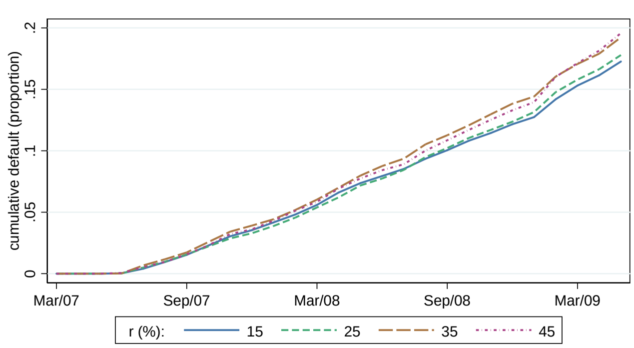
(c) Payments



(d) Cancelled accounts



(e) Accounts in default (cumulative)



Note: The figures plot five different outcomes over time separately for borrowers in each of the four interest rate arms (pooling over the minimum payment arms). Figure OA-19d graphs the cumulative cancelled cards and Figure OA-19e graphs cumulative defaults.

C.4.2 Default and Delinquency with CB data

Table OA-16: Treatment Effects for Credit Bureau Measured Delinquencies

	Delinquencies		Cummulative delinquencies	
	30 days (1)	90 days (2)	30 days (4)	90 days (5)
I:15%, P:5%	-0.033***	-0.025***	-0.025***	-0.026***
	(0.009)	(0.008)	(0.009)	(0.007)
I:15%, P:10%	-0.045***	-0.033***	-0.025***	-0.030***
	(0.009)	(0.008)	(0.009)	(0.007)
I:25%, P:5%	-0.022**	-0.017**	-0.019**	-0.017**
	(0.009)	(0.009)	(0.009)	(0.008)
I:25%, P:10%	-0.031***	-0.019**	-0.020**	-0.018**
	(0.009)	(0.009)	(0.009)	(0.008)
I:35%, P:5%	-0.002	0.000	0.002	0.001
	(0.010)	(0.009)	(0.009)	(0.008)
I:35%, P:10%	-0.019**	-0.012	-0.007	-0.017**
	(0.010)	(0.009)	(0.009)	(0.008)
I:45%, P:10%	-0.003	0.005	-0.000	-0.003
	(0.010)	(0.009)	(0.009)	(0.008)
Constant	0.225***	0.181***	0.295***	0.208***
	(0.007)	(0.007)	(0.007)	(0.006)
p-value Treatment	0.000	0.000	0.001	0.000
p-value Strata	0.000	0.000	0.000	0.000
Observations	97,130	97,130	144,000	144,000
R-squared	0.041	0.040	0.041	0.038
Dependent Variable Mean	0.189	0.151	0.261	0.174

Notes: This table provides alternative measures for delinquencies using data from the credit bureau (robust standard errors are shown in parentheses). The first three columns shows results from regressions where the dependent variable is equal to one if the account is delinquent in May 2009, zero if it has not been delinquent and has a missing value if the account no longer updates its information to the credit bureau. The last three columns show results from regressions where the dependent variable is equal to one if the account has ever been delinquent between March 2007 and May 2009. Columns (1) and (4) define as delinquency being past due by 30 days or more on the study card. Columns (2) and (5) define as delinquency as being 90 days or more past due on the study card.

C.4.3 Debt, Purchases and Payments: Methodology

In Section 5 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.[97] In addition, when useful, we also examine treatment effect heterogeneity

[97] These are currently presented in graphical form. Tables available upon request.

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[98] (10% of the population) and(c) the "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.[99]

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.[100] We include strata dummies $\{S_{ji}\}_{j=1}^{9}$ and probability weights in all specifications.[101]

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 $t = 1 \ldots 26$

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

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

[98]viz. their average payments prior to January 2007 were less than 1.5 times the average minimum payments during this period.

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

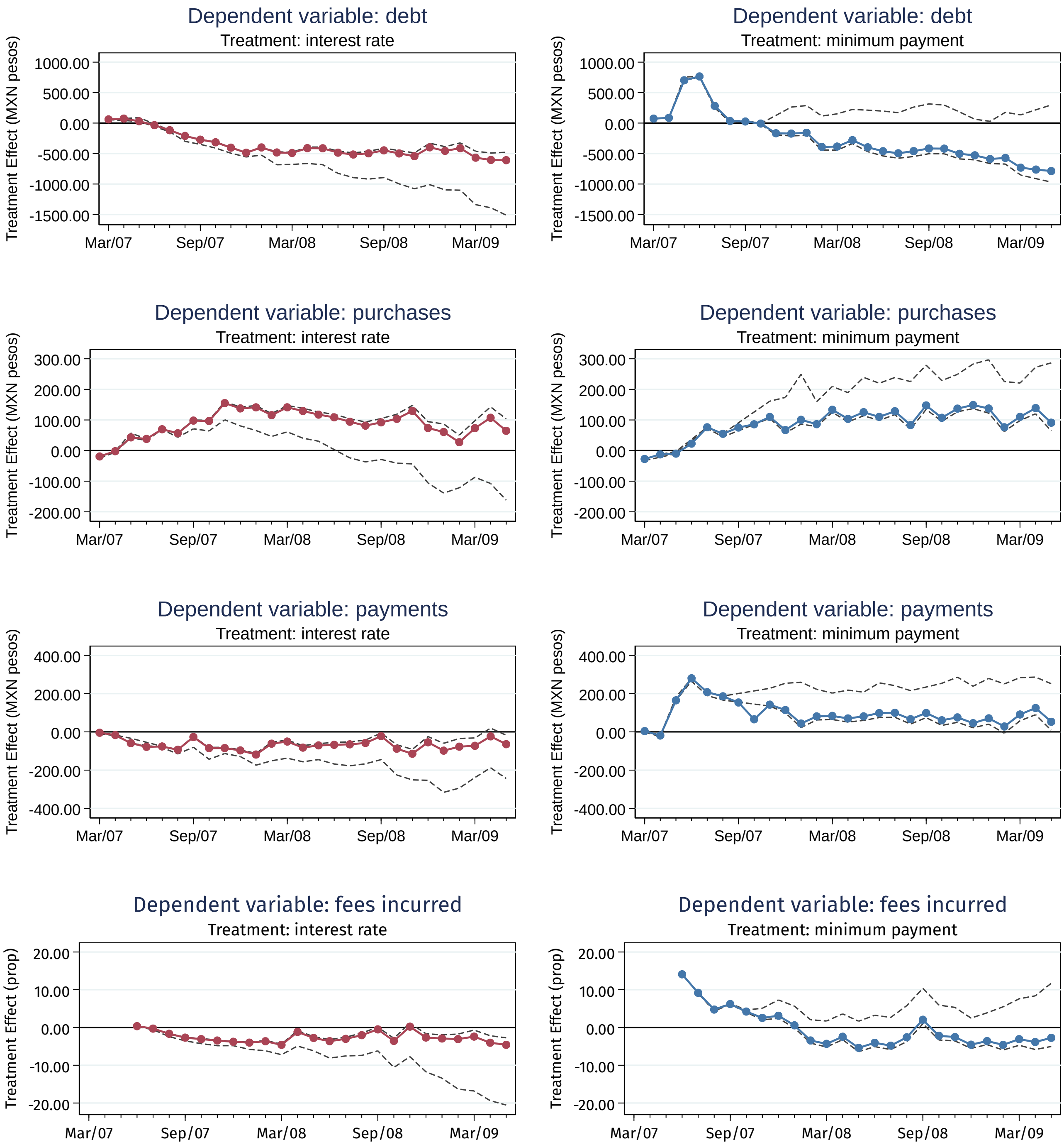
[100]As mentioned earlier, the issue with the control arm is that we do not observe the different interest rates faced by borrowers in the arm.

[101]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.

and in both cases the excluded arm is the $(45\%, 5\%)$ arm.[102] 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.

[102] 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).

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 $[0.28, 0.42]$.[103] 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.

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.[104] 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.[105]

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.4.7 and C.4.9 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 $[-0.38, +0.25]$.[106] Second, monthly payments declined modestly in response to the interest rate decreases with the long-term bounds estimated to be $[+0.04, +0.39]$.[107] 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.[108] 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

[103]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.

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

[105]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.

[106]The short-term effects have tighter bounds of $[-0.38, -0.18]$ that suggest modest increases in purchases. More details are in Table OA-19.

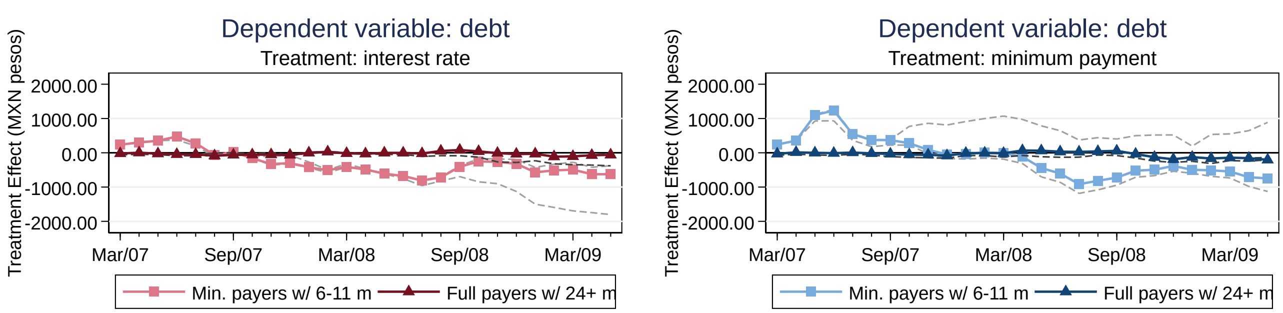
[107]Bounds for the short-term are qualitatively similar at $[+0.06, +0.24]$. See Table OA-20 for more details.

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

out by 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.[109] 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 and at best 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 "Full,24M+" stratum and the light diamonds (red or blue) correspond to the "Min,6-11M" 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.

C.4.6 Effect on Total Debt

Finally, in Table OA-18 we examine the effect of the two experimental interventions on overall debt as measured by the credit bureau. We measure debt in three different ways – total outstanding bank debt (e.g. credit card, auto, mortgage), total amount in arrears and the total of all credit lines. In all cases, we find that experimental variation in contract terms had no statistically or substantively significant effect on overall debt load.

[109]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-21 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-17: 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. * denotes statistical significance at the 5% level, ** at the 1% level and *** at the 0.1% level.

Table OA-18: Longer Term Effects of the Experiment on Aggregate Formal Credit (Credit Bureau Data)

	Outstanding Bank Debt		Bank Debt in Arrears		Credit Line	
	Total (1)	CC (2)	Total (3)	CC (4)	Total (5)	CC (6)
r = 15, MP = 5	4,888	514	-1,527	-1,451	6,399*	1,176
	(2,782)	(1,782)	(1,011)	(916)	(3,240)	(2,030)
r = 15, MP = 10	3,689	700	-1,075	-974	5,756	2,233
	(2,762)	(1,800)	(1,021)	(928)	(3,181)	(2,039)
r = 25, MP = 5	3,091	-272	-967	-871	3,968	1,225
	(2,762)	(1,772)	(1,039)	(939)	(3,151)	(2,040)
r = 25, MP = 10	1,831	-31	-1,457	-1,292	2,019	698
	(2,713)	(1,788)	(1,022)	(926)	(3,100)	(2,031)
r = 35, MP = 5	3,750	268	-1,127	-800	5,031	1,707
	(2,790)	(1,777)	(1,023)	(935)	(3,214)	(2,040)
r = 35, MP = 10	-3,428	-2,875	-1,892	-1,841*	-3,183	-2,839
	(2,597)	(1,722)	(1,004)	(904)	(2,998)	(1,952)
r = 45, MP = 10	171	-1,581	-805	-615	1,351	-660
	(2,699)	(1,790)	(1,042)	(948)	(3,132)	(2,029)
Constant (r = 45, MP = 5)	74,043***	52,402***	20,696***	18,588***	92,248***	64,724***
	(1,848)	(1,247)	(736)	(668)	(2,126)	(1,401)
Observations	136,441	136,441	136,441	136,441	136,441	136,441
R-squared	0.000	0.000	0.000	0.000	0.000	0.000

Notes: The table presents the effects of the treatment on broad measures of formal credit sector participation as measured in the Credit Bureau. Standard errors are shown in parentheses. * denotes statistical significance at the 5% level, ** at the 1% level and *** at the 0.1% level.

C.4.7 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-19. Figure OA-20 shows monthly treatment effects over the course of the experiment and Table OA-19 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-19 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-19 col (2)). Imputing zeros to purchases for all card cancellers reduces the upper bound, but it remains positive (bottom of Table OA-19 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.[110] 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 *Comparte-*

[110] 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.

mos. 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).

Table OA-19: Treatment Effects on Monthly Purchases

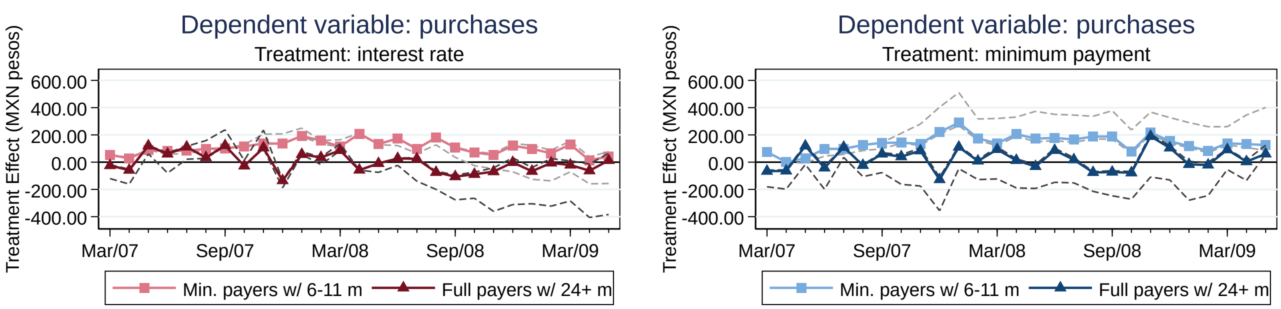
	Standard Outcome			Deflated by Amount Due in $t-1$		Selected Strata (May/09)		
	Sep/07 (1)	May/09 (2)	May/09 w/zeros (3)	Sep/07 (4)	May/09 (5)	Min.Pay,6-11M (6)	Full Pay,24+M (7)	Min.Pay, 24+ M (8)
r = 15, MP = 5	98.225***	63.111***	75.424***	0.018***	0.007***	42.462	12.398	73.787
	(14.889)	(8.567)	(5.671)	(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. * denotes statistical significance at the 5% level, ** at the 1% level and *** at the 0.1% level.

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 "Full,24M+" stratum and the light diamonds correspond to the "Min,12M–" stratum.

C.4.8 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.[111] Alternatively, since higher minimum payments imply, ceterus 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 re-

[111]Results available upon request.

ductions in debt. We explore this implication by 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-19) 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.4.9 Payments: Effect of Interest Rates

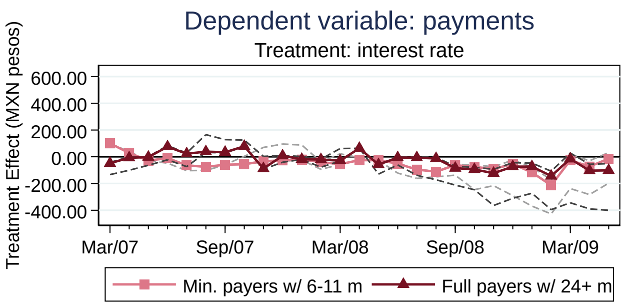
Figure OA-20 presents the Lee bounds along with the point estimates from equation (8) for each month in the experiment. We see that there is a gradual decline in monthly payments during the first six months and the bounds at the six-month mark are $[-103, -24]$ pesos with implied elasticity bounds of $[.06, .24]$ 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 "Full,24M+" stratum and the light red diamonds correspond to the "Min,12M–" stratum.

C.4.10 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[112] (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.

[112] 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-20: 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. * denotes statistical significance at the 5% level, ** at the 1% level and *** at the 0.1% level.

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.4.11 Effect on Fees

The effect of the interventions on card fees are summarized in Table OA-21 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).[113] 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 $+0.47$.

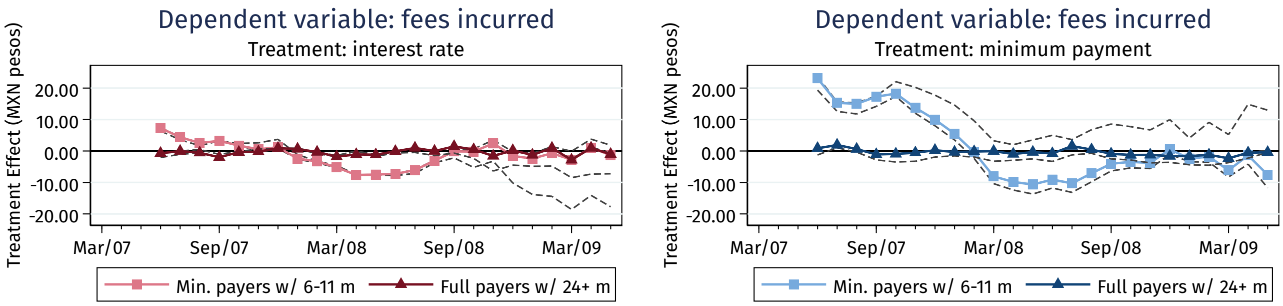
Table OA-21: Treatment Effects on Fees

	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.

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

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



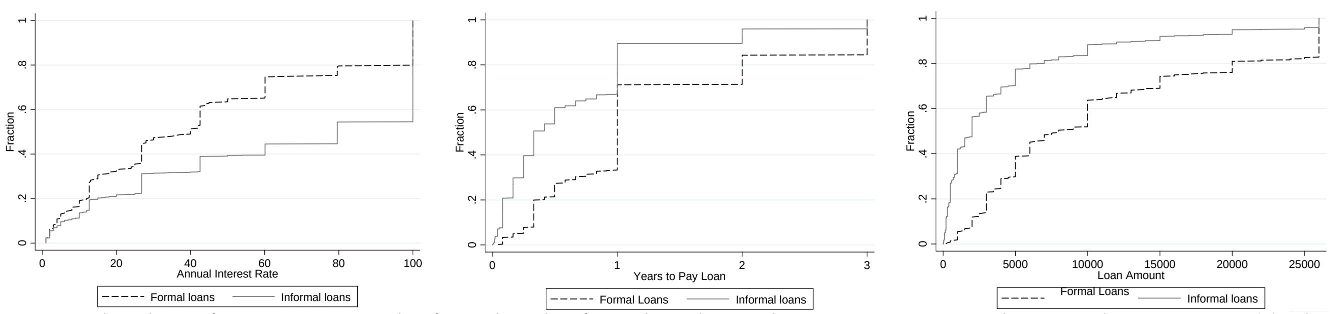
For each stratum, for each month in the experiment we regress fees on a treatment dummy. Each dot corresponds to the coefficient on the treatment dummy for that month along with point-wise Lee bounds. For simplicity the comparison group here is the $(45\%, 5\%)$ ground and the treatment group for the 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 "Full,24M+" stratum and the light diamonds correspond to the "Min,12M–" stratum.

Appendix D. Mechanisms

D.1 Consequences of default

Figure OA-25: 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-22: 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***	-0.02***	-0.01***
	(0.00)	(0.00)	(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.