# Mitigating the Risks of Financial Inclusion with Loan Contract Terms: Experimental Evidence from Mexico*

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PRELIMINARY

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**Abstract**

We use observational and experimental data over 27 months to study a large Mexican bank's experience of lending to borrowers with limited formal credit histories. The population is characterized by high default risk with about two-fifths of all study cards being cancelled or revoked during this period. We construct measures of revenues per customer and find them to be variable and difficult to predict. Borrowers generating good credit history with the bank are more likely to switch banks or add cards providing suggestive evidence of a lending externality, which by itself might be an important barrier to financial inclusion. We then use a large scale randomized experiment on a representative sample of the bank's marginal borrowers and find that large experimental changes in contract terms (interest rates and minimum payments) have small effects on defaults both in the short and long-term (27 months). The large experiment induced changes in contract terms also have small effects on other outcomes including purchases, payments and debt. Muted borrower responses are consistent with limited outside options.

**Keywords:** Financial Inclusion, Credit cards, Default risk, Mexico.

**JEL:** D14, D18, D82, G21.

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# 1 Introduction

According to the World Bank, nearly 60 percent of adults in developing countries do not use any formal financial services (Demirgüç-Kunt and Klapper, 2012). There is also a growing body of evidence linking financial development to improved economic outcomes. 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 (from India and Mexico respectively) that this relationship is causal. Perhaps unsurprisingly, developing country governments and international development institutions have made financial inclusion a key policy priority with the World Bank president calling for Universal Financial Access for all by 2020.[1] In Mexico itself, a 2011 presidential decree established the National Council for Financial Inclusion to expand financial access to underserved populations.

The precise mechanics of inclusion particularly for large financial institutions – who are often seen as key players – have not received much attention in the literature. In this paper we use a large Mexican bank's experience of lending to borrowers with limited credit histories to shed light on financial inclusion as practiced by large financial institutions. Mexico provides an appropriate setting for examining financial inclusion. Less than a third of all adults reported having an account at a financial institution and only 13 percent reported having a credit card of any sort (Demirgüç-Kunt and Klapper, 2012). In this paper we focus on credit cards (the most common formal borrowing instrument in the country) and in particular on a specific card that accounts for approximately 15% of all first-time formal sector loan products nationwide.

We begin by using a range of data sources to describe the characteristics and challenges of financial inclusion in Mexico. We show that new to banking borrowers (NTB borrowers henceforth) are a risky proposition: they are likely to default and generate variable and unpredictable revenue streams. A natural question then is whether banks can use contract terms to mitigate this borrower risk. We use a large-scale randomized experiment to answer this question and examine the effectiveness of contract terms in changing borrower behavior.

We find that providing credit cards to NTB borrowers involves substantial risk. 44 percent of the control group exited the bank over the 27 months of the experiment. We construct a proxy for net bank revenues and find that the average net present value of the 27 month net revenue per-account is 623 pesos (standard deviation 2580 pesos). Using a large set of observables we can explain less than five percent of the variation in card default, exit and profits. These two facts together lead us to conclude that lending to NTB borrowers is risky and much of this risk is hard to predict at the time the card is issued.

Since we follow NTB borrowers through their first couple of years with a credit card, we observe how these individuals generate credit histories through time, and what are the consequences of these histories. We find evidence for a rarely-discussed barrier to financial inclusion. Stiglitz (1993) notes that information about a borrower has public good characteristics. We show that this

[1] See e.g. https://goo.gl/R8YZTn for an overview of the World Bank's Universal Financial Access by 2020 strategy.

may be quantitatively important in developing countries undertaking financial inclusion. We find that 28% of NTB clients with no default on their first card in the first year get a second card with a different bank in that same year whereas the corresponding figure for clients who default in the first year is 2%. This "poaching" of clients may create a disincentive to give loans to borrowers with no credit history, thereby hindering financial inclusion, since the bank is aware that NTB borrowers who establish a good credit history are more likely to be poached by other banks. We also carry out a rough calculation to quantify this externality in terms of lost revenue for the first lender and find that the amount lost is large.

A natural next question is whether banks can use contract terms to mitigate this risk ex-post. We use a large randomized experiment to answer this question. Specifically, we test whether default is mitigated by lower interest rates and higher minimum payments – key components of the card contract. The experiment allocated 162,000 relatively NTB clients from one of Mexico's largest banks to 8 treatment arms by varying annual interest rates ($r$) from $45\%$ to $15\%$ (the decrements were {15%,25%;35%;45%}) and minimum payments ($MP$) between $5\%$ and $10\%$ for 27 months. To our knowledge, this is the first paper that experimentally varies the minimum payment in credit card contracts.

We report several interesting findings. First, reducing interest rates by a factor of three reduces card default by $2.5$ percentage points (on a base rate of $20$ percent) over the 27 month experiment. The implied elasticity is a relatively small $0.18$ reflecting the fact that the intervention was a relatively large reduction in interest rates.[2] Second, we find that lower interest rates **reduce** debt modestly because of interest compounding– our preferred bounds for the implied elasticity are $[0.34, 0.74]$. Our overall conclusion is that even relatively large changes in annual interest rates have limited effects on card debt and default for NTB borrowers. These result are somewhat surprising. A positive correlation between default and interest rates (or loan 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 borrowers to borrow excessively leading to default and welfare losses.[3] Higher minimum payments could lead to lower defaults in the long-term both through an incentive effect of a lower debt overhang and also through a selection effect, as weaker borrowers exit the bank (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

[2] 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 (2016) 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.

[3] See e.g. Warren (2007); Bar-Gill (2003). Several countries including Mexico have considered mandating higher minimum payments over concerns of borrower over-indebtedness. The Mexican Central Bank has expressed concern over the size of the minimum payments (https://goo.gl/MkYbV0) and its relation to debt. 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) and 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).

are more expensive than the card interest rate. Our third finding is that doubling the minimum payment from 5 to 10 percent (of amount outstanding) has a small negative effect on debt – our preferred bounds for the implied elasticity are $[-0.44, -0.01]$ – and reduces default by 1 percentage point (the implied elasticity is $.06$). Our overall conclusions for the minimum payment interventions are then similar to those for the interest rate changes – large changes in the required minimum payment had small effects of borrower debt and default for NTB borrowers.

We conjecture that this limited response to large changes in contract terms arises from the adverse consequences of default, including the onerous terms of the informal credit market. We find that defaulting on the bank card is associated with a 98 point reduction in the credit score (borrowers had an average score of 540) and an associated 2.5 percentage point (70%) reduction in the likelihood of getting a formal loan in the next 12 months. Furthermore, using a nationally representative sample, we find that relative to informal loans, formal loans are on average a third cheaper, more than one and a half times larger and have a repayment period that is twice as long.

This lack of responsiveness of default to contract term variation coexists with high average default levels and we next attempt to study the causes for this high default and attrition. Unfortunately we lack individual level data on important economic indicators (e.g. employment status and income) for the 2007-2009 period and therefore provide two different sets of exercises. First, we rely on municipality level data for the 2007-2009 period with our experimental sample. Our results suggest that variations in municipality level economic conditions affect card default rates pointing the to vulnerability of our population to macro shocks. Second, we perform individual level regressions on the 2010-2014 period using a representative sample of individuals with at least one bank loan in Mexico and find that individual-level unemployment is positively correlated with default as well.

In addition to the papers cited above, this paper connects with two strands in the empirical finance and credit literature. The first is a literature on asymmetric information and market failures in consumer credit markets and the second is a relatively new literature on financial inclusion particularly in developing countries. There is a substantial empirical literature documenting the existence of liquidity and credit constraints[4] and some evidence that these arise from the inability of (poor) individuals to borrow on desired terms.[5] 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.[6] 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.[7] The experimental variation in minimum payments in turn allows us to cleanly measure borrower response[8] as well as examine the importance of liquidity constraints.

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

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

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

[7]see Karlan and Zinman (2016), Attanasio et al. (2008) and Dehejia et al. (2012) for similar exercises in Mexico, the United States, and Bangladesh respectively.

[8] Keys and Wang (2016) and d'Astous and Shore (2015) use observational data from credit card markets in the U.S.

A more recent, policy-motivated, literature identifies lack of access to formal financial services as a general problem in developing countries and advocates for 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[9] documenting for instance the large numbers of people world-wide without access to formal banking services. Dupas et al. (2016) provide some experimental evidence from a multi-country trial that a focus on expanding access to bank accounts by itself may only have limited welfare impacts. The experience of our sample of new to banking borrowers with limited credit histories with a large private bank provides some insights into the challenges associated with expanding financial access through large formal institutions.

The paper proceeds as follows: Section 2 describes the various data sets and Section 3 provides relevant institutional context and describes some facts about financial inclusion in Mexico. Section 4 describes the experiment while Section 5 uses it to examine the effect of variation in contract terms on borrower behavior. Section 6 explores some mechanisms which may influence default and Section 7 concludes. Due to space constraints some robustness analyses and secondary figures and tables are reported in the Online Appendix (OA).

## 2 Data

We use four different data sources. The first comprises two different data-sets from the Mexican credit bureau. We have a large representative sample of one million consumers from the Credit Bureau from 2010 that allows us to make economy wide statements. The second is credit bureau data for our experimental sample (described below). The second source of data comes from a randomized experiment with 162,000 borrowers carried out by a large Mexican private bank (referred to as Bank A from now on). The third source comprises three nationally representative surveys and our final source is the Mexican social security data. We describe each in turn.

### 2.1 Credit Bureau Data (CB data)

We were able to obtain two random samples of one million borrowers each from the credit bureau (CB) representative of the borrowers in June 2010 and in May 2014. A borrower appears in the credit bureau if she has or has had a loan with a formal financial intermediary within the past 6 years. This intermediary is most often a bank, but can also be a phone company, a department store or a credit card issuer. For each borrower we observe the date of loan initiation, lender's name, type of loan, total debt outstanding, amount in arrears and delinquency and default history.[10] 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

to examine borrower response to changes in minimum payments.

[9] e.g. Demirgüç-Kunt and Klapper (2012), though Dabla-Norris et al. (2015) is a notable exception.

[10] We only have limited and inconsistent information on loan amounts and no information on the interest rate.

characteristics of first-time and recent borrowers, their sources of credit and repayment history. In addition we make use of a third sample from the credit bureau, where we are able to match our experimental sample to credit bureau data at an annual frequency (see below) which allows us to observe the sample's other formal sector transactions outside the experiment.

## 2.2 Experimental Data

We use data from a 27 month randomized experiment with 162,000 clients of Bank A to examine the effect of contract term variation on borrower behavior. The sample frame consisted of Bank A's borrowers who held a particular type of store credit card. In private conversations, bank officials noted that the card targeted low-income populations with limited credit histories.[11] The store credit card had an initial credit limit of approximately 7,000 pesos, an annual interest rate of the base rate[12] plus 55 percentage points and a monthly minimum payment of 4% of the total amount outstanding. By 2007, Bank A had close to 2 million clients with this store card and by 2010 the card accounted for approximately 15% of all first-time formal sector loan products.

Consumers in our 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. We discuss the implications of this sample selection for external validity below. The sampling frame was then partitioned into nine strata based on two pre-intervention characteristics that the bank uses internally as predictors of default and are thoroughly described in Section 4.1. The bank then randomly selected a sample of 18,000 clients per stratum – we use stratum weights in all regressions to ensure our results are representative of the population – and within each stratum clients were randomly assigned to one of nine study arms (this is described in more detail below) for a total of 2000 clients per treatment arm within a stratum. In what follows we will often restrict attention to 8 primary study arms which gives us a total sample of 144,000 clients across all strata. The resulting sample is geographically widespread – covering all 32 states, 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 credit limits, debt, purchases, payments, delinquencies and default. We also obtained data on the same variables for the sample about a year after the experiment (in June 2010). In addition to this detailed transaction information we also observe some basic demographic variables – age, marital status and place of residence. We also match our experimental sample to credit bureau data at seven points in time – June 2007-13. This provides us with a complete formal credit history for each experimental sample card-holder at five different points in time – we will refer to this data as the *matched* CB data.

[11] Internally the bank referred to them as the C, C- and D customer segments, similar to the UK's NRS social grades classification.

[12] This is the inter-bank rate also known as the TIIE in Mexico.

### 2.3 Survey Data

We also draw upon three 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[13] to measure loan terms for both formal and informal loans.Finally we use Mexico's employment survey (ENEU) to examine the relationship between unemployment rates and borrower behavior. The data is at the municipality (covering 1,331 municipalities) and quarterly level (for 40 quarters).

### 2.4 Social Security Data

We were also able to merge our sample with the government's social security records from November 2011 to May 2014 to obtain information on occupational status and income conditional on formal employment status of the sample that worked in the formal sector and was hence covered by the IMSS. For our experimental data, we observe income for the 18% of the 162,000 individuals. For the credit bureau samples, we observe income for the 13% and 15% of individuals, for the 2010 and 2014 samples, respectively.

### 2.5 Summary Statistics

Table 1 reports summary statistics for the 162,000 borrowers in our experimental sample and compares them to those for selected sub-samples of borrowers from the larger and more representative CB data. We present summaries of financial variables measured when the experiment began (March 2007) and some basic demographic characteristics.

In Column 2 we report summary statistics for the sub-sample of the CB data that had at at least one active credit card in June 2010 (note that our CB data is a single cross-section from 2010). This is a sample that is nationally representative of the population of borrowers with at least one credit card in 2010. Given that 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. The details of the matching procedure can be found in the Online Appendix. Finally, in Column 4 we restrict the CB sample to experienced borrowers – namely those who have had a credit history of at least 8 years.

We see (Panel C) that the experimental sample is approximately half male with an average age just under 40 and about three-fifths of the sample is married. Other than marriage rates (which are substantially lower) 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. Instead, we only observe it for the subset of individuals who also have records in the social security database (i.e. those in the formal sector) which is approximately 18% in our experimental sample and about 13%

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

in the CB data. With this selection issue in mind, we now discuss the income data. The experimental sample has an average monthly income of 13,855 pesos, compared to an average of 14,759 for recent borrowers and 22,641 for experienced borrowers.[14] Our experimental sample is thus somewhat less well-off relative to a comparable sub-group in the CB data at least when looking at those who participate in the formal sector. [15] The Online Appendix shows that the distribution function for income for the experimental sample is first-order stochastically dominated by corresponding distribution for the CB sample from Column 2.

Turning to Panel B, we present credit scores for our experimental sample in June 2007. The mean credit score for consumers in our experimental sample is 645 which is at the low end of the "financial inclusion frontier".[16] Figure 2 plots the distribution of these credit scores and for comparison, we also plot the distribution of scores for the a random sample from the credit bureau.[17]

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.4 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). As we will see this is consistent with the substantial financial inclusion effort in Mexico during this period. Other (i.e. non-card) forms of formal borrowing remained relatively rare by comparison.

We next summarize 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. For comparison we compare overall card limits for cards held by the different sub-samples of the CB data outlined above at the date closest to March 2007 (which is June 2010). The mean card limit was $49,604$ pesos for the CB sample as a whole, $22,082$ pesos for the CB NTB sub-sample and $56,187$ pesos for the experienced sub-sample. These figures suggest that our study sample was, expectedly, at the low-end of borrowing ability in the CB data.

Turning next to borrower behavior with Bank A we first define delinquency and default using bank's definitions which appear to be standard in the credit card sector in Mexico. Delinquency is defined as the failure to pay the minimum payment for more than 30 days (i.e. one billing cycle). Default is defined as delinquency for three consecutive months. Although all borrowers in the experiment had no delinquencies in January 2007, a small number (1 percent) were delinquent as

[14]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.

[15]Though they have approximately the same reported income when the CB sample is taken as a whole.

[16]In Mexico, a score lower than 670 is typically ineligible for standard credit card products. The credit score of the Mexican CB was designed by Trans Union and takes values from 400 to 800. It is however not directly comparable to the credit score ranges in the United States.

[17]The credit scores data from the CB is, unfortunately, only for 2016. This is because we were unable to obtain credit scores for the large 2010 cross-sectional CB data (for reasons of confidentiality).

of March 2007 when the experiment began. 22 percent of our consumers also have positive arrears in the CB data by June 2007 – that is an amount owed to formal sector creditors that is past due. Conditional on being in arrears, the average amount past due was 9,738 pesos. For comparison, this is less than half of the amount in arrears reported for the CB sample as a whole or the CB sub-sample of comparable borrowers (albeit in 2010).

We next attempt to quantify the revenue generated by a study borrower over the course of the study while accounting for default. This is a useful initial exercise since it provides one rough measure of the attractiveness of NTB borrowers to Bank A. We proceed as follows for each borrower: for each month we compute the revenue earned by the bank by adding the interest accrued over the month and any late fees and overdraft fees charges. We then subtract from this figure the costs to the bank. The most important of these is the cost of funds loaned to the card holder. We proxy this by computing the monthly average of daily outstanding balances in the account and multiplying it by the interest rate paid by the bank on deposits, which is approximately 1% per year. The other potential cost is the consequences of card default (or card exit more generally) and we account for it by counting the entire amount outstanding as a cost in the period in which the card exits the system (i.e when the card is revoked). We then sum and discount (using a discount rate of 5%) these net revenues over the 27 months of the experiment. This measure is informative as a proxy for profits although it clearly has shortcomings. On the revenue side, it does not for instance, include income from interchange fees or annuity fees. On the cost side it does not include the cost of promotion, advertising or the costs associated with customer acquisition. Finally, we can only use the 27 months for which we have data rather than the complete life of the card (we attempt to address this last concern below). These omitted costs are, however, presumably orthogonal to treatment assignment. Further, while we clearly omit important costs we do capture the main sources of revenue and also account for default. Our calculation yields a net revenue of approximately 623 pesos for the 27 months of the intervention or about 24 pesos per month per borrower. Figure 3(a) plots a histogram of the calculated revenues and shows that the distribution is quite dispersed with a standard deviation of 3,521 pesos.

## 3 Financial Inclusion in Mexico

Formal credit penetration is low in Mexico. The ratio of private credit provided by banks to GDP ratio was 25 percent in 2015, which is low even by Latin American standards.[18] Credit card penetration is likewise low – the percentage of adults with at least one credit card is 8.1% compared to about 52% for the US. The credit card market is also highly concentrated, with the five largest banks jointly controlling approximately 90% of the market for revolving debt.[19] APRs average about 24% (compared to 13% for the US) while non-performing credit card debt was about 8% in

[18] According to the World Bank (http://data.worldbank.org/indicator/FD.AST.PRVT.GD.ZS%?year_high_desc=true) Chile (81%), Panama (80%), Brazil (69%), Bolivia (51%), Colombia (48%) and Peru (38%) all have higher ratios in the same year.

[19] The analogous number for the US is about 62%

2009 in Mexico compared to 6% for the US at its peak.[20]

The importance of credit cards for financial inclusion in Mexico is perhaps best highlighted by the fact that banks primarily use credit cards as the first loan instrument to introduce consumers to formal financing. Panel B of Figure 1 shows that more than 70 percent of first time loans are via credit card borrowing. Interviews with bank staff suggested that expanding financial access using credit cards is a relatively recent and growing phenomenon. The number of credit cards grew from 10 million in the first quarter of 2004 to 24.6 million in the last quarter of 2011.[21] As higher income individuals were the most likely to have obtained cards early [22] a substantial part of the growth in card holders was concentrated on lower income individuals. Panel (a) of Figure 1 shows that from 2004 to 2012 the growth rate of credit card ownership for the lowest two deciles of the income distribution was 200%. Penetration remains low however, with less than 15 percent of below median income households owning any kind of card. In spite of this growth, anecdotal evidence suggests card application rejection rates average about 50 percent with the figure rising to about 70 percent for NTB clients.

### 3.1 Are NTB Borrowers Credit Constrained?

The high rejection rates and limited participation in the formal credit sector raises the possibility that NTB clients continue to be credit constrained. We explore this by examining how card debt in our experimental sample responds to increases in credit limits.[23] If borrowers were credit constrained one would expect they would take advantage of the expanded credit limit and increase borrowing. However, increases in borrowing following credit limit expansions could also be consistent with *lack* of credit constraints if borrowers are replacing costlier non-card debt with cheaper card debt. We cannot rule out this possibility though we discuss its relevance below.

Using panel data on debt and credit limits We regress changes in debt on lagged changes in credit limits and include a set of time-varying covariates and time dummies. Specifically, let $\text{Debt}_{it}$ be the amount of debt held by card $i$ at the end of month $t$ and let $\text{Limit}_{it}$ denote the credit limit for account $i$ at the beginning of month $t$. We estimate regressons of the form

$$\Delta\text{Debt}_{i,t} = \delta_t + \sum_{j=0}^{12} \beta_j \Delta\text{Limit}_{i,t-j} + \gamma' X_{i,t} + \epsilon_{i,t} \tag{1}$$

$\beta_0$ represents effect of a change in credit limits between period $t-1$ and $t$ on debt changes between $t-1$ and $t$. $\beta_j$ represents the incremental increase in debt between periods $t-1$ and $t$ due to a unit change in credit limit $j$ months ago. The quantity $\sum_{j=0}^{12} \beta_j$ then provides us with a measure of the long-run (12 month) total effect of credit limit on debt.

[20] The numbers in 2015 are close to 6% and 2% for Mexico and the US respectively. The online appendix contains more comparative statistics and the data sources for these figures.

[21] Banco de México (2016).

[22] Conditional on having a card, the average number of cards per person was already 4.2 in 2007 (Seira et al., 2016).

[23] This follows the strategy in Gross and Souleles (2002).

We find that after 12 months a credit limit increase of 100 pesos translates into 35 pesos of extra interest paying debt.[24] This propensity to consume out of increases in the credit limit is about thrice as large as the figure for the US in Gross and Souleles (2002), suggesting that these Mexican borrowers are credit constrained and significantly more so than their US counterparts. This evidence on credit constraints will be helpful in understanding the experimental results in the sequel.

### 3.2 The perils of financial inclusion

While there is a substantial literature documenting the effects of credit on borrowers much less is known 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 descriptive evidence on the difficulty of expanding formal credit to NTB borrowers.

Three broad categories of explanation have been put forward in this context. The first is that information on NTB borrowers is limited and asymmetric –it is hard to identify creditworthy borrowers. Second, NTB borrowers typically demand small loan amounts which makes it difficult for lenders to recoup fixed costs incurred irrespective of loan size. Third, the credit history established by NTB borrowers with their first lending bank has a public good aspect to it since credit history established with the initial lender generates a positive externality for competing banks who can use that information to offer products to NTB clients with good credit histories. The initial lender in such a situation may either lose good borrowers or be adversely affected if its clients are thus enabled to borrow from other sources and default is a function of total amount borrowed. In such situations wherein such externalities are not interalized, financial inclusion will be lower than is socially optimal.

We present evidence that supports the first and third of these explanations. First, we document that NTB borrowers are indeed risky – characterized by high default rates and variable and unpredictable revenues for the bank. Second, we provide some empirical evidence of the externality wherein NTB clients who have established a good credit history with Bank A leave for another bank.

#### 3.2.1 High Exit and Unpredictable Revenues

Card exit is typically bad news for a bank as they cannot make profits from them and recover the fixed promotion cost required to open the card in the first place.[25] During the 27 month experiment approximately 44 percent of the control group accounts exited the experiment; 19 percent defaulted (i.e. stopped making payments and subsequently had their card revoked by the bank) and 16

[24] We also use alternative specifications and find, if anything, larger coefficients. Since the change in limit may be requested by the borrower or related to future changes in debt, we use dummies for the time since the last limit increase as instrumental variables (as in Gross and Souleles (2002)). This instrument is plausible since the bank does not evaluate all cards for a limit increase at the same time. See the Online Appendix for details and results.

[25] The bank calculates a cost of about 100 pesos for promotion to open a card.

percent cancelled their cards[26] while another 9 percent exited for other reasons. Figure 4(b) shows that these different causes of card exit evolved smoothly over time during the experiment. This high level of exist is typical of NTB borrowers. One way to assess this is to look at the rate of cexit for credit card holders as a function of their credit limit, since small credit limits are a proxy for being NTB. Figure 4(a) examines this relationship in the CB sample. It shows a kernel regression of an indicator for card closure within 27 months of being opened on the initial credit limit for the card. We see strikingly similar exit rates in the CB data and in our experimental sample.

We next attempt to quantify the net revenue generated for Bank A by the experimental sample during the study period. We use the net revenue defined on p.9. Note that this measure of ex-post profit already takes card exit into account. It also accounts for the probability of default and its severity in a rough manner by using realized default and subtracting that as a cost to the bank. The net revenue is about $24$ pesos per month per borrower. Figure 3 plots a histogram of the net revenue measure showing considerable dispersion as well as the fact that revenue is also hard to predict. A regression of the revenue measure on a set of variables observed by the bank at the time of application yields a $R^2$ less than $.01$ – adding other variables observed post-origination such as debt, payments and purchases at the start of the experiment improve it to $0.18$. Moreover, we split our sample in halves and try to predict in one of the halves the flow of revenues from March 2007 to May 2009 using March 2007 observables and actual observed revenues from the other half; we find that the correlation between the actual observed values and the predicted ones is $0.04$ (see Table OA.16). All in all, this suggests that revenues vary from card to card, and are hard to predict before using observables.

Figure 3(b) plots a kernel regression of the revenue measure on 2007 credit scores. Our revenue measures are quite low at low levels of credit scores and then rise rapidly before flattening out and falling at higher levels. At high credit score levels, net revenues are low since such borrowers typically do not revolve debt or can substitute more easily towards other cards. Revenues are also lower for borrowers with very low credit scores (e.g 550 or below where 5.3% of sample lies).[27]

#### 3.2.2 Client "Poaching" and the First Lender Externality

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".[28] We attempt to document the existence of such an externality and quantify its importance in the Mexican context. We proceed in three steps.

[26] The bank charges no fees for the card so there are no direct costs to keeping the card. However, we conjecture that there are other reasons for cancelling a card. In other work, 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.

[27] Since we calculate profit after the score is measured there is no issue of reverse causality. Note also that the confidence interval is an interval for the *mean* of profit and therefore the fact that it is tight does not imply that profit for a single person is easy to predict.

[28] Consistent with this hypothesis, Petersen and Rajan (1995) showed for the US that young firms in concentrated markets — where it is arguably harder to steal good borrowers ex-post — receive more finance than do similar firms in more competitive markets.

First, we show that for NTB client, building a good (bad) credit history/credit score with the first "inside" bank is a good predictor of (not) getting a second card with a different "outside" bank. This is evidence that the public history in the CB is being used. Second, we show that this hurts the profit of the inside bank. Given the informational content of credit histories, the adverse selection problem need not eliminate the profitability of poaching.

We have already documented the high rate of voluntary card cancellations (i.e. cards that are closed by the borrower after settling all amounts outstanding) in our study in Figure 4(b). Next, in order to focus attention on the most recent set of borrowers, we restrict attention to borrowers for whom the store card was their first card and who have been with the bank for 6-11 months. These are precisely the set of clients for whom credit history generated by the bank would be most critical.

In Figure 5(a) we plot non-parametric regressions of voluntary client cancellation and bank revocations (between June 2008 and May 2009) against the **change** in credit scores in the previous year (i.e. between June 2007 and June 2008). Its apparent that decreases in the credit score are correlated with subsequent bank initiated revocations, and that conversely larger increases in the credit scores are associated with somewhat higher borrower initiated voluntary cancellations. Next we examine these borrowers card-seeking behavior in Figure 5(b). 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 (i.e between June 2007 and June 2008). If there is poaching as a result of public history information revealing that a client is good, we would expect that borrowers with larger improvements in their credit scores are more likely open a card at other bank. We indeed see this, consistent with the idea that other lenders are using improvements in credit history with Bank A to screen borrowers. Bank A also gives them a new card, partly to guard off and limit some of the poaching.

A natural question then is how much revenue the first bank loses in such a situation. To assess this we need to estimate for each client bank A loses to other banks how much would Bank A would have earned from this client had he stayed. We first identify as switchers those consumers who cancel their card with Bank A on any month in the experiment and open a card in another bank within 6 months of closing the card. Denote by $\widehat{Rev}_{i,t}$ the revenue that switcher $i$ would have given to the Bank A in month $t$ had him not switched. Denote by $\tau_i$ the month at which individual $i$ closes its card. Thus, our job is to predict $(\widehat{Rev}_{i,t})_{t=\tau}^{T}$. For switcher $i$ in month $t \geq \tau$, we search – among clients whose for whom the tenure with the credit card, prior payment behavior, and treatment group are exactly the same as the one of switcher $i$ – for the two clients $j_t = (j_{1t}, j_{2t})$ most similar to the switcher in terms of debt, purchases, payments, and credit limit as measured on the month $\tau - 1$, whose card is still open in $t$. This is effectively just a matching technique on euclidean distance of the monetary variables, with an exact match on card tenure and payment behavior. We face the caveat that clients $j_t$ themselves may have exited the experiment in $t + 1$, in which case we search for different counterfactual clients active in period $t + 1$. Counterfactual revenue may be negative if the control clients $j_t$ defaulted on their debt for instance, so that this is a net-of-default revenue. Note that we do this for all periods until the end of the experiment. We then take the present value of this revenue not earned and use it as our measure of revenue lost.[29] We calculate that average revenue forgone for each borrower that switches is 246 pesos per account or 40 percent of the net of present value of revenue. All of our alternative specifications yield even higher revenue losses from 427 to 629 pesos per account.[30]

# 4 Using Contract Terms to Change Behavior

The previous section demonstrated the high rates of default and exit in our NTB experimental sample from Bank A. In general, banks can use both ex-ante screening to select better borrowers and contract terms to guide borrower behavior after selection. Screening, however, is particularly difficult with NTB borrowers given their limited credit history. In addition, as we argued earlier (see p. 12) borrower characteristics explain a very small fraction to the variation in subsequent revenues and borrower exit. This limited ability to identify good borrowers ex-ante then leads banks to rely more on contract terms – the most important being the interest rate on debt, the credit limit and the minimum payment required – to limit default.[31] For instance reductions in the interest rate may be used to reduce moral hazard (or more proximately the debt burden). Similarly, increases in the minimum required payment can be used to try to limit indebtedness and consequent default.

Whether and to what extent such variation in contract terms can mitigate default and exit for NTB borrowers is an open empirical question. This is both because (a) contract terms are endogenous to expected default (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 in interest rates and minimum payments (the bank did not experimentally vary credit limits).[32] We use this experiment to transparently answer the question of the extent to which contract terms mitigate default for NTB borrowers.

[29]We are matching at the individual-month level using information generated on the month previous to switching, and this allows for better prediction than the OLS presented in Table OA.16 and discussed in Section 3.2.1. As any matching on observables, we may have biased counterfactuals if unobservables are correlated with future client revenue and client exit. To test the accuracy of the matching technique we generate placebo exit of non-exiters by using only those individuals who never cancel their card, assigning them random closing dates and testing the accuracy of our matching estimator on a sample of individuals for whom we do observe the true revenue flow. The R-squared between our prediction and the true value is 0.26. See OA.11 for more information.

[30]See Section C.1 of the Online Appendix for more information and for alternative specifications. Table OA.11 presents our results.

[31]Banks do not use menus of contracts to screen borrowers by self-selection.

[32]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. We asked permission to the bank to analyze the data and write an academic paper. 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 the bank and the Central Bank regarding the causes of high card default rates. Banks run randomized experiments to test products as a matter of course and the current experiment was one of many run by the bank during this period.

### 4.1 Description of the Experiment

As outlined in the data section, the bank divided its sampling frame (of over one million clients) into nine different strata based on two pre-intervention characteristics which it 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.[33] Each borrower was classified into one of three categories of duration of time 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 always paid bills in their entirety in each of the previous 12 months and hence accrued no debt, (b) a "partial payer" whose average payment over the past 12 months was greater than $1.5$ times the average of the minimum payments required from 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 (see Online Appendix for details) and can thus make statements about the entire sampling frame.

Within each stratum, the bank randomly allocated 2000 members to each of 8 intervention arms and one control arm. Each treatment arm is a combination of two contract characteristics: (a) a required minimum payment the borrower is expected to pay every month based on his outstanding debt on the card and (b) the interest rate on the amount outstanding. The minimum payment was set at either $5\%$ or $10\%$. About 70% of our study sample paid less than 10% of their amount outstanding (debt) in March 2007 suggesting that the higher minimum payment of $10\%$ should affect consumer behavior. The minimum payment for the product prior to the study was $4\%$. The manipulated interest rate could take on one of four values – $15\%, 25\%, 35\%, 45\%$. The interest rate for the product in the period prior to the study was approximately about $55\%$ so all the experimentally assigned interest rates would have been viewed as interest rate reductions by our sample. 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. The sheer number of treatment effects necessarily requires some data compression in presentation. 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. Because of this we do not use the control group in most of the analysis below. We are explicit in the sequel about which group serves as

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

the reference or comparison group. In most cases we use the $5\%$ minimum payment and the $45\%$ interest rate group (abbreviated to $(5\%, 45\%)$) as the comparison group.

Figure 6 shows the timeline of the experiment, as well as measurement dates. The 9 strata were defined with information of 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 which was in charge of deciding credit limits.[34] The experiment ended in May 2009 at which point the study participants received a letter setting out their new contract terms. These terms were the standard conditions with an interest rate of approximately $55\%$ and a minimum payment of $4\%$.

Finally, Table OA.4 in the Online Appendix tests the randomization procedure and shows that treatment assignment is uncorrelated with out main outcome variables (as of March 2007) as well as demographics.

### 4.2 Card Exit

There is substantial card exit during the experiment as documented in Section 3.2.1 – approximately $19\%$ of the control group defaulted on their debt during this time and had their card revoked by the bank and an additional $16\%$ paid off their debt, cancelled their cards and left the bank. Another $9\%$ left the bank for other reasons (primarily lost cards or due to member death). While in Section 3.2.1 this was an outcome of interest in itself, here card exit threatens the internal validity of the experiment. 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 for a range of outcomes. 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 assess whether the attrition is differential across treatment arms by estimating

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

where $A_i$ is equal to 1 if the borrower exits at some point during the experiment and 0 otherwise. The right hand side variables are a set of full treatment and stratum dummies. Table 3 shows the

[34] Statistically we cannot reject the null of no differences in credit limits across treatment arms at baseline and endline.

results for all treatment arms though we focus discussion on only tree arms below. Exit in the $(45\%, 5\%)$ group was $40\%$ while exit in the $(45\%, 10\%)$ arm was four percentage points higher and exit in the primary lowest interest rate arm $(15\%, 5\%)$ was five percent lower.

### 4.2.1 Default

Turning next to default, a substantial part (20%) of the base group (i.e. the $(45\%, 5\%)$) defaulted over the course of the experiment. By comparison, the effects of the interventions were quite small. Reducing the interest rate to a third of the base group rate reduced defaults by approximately two percentage points over 27 months. The implied elasticity of default with respect to the interest rate is a relatively low $.18$.[35]

Many models of asymmetric information[36] imply a positive correlation between risk and prices. This occurs either due to (a) adverse selection wherein borrowers of a riskier "type" are more likely to be attracted by higher interest rates and (b) moral hazard wherein (holding type constant) higher interest rates induce borrowers to take actions that make them more likely to default. A common statistic for the presence of asymmetric information is therefore the correlation between interest rates and default. The large variation in interest rates induced by the experiment – from 45% to 15% – allows us to test for the presence of asymmetric information – more specifically for moral hazard. As we see above, the experimental variation in prices leads to relatively small changes in default suggesting weak evidence of moral hazard in this high risk population (in Section 6 we attempt to provide a rationalization for this) . Unfortunately, in general the size of the correlation bears no relation to the welfare costs. [37]

Einav and Finkelstein (2011) also suggest that since default severity is the main component of marginal cost the, one could use price variation to estimate demand, marginal cost, and average cost and then calculate welfare loss. We now attempt such a calculation. Adverse selection manifests in the slope of the marginal cost curve, while moral hazard is reflected in the x-axis. Overall, we find that on average an decrease of 30 pp in the interest rate causes an decrease of 352 pesos in the present value the default severity. This is about 24 percent of severity cost for the control group. If we condition on non-attriters to hold constant the population, we find that an decrease of 30 pp in the interest rate causes a decrease of 118 pesos (or 32 percent) in the present value of the total cost.

Turning to the minimum payment intervention, doubling the minimum payment from $5\%$ to $10\%$ (for the $45\%$ interest rate group) increased default by about one percent. The implied elasticity .06[38] that we judge to be small relative to the amount of policy attention paid to low minimum

[35]This is lower than the delinquency elasticity of $1.8$ implied by Karlan and Zinman (2016) and also lower than the default elasticity of $0.39$ implied by the interventions in Karlan and Zinman (2009).

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

[37]Einav and Finkelstein (2011)

[38]The results are lower than those for delinquency in Keys and Wang (2016) but of the same order of magnitude as those for defaut documented by d'Astous and Shore (2015). Both studies employ a quasi-experimental design to estimate causal effects using observational data from the United States. The latter document that an increase in minimum payments of $2\%$ on average over a base-rate of 3% increased default rates by $4\%$ over two years (which also implies an

payments as drivers of default.

#### 4.2.2 Card Cancellations

Turning next to cancellations, increases declines in the interest rate had stronger effects on cancellations both in an absolute and relative sense (compared to revocations) with card cancellations declining by three percent (relative to comparison rate of $13\%$) for an implied elasticity of $0.39$. Changes in the minimum payment led to long-term increases in default of $1.7$ percentage points for an implied elasticity of $0.12$.

Thus, the interest rate and minimum payment interventions affected both bank initiated defaults as well as borrower initiated cancellations. In the sequel we will often treat card cancellations conceptually differently from default since the former is an active borrower decision to reduce debt to zero and forego the opportunity of make more purchases (or accumulate more debt) on the card.

We next examine the month-wise treatment effects for both outcomes in Figure 7 and some broad patterns are evident. The effect of the interest rate decrease on both revocations and cancellations increases over time in a manner consistent with the hypothesis that interest rate reductions work through decreasing the rate at which debt increases. On the other hand, the effect of the minimum payment change tends to stay relatively constant over time (particularly after the first few months).

Finally, we also examine heterogeneity in treatment effects by comparing treatment effects across two strata in Figure 7. Perhaps unsurprisingly, neither intervention has any effect on the "full-payers,24m+" strata. Since borrowers in these strata tend to pay in full each month and accumulate no debt, both interventions should be predicted to have no effect and that is indeed the case. Conversely, the treatment has much stronger effects on borrowers who have been with the bank for less than a year and historically paid very low monthly payments (the "minimum-payer, 6-11m" stratum).

To summarize, there was substantial card exit during the experiment and there were differences in exit rates both across treatment arms and across strata. These results suggest particular caution when estimating and interpreting experimental effects on other outcomes of interest – purchase, payments and debt – discussed below.

## 5 Experimental Results

### 5.1 Methodology

We present both short-term (at the six month horizon) as well as long-term effects (after 27 months at the end of the experiment). We also present month-by-month treatment effects for each of the 27 months of the experiment.[39] In addition, when useful, we also examine treatment effect heterogeneity by presenting stratum-specific treatment effects for three strata – (a) the "Full, 24M+"

elasticity of .06).

[39]in graphical form. Tables available upon request.

stratum comprising borrowers who had been with the bank for at least 24 months before January 2007 and 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[40] (10% of the population) and(c) the “Min, 24M+” stratum comprising the longest term borrowers in the poorest repayment category (62% of the population).

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.[41]

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:

1. 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)

2. 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

$$Y_i = \sum_{j=1}^{7} \beta_j T_{ji} + \sum_{s=1}^{9} \delta_s S_{ji} + \epsilon_i \tag{2}$$

where $Y_i$ is the outcome measured either in the last month of the experiment or six months after the experiment began. 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.[42] We include strata dummies $\{S_{ji}\}_{j=1}^{9}$ in all specifications and use probability weights.[43]  this method we present treatment effects for all arms but focus discussion only on the two contrasts specified above.

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

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

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

[42]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.

[43]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.

separately for $t = 1 \ldots 27$

$$Y_{it} = \alpha_{1t} + \beta_{1t} T_i^{(15\%,5\%)} + \nu_{1it} \tag{3}$$

$$Y_{it} = \alpha_{2t} + \beta_{2t} T_i^{(45\%,10\%)} + \nu_{2it} \tag{4}$$

and in both cases the excluded arm is the $(45\%, 5\%)$ arm.[44] We then graph the estimates of $\beta_{1t}$ and $\beta_{2t}$ against time along with the corresponding Lee bounds in Figure 9. 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 (27 month) effects. Having described the general methodology we next turn to describing the effects of the interventions on each outcome of interest in turn.

## 5.2 Effects on Purchases

### 5.2.1 Effect of Interest Rate Decrease on Purchases

We begin by examining the effect of the experimental variation in interest rates on purchases in Figure 9 and Table 4. Figure 9 shows monthly treatment effects over the course of the experiment and Table 4 presents short- and long-term regression results accounting for attrition. We see in Figure 9 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 4 col (1)) are $[-0.38, -0.18]$ indicating a small negative effect on purchases. 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 4 col (2)). Imputing zeros to purchases for all card cancellers reduces the upper bound, but it remains positive (bottom of Table 4 col (3)).

The long-term elasticity bounds are wide but even at the lower bound they are substantially smaller than those found in other developing country studies that examine the effect of interest rate changes on total loan quantity.[45] For instance, Karlan and Zinman (2016) compute a two year elasticity of $-2.9$ of loan quantity with respect to interest rate in an experiment in Mexico with *Compartamos*. 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

[44]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.

[45]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.

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). In summary, the effect of interest rate reductions on purchases appears to be relatively small, even after accounting conservatively for attrition.

Perhaps unsurprisingly, there is considerable variation across strata in the treatment effects. "Full,24M+" borrowers do not respond to the changes in interest rates for the entire duration of the intervention. At the other extreme, "Min,12M–" borrowers increasing purchases by around 50 pesos although the effects are imprecisely estimated.

We also examined treatment effects after normalizing purchases by the amount due and obtained broadly 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 significant declines and small increases.

#### 5.2.2 Effect of Minimum Payment Increase on Purchases

Doubling the minimum payment led to an *increase* in monthly purchases. Figure 9 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[46]. 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 reductions in debt. We explore this implication by examining the changes in purchases across the various interest rate arms keeping the required payment fixed at $10\%$

[46]Results available upon request.

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 4) 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.

## 5.3 Effects on Monthly Payments

### 5.3.1 Effect of Interest Rate Decrease on Monthly Payments

Figure 9 presents the Lee bounds along with the point estimates from equation (4) 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 explore effects 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.

### 5.3.2 Effect of Minimum Payment Increase on Monthly Payments

It is reasonable to expect that the most direct effect of the minimum payment intervention would be on monthly payments. Figure 9 documents a sharp increase in monthly payments in the treatment group in the third month of the experiment[47] (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

[47]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.

$[.24, .29]$ 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.

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

## 5.4 Effects on Debt

### 5.4.1 Effect of Interest Rate Decrease on Debt

Debt follows an interesting and, at first-glance, a somewhat counter-intuitive pattern. Figure 9 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 we find that the implied elasticity bounds are relatively tight at $[0.28, 0.42]$ suggesting real, though modest, effects.[48] The bounds begin to widen after the first year but remain consistently negative and even the lower 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 with the upper bound being tightened to $0.74$. These results suggest a robust, modest negative effect of interest rate reductions on total debt.

As might be expected, Figure 12 shows there is variation across strata in the treatment effects. Debt for the "Full,24M+" borrowers does not respond to the changes in interest rates while the effects are much more pronounced for the "Min,12M–" category.

The negative effect of interest rate declines on debt may seem counter-intuitive initially since agents seem to respond to price declines by decreasing quantities. It appears, however, that the

[48]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.

effect arises from the fact that lower interest rates result in borrower debt outstanding being compounded at a correspondingly lower rate. This decline more than offsets any increase in purchases by borrowers as well as the decline in monthly payments observed earlier. To summarize, there is a fairly robust and moderate decline in total debt outstanding as a result of the interest rate decrease.

#### 5.4.2 Effect of Minimum Payment Increase on Debt

Debt response to the minimum payment increase follows an interesting pattern. Figure 9 show that debt increases markedly in the third and fourth month of the experiment, increasing by almost 750 pesos by June 2007. However, there is an similarly precipitous decline soon after with the increase being wiped out by September so that the six-month effects are very small – the bounds for the implied elasticities are quite small at $[0.02, 0.08]$.

The increase in debt in the first months of the experiment arises mostly from late payment fees[49] and are added to the borrower's debt. 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 modest 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 at best a modest effect on overall debt. Examining heterogeneity in the treatment effects by strata (see Figure 12) yields similar results as above and we omit the discussion here.

Ton 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.

#### 5.4.3 Effect on Total Debt

Finally, we can also examine the effect of the two experimental interventions on total debt as measured by the credit bureau.

### 5.5 Effect on Bank Revenues

Figure 14 plots means of our revenue measure (computed as described in Section 2.5) by treatment group. Revenue is decreasing in minimum payments and increasing in the interest rate. Column (5) of Table OA.19 presents these differences in regression form and finds them to be statistically significant. Within the support of the experimental manipulations therefore Bank A therefore

[49] The late payment fee is 350 pesos for any payment less than the minimum required payment.

finds it profitable to maximize interest rates and minimize minimum payments.[50] Extrapolating from these points suggests that increasing interest rates while minimizing the minimum required monthly payments may be a profitable strategy for the bank. Indeed, according to narrative accounts, the assumption that banks were playing this strategy *at the expense of borrower welfare (as proxied by default)* was the reason for the Mexican Central Bank's engagement with Bank A and the subsequent experiment.

To summarize this section, the experiment provides sobering evidence on the difficulty of using contract terms to alter consumer behavior. Large changes in interest rates and minimum payments had only limited effects on borrower default, a key outcome for policymakers. The next section attempts to address why such large changes had such small effects.

# 6 Mechanisms

The previous section documents that even large changes in interest rates and minimum payments have muted effects on for our NTB borrowers and in particular on their default rates. One plausible explanation is that default leads to significant and swift punishment by sharply decreasing access to the relatively attractive formal credit sector. In this section we flesh out the various components of this argument.

## 6.1 Consequence of Default

A primary rationale for the establishment of credit bureaus is to provide information about borrower behavior – including default – to all lenders in the market. Prospective lenders can then use this information to make loan decisions which fact in turn provides an ex-ante incentive for the borrower to limit default.[51] In order for this incentive to be effective, the consequences of default must be sufficiently dire. Here we evaluate whether this is indeed the case.

Our first approach is to assess the extent to which default in the study card affects subsequent formal sector credit via its effect on credit scores. We use an instrumental variables strategy that instruments credit scores with default on the study card during the experiment.

We begin by first regressing credit scores in period $t$[52] against a default indicator equal to 1 if the borrower has defaulted at any point between since the start of the experiment and period $t$.

$$\mathbb{1}(\text{gets loan})_{it} = \beta_0 + \beta_1 \times \text{credit score}_{it} + \epsilon_{it} \tag{5}$$

$$\text{credit score}_{it} = \delta_0 + \delta_1 \times \mathbb{1}(\text{default})_{it} + \nu_{it} \tag{6}$$

[50]Note that this measure of profit does no suffer from attrition bias, as somebody who attrits is correctly counted as not reporting profits in the months after attrition.

[51]The theoretical argument for bureaus is strongest for environments not characterized purely by adverse selection.

[52]We use June 2007 and June 2008 CB data as these are the only times of overlap with the experiment. After matching, we have data for 158,114 individuals of whom 80 percent are observed in both periods.

Columns (3) and (6) of Table 7 shows that while on average 3.3 percent of non-defaulting borrowers get a new credit card and 5.2 percent get a new loan in each period, only 1.2 and 1.9 of defaulters get new credit cards and new loans, respectively. This means that default has a negative effect of 65 and 63 percent on the likelihood of getting a new card and loan, respectively. These effects are 76 and 69 percent if we look at the likelihood of getting a card and loan in the next 6 months. Column (7) shows that this effect is also reflected in the credit score. The first stage regression shows that the credit score decreases by 103 points in the month when default happens, and this associated in the IV regression to about a 2.1 pp lower likelihood of getting a loan. The direct OLS regression has a smaller coefficient, suggesting that the effect of default operates directly and not only through a change in the credit score. We conclude that defaulting does imply a significant exclusion from the formal loan market.

### 6.2 Informal Loan Terms

A natural next issue is to quantify the costs of being excluded from the formal credit sector. We do so by briefly describing the contract terms for informal loans and documenting that they are typically worse on all dimensions relative to our store card. We rely on survey data as informal loans (by definition) 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 when it is given by a bank and informal otherwise.[53] Consistent with the evidence from a range of developing countries in Banerjee and Duflo (2010) only 6% of borrowers have any formal loans and 91% of borrowers have informal loans only.

Informal loan have significantly worse terms than formal loans. Figure 15 shows that the distribution of interest rates for informal loans stochastically dominates that for informal loan interest rates while the opposite is true for loan terms and loan amounts. Table 8 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.[54] The results on loan terms and duration survive the addition of household fixed effects.[55] Based on these results we conclude that it is costly to be excluded from the formal loan market.

[53]Informal loan sources include: Cooperativas (13%), Prestamista (8%), Relatives (38%), Acquiantances (20%), Work (11%), casa de préstamo (5%), and others (5%).

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

[55] Only about 3 percent of households hold both formal and informal sector leans 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 then are default rates so high? We speculate that the answer is partly that NTB borrowers are vulnerable to frequent and large economic shocks that precipitate default.

Unfortunately we do not observe individual level shock data for our sample. We attack the issue by providing two complementary exercises. First, we aggregate our experimental data to the municipality level and measure the correlation between municipality level unemployment and municipality level default in the previous quarters. The second exercise is to use our Credit Bureau random sample of 2014 to match it to social security data from 2010 to 2014 to measure individual-level formal sector employment (and unemployment) to correlate individual-level formal sector employment status with individual-level default in the previous months. Both set of exercises suggest a large and statistically significant correlation between unemployment and default.

#### 6.3.1 Municipality level correlation using experimental data at the municipality level

We examine the correlation between aggregate local – municipality level – shocks as proxied by unemployment rates and default.[56] We estimate the following regression

$$D_{ijt} = \alpha_t + \beta ln(1 + U_{j,t-k}) + \nu_{i,t} \tag{7}$$

where $I_{i,t}$ is an indicator for default, cancellation by client, or attrition of borrower $i$ in municipality $j$ in quarter $t$; $\alpha_t$ represent quarter dummies, and $U_{j,t-k}$ is the unemployment rate at municipality $j$ lagged $k$ quarters and is entered in logs. Standard errors are clustered at the municipality level. We estimate this regression for three specifications (rows: no controls, with quarter dummies, and with quarter dummies and municipality fixed effects), in two samples: the experimental sample (columns 1-3) and the credit bureau sample (columns 4-6). Each cell in Table 9 corresponds to a coefficient $\beta$ for one regression. We use a lag of $K$=3 quarters but results are robust to using different lags.

Although explained variance is low (at most 4%) we find large and statistically significant correlations between unemployment and default. For example, in column 1 row 2 the coefficient of 0.147 implies that a 10 percent increase in unemployment is associated with 1.4 percentage points increase in default, from a mean of 2.3pp. The coefficient is not longer statistically significant once we control for municipality fixed effects, but we suspect this arises in part because in this case we rely on within municipality time variation in unemployment and we only have 39 months in the experimental sample and 120 months in the credit bureau sample. We find the effect of the same sign but smaller when we use the credit bureau sample (column 3). Given the larger sample size we still find a statistically significant effect even when we control for quarter and municipality fixed effects. In this case an increase of 10% in unemployment is associated with a 0.5pp increase

[56] There are 1415 municipalities in our experimental sample and 303 in our credit bureau sample. Mexico's unemployment survey covers 2343 municipalities and is available quarterly.

in default from a baseline of 1.5pp, i.e. a 33% increase.

One would expect that unemployment does not drive voluntary cancellations as these are most likely the result of finding better terms at other banks. So using cancellations as a dependent variable can be seen as a placebo check. We indeed find that there is no effect of unemployment in this variable.

#### 6.3.2 Individual-level unemployment using a representative dataset

We make use of two of our data sources for this exercise. First, we use our 2014 Credit Bureau representative sample of individuals in the credit bureau in May 2014 and their corresponding credit information. Among the million observed individuals, for 542,959 of them: (1) we observe their Tax ID (RFC) and (2) they have had a bank loan at least once between January 2011 and May 2014.[57] Second, we use the national social security records from October 2011 to May 2014 for all of the population in Mexico. This gives us a match of the following characteristics: we observe income for at least one month for 86,363 individuals; we observe income for all months for 63,963 individuals. This dataset will allow us to draw inferences representative of all of those individuals in Mexico who have had at least one loan between January 2011 and May 2014.

By exploiting the high frequency and detailed information of the credit bureau we construct the following dependent variables. We define a categorical value at the individual level which is equal to 1 when individual $i$ in time $t$ has at least one of his loans in arrears for more than $m$ months and 0 if all of their loans have strictly less than $m$ months in arrears – including zero days. We use $k = 2, 4, 12$. Define these variables by $\mathbb{1}(m \text{ months in arrears})_{it}$ for individual $i$ at time $t$. We use the following regression equations:

$$\mathbb{1}(m \text{ months in arrears})_{it} = \alpha_i + \gamma_{s,t} + \beta \cdot \mathbb{1}(\text{formal employment})_{i,t-k} + \epsilon_{it} \tag{8}$$

$$\mathbb{1}(m \text{ months in arrears})_{it} = \delta_i + \phi_{s,t} + \rho \cdot \log(\text{monthly\_wage})_{i,t-k} + \eta_{it} \tag{9}$$

where $\mathbb{1}(\text{formal employment})_{i,t}$ is a dummy of the formal employment status for individual $i$ at time $t$; $\text{monthly\_wage}_{it}$ is the monthly wage of individual $i$ at time $t$, conditional on him being employed; $\alpha_i, \delta_i$ are individual fixed effects; and $\gamma_{s,t}, \phi_{s,t}$ are state of residence $\times$ month fixed effects to control for differential macroeconomic trends across states. Note that those who never and those who always have a formal employment help us identify the state cross time trends. Those who gain and those who lose their jobs at the formal sector allow us to identify $\beta$. The long length nature of our panel data allow us to identify the individual fixed effects as well. Thus, $\beta$ will identify the probability change of having a formal employment on default of the same individual living in the same state in month $t$. Analogously $\rho$ identifies the effect marginal effect of a peso increase in the wage the employed individuals. We use these regressions to talk about both the extensive and intensive margin of default.

[57] This is the intersection between the two variables. For 84.4 percent we observe their Tax ID (RFC), whereas 64.3 percent have had a loan with a bank in the corresponding period.

Table 10 provides the results for $m = 4$ for different levels of $k$, and Tables OA.13 and OA.14 in the OA provide the results for $m = 2$, and $m = 12$, respectively. The same individual, in the same state×month is associated with a 1.6 pp. lower probability of default if was is employed in the formal sector one month ago compared to the situation where the same individual was not employed. The 1.6 pp. correspond to roughly to 11 percent of the unconditional mean. Moreover, the coefficient of $\beta$ is increasing (in absolute value) the larger the $k$, and remains roughly constant after $k = 7$. On the other hand, when looking at the intensive margin, a one percent increase in the wage of individual $i$ a month ago is associated with a 1.3 pp. lower probability of default today. Again, the coefficient is increasing (in absolute value) in $k$ up until $k = 7$, after which the effect diminishes but remains negative. As shown in the OA, the result is kept for the different possible pairs of $m$ and $k$. We use this exercise to starkly conclude that the loss of employment in the formal sector and the decrease in the wage received conditional on being employed are associated with a higher probability of default.

## 7 Conclusion

In this paper we used a range of data sets to document several facts about formal financial sector credit to borrowers relatively new to formal credit. We find that borrower turnover is high and that bank revenue from borrowers is variable and difficult to predict. Next, we showed that there is some evidence of a first-lender externality in that new to banking borrowers who generate good credit histories with their first bank are more likely to leave the bank than borrowers who do not generate comparable records. A natural next question is whether and how the bank can structure ex-post contract terms to limit default.

We use a large-scale randomized experiment to answer this question. We examine the effects of variations in contract terms – interest rates and minimum payments – on borrower outcomes. We find that even large changes in contract terms have very small effects on default relative to the high default rates in the population. Similarly, we find relatively small effects on purchases, payments as well as debt. Finally, we attempted to reconcile these limited responses to the high underlying default rates. We conjectured that unattractive outside options along with vulnerability to shocks could help reconcile the two. We find some evidence that terms for informal loans are much worse than those offered by the store card and that municipality level shocks are weakly correlated with card default. Moreover, individual level employment shocks are also correlated with default.

Because we show that the mechanical part of default is important, a natural next question is to quantify the extent to which strategic default also plays a role. The next step for this research is to develop a realistic model that allows us to separate between these two. Since both of them ask for different policy remedies, being able to correctly measure the strategic and mechanical part of default should be an important line of research in the future.

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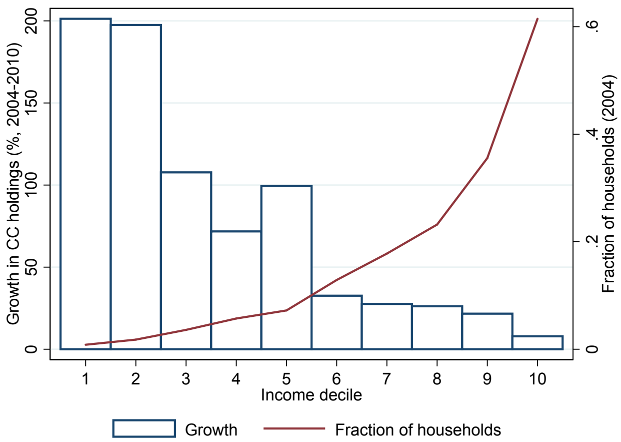
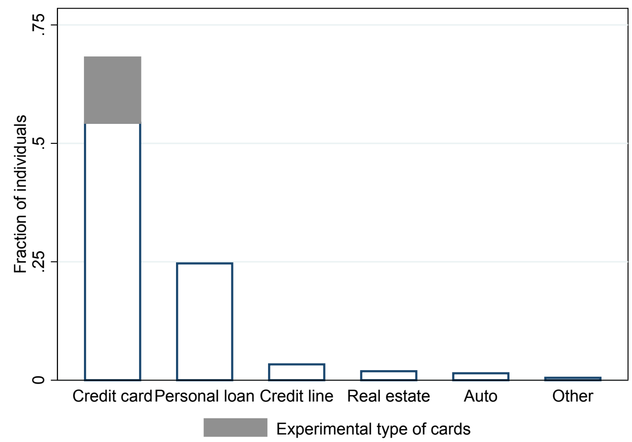
**Figure 1:** Credit Card Growth and First Loan by Type



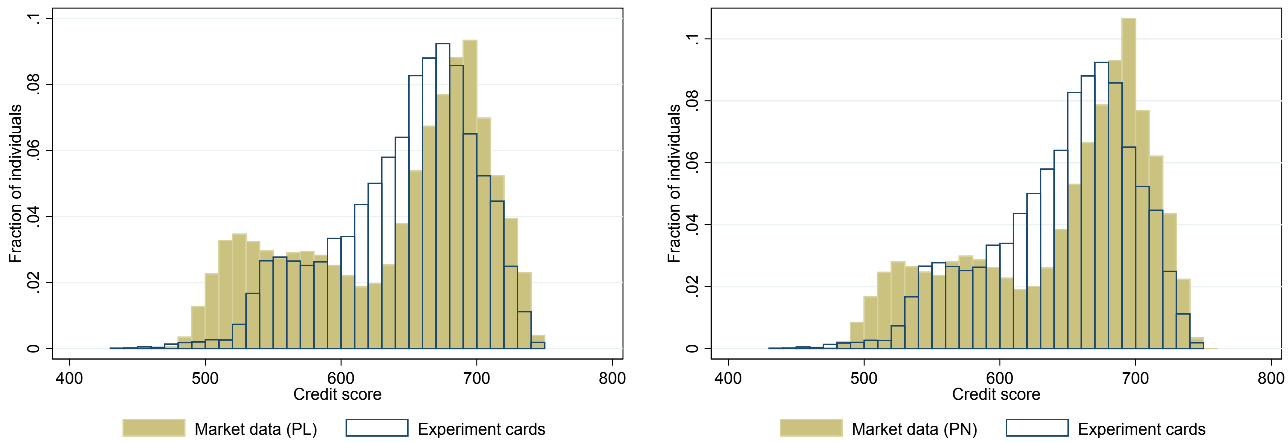

**(a)** Cardholders' growth by income decile




**(b)** First loan distribution by type of credit

*Notes:* Figure (a) is constructed using data from the National Income Expenditure Survey (ENIGH). The bar graph is read in the left axis; it shows the percentage growth from 2004 to 2010 in credit card holdings by decile. The line graph is read in the right axis; it plots the fraction of households with at least a credit card per income decile. Figure (b) is constructed using the credit bureau 2010 information. For each consumer, we check what is the type of the oldest loan available and plot the proportion of consumers for whom each type of loan is the first. The other type of loans category includes loans for furniture, (car) leasing, home equity loans, miscellaneous, guaranteed cards, and unsecured loans. The grey area represents the cards that are identified by the same 6-digit product number as our experimental cards that are issued by Bank A, our cooperating bank.

**Figure 2:** Distribution of Credit Scores of experimental sample in June 2007 and population in 2016

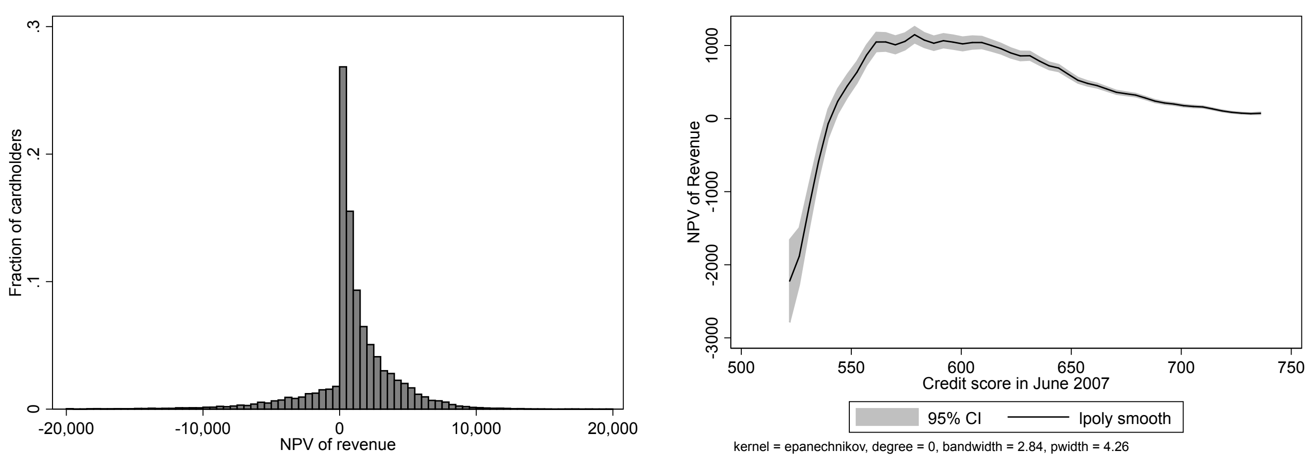



**(a)** Individuals with personal loans

**(b)** Individuals with payroll loans

*Notes:* This figure compares the credit score distribution of those individuals in the experimental sample against two different random samples taken in 2016. Figure (a) shows in dark a 2.5 million random sample of individuals with at least one personal loan coming from the 12 biggest banks in Mexico. Figure (b) shows in dark a 2.5 million random sample of individuals with at least one payroll loan from the 30 biggest banks in Mexico. Both figures show in transparent bars the credit score of the experimental individuals in 2007. Unfortunately we could not get information for a representative sample in 2007. Our guess is that market data will be shifted to the right as personal and payroll portfolios have deteriorated.

**Figure 3:** Ex Post Profit Distribution and Relationship to Scores

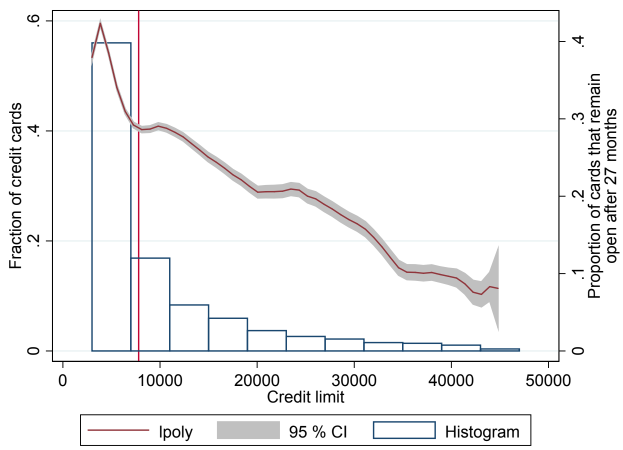



**(a)** Distribution of NPV of Revenue

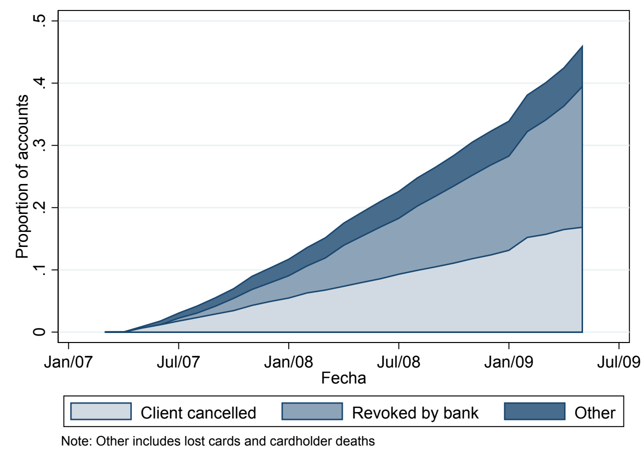
**(b)** NPV of Revenue by Credit Score

*Notes:* Figure (a) represents the distribution of the revenue obtained from the experiment subjects. The Sampling weights are used to represent the population. For readibility, the graph excludes those individuals with NPV of revenue equal to zero. Figure (b) shows a kernel regression for the NPV of revenue based on the credit score in June 2007. The analogous graphs for our extreme strata can be found in Figure OA.5.

**Figure 4:** Card closings in the population and in the experiment



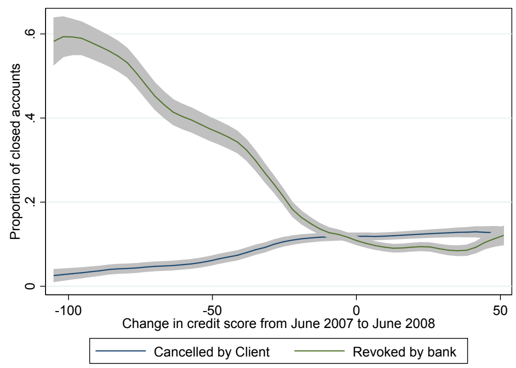

**(a)** Comparison between experiment and population



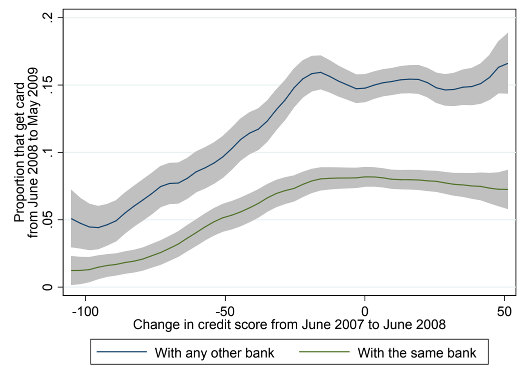

**(b)** Attrition in the experiment

*Notes:* Figure (a) is a kernel regression of a binary variable equal to 1 if a credit card is closed within 27 months of being opened against the credit limit of the card (using the credit bureau random sample shown in Column (2) of Table 1). The vertical solid red line shows the mean initial credit limit for the cards in the experiment. Figure (b) plots card exit rates over the course of the experiment for the control group. Card closures are subdivided (a) bank initiated revocations (or default) (b) borrower initiated cancellation or (c) other reasons (primarily lost cards or death). For comparison, Figure OA.14 in the online appendix plots the analogous graphs to Figure (b) for two different strata.

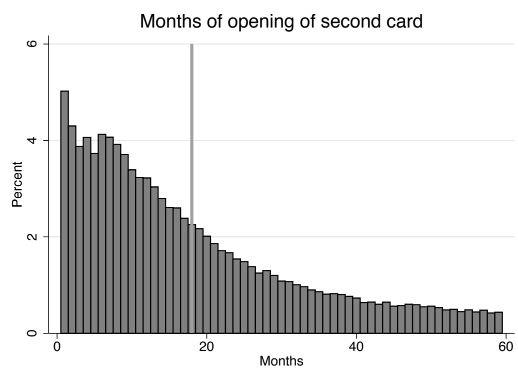
**Figure 5:** Client Poaching




**(a)** Cancellation and Revocation and Changes in Credit Scores



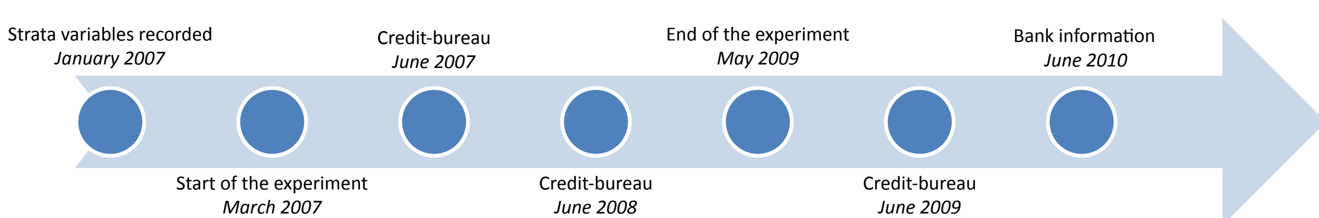

**(b)** New Cards and Changes in Credit Scores




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

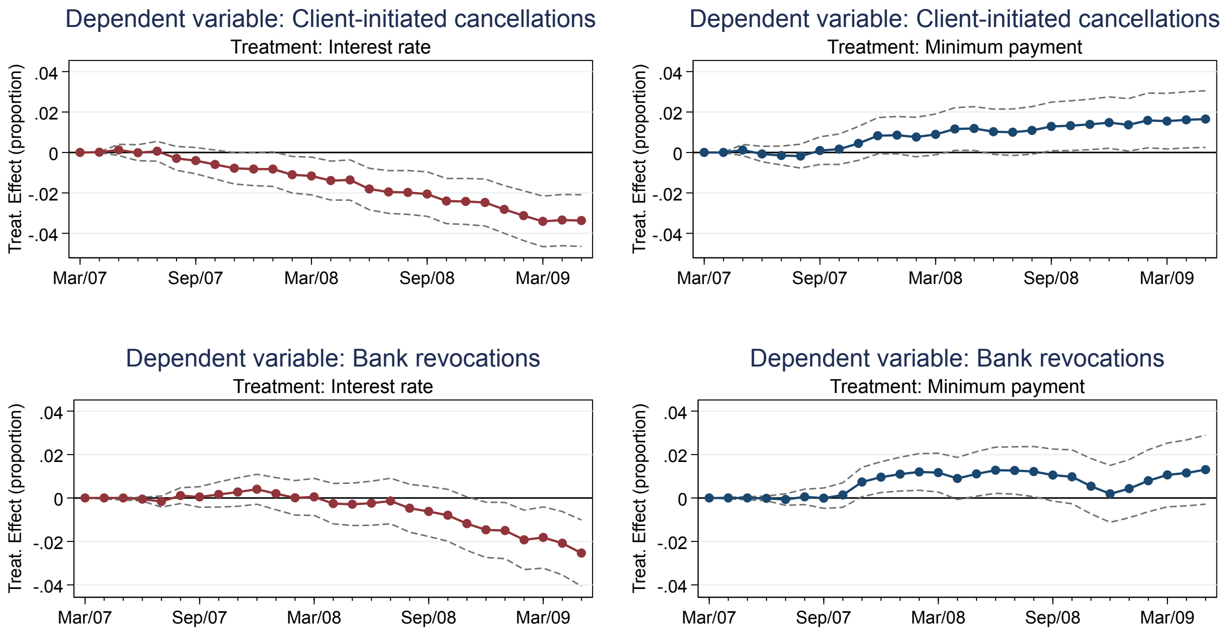
*Notes:* Panel A shows kernel regressions of changes in scores from June 2007 to June 2008 versus a dummy of revocation by Bank A or voluntary cancellation by the client in the period June 2008 to June 2009. Panel C shows the number of months lapsed for the month from when the clients get their first card to when when they get their second card in a different bank as a measure of switching. Vertical line shows the median of the distribution.

**Figure 6:** Timeline for the Experiment



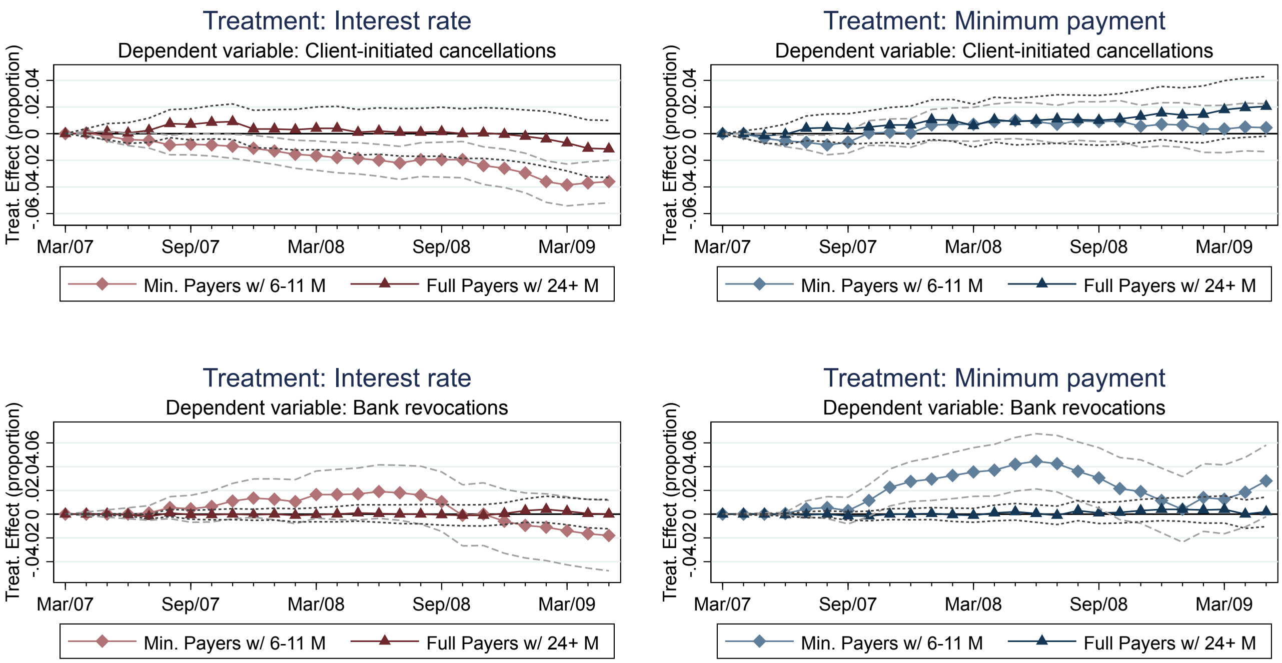

*Notes:* This is the timeline for the experiment. It provides a summary of the dates for the experimental part of the paper. The full description of the experiment can be found in Section 4.1.

**Figure 7:** Card Exit Over Time



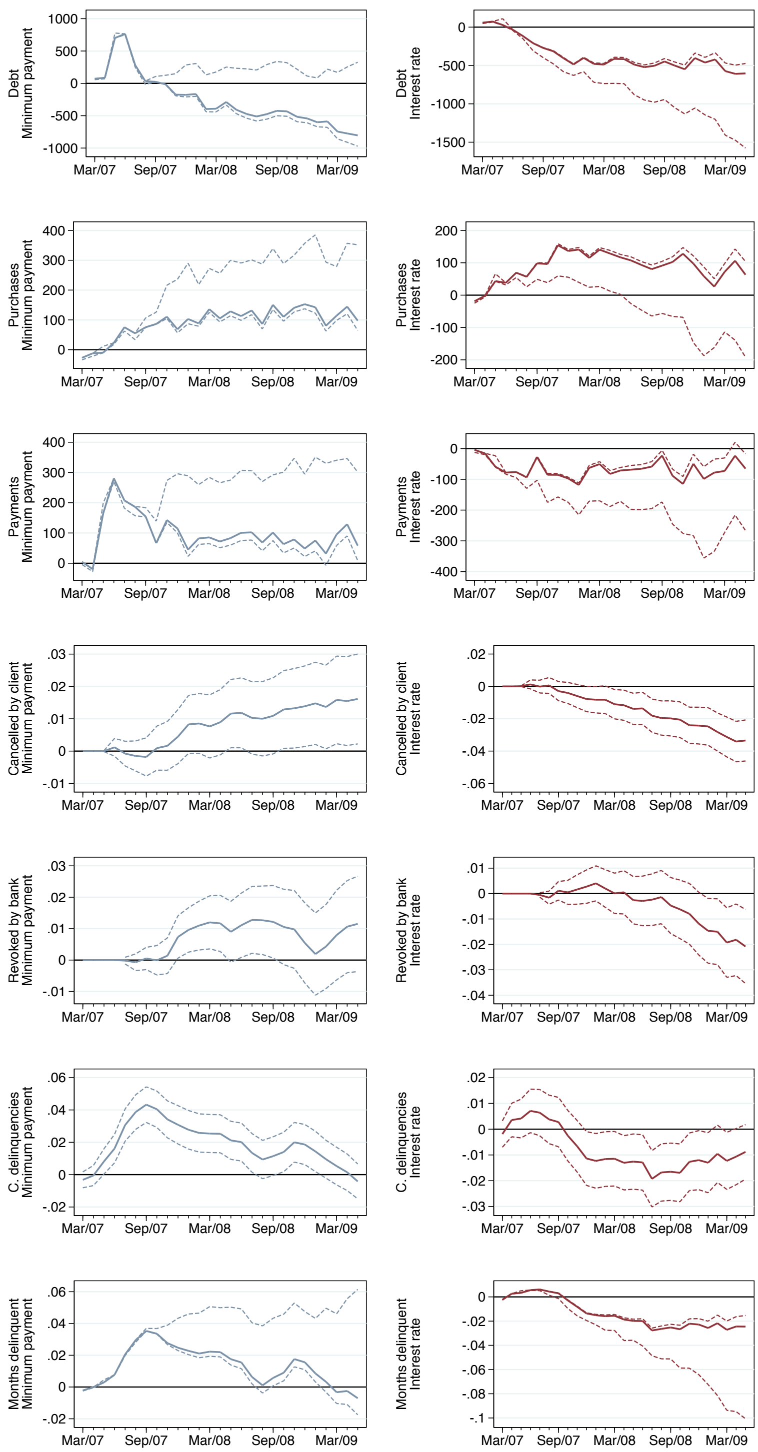

For each month in the experiment we regress the outcome (revocation or cancellation) on a treatment dummy. Each dot corresponds to the coefficient on the treatment dummy for that month along with point-wise 95% confidence intervals. For simplicity the comparison group here is the $(45\%, 5\%)$ ground and the treatment group for the interest rate change is the $(15\%, 5\%)$ ground and the $(45\%, 10\%)$ group for the minimum payment intervention.

**Figure 8:** Card Exit Across Strata and Time



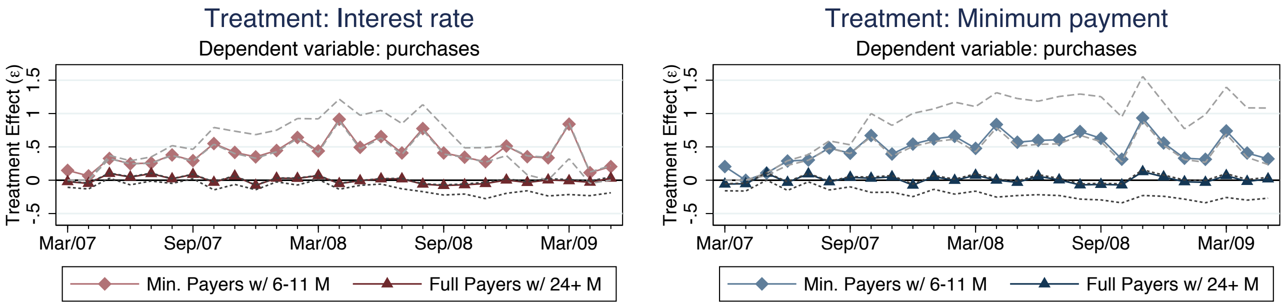

For each month in the experiment we regress the outcome (revocation or cancellation) on a treatment dummy. Each dot corresponds to the coefficient on the treatment dummy for that month along with point-wise $95\%$ confidence intervals. For simplicity the comparison group here is the $(45\%, 5\%)$ ground 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 red triangles correspond to the "full-payer,24m+" stratum and the light red diamonds correspond to the "minimum payer, 6-11m" stratum.

**Figure 9:** Treatment effect estimates



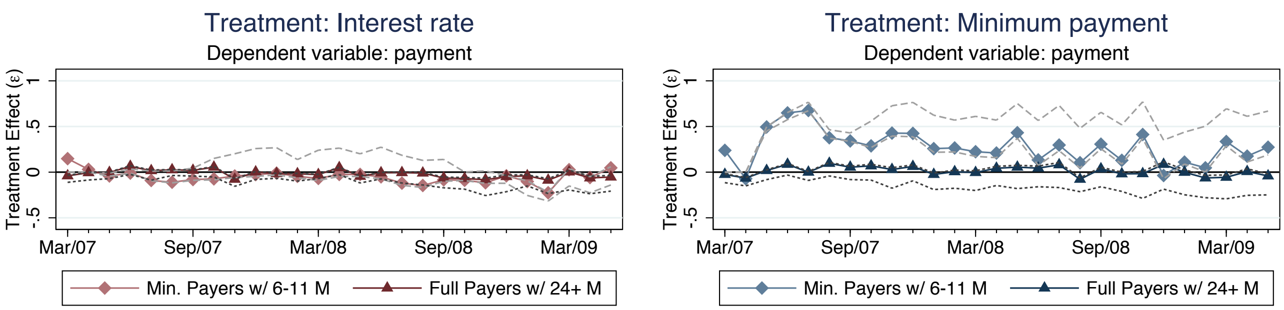

*Notes:* The left side of the panel shows the effect of increasing the minimum payment to 10% relative to the 5% group. The right side of the panel shows the effect of decreasing the interest rate from 45% to 15%. For each month $t$ in the experiment, we run $y_{it} = \alpha_t + \beta_t T_i + \epsilon_{it}$ 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, a dummy indicating borrower initiated cancellation and a dummy indicating bank initiated revocation, a ever delinquent dummy indicating whether the client has ever been delinquent in the past, and the cumulative sum of the months over the experiment that the client has been delinquent. We compute Lee bounds (Lee (2009)) for debt, purchases, payments and total months delinquent which are drawn as dashed lines. The dashed lines for cancellations, revocations and delinquencies are standard pointwise 95% confidence intervals.

**Figure 10:** Effect on Purchases: Heterogeneity Across Strata and Time



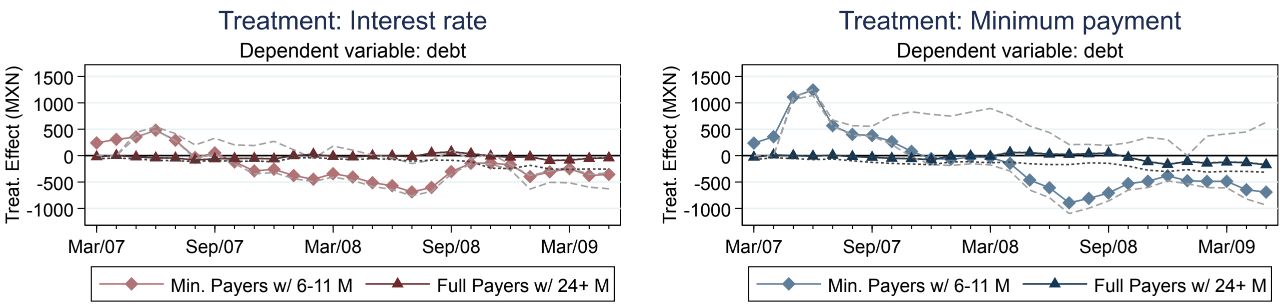

For each stratum, for each month in the experiment we regress purchases on a treatment dummy. Each dot corresponds to the coefficient on the treatment dummy for that month along with point-wise Lee bounds. For simplicity the comparison group here is the $(45\%, 5\%)$ ground and the treatment group for 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.

**Figure 11:** Effect on Payments: Heterogeneity Across Strata and Time




For each stratum, for each month in the experiment we regress payments 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 red triangles correspond to the "Full,24M+" stratum and the light red diamonds correspond to the "Min,12M–" stratum.

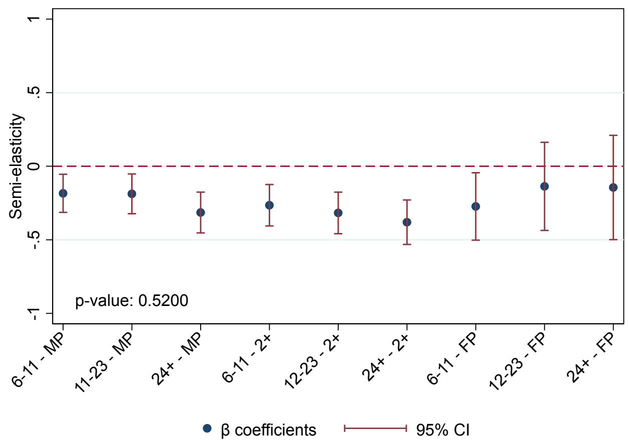
**Figure 12:** Effect on Debt: Heterogeneity Across Strata and Time



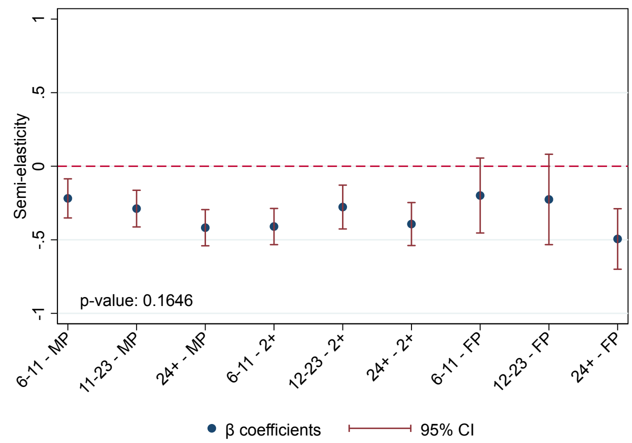

For each stratum, for each month in the experiment we regress debt on a treatment dummy. Each point (triangle or diamond) corresponds to the coefficient on the treatment dummy for that month along with point-wise Lee bounds. For simplicity the comparison group here is the $(45\%, 5\%)$ ground and the comparison group for the interest rate change is the $(15\%, 5\%)$ arm and the comparison arm for the minimum payment inrease is $(45\%, 10\%)$ 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,12M–" stratum.

**Figure 13:** Treatment effect heterogeneity across strata in May 2009

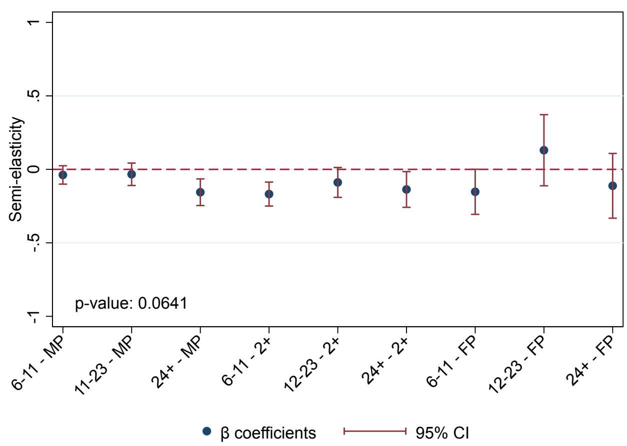
**(a)** Debt - Interest rate



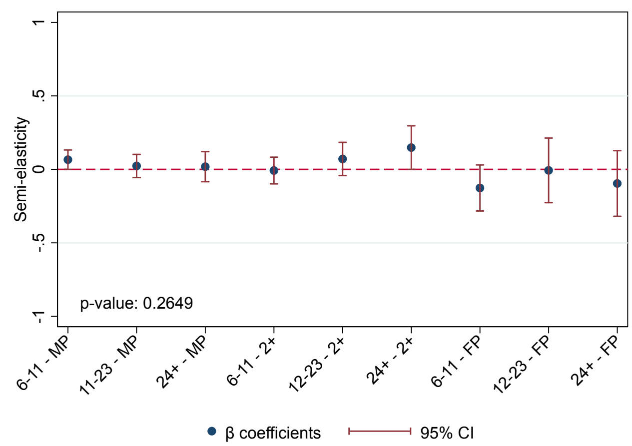

**(b)** Debt - Minimum payment



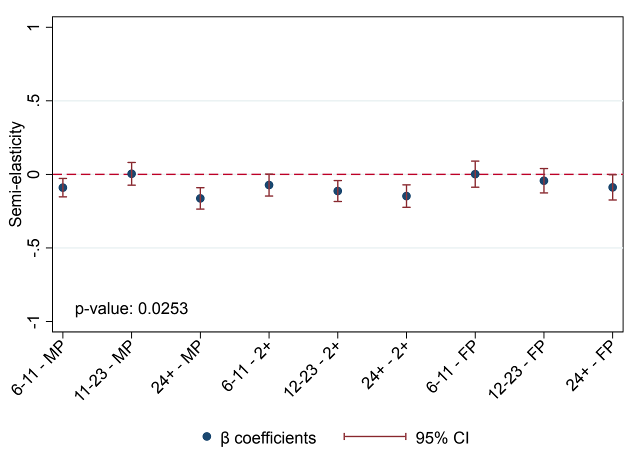

**(c)** Cumulative delinquencies - Interest rate



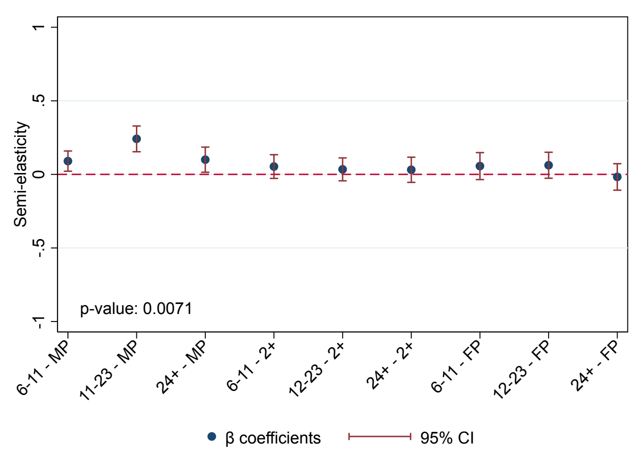

**(d)** Cumulative delinquencies - Minimum payment




**(e)** Attrition - Interest rate

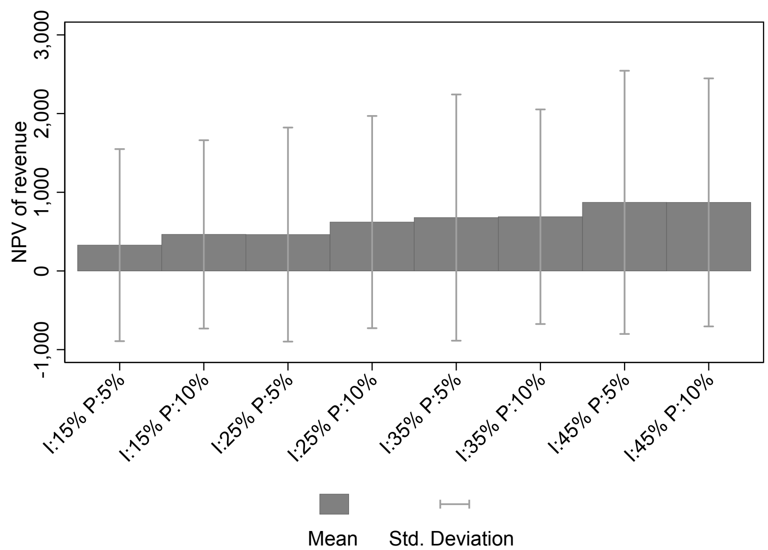



**(f)** Attrition - Minimum payment



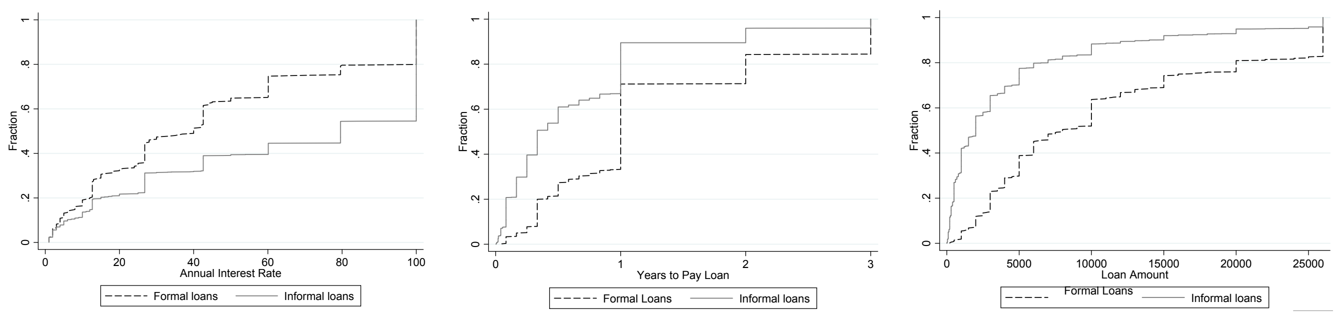

*Notes:* The figures represent the semielasticity estimation of the interest rate and the minimum payment by strata. All variable definitions follow those from Table OA.19. Confidence intervals are given by the upper and lower bars shown in each graph. The complete methodology to obtain these semielasticities is shown in the Online Appendix.

**Figure 14:** Mean and standard deviation of profits




*Notes:* This figure shows the mean and standard deviation of the net of present value of revenue for different interest rate and minimum payment combinations. For comparison, Figure OA.6 shows the analogous graph for our extreme strata.

**Figure 15:** Comparison formal and informal loan market in Mexico




*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.

# Tables

**Table 1:** Summary statistics and baseline characteristics

| | Experimental | Credit bureau | | |
|---|---|---|---|---|
| | | All individuals (random) | New borrowers (matched) | Experienced borrowers |
| | (1) | (2) | (3) | (4) |
| *Panel A. Information from the experimental credit card* | | | | |
| Month of measurement | March 2007 | | | |
| Payments | 711 | - | - | - |
| | (1,473) | | | |
| Purchases | 338 | - | - | - |
| | (1,023) | | | |
| Debt | 1,198 | - | - | - |
| | (3,521) | | | |
| Credit limit | 7,879 | - | - | - |
| | (6,117) | | | |
| Net present value of revenue | 623 | - | - | - |
| | (2,850) | | | |
| Number of cards when experimental card was opened | 0.90 | - | - | - |
| | (1.44) | | | |
| First card (Percentage) | 57 | - | - | - |
| Default (03/07 - 05/09, Control Group) | 19 | - | - | - |
| Cancellations (03/07 - 05/09, Control Group) | 16 | - | - | - |
| *Panel B. Information from the credit bureau* | | | | |
| Month of measurement | June 2007 | June 2010 | June 2010 | June 2010 |
| Credit score | 645 | - | - | - |
| | (52) | | | |
| Mean credit card limit | 15,776 | 49,604 | 22,082 | 56,187 |
| | (15,776) | (32,596) | (28,710) | (43,032) |
| Total credit line | 53,652 | 53,718 | 49,348 | 139,804 |
| | (70,292) | (103,503) | (87,855) | (162,568) |
| Number of credit cards open | 2.75 | 1.94 | 2.04 | 2.69 |
| | (1.90) | (1.60) | (2.04) | (2.11) |
| Number of banks interacted with† | 2.63 | 1.44 | 1.49 | 1.80 |
| | (1.25) | (0.80) | (1.49) | (1.00) |
| Tenure in months of oldest credit | 68 | 79 | 68 | 206 |
| | (54) | (87) | (57) | (85) |
| Total amount in arrears given that it is positive | 9,738 | 20,349 | 20,682 | 56,266 |
| | (49,604) | (52,759) | (48,263) | (96,039) |
| Pct. of accounts with positive amount in arrears | 22 | 24 | 28 | 18 |
| *Panel C. Demographic information* | | | | |
| Month of measurement | June 2007 | June 2010 | June 2010 | June 2010 |
| Male consumers | 52 | 47 | 47 | 53 |
| Married consumers | 62 | 50 | 48 | 47 |
| Consumers with information in the social security database | 18 | 13 | 14 | 13 |
| Age | 39 | 42 | 41 | 51 |
| | (6) | (13) | (12) | (12) |
| Monthly income‡ | 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 the credit bureau sample. Column 1 shows the information of our experimental sample. Column 2 takes the credit bureau random sample and computes the analogous estimations for those individuals with at least one credit card open by the month of measurement. Column 3 matches the experimental sample by the tenure in months of oldest credit. Thus, it shows a (random stratified) sample of individuals, with at least one credit card open by the month of measurement, that is comparable to our experimental sample. Column 4 keeps only individuals who have at least 8 years of information in the credit bureau and have a credit card open by the month of measurement. Not all variables are available in the credit bureau data. † 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 taken from the social security data and is measured in October 2011 for all consumers. This is the actual income reported by their firms to the social security database; we also report the percentage of consumers that are matched in the social security database.

**Table 2:** Stratum 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 3:** Card Exit

| | (1) Cancellations | (2) Revocations | (3) Exit |
|---|---|---|---|
| r = 15, MP = 5 | -0.034*** | -0.025** | -0.051*** |
| | (0.004) | (0.007) | (0.009) |
| r = 45, MP = 10 | 0.017** | 0.013* | 0.043*** |
| | (0.005) | (0.005) | (0.007) |
| Constant (r = 45, MP = 5) | 0.131*** | 0.204*** | 0.406*** |
| | (0.002) | (0.005) | (0.005) |
| Observations | 144,000 | 144,000 | 144,000 |
| R-squared | 0.002 | 0.001 | 0.003 |
| p-value MP | 0.000 | 0.000 | 0.000 |
| p-value IR | 0.000 | 0.000 | 0.000 |

All outcomes measured in May 2009. A cancellation is a borrower initiated exit after paying off all debts. A revocation is a bank initiated action that closes a borrower's card after three months of delinquency. The Other category (not shown) consists of card exits for other reasons (primarily card loss).

**Table 4:** 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] |
| $\epsilon$ 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] |
| $\epsilon$ 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 purchases at the end of the experiment (27 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.

**Table 5:** 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] |
| $\epsilon$ 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] |
| $\epsilon$ 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 (27 months). Columns (2) and (5) drop all card exits. The Lee bounds compare (r=15, MP=5) and (r=45,MP=10) arms against the (r=45, MP=5) arm. informative than the point estimates. Columns (3) and (6) assigns a zero for all outcomes for card cancellers and the resulting Lee bounds are also informative. Columns (7),(8) and (9) estimate the endline regressions for three different strata – (a) "Min Payers, <12" borrowers who were with the bank for less than six months in January 2007 and were in the lowest payment category ;(b) "Full Payers,>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 Payers,>24M" borrowers who had been with the bank for more than 2 years by January 2007 and were in the lowest payment category.

**Table 6:** Treatment Effects on Total Debt

| | Standard Outcome | | | Deflated by Amount Due in $t-1$ | | Selected Strata (May/09) | | |
|---|---|---|---|---|---|---|---|---|
| | Sep/07<br>(1) | May/09<br>(2) | May/09 w/zeros<br>(3) | Sep/07<br>(4) | May/09<br>(5) | Min.Pay,6-11M<br>(6) | Full Pay,24+M<br>(7) | Min.Pay, 24+ M<br>(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] |
| $\epsilon$ 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] |
| $\epsilon$ 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 (27 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.

**Table 7:** Probability of getting a loan against default

| | New credit card in $t$ | | | New credit in $t$ | | | Credit score |
|---|---|---|---|---|---|---|---|
| | OLS | IV | Reduced Form | OLS | IV | Reduced Form | First Stage |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Credit score | 0.0001*** | 0.0002*** | | 0.0002*** | 0.0004*** | | |
| | (0.0000) | (0.0000) | | (0.0000) | (0.0000) | | |
| Default | | | -0.0218*** | | | -0.0328*** | -103*** |
| | | | (0.0021) | | | (0.0025) | (0.6089) |
| Constant | -0.0480*** | -0.1236*** | 0.0335*** | -0.0598*** | -0.1848*** | 0.0517*** | 657*** |
| | (0.0053) | (0.0152) | (0.0007) | (0.0066) | (0.0178) | (0.0008) | (0.1312) |
| R-squared | 0.0017 | | 0.0007 | 0.0020 | | 0.0010 | 0.1208 |
| Observations | 258,102 | 258,102 | 258,102 | 258,102 | 258,102 | 258,102 | 258,102 |
| Dependent Variable Mean | 0.0325 | 0.0325 | 0.0325 | 0.0501 | 0.0501 | 0.0501 | 642 |

*Notes:* This table shows the relation between getting a loan and default. The dependent variable is shown above the column number. For columns (1) to (6), the dependent variable is an indicator variable if consumer $i$ gets a new credit card / new credit in period $t$, respectively. Columns (2) and (5) instrument the credit score using an indicator variable that is equal to 1 if consumer $i$ defaults in period $t$. Columns (3) and (6) show the reduced form relationship between default and the corresponding dependent variables. Column (7) shows the first stage coefficient (the regression between credit score and default). Errors are clustered at the individual level. ***, ** and * show statistical significance at the 1, 5 and 10 percent, respectively.

**Table 8:** 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 |
| Individual 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:* This table shows the estimated effect the formality of a credit has on interest rates in Columns (1) to (3); the amount of loans in pesos in Columns (4) to (6) and the loan duration in years in Columns (7) to (9). Regression specifications varies from only including year sample dummies (Columns 1, 3, and 6), to including also education dummies, age, monthly expenditure and some dummy variables which indicate whether the household owns certain objects (Columns 2, 4, 7), to a final regression where we include household fixed effects. We consider a credit to be formal when given by a banking institution and informal on any other case, such as personal loans, non-banks loan institutions, or government programs. Standard errors are shown in parenthesis. ***, ** and * show statistical significance at the 1, 5 and 10 percent, respectively. The source for this table is the National Survey of Household Living Standards (Rubalcava and Teruel, 2006).

**Table 9:** Efects of unemployment on bureau and experimental samples

| | Experiment Sample | | Credit Bureau Sample |
|---|---|---|---|
| | Default | Cancelled | Default |
| | (1) | (2) | (3) |
| No controls | 0.199* | 0.007 | 0.04** |
| | (0.104) | (0.011) | (0.019) |
| Quarter effects | 0.147** | -0.004 | 0.005 |
| | (0.063) | (0.009) | (0.017) |
| Quarters and Municipality | 0.045 | -0.0006 | -0.002 |
| | (0.036) | (0.014) | (0.023) |
| R-squared (No controls) | 0.0002 | 0.00005 | 0.0003 |
| R-squared (Quarter) | 0.042 | 0.006 | 0.006 |
| R-squared (Quarters and Municipality) | 0.046 | 0.008 | 0.024 |
| Mean of independent variable | 0.044 | 0.044 | 0.047 |
| Mean of dependent variable | 0.064 | 0.013 | 0.002 |
| Observations | 1250695 | 1250695 | 23718 |

*Notes:* Each cell in the first panel represents a coefficient in a different regression. The dependent variable is shown in the row above the column numbers. The independent variable is the logarithm of 1 + unemployment rate in each municipality six months before. The first row shows the reduced form relation. The second row controls for quarter of observation fixed effects. The third row controls for quarter and municipality fixed effects. Standard errors are clustered at the individual level. Columns (1) to (3) regress on the experimental sample. Columns (4) to (6) regress on the Credit Bureau random sample. The experiment sample taken is all the quarters where each experiment card was open during the experiment, when the card closes we take one last quarter for that specific card where we account for the closing. On the other hand, the bureau sample takes quarters from the years 2005-2014 for open cards on the data range, plus the last quarter on closing if its the case. For comparison, the last panel shows the mean of both the independent and dependent variables.

**Table 10:** Unemployment regressions at the individual level with $m = 4$

| Value of $k$ | (1) k = 1 | (2) k = 3 | (3) k = 5 | (4) k = 7 | (5) k = 9 | (6) k = 11 |
|---|---|---|---|---|---|---|
| *Panel A. Extensive margin regressions* | | | | | | |
| formal employment in $t-k$ | -0.016 | -0.019 | -0.024 | -0.028 | -0.027 | -0.026 |
| | (0.0003) | (0.0003) | (0.0003) | (0.0003) | (0.0003) | (0.0003) |
| Observations | 15.7 million | 15.1 million | 14.4 million | 13.7 million | 13.0 million | 12.3 million |
| R-squared | 0.030 | 0.030 | 0.029 | 0.029 | 0.028 | 0.028 |
| *Panel B. Intensive margin regressions* | | | | | | |
| monthly wage (thousands) in $t-k$ | -0.013 | -0.019 | -0.023 | -0.023 | -0.018 | -0.013 |
| | (0.0006) | (0.0006) | (0.0006) | (0.0007) | (0.0007) | (0.0007) |
| Observations | 2.1 million | 2.0 million | 1.9 million | 1.8 million | 1.6 million | 1.5 million |
| R-squared | 0.015 | 0.015 | 0.015 | 0.014 | 0.013 | 0.012 |
| *Panel C. Additional information* | | | | | | |
| unconditional mean dep. variable | 0.148 | 0.148 | 0.148 | 0.148 | 0.148 | 0.148 |
| proportion formally employed in a given month | 0.117 | 0.117 | 0.117 | 0.117 | 0.117 | 0.117 |
| mean wage (conditional on being employed) | 12,464 | 12,464 | 12,464 | 12,464 | 12,464 | 12,464 |
| Individual fixed effects | yes | yes | yes | yes | yes | yes |
| State × month fixed effects | yes | yes | yes | yes | yes | yes |

*Notes:* Each column within each panel represents a difference regression. An observation is an individual-month. The dependent variable is a categorical variable equal to 1 if individual $i$ in month $t$ has had at least one loan in arrears for 4 months. The independent variable in Panel A is a categorical variable equal to 1 if individual $i$ in month $t-k$ is employed in the formal sector. In panel B, the independent variable is the logarithm of the wage. Panel B is only ran in those individuals whose income we know (ie. those individuals in the formal sector). Columns (1) through (6) show different levels of $k$. All specifications include individual and state× month fixed effects. Standard errors are shown in parenthesis.

# Online Appendix for Mitigating the Risks of Financial Inclusion with Loan Contract Terms

Sara G. Castellanos    Diego Jiménez    Aprajit Mahajan    Enrique Seira

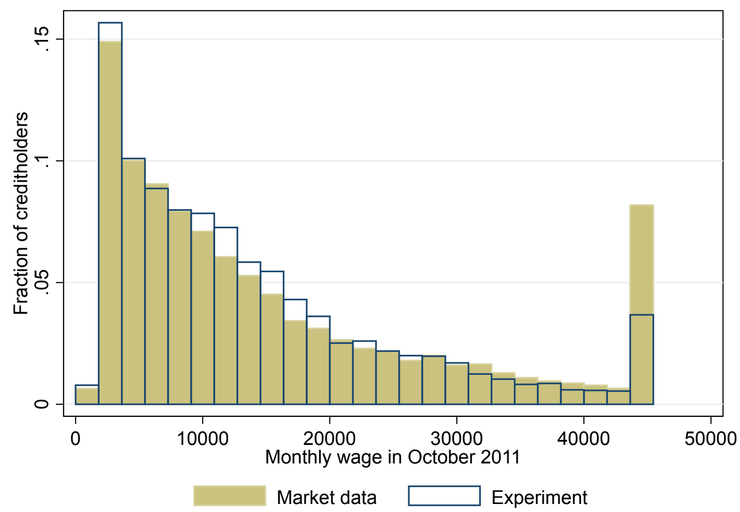
## A Financial Inclusion in Mexico

### A.1 Additional figures and tables

**Figure OA.1:** Example of stands used for bancarization

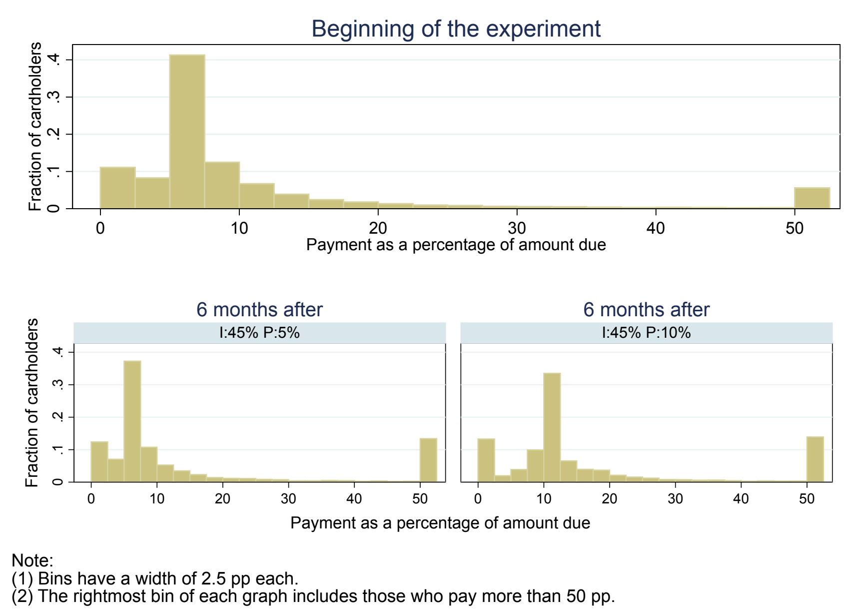

*Notes:*

**Figure OA.2:** Creditholders by income in October 2011




*Notes:* The histogram plots in dark bars the distribution of a random sample of consumers in the credit bureau with at least one credit card. The light bars show the distribution of the income of consumers in the experiment. Sampling weights are used for the experiment population. The histogram is censored at 45,000 pesos. We find 18 and 13 percent of our consumers from the experimental and random sample datasets in the social security database, respectively.

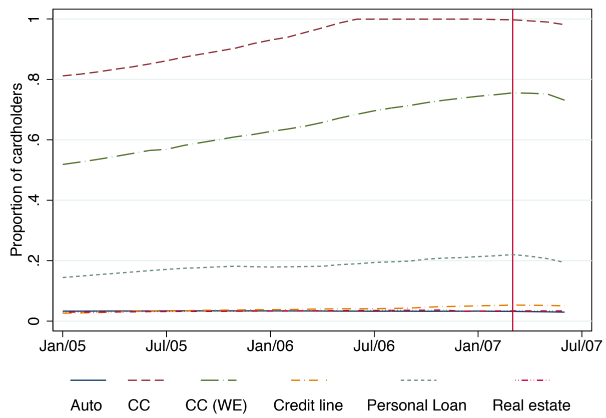
**Figure OA.3:** Payment as a fraction of debt before the experiment



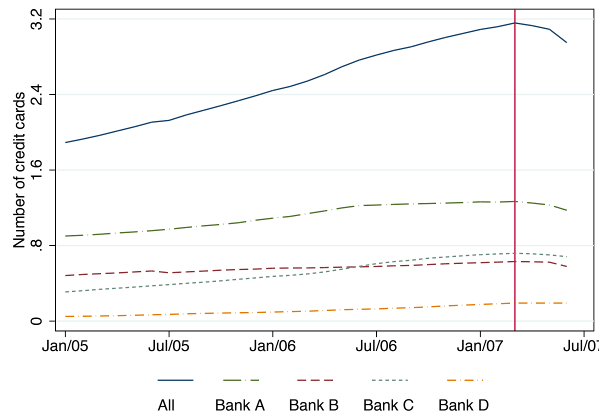

*Notes:* Payment as a percentage of the amount due is defined as the quotient between the payments in April 2007 and the amount due in March 2007. The variable in the X-axis is only an approximation to the minimum payment since the minimum payment may include some fees or discounts that we do not observe.

**Figure OA.4:** Prior loans for the experimental sample

**(a)** Prior loans

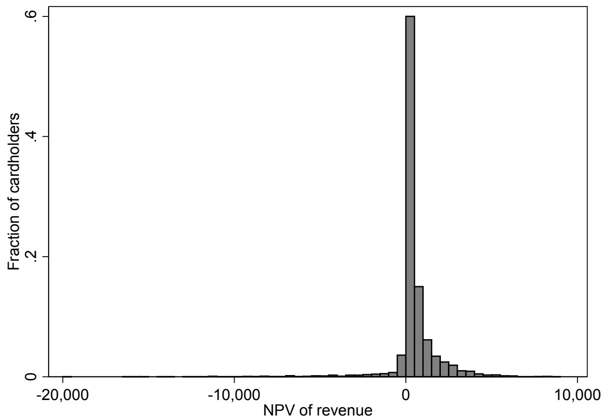



**(b)** Number of credit cards

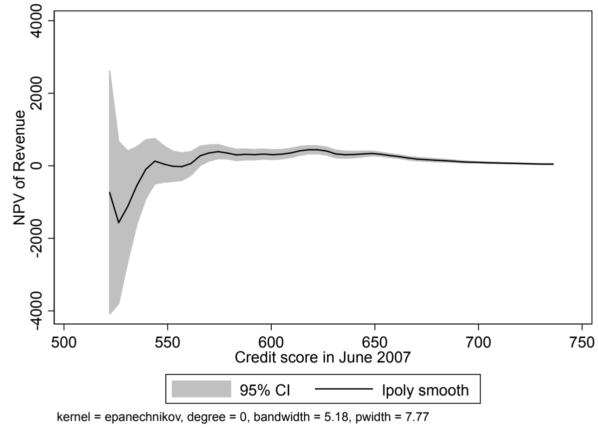



*Notes:* The vertical line marks the month when the experiment started. Panel (a) shows the proportion of cardholders that, prior to month $t$ have had at least one credit of each type of loans. We plot the five most popular types of loans in our experimental sample. CC stands for credit cards and WE stands for without experimental cards. Excluded from this figure are loans for furniture, (car) leasing, home equity loans, miscellaneous, guaranteed cards, and unsecured loans. These loans, 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. Bank A represents our cooperating bank. We exclude 5 banks that represent 0.1 cards altogether by the beginning of the experiment and less than 0.1 before.

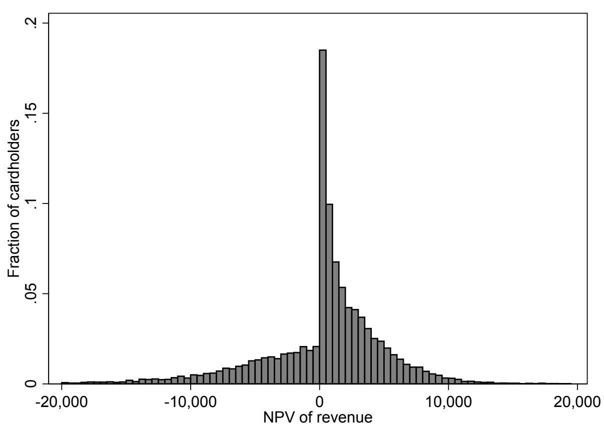
**Figure OA.5:** Ex Post Profit Distribution and Relationship to Scores for selected strata



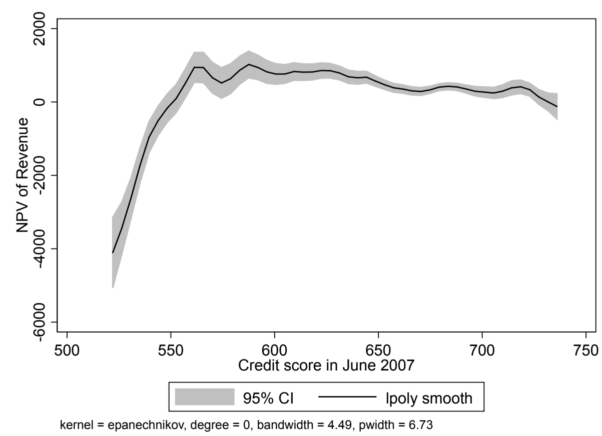

**(a)** Distribution of NPV of Revenue
Full payers with 24+ months with the credit card




**(b)** NPV of Revenue by Credit Score
Full payers with 24+ months with the credit card



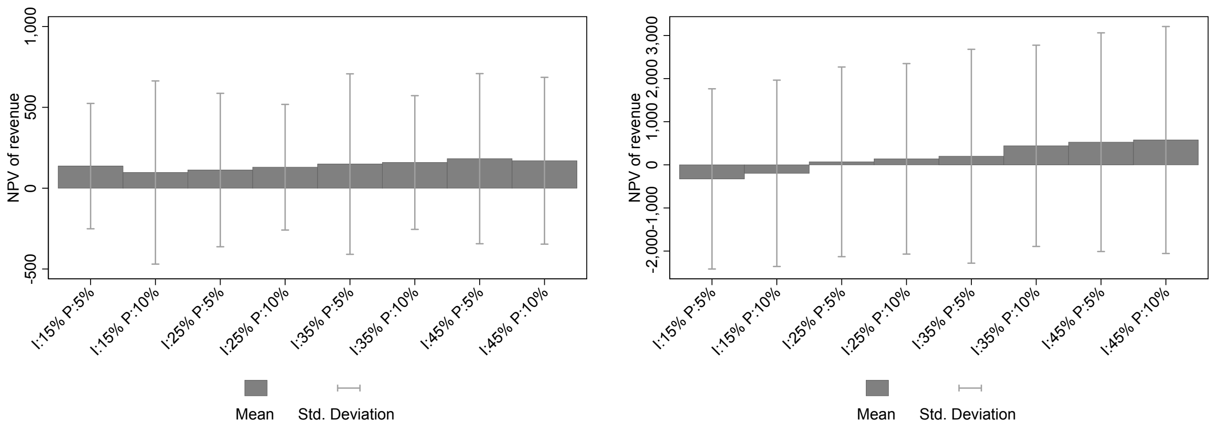

**(c)** Distribution of NPV of Revenue
Minimal payment payers with 6-11 months with the card




**(d)** NPV of Revenue by Credit Score
Minimal payment payers with 6-11 months with the card

*Notes:* Figures (a) and (c) represent the distribution of the revenue obtained from the experiment subjects for different strata levels. For readibility, the graph excludes those individuals with NPV of revenue exactly equal to zero. Figures (b) and (d) shows a kernel regression for the NPV of revenue based on the credit score in June 2007.

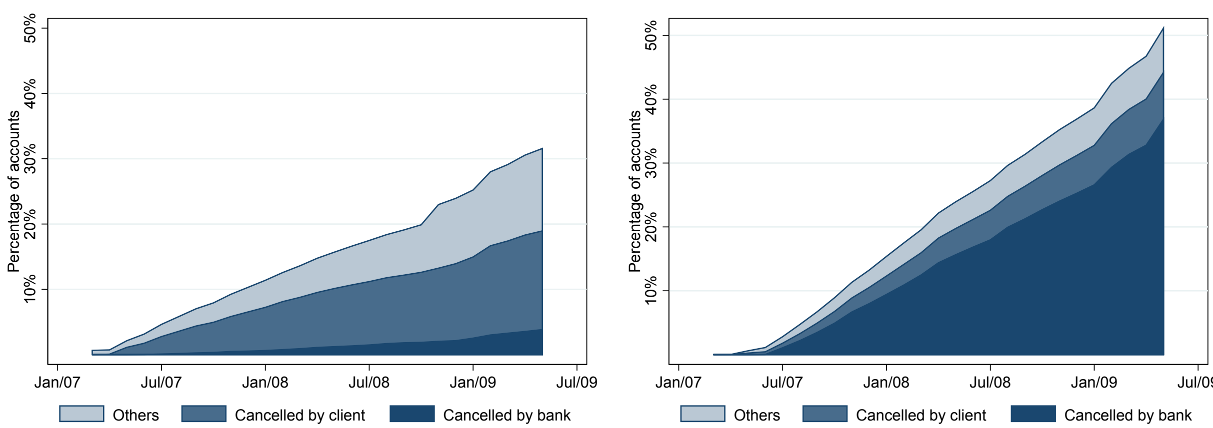
**Figure OA.6:** Mean and Standard Deviation of profits for Selected Strata




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

*Notes:* The figure shows the mean and standard deviation of the net of present value of revenue for different interest rate and minimum payment combinations for the subjects who pay their debts fully and have had their cards for more than two years in panel (a) and for the minimal payement subjects with less than 11 months with their card in panel (b).

**Figure OA.7:** Attrition in the Experiment for 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 study cards. Card closures are subdivided into whether it was (a) the bank's decision (the card is revoked or frozen) (b) the borrower's decision (cancellation) or (c) other reasons (primarily lost cards or death). This graph is analogous to Figure OA.11 on panel (b) in the main paper.

## A.2 Liquidity constraints

We follow Gross & Souleles (2002) and test for liquidity constraints in our experimental sample. Their baseline specification follows the equation:

$$\Delta Debt_{i,t} = \delta_t + \sum_{j=0}^{12} \beta_j \Delta Limit_{i,t-j} + \gamma' X_{i,t} + \epsilon_{i,t} \qquad (1)$$

where the coefficient of interest is $b_{tot} = \sum_{j=0}^{12} \beta_j$. Table 2 in their paper shows their regression results. For comparison, their baseline $b_{tot}$ estimate is $0.126$ $(0.021)$ and their IV estimate is $0.111$ $(0.018)$. The following Table summarizes our results.

**Table OA.1:** Gross & Souleles (2002) estimation results

| | | 6-11 months | | | 12-23 months | | | 24+ months | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | All | Min | Two + | Full | Min | Two + | Full | Min | Two + | Full |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| Baseline | 0.35 | 0.74 | 0.43 | 0.20 | 0.61 | 0.46 | 0.13 | 0.35 | 0.15 | 0.03 |
| | (0.03) | (0.05) | (0.03) | (0.02) | (0.05) | (0.04) | (0.02) | (0.05) | (0.03) | (0.01) |
| With # changes | 0.37 | 0.73 | 0.47 | 0.24 | 0.60 | 0.49 | 0.14 | 0.37 | 0.15 | 0.03 |
| | (0.03) | (0.05) | (0.04) | (0.03) | (0.06) | (0.05) | (0.02) | (0.05) | (0.03) | (0.01) |
| With # changes IV | 0.73 | 2.13 | 1.26 | 0.53 | 1.60 | 1.03 | 0.09 | 0.63 | 0.52 | -0.08 |
| | (0.14) | (0.31) | (0.28) | (0.37) | (0.28) | (0.39) | (0.09) | (0.19) | (0.28) | (0.14) |
| Observations | 1,640,189 | 163,674 | 179,821 | 188,817 | 172,646 | 180,383 | 187,382 | 184,688 | 187,058 | 195,720 |
| Observations in IV | 1,384,908 | 120,706 | 145,961 | 173,560 | 128,061 | 146,907 | 176,744 | 147,946 | 157,084 | 187,939 |

*Notes:* Standard errors are shown in parenthesis. Errors are clustered at the individual level. Each cell represents a different regression. This table shows the $b_{tot}$ estimates for different populations and different specifications. Each column represents a different population of interest, based on the strata variables for the experiment. Column (1) estimates include probability weights based on the size of each of the strata in the population. All regressions include time fixed effects. The first row shows the baseline estimates, which identically mimics the estimation by Gross & Souleles (2002). The second row shows estimations that control by number of credit limit increases and number of credit limit decreases. The third row shows the instrumental variable approach that uses as instrument the months since the last credit limit change. Following GS, the third row also controls by number of credit limit increases and decreases as well.

# B Mitigating default and exit with contract terms

## B.1 Description of the experiment and randomization

**Table OA.2:** Experimental Design

*Panel A: Stratification*

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

*Panel B: Randomization within strata (for each stratum)*

| Interest Rate | Minimum payment | |
|---|---|---|
| | *10%* | *5%* |
| *15%* | 2 | 2 |
| *25%* | 2 | 2 |
| *35%* | 2 | 2 |
| *45%* | 2 | 2 |
| Control | 18 | |

**Table OA.3:** Sampling weights

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

**Table OA.4:** Randomization - 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) tabulates the mean and standard deviation in parenthesis for the selected randomization groups in the experiment. The standard error for the mean estimates can be computed by dividing the standard deviation by the number of individuals in each randomization group. Time-changing variables are measured at the beginning of the experiment. Panel A includes all individuals, whereas Panel B excludes those individuals who leave the experiment for any reason prior to when the experiment concluded. Column (11) shows the mean and standard deviations of the complete sample. Column (12) shows the p-value of a test of all means from (1) to (10) being equal.

## B.2 Treatment effect robustness results

**Table OA.5:** Treatment Effect Estimation in May 2009 - Delinquencies measured in Credit Bureau

| | Delinquencies | | | Cummulative delinquencies | | |
|---|---|---|---|---|---|---|
| | 30 days (1) | 90 days (2) | 150 days (3) | 30 days (4) | 90 days (5) | 150 days (6) |
| I:15%, P:5% | -0.033*** | -0.025*** | -0.022*** | -0.025*** | -0.026*** | -0.025*** |
| | (0.009) | (0.008) | (0.008) | (0.009) | (0.007) | (0.007) |
| I:15%, P:10% | -0.045*** | -0.033*** | -0.027*** | -0.025*** | -0.030*** | -0.026*** |
| | (0.009) | (0.008) | (0.008) | (0.009) | (0.007) | (0.007) |
| I:25%, P:5% | -0.022** | -0.017** | -0.020** | -0.019** | -0.017** | -0.019*** |
| | (0.009) | (0.009) | (0.008) | (0.009) | (0.008) | (0.007) |
| I:25%, P:10% | -0.031*** | -0.019** | -0.017** | -0.020** | -0.018** | -0.016** |
| | (0.009) | (0.009) | (0.008) | (0.009) | (0.008) | (0.007) |
| I:35%, P:5% | -0.002 | 0.000 | 0.001 | 0.002 | 0.001 | -0.004 |
| | (0.010) | (0.009) | (0.008) | (0.009) | (0.008) | (0.007) |
| I:35%, P:10% | -0.019** | -0.012 | -0.010 | -0.007 | -0.017** | -0.014** |
| | (0.010) | (0.009) | (0.008) | (0.009) | (0.008) | (0.007) |
| I:45%, P:10% | -0.003 | 0.005 | 0.006 | -0.000 | -0.003 | -0.001 |
| | (0.010) | (0.009) | (0.008) | (0.009) | (0.008) | (0.007) |
| Constant | 0.225*** | 0.181*** | 0.157*** | 0.295*** | 0.208*** | 0.173*** |
| | (0.007) | (0.007) | (0.006) | (0.007) | (0.006) | (0.005) |
| p-value Treatment | 0.000 | 0.000 | 0.000 | 0.001 | 0.000 | 0.000 |
| p-value Strata | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| Observations | 97,130 | 97,130 | 97,130 | 144,000 | 144,000 | 144,000 |
| R-squared | 0.041 | 0.040 | 0.037 | 0.041 | 0.038 | 0.034 |
| Dependent Variable Mean | 0.189 | 0.151 | 0.129 | 0.261 | 0.174 | 0.140 |

*Notes:* This table provides alternative measures for delinquencies using the credit bureau information. Robust standard errors are shown in parenthesis. The first three columns show estimates that are equal to one whenever the account is delinquent in May 2009, zero if it is not and has a missing value if the account no longer updates its information to the credit bureau. The last three columns show estimates that are equal to one if the account has been delinquent between March 2007 and May 2009 at least once. Columns (1) and (4) define as delinquency having 30 days or more with amount past due. Columns (2) and (5) define as delinquency having 90 days or more with amount past due. Columns (3) and (6) define as delinquency having 150 days or more with amount past due.

**Table OA.6:** Treatment Effects in May 2009 Imputing Missing Values

| | Payments (1) | Purchases (2) | Debt (3) |
|---|---|---|---|
| I: 15% P: 5% | -10.25 | 61.65*** | -281.2*** |
| | (16.02) | (16.79) | (77.78) |
| I: 15% P: 10% | 67.41*** | 155.1*** | -533.2*** |
| | (18.46) | (18.44) | (72.27) |
| I: 25% P: 5% | -15.47 | 20.78 | -119.3 |
| | (15.90) | (14.57) | (87.24) |
| I: 25% P: 10% | 48.15*** | 99.92*** | -522.0*** |
| | (16.47) | (15.84) | (71.59) |
| I: 35% P: 5% | 15.44 | 16.68 | -171.8** |
| | (18.27) | (15.83) | (78.97) |
| I: 35% P: 10% | 51.47*** | 83.81*** | -418.6*** |
| | (19.68) | (16.83) | (75.69) |
| I: 45% P: 10% | 4.769 | 35.57** | -534.2*** |
| | (16.27) | (15.30) | (71.49) |
| Constant | 393.4*** | 293.2*** | 1307.6*** |
| | (14.03) | (15.83) | (61.33) |
| Observations | 144000 | 144000 | 144000 |
| R-squared | 0.0170 | 0.0211 | 0.00831 |
| Dependent variable mean | 392.6 | 305.3 | 934.1 |

*Notes:* The table is a robustness check for the main treatment effect estimation of the paper, except that missing values are imputed with zeros for payment, purchases and debt (This makes sense as when account is closed these variables have are zero). On other hand cum. delinquency, revoked cards and cancelled cards are imputed with the last observed value before account exits the sample. Standard errors are shown in parenthesis.

**Table OA.7:** Treatment effects in May 2009: Heterogeneous effects

| | Payments | Purchases | Debt | Delinquency | Cumulative Delinquency | Revoked Cards | Cancelled Cards | Credit Bureau Score | Attrition |
|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| *Panel A. Below median income [Extrapolated income]* | | | | | | | | | |
| I: 15%, P: 5% | -62.82*** | 89.45*** | -655.91*** | -0.02*** | -0.04*** | -0.02*** | -0.02*** | 2.95*** | -0.02*** |
| | (0.11) | (0.12) | (0.42) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| I: 15%, P: 10% | 157.99*** | 298.98*** | -1231.81*** | -0.03*** | -0.03*** | -0.00*** | 0.01*** | 6.05*** | 0.02*** |
| | (0.12) | (0.11) | (0.56) | (0.00) | (0.00) | (0.00) | (0.00) | (0.01) | (0.00) |
| I: 25%, P: 5% | -74.78*** | 8.81*** | -535.65*** | -0.02*** | -0.04*** | -0.02*** | -0.01*** | 3.92*** | -0.02*** |
| | (0.22) | (0.17) | (0.58) | (0.00) | (0.00) | (0.00) | (0.00) | (0.01) | (0.00) |
| I: 25%, P: 10% | 109.40*** | 158.92*** | -1016.53*** | -0.02*** | -0.02*** | -0.01*** | 0.01*** | 5.10*** | 0.01*** |
| | (0.15) | (0.24) | (0.56) | (0.00) | (0.00) | (0.00) | (0.00) | (0.01) | (0.00) |
| I: 35%, P: 5% | -16.52*** | 43.45*** | -319.65*** | -0.02*** | -0.01*** | 0.01*** | -0.00*** | -0.14*** | 0.01*** |
| | (0.10) | (0.11) | (0.39) | (0.00) | (0.00) | (0.00) | (0.00) | (0.01) | (0.00) |
| I: 35%, P: 10% | 123.38*** | 141.21*** | -899.78*** | -0.01*** | -0.01*** | -0.01*** | 0.01*** | 4.13*** | 0.02*** |
| | (0.21) | (0.16) | (0.97) | (0.00) | (0.00) | (0.00) | (0.00) | (0.01) | (0.00) |
| I: 45%, P: 10% | 53.42*** | 111.52*** | -903.76*** | -0.01*** | 0.00*** | 0.01*** | 0.04*** | 3.53*** | 0.07*** |
| | (0.26) | (0.32) | (1.12) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| Constant | 661.11*** | 483.49*** | 2387.24*** | 0.14*** | 0.32*** | 0.21*** | 0.10*** | 609.76*** | 0.38*** |
| | (24.06) | (8.40) | (67.27) | (0.01) | (0.00) | (0.00) | (0.00) | (0.67) | (0.01) |
| Observations | 49094 | 49094 | 49094 | 56924 | 85107 | 80366 | 80366 | 80351 | 85107 |
| R-squared | 0.0141 | 0.0186 | 0.0239 | 0.0416 | 0.0325 | 0.00605 | 0.00870 | 0.0503 | 0.00511 |
| Dependent Variable Mean | 691.9 | 578.9 | 1539.8 | 0.175 | 0.310 | 0.260 | 0.129 | 614.4 | 0.341 |
| *Panel B. More than two credit cards* | | | | | | | | | |
| I: 15%, P: 5% | -63.28*** | 84.02*** | -646.62*** | -0.03*** | -0.04*** | -0.02*** | -0.03*** | 2.66*** | -0.05*** |
| | (0.11) | (0.15) | (0.67) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| I: 15%, P: 10% | 131.88*** | 307.05*** | -1019.81*** | -0.04*** | -0.04*** | -0.01*** | -0.02*** | 5.92*** | -0.01*** |
| | (0.12) | (0.10) | (0.50) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| I: 25%, P: 5% | -57.54*** | 42.07*** | -319.17*** | -0.02*** | -0.04*** | -0.02*** | -0.02*** | 4.44*** | -0.04*** |
| | (0.15) | (0.20) | (0.75) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| I: 25%, P: 10% | 121.59*** | 220.43*** | -1018.47*** | -0.03*** | -0.02*** | -0.00*** | -0.01*** | 3.66*** | 0.00*** |
| | (0.11) | (0.11) | (0.43) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| I: 35%, P: 5% | 19.90*** | 62.76*** | -422.94*** | -0.02*** | -0.01*** | -0.00*** | -0.02*** | 1.58*** | -0.02*** |
| | (0.18) | (0.14) | (0.67) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) | (0.00) |
| I: 35%, P: 10% | 131.53*** | 191.57*** | -809.60*** | -0.01*** | -0.00*** | 0.00*** | -0.01*** | 3.07*** | 0.01*** |
| | (0.07) | (0.14) | (0.57) | (0.00) | (0.00) | (0.00) | (0.00) | (0.01) | (0.00) |
| I: 45%, P: 10% | 44.63*** | 125.80*** | -933.06*** | -0.01*** | 0.01*** | 0.02*** | 0.00*** | 2.55*** | 0.04*** |
| | (0.27) | (0.36) | (1.31) | (0.00) | (0.00) | (0.00) | (0.00) | (0.01) | (0.00) |
| Constant | 736.56*** | 516.17*** | 2576.15*** | 0.14*** | 0.30*** | 0.18*** | 0.09*** | 599.76*** | 0.33*** |
| | (14.68) | (20.78) | (76.81) | (0.01) | (0.01) | (0.01) | (0.00) | (1.18) | (0.01) |
| Observations | 58345 | 58345 | 58345 | 64382 | 88955 | 84720 | 84720 | 86204 | 88955 |
| R-squared | 0.0235 | 0.0314 | 0.0207 | 0.0355 | 0.0346 | 0.00539 | 0.00423 | 0.0706 | 0.00464 |
| Dependent Variable Mean | 871.9 | 767.4 | 1333.7 | 0.122 | 0.261 | 0.204 | 0.114 | 621.7 | 0.283 |

*Notes:* Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

**Table OA.8:** Treatment effects in May 2009 for Full payers with the card for more than 2 years

| | Payments | Purchases | Debt | Net revenue | Cost | Delinquencies | Cumulative delinquencies | Revoked cards | Cancelled cards | Credit score | Attrition |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
| I:15% P:5% | -102 | 12 | -60 | -46 | -60** | -0.010* | -0.007 | 0 | -0.011 | 2.26 | -0.028* |
| | -102 | -101 | -81 | -29 | -30 | -0.006 | -0.007 | -0.006 | -0.011 | -2.04 | -0.015 |
| I:15% P:10% | -14 | -15 | -99 | -86** | -19 | -0.004 | -0.003 | -0.006 | -0.005 | 0.39 | -0.032** |
| | -110 | -101 | -79 | -35 | -37 | -0.006 | -0.008 | -0.006 | -0.011 | -2.01 | -0.014 |
| I:25% P:5% | -103 | -5.05 | -34 | -70** | -31 | -0.012** | -0.019*** | -0.010* | 0.011 | 3.90** | -0.01 |
| | -111 | -106 | -92 | -32 | -34 | -0.006 | -0.007 | -0.006 | -0.011 | -1.98 | -0.015 |
| I:25% P:10% | -75 | -94 | -179** | -53* | -55* | -0.007 | -0.009 | -0.004 | 0.005 | 4.85** | -0.02 |
| | -103 | -94 | -71 | -29 | -31 | -0.006 | -0.007 | -0.006 | -0.011 | -1.97 | -0.015 |
| I:35% P:5% | 20 | 64 | -71 | -33 | -51 | -0.012** | -0.017** | -0.007 | 0.002 | 5.5*** | -0.019 |
| | -112 | -106 | -88 | -34 | -37 | -0.006 | -0.007 | -0.006 | -0.011 | -1.97 | -0.015 |
| I:35% P:10% | 95 | 199 | 53 | -24 | -45 | -0.006 | -0.004 | -0.001 | 0.023** | 4.15** | 0.023 |
| | -161 | -162 | -97 | -30 | -32 | -0.006 | -0.008 | -0.006 | -0.011 | -1.99 | -0.015 |
| I:45% P:10% | -23 | 61 | -204*** | -13 | -28 | -0.013** | -0.006 | 0 | 0.019* | 3.90* | -0.006 |
| | -108 | -118 | -67 | -33 | -36 | -0.006 | -0.007 | -0.006 | -0.011 | -2 | -0.015 |
| Constant | 1402*** | 1341*** | 413*** | 183*** | 150*** | 0.030*** | 0.063*** | 0.039*** | 0.142*** | 677*** | 0.317*** |
| | -86 | -73 | -59 | -24 | -26 | -0.005 | -0.005 | -0.004 | -0.008 | -1.44 | -0.01 |
| Observations | 1 | 1 | 0 | 0 | 1 | 0 | 0 | 1 | 0 | 0 | 0.005 |
| p-value Treatments | 10948 | 10948 | 10948 | 16000 | 16000 | 10948 | 16000 | 16000 | 16000 | 15077 | 16000 |
| R-squared | 0 | 0.001 | 0.001 | 0.001 | 0 | 0.001 | 0.001 | 0 | 0.001 | 0.001 | 0.001 |
| Dependent Variable Mean | 1376 | 1367 | 338 | 142 | 113 | 0.021 | 0.054 | 0.035 | 0.148 | 680 | 0.306 |

*Notes:* Each column represents a separate, cross-sectional, regression. Dependent variables are shown above the column number; the independent variables are the treatments, where the IR: 45% and MP: 5% group is excluded. All regressions are weighted by the values given in Table **??**. Robust standard errors are shown in parenthesis. ***, ** and * show statistical significance at the 1, 5 and 10 percent, respectively. Monetary variables (1) to (3) are measured in constant — March 2007 — MXN pesos. Column (4) shows the net present value of revenue by account for the experiment. Column (5) shows the present value of the cost as explained in the paper. Column (6) dependent variable is equal to one if the account is reported to be delinquent in May 2009, zero if not and missing if the account has already exited the sample. Column (7) dependent variable is equal to one if the account has been reported to be delinquent at least once before or on May 2009, and zero otherwise. Columns (8) dependent variable is an indicator variable that show which accounts have exited the sample by May 2009 whose reason to attrit can be attributed to being voluntarily cancelling the card. Column (10) dependent variable is equal to one if the account has attrited from the experiment irrespective of the reason for attrition and zero otherwise. The p-value of the treatment variables tests the null hypothesis that all treatment arms – except for the constant – are statistically zero. The p-value of the strata variables tests the null hypothesis that all variables have equal means – which is equivalent to all strata dummies being equal to zero.

**Table OA.9:** Treatment effects in May 2009 for Minimal payment payers with less than 12 months with the card

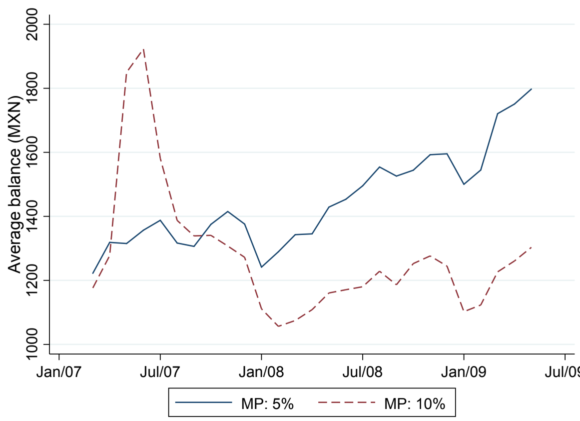
| | Payments | Purchases | Debt | Net revenue | Cost | Delinquencies | Cumulative delinquencies | Revoked cards | Cancelled cards | Credit score | Attrition |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) | (11) |
| I:15% P:5% | -14 | 42 | -633** | -852*** | -494*** | -0.023 | -0.018 | -0.017 | -0.037*** | 0.366 | -0.042*** |
| | -47 | -50 | -254 | -147 | -158 | -0.018 | -0.016 | -0.015 | -0.008 | -2.84 | -0.016 |
| I:15% P:10% | 125** | 209*** | -1303*** | -722*** | -722*** | -0.026 | 0.007 | 0.014 | -0.026*** | -3.17 | 0.004 |
| | -49 | -56 | -246 | -149 | -159 | -0.018 | -0.016 | -0.015 | -0.008 | -2.86 | -0.016 |
| I:25% P:5% | -23 | -16 | -160 | -456*** | -531*** | -0.009 | -0.025 | -0.014 | -0.026*** | -0.255 | -0.021 |
| | -49 | -50 | -271 | -150 | -161 | -0.018 | -0.016 | -0.015 | -0.008 | -2.84 | -0.016 |
| I:25% P:10% | 99** | 135** | -1137*** | -386** | -651*** | -0.016 | 0.011 | 0.001 | -0.024*** | -3.33 | -0.006 |
| | -50 | -56 | -251 | -150 | -160 | -0.019 | -0.016 | -0.015 | -0.008 | -2.84 | -0.016 |
| I:35% P:5% | -33 | -48 | -99 | -326** | -69 | -0.001 | 0.009 | 0.007 | -0.024*** | -5.2* | -0.018 |
| | -43 | -56 | -270 | -159 | -168 | -0.019 | -0.016 | -0.015 | -0.008 | -2.86 | -0.016 |
| I:35% P:10% | 145*** | 118** | -1018*** | -84 | -604*** | -0.006 | -0.007 | -0.012 | -0.004 | -2.14 | 0.012 |
| | -48 | -53 | -256 | -154 | -165 | -0.019 | -0.016 | -0.015 | -0.009 | -2.87 | -0.016 |
| I:45% P:10% | 163*** | 125** | -750*** | 50 | -172 | 0.033* | 0.032** | 0.018 | 0.005 | -3.44 | 0.043*** |
| | -57 | -56 | -264 | -164 | -174 | -0.02 | -0.016 | -0.015 | -0.009 | -2.86 | -0.016 |
| Constant | 530*** | 354*** | 3433*** | 526*** | 2954*** | 0.218*** | 0.485*** | 0.328*** | 0.089*** | 576*** | 0.471*** |
| | -33 | -43 | -199 | -113 | -121 | -0.013 | -0.011 | -0.011 | -0.006 | -2 | -0.011 |
| Observations | 7820 | 7820 | 7820 | 16000 | 16000 | 7820 | 16000 | 16000 | 16000 | 14977 | 16000 |
| p-value Treatments | 0 | 0 | 0 | 0 | 0 | 0.094 | 0.011 | 0.149 | 0 | 0.445 | 0 |
| R-squared | 0.005 | 0.006 | 0.008 | 0.004 | 0.003 | 0.002 | 0.001 | 0.001 | 0.003 | 0 | 0.002 |
| Dependent Variable Mean | 585 | 422 | 2806 | 179 | 2548 | 0.212 | 0.487 | 0.328 | 0.072 | 574 | 0.467 |

*Notes:* Each column represents a separate, cross-sectional, regression. Dependent variables are shown above the column number; the independent variables are the treatments, where the IR: 45% and MP: 5% group is excluded. All regressions are weighted by the values given in Table **??**. Robust standard errors are shown in parenthesis. ***, ** and * show statistical significance at the 1, 5 and 10 percent, respectively. Monetary variables (1) to (3) are measured in constant — March 2007 — MXN pesos. Column (4) shows the net present value of revenue by account for the experiment. Column (5) shows the present value of the cost as explained in the paper. Column (6) dependent variable is equal to one if the account is reported to be delinquent in May 2009, zero if not and missing if the account has already exited the sample. Column (7) dependent variable is equal to one if the account has been reported to be delinquent at least once before or on May 2009, and zero otherwise. Columns (8) dependent variable is an indicator variable that show which accounts have exited the sample by May 2009 whose reason to attrit can be attributed to being voluntarily cancelling the card. Column (10) dependent variable is equal to one if the account has attrited from the experiment irrespective of the reason for attrition and zero otherwise. The p-value of the treatment variables tests the null hypothesis that all treatment arms – except for the constant – are statistically zero. The p-value of the strata variables tests the null hypothesis that all variables have equal means – which is equivalent to all strata dummies being equal to zero.

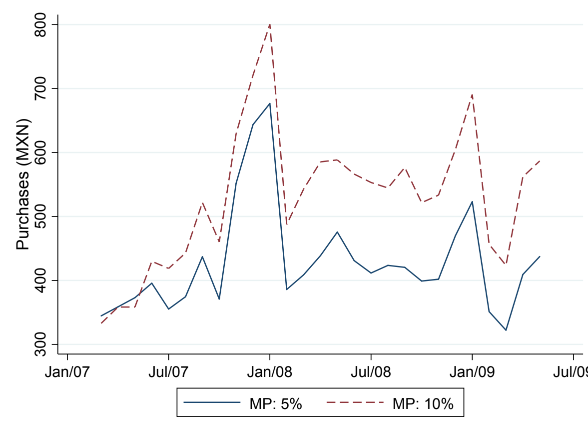
## B.3 Monthly averages for different treatment groups

**Figure OA.8:** Minimum payment variations: Results

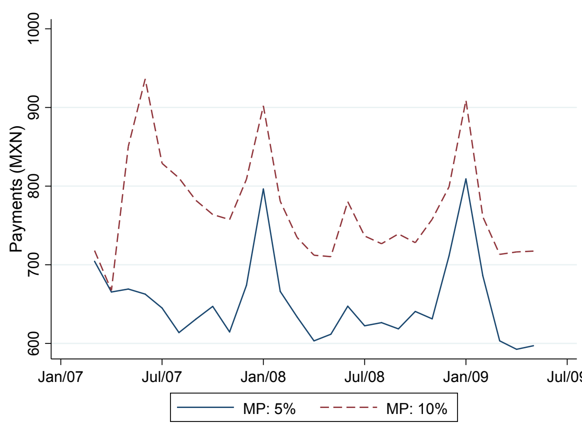
**(a)** Average Balance



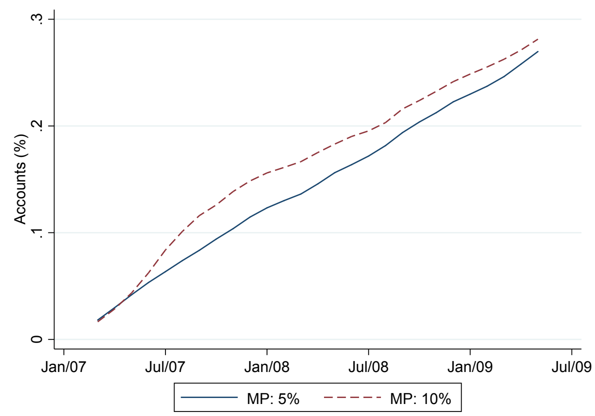

**(b)** Purchases



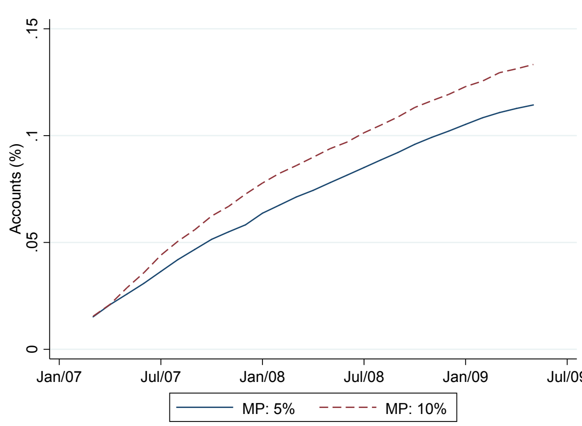

**(c)** Payments



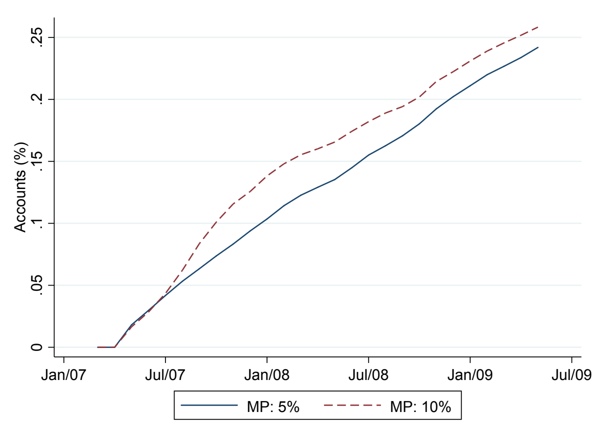

**(d)** Accounts in delinquency (cumulative)




**(e)** Cancelled accounts



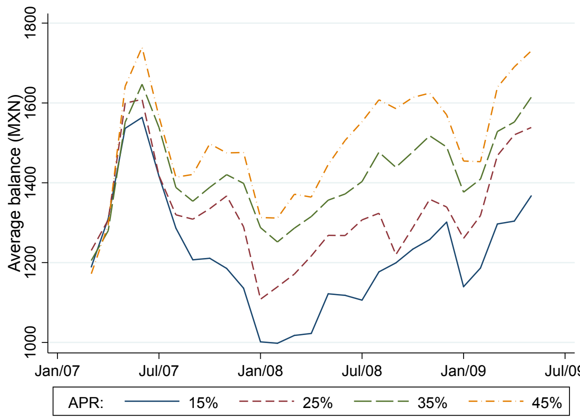

**(f)** Accounts in default (cumulative)



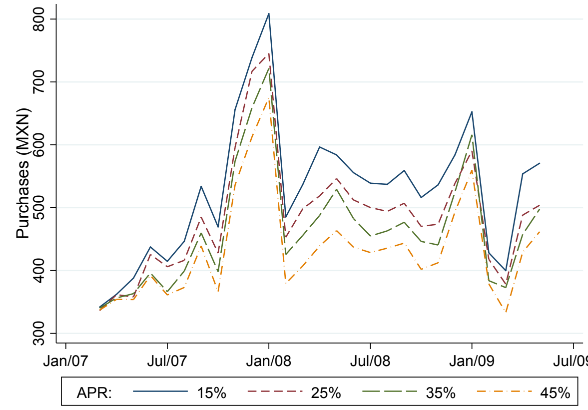

*Note:* **??** refers, to the % of accounts that are delinquent or that have fallen in delinquency at least once before. **??** refers to the cumulative % of accounts that are cancelled. **??** and **??** refer, for each month, to the % of accounts that have defaulted at least once.

**Figure OA.9:** Annual percentage rate variations: Results

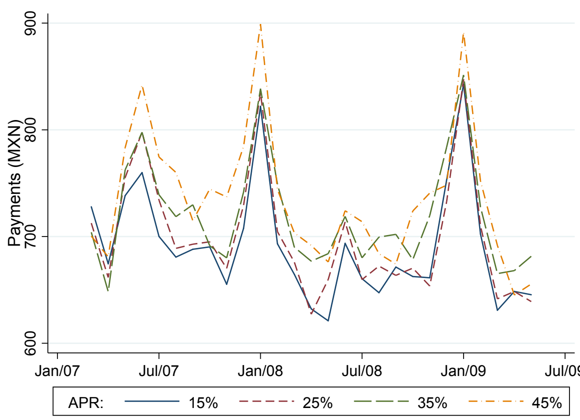
**(a)** Average Balance



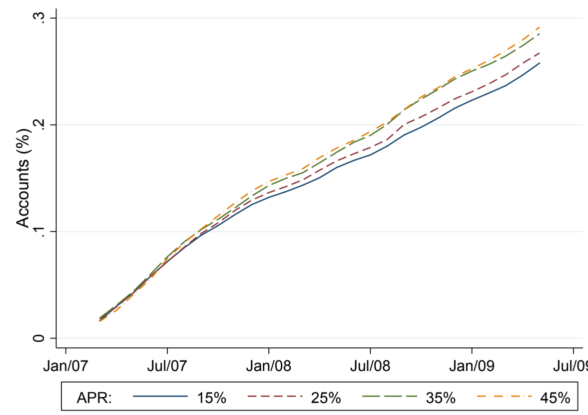

**(b)** Purchases



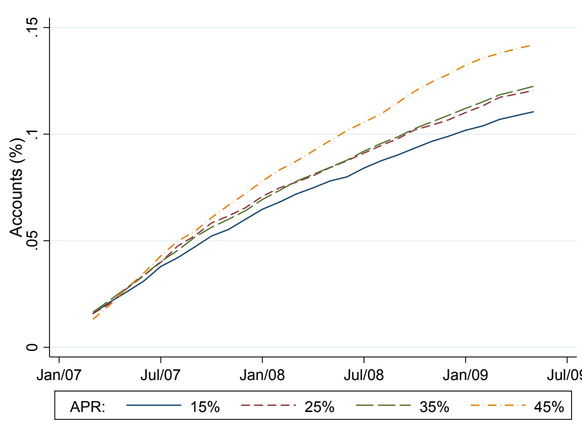

**(c)** Payments



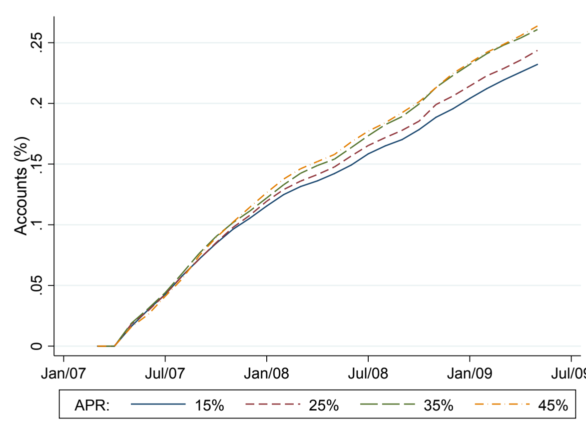

**(d)** Accounts in delinquency (cumulative)




**(e)** Cancelled accounts




**(f)** Accounts in default (cumulative)




*Note:* OA.9d refers, to the % of accounts that are delinquent or that have fallen in delinquency at least once before. OA.9e refers to the cumulative % of accounts that are cancelled. **??** and OA.9f refer, for each month, to the % of accounts that have defaulted at least once.

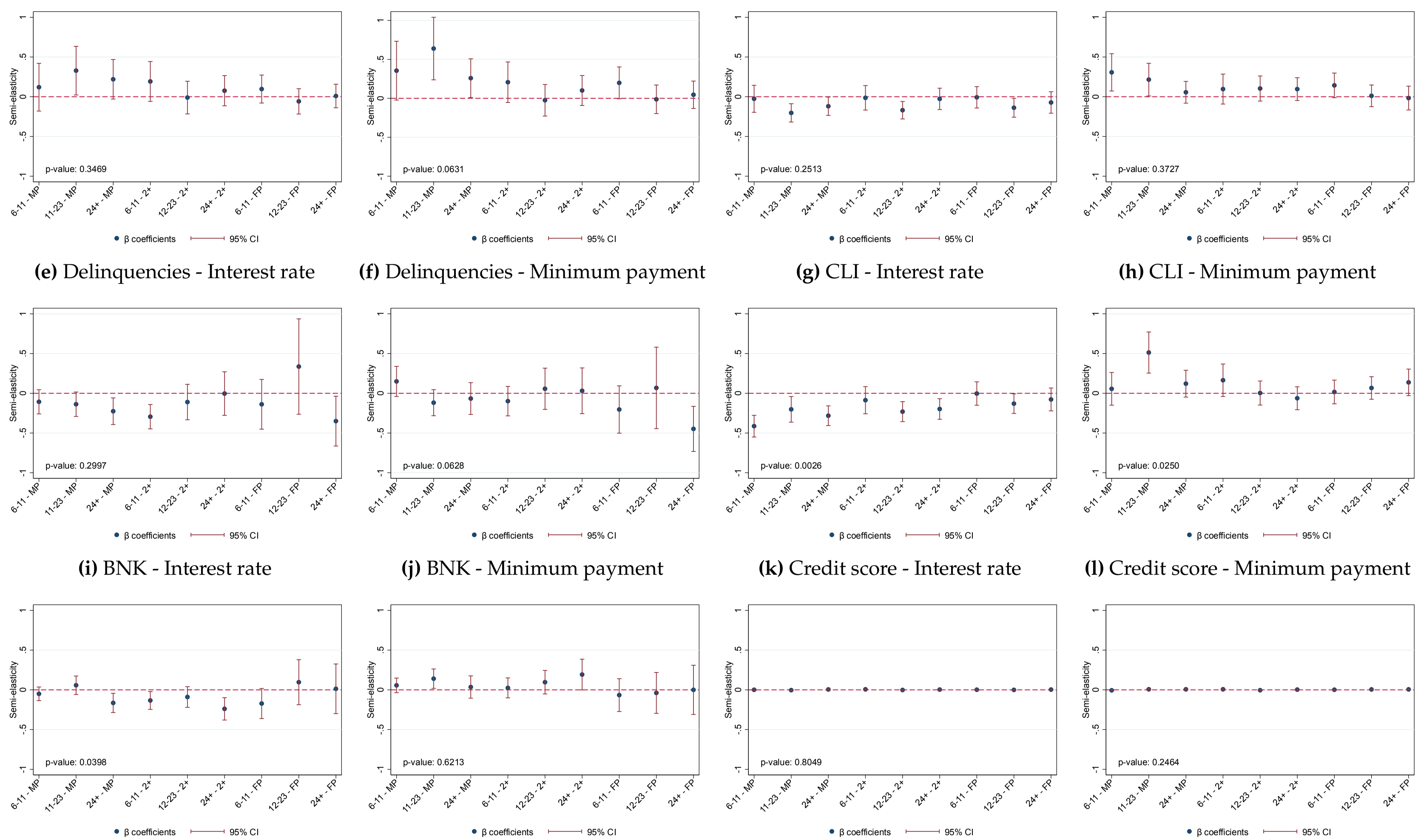
## B.4 Semielasticity estimation

Semielasticities are computed using the following specification:

$$dep\_var = \sum_{(i,j,k)\in\{15\%,\ldots,45\%\}\times\{5\%,10\%\}\times\{1,\ldots,9\}} \beta_{i,j,k}\mathbb{1}(\text{r} = i) \times \mathbb{1}(\text{MP} = j) \times \mathbb{1}(\text{strata} = k) \tag{2}$$

where r and MP denote the interest rate and minimum payment assigned, and the strata variable covers all groups described in the experiment. For simplicity, we eliminate the constant so that all groups can be included without collinearity. Thus, for strata $k$, the semielasticities for interest rate and minimum payments are computed by $\beta_{15\%,5\%,k}/\beta_{45\%,5\%,k} - 1$ and $\beta_{45\%,10\%,k}/\beta_{45\%,5\%,k} - 1$, respectively. The following Figure shows the treatment effect semielasticity estimation for the variables not shown in the paper.

**Figure OA.10:** Treatment effect heterogeneity across strata in May 2009




**(a)** Purchases - Interest rate

**(b)** Purchases - Minimum payment

**(c)** Payments - Interest rate

**(d)** Payments - Minimum payment

**(e)** Delinquencies - Interest rate

**(f)** Delinquencies - Minimum payment

**(g)** CLI - Interest rate

**(h)** CLI - Minimum payment

**(i)** BNK - Interest rate

**(j)** BNK - Minimum payment

**(k)** Credit score - Interest rate

**(l)** Credit score - Minimum payment

*Notes:* The figures represent the semielasticity estimation of the interest rate and the minimum payment by strata. All variable definitions follow those from Table OA.19. Standard errors are given by the upper and lower bars shown in each graph. Probability weights are used following Table OA.3. The complete methodology to obtain these semielasticities is shown in the Online Appendix.

### B.5 Compounding of interest rate

We argue the following relation holds in our data:

$$\text{amount due}_{i,t} = \text{amount due}_{i,t-1} + \text{purchases}_{i,t} - \text{payments}_{i,t} + \text{fees}_{i,t} + \text{debt}_{i,t} \times \text{interest rate}_i \quad (3)$$

To test such an equation we use our data. We ran two specifications, the first where we only 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 and the second with all observations for completeness. The following Table summarizes our results:

**Table OA.10:** Compounding of interest rate

| | (1) | (2) |
|---|---|---|
| Amount Due$_{i,t-1}$ | 1.011*** | 0.996*** |
| | (0.000323) | (0.000248) |
| Payments$_{i,t}$ | -1.000*** | -1.000*** |
| | (0.000627) | (0.000363) |
| Purchases$_{i,t}$ | 0.982*** | 1.008*** |
| | (0.000836) | (0.00102) |
| 15% x Debt$_{i,t}$ | -0.00973*** | 0.179*** |
| | (0.00322) | (0.00343) |
| 25% x Debt$_{i,t}$ | 0.0884*** | 0.279*** |
| | (0.00330) | (0.00356) |
| 35% x Debt$_{i,t}$ | 0.186*** | 0.380*** |
| | (0.00348) | (0.00370) |
| 45% x Debt$_{i,t}$ | 0.280*** | 0.476*** |
| | (0.00424) | (0.00474) |
| Fees$_{i,t}$ | 0.480*** | 0.495*** |
| | (0.00232) | (0.00178) |
| P values | | |
| Coef(15% x Debt$_{i,t}$)=.15+TIIE | 0 | 1.81e-22 |
| Coef(25% x Debt$_{i,t}$)=.25+TIIE | 0 | 3.16e-20 |
| Coef(35% x Debt$_{i,t}$)=.35+TIIE | 0 | 1.31e-17 |
| Coef(45% x Debt$_{i,t}$)=.45+TIIE | 0 | 3.76e-14 |
| R-squared | 0.991 | 1.000 |
| Observations | 3057459 | 483536 |

# C Mechanisms

### C.1 The cost of business stealing

We document the loss in revenue suffered by our cooperating bank that can be attributed to business stealing. For this, we first identify all individuals who close their card with Bank A and open a new card in from $\pm k$ months, where $k$ 6 months in our baseline specification. Our exercise relies on a matching estimator. The potential outcome to compute is what the month revenue would have been had individuals not opened a new card. Thus, for each individual $i$, for each period $t$ after they have opened a new card, we construct a matching estimator that takes the remaining individuals. The non-exact matching covariates are purchases, payments, debt and credit limit, all measured in in the last month where the closed card was still open. The exact matching covariates are our strata variables, the treatment group, as well as the date of the observation to eliminate time effects. The estimates are summarized in the Table below.

**Table OA.11:** Cost of business stealing

| | Baseline (1) | Less months (2) | Non-attriters (3) | Less neighbors (4) | More neighbors (5) |
|---|---|---|---|---|---|
| | *Panel A. Matching procedure characteristics* | | | | |
| Treatment group | +- 6 months | +- 3 months | +- 6 months | +- 6 months | +- 6 months |
| Control group | All individuals | All individuals | Non-attriters | All individuals | All individuals |
| Covariates | | | | | |
| Purchases | yes | yes | yes | yes | yes |
| Payments | yes | yes | yes | yes | yes |
| Debt | yes | yes | yes | yes | yes |
| Credit limit | yes | yes | yes | yes | yes |
| Minimum matches | 2 | 2 | 2 | 1 | 3 |
| | *Panel B. Matching results* | | | | |
| Individuals in treatment | 3,775 | 2,268 | 3,775 | 3,775 | 3,775 |
| Individuals in control | 140,225 | 141,732 | 99,164 | 140,225 | 140,225 |
| Mean loss by account | 246 | 450 | 619 | 427 | 588 |
| SD of loss by account | 1,417 | 1,104 | 1,042 | 1,947 | 1,429 |
| Pct. of accounts w/negative pred. revenue | 30 | 25 | 21 | 48 | 20 |
| Placebo prediction $\beta$ | 0.5 | 0.51 | 0.5 | 0.49 | 0.5 |
| Placebo prediction R-squared | 0.26 | 0.3 | 0.26 | 0.21 | 0.31 |

**Table OA.12:** Probability of getting a loan against default

| | New credit card between $t$ and $t+6$ | | | New credit between $t$ and $t+6$ | | |
|---|---|---|---|---|---|---|
| | OLS (1) | IV (2) | OLS (3) | OLS (4) | IV (5) | OLS (6) |
| Credit score | 0.0006*** | 0.0013*** | | 0.0008*** | 0.0016*** | |
| | (0.0000) | (0.0000) | | (0.0000) | (0.0001) | |
| Default | | | -0.1145*** | | | -0.1466*** |
| | | | (0.0035) | | | (0.0045) |
| Constant | -0.2093*** | -0.6758*** | 0.1498*** | -0.2797*** | -0.8450*** | 0.2126*** |
| | (0.0112) | (0.0249) | (0.0014) | (0.0137) | (0.0321) | (0.0016) |
| R-squared | 0.0081 | . | 0.0048 | 0.0116 | . | 0.0060 |
| Observations | 258,102 | 258,102 | 258,102 | 258,102 | 258,102 | 258,102 |
| Dependent Variable Mean | 0.1443 | 0.1443 | 0.1443 | 0.2056 | 0.2056 | 0.2056 |

*Notes:* This table shows the relation between getting a loan and default. The dependent variable is shown above the column number. For columns (1) to (6), the dependent variable is an indicator variable if consumer $i$ gets a new credit card / new credit between the periods $t$ and $t+6$, respectively. Columns (2) and (5) instrument the credit score using an indicator variable that is equal to 1 if consumer $i$ defaults in period $t$. Columns (3) and (6) show the reduced form relationship between default and the corresponding dependent variables. Errors are clustered at the individual level. ***, ** and * show statistical significance at the 1, 5 and 10 percent, respectively.

## C.2 Unemployment robustness regressions

**Table OA.13:** Unemployment regressions at the individual level with $m = 2$

| Value of $k$ | (1) k = 1 | (2) k = 3 | (3) k = 5 | (4) k = 7 | (5) k = 9 | (6) k = 11 |
|---|---|---|---|---|---|---|
| | *Panel A. Extensive margin regressions* | | | | | |
| formal employment in $t-k$ | -0.0183 | -0.0231 | -0.0293 | -0.0296 | -0.0280 | -0.0257 |
| | (0.0004) | (0.0004) | (0.0004) | (0.0004) | (0.0004) | (0.0004) |
| Observations | 15.7 million | 15.1 million | 14.4 million | 13.7 million | 13.0 million | 12.3 million |
| R-squared | 0.0279 | 0.0277 | 0.0271 | 0.0267 | 0.0261 | 0.0256 |
| | *Panel B. Intensive margin regressions* | | | | | |
| monthly wage (thousands) in $t-k$ | -0.021 | -0.029 | -0.030 | -0.025 | -0.017 | -0.010 |
| | (0.0007) | (0.0007) | (0.0008) | (0.0008) | (0.0009) | (0.0009) |
| Observations | 2.1 million | 2.0 million | 1.9 million | 1.8 million | 1.6 million | 1.5 million |
| R-squared | 0.015 | 0.015 | 0.014 | 0.013 | 0.012 | 0.011 |
| | *Panel C. Additional information* | | | | | |
| unconditional mean dep. variable | 0.148 | 0.148 | 0.148 | 0.148 | 0.148 | 0.148 |
| proportion formally employed in a given month | 0.117 | 0.117 | 0.117 | 0.117 | 0.117 | 0.117 |
| mean wage (conditional on being employed) | 12,464 | 12,464 | 12,464 | 12,464 | 12,464 | 12,464 |
| Individual fixed effects | yes | yes | yes | yes | yes | yes |
| State $\times$ month fixed effects | yes | yes | yes | yes | yes | yes |

*Notes:* Each column within each panel represents a difference regression. An observation is an individual-month. The dependent variable is a categorical variable equal to 1 if individual $i$ in month $t$ has had at least one loan in arrears for 2 months. The independent variable in Panel A is a categorical variable equal to 1 if individual $i$ in month $t-k$ is employed in the formal sector. In panel B, the independent variable is the logarithm of the wage. Panel B is only ran in those individuals whose income we know (ie. those individuals in the formal sector). Columns (1) through (6) show different levels of $k$. All specifications include individual and state$\times$ month fixed effects. Standard errors are shown in parenthesis.

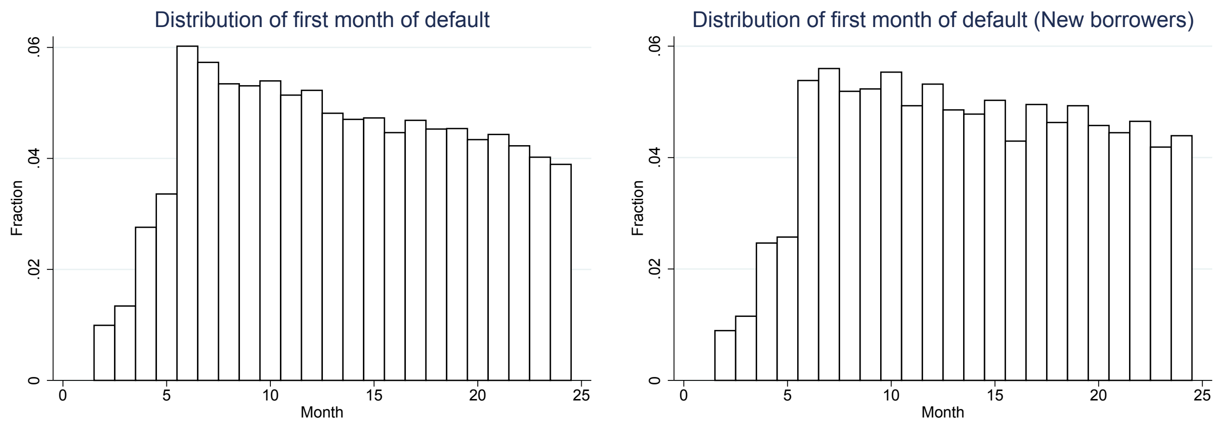
**Table OA.14:** Unemployment regressions at the individual level with $m = 12$

| Value of $k$ | (1) k = 1 | (2) k = 3 | (3) k = 5 | (4) k = 7 | (5) k = 9 | (6) k = 11 |
|---|---|---|---|---|---|---|
| *Panel A. Extensive margin regressions* | | | | | | |
| formal employment in $t-k$ | -0.0138 | -0.0155 | -0.0186 | -0.0224 | -0.0248 | -0.0239 |
| | (0.0003) | (0.0003) | (0.0003) | (0.0003) | (0.0003) | (0.0003) |
| Observations | 15.7 million | 15.1 million | 14.4 million | 13.7 million | 13.0 million | 12.3 million |
| R-squared | 0.0297 | 0.0289 | 0.0284 | 0.0280 | 0.0275 | 0.0269 |
| *Panel B. Intensive margin regressions* | | | | | | |
| monthly wage (thousands) in $t-k$ | -0.007 | -0.012 | -0.016 | -0.019 | -0.019 | -0.016 |
| | (0.0005) | (0.0005) | (0.0006) | (0.0006) | (0.0006) | (0.0006) |
| Observations | 2.1 million | 2.0 million | 1.9 million | 1.8 million | 1.6 million | 1.5 million |
| R-squared | 0.015 | 0.015 | 0.014 | 0.013 | 0.012 | 0.012 |
| *Panel C. Additional information* | | | | | | |
| unconditional mean dep. variable | 0.148 | 0.148 | 0.148 | 0.148 | 0.148 | 0.148 |
| proportion formally employed in a given month | 0.117 | 0.117 | 0.117 | 0.117 | 0.117 | 0.117 |
| mean wage (conditional on being employed) | 12,464 | 12,464 | 12,464 | 12,464 | 12,464 | 12,464 |
| Individual fixed effects | yes | yes | yes | yes | yes | yes |
| State $\times$ month fixed effects | yes | yes | yes | yes | yes | yes |

*Notes:* Each column within each panel represents a difference regression. An observation is an individual-month. The dependent variable is a categorical variable equal to 1 if individual $i$ in month $t$ has had at least one loan in arrears for 12 months. The independent variable in Panel A is a categorical variable equal to 1 if individual $i$ in month $t-k$ is employed in the formal sector. In panel B, the independent variable is the logarithm of the wage. Panel B is only ran in those individuals whose income we know (ie. those individuals in the formal sector). Columns (1) through (6) show different levels of $k$. All specifications include individual and state$\times$ month fixed effects. Standard errors are shown in parenthesis.

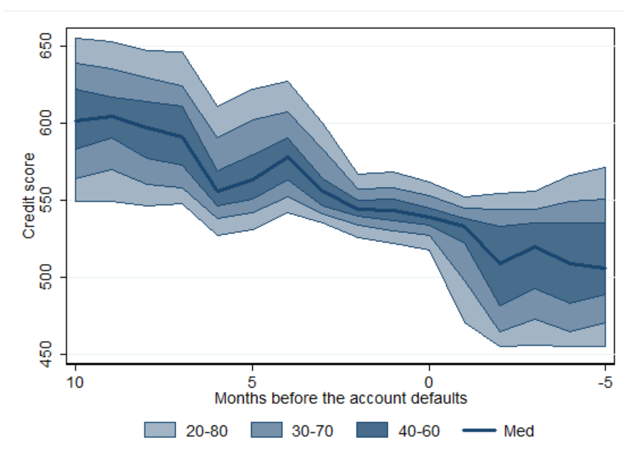
# D Other

**Figure OA.11:** Distribution of first month of default given default




*Notes:* Both figures represent the distribution of the first month of default (given default on the first two years) for all borrowers (a) and new borrowers (b). 8.56% of borrowers commited default on the period, whereas the percentage falls to 8.0% on new borrowers. On the first 12 months the according percentages are 3.99% and 3.54%. *Do file: MonthOfFirstDefault*

**Figure OA.12:** Punishment for default.




*Note:* Figure shows punishment for defaulting in the credit score. It shows the distribution of credit vs months before the account defaults.

**Table OA.15:** Attrition Results

| | (1) | (2) |
|---|---|---|
| 06-11 M × Part-balance | -0.10*** | -0.09*** |
| | (0.01) | (0.01) |
| 06-11 M × Full-balance | -0.16*** | -0.15*** |
| | (0.01) | (0.01) |
| 12-23 M × Part-balance | -0.11*** | -0.10*** |
| | (0.01) | (0.01) |
| 12-23 M × Full-balance | -0.15*** | -0.14*** |
| | (0.01) | (0.01) |
| 12-23 M × Minimum | -0.05*** | -0.05*** |
| | (0.01) | (0.01) |
| 24+ M × Part-balance | -0.15*** | -0.14*** |
| | (0.01) | (0.01) |
| 24+ M × Full-balance | -0.20*** | -0.18*** |
| | (0.01) | (0.01) |
| 24+ M × Minimum | -0.13*** | -0.11*** |
| | (0.01) | (0.01) |
| I:15%, P:5% | -0.05*** | -0.05*** |
| | (0.01) | (0.01) |
| I:15%, P:10% | -0.00 | -0.01 |
| | (0.01) | (0.01) |
| I:25%, P:5% | -0.04*** | -0.04*** |
| | (0.01) | (0.01) |
| I:25%, P:10% | 0.01 | 0.01 |
| | (0.01) | (0.01) |
| I:35%, P:5% | -0.01 | -0.02 |
| | (0.01) | (0.01) |
| I:35%, P:10% | 0.01 | 0.01 |
| | (0.01) | (0.01) |
| I:45%, P:10% | 0.04*** | 0.04*** |
| | (0.01) | (0.01) |
| log(1+debt) | | 0.02*** |
| | | (0.00) |
| log(1+payment) | | -0.03*** |
| | | (0.00) |
| log(1+purchases) | | -0.01*** |
| | | (0.00) |
| Constant | 0.52*** | 0.64*** |
| | (0.01) | (0.01) |
| Dependent variable mean | 0.41 | 0.41 |
| Observations | 144,000 | 143,626 |
| R-squared | 0.012 | 0.055 |

*Notes:* Robust standard errors are shown in parenthesis. Probability weights are used according to sample proportions. The dependent variable is a discrete variable which is equal to one if the account attrits during the experiment.

**Table OA.16:** Net Present Value of Revenue Regressions

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| Male | -98.74*** | -93.37** | -90.27** | | -12.61 |
| | (37.46) | (38.46) | (38.48) | | (114.1) |
| Married | 58.88 | 57.55 | 59.61 | | 194.9* |
| | (39.98) | (40.57) | (40.53) | | (103.5) |
| Number of credits (at opening) | -106.0*** | -115.6*** | -108.0*** | -108.9*** | -107.9*** |
| | (16.99) | (16.68) | (16.74) | (16.68) | (33.05) |
| Income | | | | | 0.0106*** |
| | | | | | (0.00373) |
| C: 2 A: 12-23 | | | 290.7*** | 275.5*** | 118.5 |
| | | | (37.03) | (37.22) | (103.2) |
| C: 2 A: 24+ | | | 198.6*** | 195.2*** | -79.10 |
| | | | (36.45) | (36.94) | (100.4) |
| C: T A: 6-11 | | | -331.5*** | -271.9*** | -444.3*** |
| | | | (33.71) | (36.57) | (100.6) |
| C: T A: 12-23 | | | -314.3*** | -266.6*** | -331.1*** |
| | | | (32.87) | (35.74) | (96.78) |
| C: T A: 24+ | | | -363.5*** | -279.6*** | -469.3*** |
| | | | (33.84) | (37.72) | (105.3) |
| C: NM A: 6-11 | | | -276.4*** | -333.0*** | -139.6 |
| | | | (49.40) | (49.59) | (128.2) |
| C: NM A: 12-23 | | | 329.6*** | 273.5*** | 259.7** |
| | | | (40.08) | (40.80) | (112.0) |
| C: NM A: 24+ | | | 201.2*** | 163.0*** | -47.39 |
| | | | (36.64) | (38.11) | (98.02) |
| Debt March07 | | | | 0.0312** | 0.0670 |
| | | | | (0.0133) | (0.0422) |
| Purchases March07 | | | | -0.0893*** | -0.0718** |
| | | | | (0.0215) | (0.0321) |
| Payment March07 | | | | -0.0412*** | -0.0178 |
| | | | | (0.0139) | (0.0233) |
| Constant | 767.1*** | 790.3*** | 619.3*** | 666.5*** | 625.9*** |
| | (40.14) | (39.69) | (51.89) | (51.48) | (163.6) |
| Age (Quintiles) | YES | YES | YES | YES | YES |
| Bureau Tenure (Quintiles) | YES | YES | YES | YES | YES |
| Absorbing by Zip Code | NO | YES | YES | YES | YES |
| Observations | 138429 | 138429 | 138429 | 138063 | 23358 |
| R-squared | **0.00243** | **0.174** | **0.178** | **0.181** | **0.453** |
| Adjusted R-squared | - | **0.0973** | **0.102** | **0.104** | **0.269** |

*Notes:* Table reports different specifications of a regression of the net present value of revenue. The purpose of the regression is to show that it is very difficult to predict revenue solely on strata information; moreover, adding demographic information and post-application information (e.g. debt, payment, purchases and credit score) does not improves prediction accuracy. Probability weights are used according to OA.3 and robust standard errors are shown in parenthesis. ***, ** and * show statistical significance at the 1, 5 and 10 percent, respectively.
The correlation between predicted values and observed values for the out of sample is 0.0410.

**Table OA.17:** US-Mexico Comparative

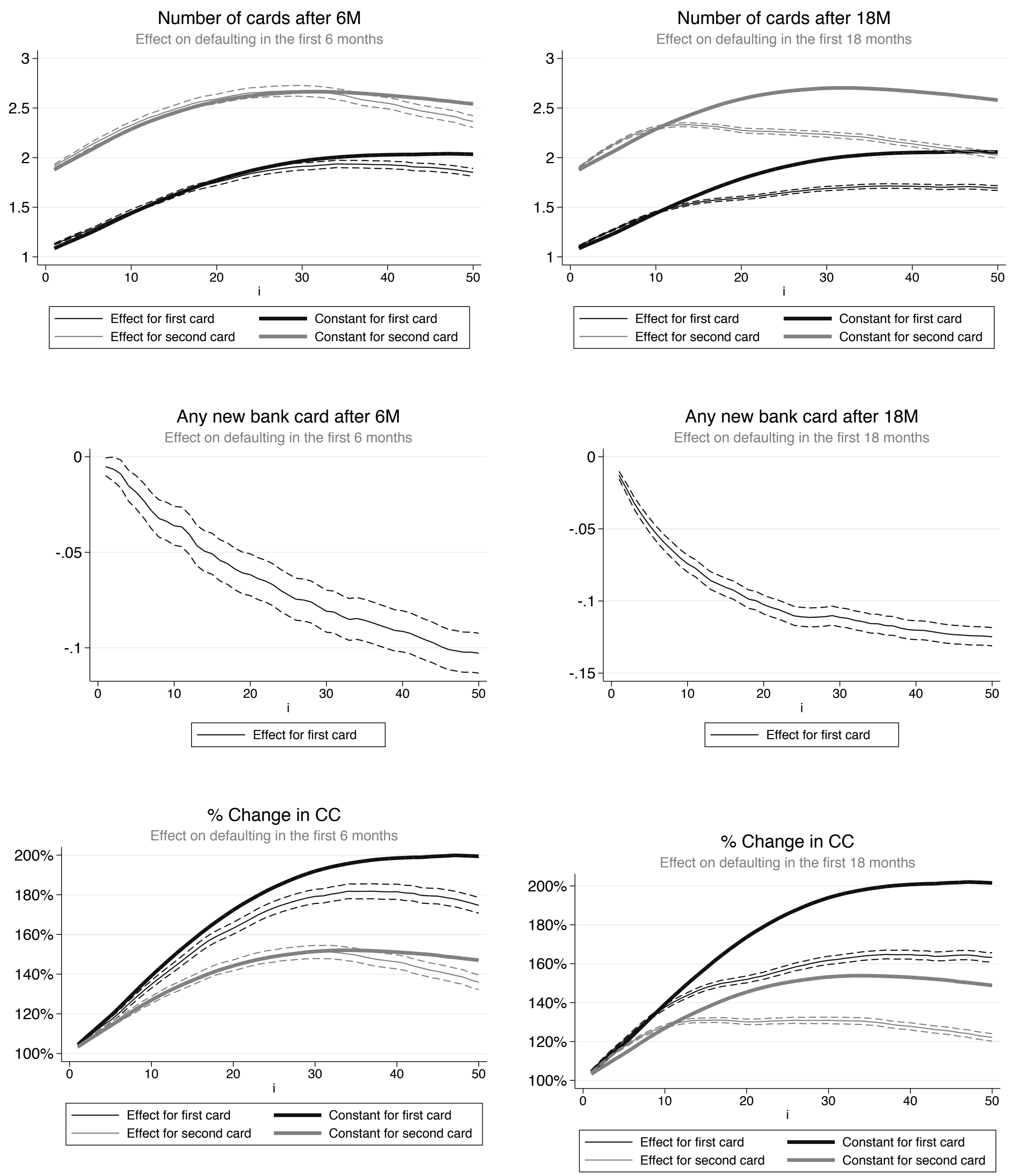
| | Mexico | US | Sources |
|---|---|---|---|
| Number of Issuers | 18 | Thousands | Mexico: Banxico, Reporte sobre las condiciones de competencia en el mercado de emisión de tarjetas de crédito 2013. US: http://www.creditcards.com/credit-card-news/credit-card-industry-facts-personal-debt-statistics-1276.php |
| Share of inhabitants with credit card | 8.1% | 56.4% | Mexico: number of cards from Mexico: Banxico, Indicadores basicos de tarjetas de credito. Population from the Mexican Census. US: http://www.statista.com/statistics/245372/number-of-cardholders-by-credit-card-type/ |
| % of consumers with at least one card | About 30% | 72.2% | Mexico: based on back of the envelope calculations. US: The 2009 Survey of Consumer Payment Choice," Federal Reserve Bank of Boston; published in 2011 |
| Average outstanding balance | 850 usd | 2,755 usd | From the Fed's G.19 release (http://www.federalreserve.gov/releases/g19/current/), based on outstanding balance. Per adult 4800, according to creditcards.com |
| Share non performing | 16% | 3.8% | Mexico: "Tasa de deterioro ajustada", from Mexican banking comission CNBV (http://portafoliodeinformacion.cnbv.gob.mx/bm1/Paginas/infosituacion.aspx.). US: "credit card charge-off rate" of to 100 banks 2012, from (https://research.stlouisfed.org/fred2/graph/?s[1][id]=CORCCT100S). The numbers are hard to compare due to different definitions, so care should be taken. |
| Market share of top 5 issuers | 89% | 62% | Mexico: Banking comission data CNBV. US: creditcards.com |
| Average number of CCs per cardholder | 4.2% | 3.7% | Mexico: Banxico, Reporte sobre el Sistema Financiero," Technical Report, Banco de Mexico 2006., US: "The 2009 Survey of Consumer Payment Choice," Federal Reserve Bank of Boston; published in 2011 |
| APR | 24% | 12.7% | Mexico: Banxico, Indicadores basicos de tarjetas de credito 2012., US: Fed's G.19 release. |

**Table OA.18:** Price comparison between cards' store and other supermarket stores

| | All foods | | | | Basic Basket | | | |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| walmart | 0.0483*** | 0.0441*** | 0.0439*** | 0.0404*** | 0.0406*** | 0.0390*** | 0.0364*** | 0.0348*** |
| | (0.0007) | (0.0011) | (0.0007) | (0.0011) | (0.0010) | (0.0016) | (0.0010) | (0.0015) |
| C | | -0.0081*** | | -0.0057*** | | -0.0048*** | | -0.0034*** |
| | | (0.0003) | | (0.0003) | | (0.0003) | | (0.0003) |
| DE | | -0.0172*** | | -0.0154*** | | -0.0126*** | | -0.0127*** |
| | | (0.0003) | | (0.0003) | | (0.0003) | | (0.0003) |
| 1.walmart*C | | 0.0023 | | 0.0017 | | -0.0013 | | -0.0017 |
| | | (0.0014) | | (0.0014) | | (0.0021) | | (0.0020) |
| 1.walmart*DE | | 0.0038* | | 0.0055*** | | 0.0013 | | 0.0044 |
| | | (0.0020) | | (0.0019) | | (0.0030) | | (0.0029) |
| Linear time trend | 0.0008*** | 0.0008*** | 0.0009*** | 0.0009*** | 0.0011*** | 0.0011*** | 0.0012*** | 0.0012*** |
| | (0.0000) | (0.0000) | (0.0000) | (0.0000) | (0.0000) | (0.0000) | (0.0000) | (0.0000) |
| Constant | 2.5340*** | 2.5438*** | 2.5352*** | 2.5433*** | 2.5377*** | 2.5445*** | 2.5390*** | 2.5451*** |
| | (0.0007) | (0.0007) | (0.0007) | (0.0007) | (0.0008) | (0.0009) | (0.0008) | (0.0008) |
| Week dummies | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| SKU dummies | Yes | Yes | No | No | Yes | Yes | No | No |
| SKU x City dummies | No | No | Yes | Yes | No | No | Yes | Yes |
| Obs | 16,479,010 | 16,477,410 | 16,478,860 | 16,477,260 | 8,345,255 | 8,344,409 | 8,345,255 | 8,344,409 |
| R-squared | 0.71 | 0.71 | 0.76 | 0.76 | 0.74 | 0.74 | 0.78 | 0.78 |

*Notes:*

**Figure OA.13:** Effects of default on new CC activations




*Note:* The graphs show the effect early default has on new credit cards in the future.

**Table OA.19:** Credit limit regressions

| | Card Limit | |
|---|---|---|
| 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) |
| Observations | 3201085 | 3201085 |
| p-value Treatments | 0.438 | 0.486 |
| p-value Strata | 0.000 | 0.000 |
| R-squared | 0.021 | 0.030 |
| Dependent Variable Mean | 11157.089 | 11157.089 |
| Month dummies | No | Yes |

*Notes:* Each column represents a different regression which estimates limit on a certain month for a certain person only using the segment. We have one version with month dummies.

**Figure OA.14:** Limits by month per segment