# Expanding Financial Access Via Credit Cards: Evidence from Mexico*

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**Abstract**

Credit card debt is increasingly common among poor and inexperienced borrowers – thus *de facto* a financial inclusion product. However, it remains relatively under-studied. We use detailed card level data and a product that accounted for 15% of all first-time formal loans in Mexico and show that default rates are high and ex-ante unpredictable for new borrowers – suggesting an important role for ex-post contract terms in limiting risk. However, using a large nation-wide experiment we find that default is unresponsive to minimum payment increases, a commonly proposed policy remedy. We provide evidence that the zero result is driven by the offsetting effects of tightened liquidity constraints and lower debt burdens. Surprisingly, we also find muted default responses to large experimental changes in interest rates – suggesting a limited role for ex-post moral hazard in our context. Finally, we use job displacements to document large effects of unemployment on default, highlighting the centrality of idiosyncratic shocks as a barrier to the expansion of formal credit among poorer populations.

**Keywords:** Financial Inclusion, Credit cards, Default risk, Mexico.
**JEL:** O16, G21, D14, D82.

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# 1 Introduction

Credit card borrowing is increasingly common in many developing countries (see Figure 1) and is often the first formal sector loan product for borrowers. For instance, in Mexico (our country of focus) it was the first loan type for 74% of all formal sector borrowers (and comprises over half of all formal sector loans). In Peru and Colombia, credit cards were the first formal loan for 83% and 51% of all formal borrowers respectively. Even in the United States, credit cards are the most common way to enter the formal credit sector, comprising about half of all first time formal loans.[1] Credit card use has been expanding to low income populations (see Figure 2(a) for Mexico), and this expansion of borrowing among often poor and inexperienced borrowers has in turn led to increased policy attention and regulation on key contract terms aimed at limiting (what is seen as) excessive debt and default risk.[2]

Despite its increasing role in deepening financial inclusion and increased policy attention and regulation, credit card borrowing in developing countries remains relatively understudied — particularly relative to other recent approaches to expanding credit access, e.g. micro-finance. As context, there were approximately 2.3 million micro-finance clients in Mexico in 2009, while the single credit card we study, targeted at borrowers with non-existent or limited credit histories, taken alone had 1.3 million customers at the time (Pedroza, 2010, and authors' calculations.) Thus, relative to their importance as an engine of credit expansion, credit cards have received relatively little attention from development economists.

In this paper, we study credit card lending to borrowers with limited credit histories in Mexico. To do so, we use detailed individual-level loan data, and a large-scale country-level randomized experiment on new borrowers (we will often use the abbreviation NTB borrowers to denote such new to banking borrowers). The experiment focuses on a specific card (henceforth the study card) targeted at NTB borrowers issued by one of the country's largest commercial banks (Bank A from now on). In 2010, the study card was a flagship financial inclusion product in Mexico and accounted for approximately 15% of *all* first-time formal sector loan products nationwide (Figure 2b). We use this experiment as a window into the challenges of financial inclusion through credit cards.

We begin by establishing two central facts about NTB credit card borrowers. First, NTB borrowers have high default rates — about 19% of our sample of Bank A borrowers defaulted on the study card over the 26 month study period. Newer borrowers are much riskier: default is

[1]The figures for Mexico are from authors' calculations. The figures for Colombia are from Banca de las Oportunidades (2016). The figure for Peru was obtained through Universidad del Pacifico and kindly provided by Mirko Daga. The numbers for the U.S. are from Haughwout et al. (2020). There does not, however, appear to be an internationally comparable database that can be used to examine this at a global level. We provide numbers from all the countries for which we were able to obtain data.

[2]For instance, in Mexico and Taiwan, the state mandates a floor on minimum payments. Singapore mandates both minimum income requirements as well as automatic credit suspension for any borrower not making their minimum payment for 60 days. Several countries mandate transparency in terms and conditions as well as limit the scope of interest rate increases and penalty fees (e.g. the United States and South Korea). See e.g. Nelson (2020); Financial Conduct Authority (2015).

twice as high for borrowers who had been with Bank A for less than a year relative to borrowers who had been with the bank for more than two years. We document similarly high default rates for NTB borrowers in a representative credit bureau sample of 1 millon borrowers, evidence that these default rates are a general feature of lending to NTB borrowers. Second, we use a large set of observables and a range of machine learning methods and find that default is difficult to predict at the time of card issuance. Given this lack of predictability, the role of ex-post contract terms in mitigating risk assumes even greater importance.

We then ask whether default can be reduced by altering two key elements of the credit card contract: minimum payments and interest rates. To our knowledge, this is the first experimental evaluation of changes in minimum payments as well as the first experimental evaluation of joint changes in interest rates and minimum payments. The experiment, carried out by Bank A, covered all 32 Mexican states, (and 1,360 out of 2348 municipalities) and randomly allocated 162,000 pre-existing study card borrowers to 8 treatment arms that varied monthly minimum payments (between $5\%$ and $10\%$) and annual interest rates (between 15%, 25%, 35% and 45%) and a control group. The magnitude of experimental variation as well as the sample sizes are substantial.

We study the 26 months of the experimental period from March 2007 to May 2009. In addition, we also follow borrowers and examine outcomes three years after the experiment ended (June 2012). The sampling scheme ensures that the experimental results are representative of the bank's national population of study card customers (about 1.3 million at the start of the study) — all borrowers had signed up for the study card under the status quo contract terms (minimum payment of $4\%$ and an annual interest rate of $55\%$), so our results are conditional on this initial selection.

We first examine the effects of the experimentally induced changes in minimum payments. Policy-makers, worried that low minimum payments could lead to excessive borrowing and increased default with negative consequences for both borrowers and the financial system, have advocated raising minimum payments.[3] As a consequence of this global policy debate, the Mexican Central Bank mandated a floor for minimum payments in 2010. Higher minimum payments, however, could have two opposing effects and it is not *apriori* clear which one will dominate. On the one hand, ceteris paribus, higher minimum payments reduce debt, easing the repayment burden, thereby reducing default. On the other hand, higher minimum payments reduce effective borrowing possibilities, tighten short run liquidity constraints, which increases default. This trade-off has been explicitly recognized in policy discussions,[4] but there is, hitherto, no evidence

[3]See e.g. Warren (2007); Bar-Gill (2003). In the United States policy makers have evaluated this possibility given that many minimum payments do not cover the finance charges (`https://goo.gl/X8ujTi`). For a detailed description of the global attention to such concerns and the various policy prescriptions around it, see Financial Conduct Authority (2015). Such prescriptions find some support in models of time-inconsistent or unaware agents (Heidhues and Kőszegi, 2010; Heidhues and Kőszegi, 2016; DellaVigna and Malmendier, 2004; Gabaix and Laibson, 2006). There is some evidence that time inconsistent preferences play a role in credit card debt accumulation (Meier and Sprenger, 2010; Laibson et al., 2003; Shui and Ausubel, 2005) and that minimum payments serve as an anchoring device (Stewart, 2009; Keys and Wang, 2019).

[4] For instance, the United Kingdom's Financial Conduct Authority notes that "The [increase in] minimum payment requirement [attempts to] ensure consumers pay off credit card debts faster, incurring less cumulative debt in the long run. It is plausible that such a measure could increase default rates in the short run, as consumers are forced to pay off bigger proportions of their balance up front." Financial Conduct Authority (2015).

on the matter. The liquidity effect may be particularly relevant for poorer borrowers – at the start of the experiment for instance, $73\%$ of borrowers' monthly payments were less than $10$ percent of the total debt.

Our first finding is that doubling the minimum payment (from 5 to 10 percent of total debt) had no effect on default over the 26 month study period. The intervention also increased card cancellations and reduced (a proxy for) bank revenues suggesting, if anything, that both borrowers and the bank were hurt by the change. This provides sobering evidence on the effectiveness of lowering default through increased minimum payments even over a relatively long period. We next attempt to unpack the null effect of the increased minimum payments by examining behavior in June 2012, three years after the experiment ended. After the experiment ended in May 2009, the second effect (tightened contemporaneous liquidity constraints) was no longer operational as all subjects were returned to a common minimum payment (4%). However, because debt was lower at the end of the experiment in the higher minimum payment arms, the repayment burden effect should be smaller in these arms. Our second finding verifies this. In stark contrast to the null effect in the short run where the opposing effects likely operated, the effect of the higher minimum payments on default was substantively (and statistically) significantly stronger in June 2012 than in May 2009. It seems reasonable to assume that during the experiment the two countervailing effects cancelled each other out, highlighting the double-edged nature of increasing minimum payments as a policy tool to limit default.

Turning to interest rates, our third finding is that reducing the interest rate from $45\%$ to $15\%$ reduced default by $2.6$ percentage points (on a base rate of $19$ percent) over the 26 month experiment. The implied elasticity is a relatively small: $+0.20$.[5] This is somewhat surprising since a positive correlation between default and interest rates (conditional on selection) is often interpreted as a measure of moral hazard[6] and our apriori expectation was that moral hazard would be a significant contributor to borrower default for NTB borrowers. This finding is potentially important since it suggests that (conditional on selection) asymmetric information in the form of moral hazard may not be a prime determinant of default in our NTB population. In the same vein, our final experimental finding, taking advantage of the stratified experimental design, is that default elasticities are increasing in tenure with the bank so that interest rate changes are least effective for those borrowers for whom the asymmetric information problem is likely the most acute (i.e. the newest NTB borrowers).

[5]This contrasts with other work (e.g. Adams et al., 2009) who find that interest rates are an important determinant of default for U.S. auto loans. Our default responsiveness is considerably smaller than the effects on delinquency rates documented in Karlan and Zinman (2017) although the authors do not report effects on default. It is also smaller than the elasticity implied by the Karlan and Zinman (2009) interest rate interventions in South Africa. See Table OA-15 for a comparison table.

[6]"[T]he theory of moral hazard suggests that high interest rates give greater reason to default, for those who have a choice" (Banerjee and Duflo, 2010). A significant literature infers moral hazard from the causal effect of interest rates on default (conditional on the selection) (see e.g. Karlan and Zinman, 2009; Adams et al., 2009). The causal effect on default arises from both a debt or repayment burden effect (higher interest rates mechanically increase the debt burden) as well as a "pure" moral hazard or incentive effect (higher interest rates increase incentives for default independently of debt burden). That the combined effect is small implies that both effects are small. Note that default is always feasible and that there is "space" for responses in both directions as baseline default is 19%.

In the last section we show that default, while unresponsive to large changes in contract terms, is very responsive to changes in employment status. Matching the universe of Mexican Social Security data with credit bureau data, we find first that 37% of our sample experienced at least one separation episode during the study period (of 31 months). Using downsizing (mass layoffs) as a proxy for involuntary separation (see e.g. Jacobson et al., 1993; Couch and Placzek, 2010; Flaaen et al., 2019) we estimate that displacement (defined as separation that occurs as part of a downsizing) leads to a 7 percentage point increase in the probability of default on a formal sector loan in the next year, corresponding to 36% of the mean default rate. These magnitudes are substantial and are consistent with the hypothesis that NTB borrowers are vulnerable to large shocks that precipitate default.[7]

We draw three lessons from these results. First, despite the regulatory emphasis on increasing minimum payments to protect inexperienced borrowers, we find them to be of limited effectiveness in limiting default. Second, ex-post contract terms do little to mitigate risk among our sample of pre-existing NTB borrowers. This is unfortunate since ex-ante screening through credit scoring methods is difficult for new borrowers given limited or non-existent credit histories. Third, the small casual effect of interest rates suggests that moral hazard – interpreted as the causal effect of interest rates on default (conditional on selection) – may be less important than other factors in contributing to default. In particular, large idiosyncratic shocks (here unemployment, though illness could also have similar effects) may be important drivers of default, suggesting a more prominent role for insurance in expanding financial access. These lessons point to difficulties in expanding credit to NTB borrowers using credit cards – in fact, after nearly a decade of expending considerable effort and resources, Bank A stopped issuing the study card entirely by 2010 and moved away from lending to NTB borrowers.

This paper connects with several strands in the literature on credit markets. First, a recent literature identifies lack of access to formal financial services as a general problem in developing countries and advocates supply-side interventions aimed at increasing financial inclusion – that is the creation of broad-based access to financial services, particularly for poor and disadvantaged populations.[8] We provide a detailed empirical analysis of the difficulties involved in expanding formal credit access through an increasingly commonly used – though under-studied – product as well as causal evidence on the effectiveness of standard policy tools at mitigating default risk. Our work also adds to an earlier literature that critiques institutional, typically state-led and agricultural, lending to the poor (see e.g Adams et al., 1984). In this literature, limited formal private sector engagement with poor borrowers is taken as *prima facie* evidence of the inability to do so profitably. Our study provides detailed evidence on a large-scale commercial bank's experience of

[7]Since the social security data only capture formal employment, our results are only about separations in the formal sector. In that sense, the effects are potentially a lower bound since absence from the social security data may not mean unemployment – workers may have found informal employment.

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

lending to poor inexperienced borrowers using an increasingly common credit instrument.

We also contribute to the empirical literature documenting the existence and gravity of informational problems in credit markets.[9] We complement this literature by focusing on a different population (borrowers who are new to formal credit), a different product (credit cards) and different contract terms (minimum payments). In addition, we examine the relative importance of liquidity constraints, repayment burden and "pure" moral hazard over a longer (five year) horizon; our research design also allows us to isolate some effects (e.g the effect of the debt repayment burden). Finally, we believe we are the first to document the effects of formal sector job displacement on default risk for a developing country.

The paper proceeds as follows: Section 2 outlines the various data sets we use and provides basic summary statistics. Section 3 provides relevant institutional context and establishes some facts about financial inclusion and credit in Mexico using a large representative sample of borrowers from the formal credit market and our sample from Bank A. Section 4 describes the experiment while Section 5 reports the effects of the experiment on default, cancellations and bank revenues, the primary outcomes of interest. Section 6 discusses some of the mechanisms driving the treatment effects documented in Section 5, Section 7 estimates the effect of job displacement on default, and Section 8 concludes. Due to space constraints some robustness analyses and secondary figures and tables are reported in the Online Appendices (OA).

## 2 Data, Main Outcomes and Summary Statistics

In this section we outline the data sets used, the primary outcomes considered and summary statistics for our experimental sample.

### 2.1 Data sets

We use six different data sets. The first data set is a monthly individual level administrative panel for a sample of 162,000 cardholders from Bank A. The second data set comes from the Credit Bureau and is an annual panel matched to the sample of 162,000 study card holders. The third group comprises a set of six large representative samples of one million consumers each from the Mexican Credit Bureau that allows us to make population level statements and comparisons. The fourth dataset is the Mexican Social Security data matched to the bank and credit bureau data. The last two data sets are nationally representative surveys (ENIGH, MxFLS). We next describe each in turn. Figure 4 provides a comprehensive view of our main data sets and their measurement dates.

**Bank Data (Experimental Sample) and Study Card:**

*Study Card*: We use detailed data from a large commercial Mexican bank and a product (the study card) that accounted for 15% of first-time loans nation-wide in 2010. The study card is a credit card that can be used at a large set of supermarkets as well as other stores. In 2011, these stores

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

that accounted for $43\%$ of all household expenditures at supermarkets and $16\%$ of all household expenditures in Mexico.[10] The card was specifically targeted at low-income borrowers with no or limited credit histories (internally the bank referred to them as the C, C- and D customer segments). It had an initial credit limit of approximately 7,000 pesos, an annual interest rate of 55 basis points over the base rate and a monthly minimum payment of 4% of the total amount outstanding. The card was initially offered in 2003, and by 2009 Bank A had approximately 1.3 million clients , a substantial financial inclusion effort in a country where there were approximately 11 millions cards at the time.

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

*Variables:* For this sample of 144,000 clients, we have monthly level data on purchases, payments, debt, credit limits, default and cancellations from March 2007 to May 2009. In addition to this detailed transaction information we also observe some basic demographic variables – age, gender, marital status and residential zip code. Finally, we also observe default status for the study sample in June 2012, three years after the end of the experiment.

**Credit Bureau Data (Matched to Experimental Sample):** We were able to match the experimental sample to the credit bureau data once each year (from June 2007 to June 2010) and once more in June 2012. This enables us to observe other formal sector transactions by the experimental sample thereby allowing us to measure effects on non-Bank A related outcomes (e.g. overall debt or overall default). We will refer to this data as the *matched* CB data. We also observe credit score in each of these snapshots, but no information on other interest rates.

**Credit Bureau Data (Representative Cross-Section):** We use six random samples of one million borrowers from the Mexican Credit Bureau (Buró de Crédito) to describe the population of NTB borrowers in the country. We observe six credit bureau representative cross-sections from June

[10]We thank Marco Gonzalez-Navarro for kindly carrying out the calculations using data from Atkin et al. (2018).

2010, June 2011, June 2012, June 2013, December 2013, and March 2014. A borrower appears in the credit bureau if she has or has had a loan with a formal financial intermediary.[11] For each borrower we observe the date of loan initiation, the source and type of loan and her monthly delinquency and default history. We have limited information on total loan amounts and no information on the interest rate, or other contract terms. Unlike the matched CB data, we do not observe credit scores for the borrowers in these random sample snapshots. We also observe some demographics – age, gender, marital status and zip code. We use this information to provide a snapshot of financial inclusion – in particular we describe the characteristics of first-time and recent borrowers, their sources of credit and their repayment history. We will refer to this as the CB data.

**Matched Social Security Data Panel:** We were also able to merge the universe of Mexican formal workers (close to 14 million) from social security agency's records from November 2011 to March 2014 to obtain information on formal employment. We observe the formal sector wage for those employed in the formal sector in this dataset.

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

### 2.2 Main outcomes

**Default.** Default is defined as three consecutive monthly payments that are each less than the minimum payment due. In such instance, the bank revokes the card automatically. Therefore we will refer to default and revocation interchangeably. Default is enormously relevant for policy makers since it can be an important source of financial instability and is particularly worrisome in poor populations. In fact, the experiment described in this paper was driven in part by the Mexican Central Bank's concern over default rates among NTB borrowers. Default is clearly important for banks as well since it directly affects profits. Finally, the literature on credit market imperfections has focused on default in testing for moral hazard.

**Client cancellation.** Cancellation refers to the event when a borrower voluntarily cancels their card. Cancelling a card is an active decision which the client can take after repaying the debt outstanding on the card. Cancellations are of direct interest because they provide some revealed preference evidence on the change in the study card's attractiveness as a result of contract term changes and because they typically reduce bank revenues.

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

**Revenue.** Measures of bank revenues (and profits) from the study card – difficult to observe – are critical for understanding the long term viability of expanding financial access through credit cards. They are also informative about the commercial feasibility of the experimental contract terms. We quantify bank revenue from the study card using the detailed data on purchases, payments and debt over the 26 month experiment.[12] We define revenue for card $i$ as

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

where PV($\cdot$) stands for the present value of the stream of payments inside parentheses that are discounted at the TIIE (the Mexican inter-bank rate).[13] If we observed a card from inception until closure, the exercise above would reduce to subtracting the net present value of payments from the net present value of purchases. Unfortunately, we only observe cards for a 26 month window so have to account for card usage before and after the experiment. We account for pre-study behavior by subtracting the amount due from card $i$ at the start of the experiment (March 2007). We assume that post-study, borrowers make no further purchases and default on their outstanding debt with probability $\phi_i$ in which case the bank recovers $10\%$ of the amount due.[14]

Several features are worth noting. First, this measure of revenue accounts, albeit mechanically, for both default and cancellations. By the same reasoning it incorporates interest and fees. Second, it is not a comprehensive measure of profit since it does not include promotion costs, the cost of the physical card and maintenance or administrative expenses or any income earned by merchant discount fees or interchange fees. Nevertheless, in our estimation, it provides a useful proxy for card-level bank revenue.

Figure OA-10 plot histograms of our revenue measure. The measure shows considerable dispersion — the standard deviation is 7,347 pesos, considerably larger than the mean of 4,197 pesos. Newer borrowers exhibit even greater dispersion. In private conversations, Bank A officials confirmed that average revenue, its dispersion, and its relation to credit scores are reasonable. To assess its reasonableness, we examine correlations with credit scores. Revenue displays an inverted-U pattern with respect to initial credit scores. This inverted-U shaped relationship between bank revenues has been documented in other markets which gives us further confidence in our construct (e.g. Fig. II.E in Agarwal et al. (2015) plots a similar inverted-U relationship using credit card data for the United States).

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

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

[14] For each card $i$, $\phi_i$ is modeled as a function of its credit score using a non-parametric regression of default (during the 26 month window 03/07-05/09) against the credit score in June 2007 for the control group. We then assigned $\phi_i$ based on the estimated regression evaluated at the credit score for $i$ in June 2009 (see figure OA-9). The 10% figure for debt collections is based on conversations with bank officials. The expected fraction (of the amount due) that would be recovered then is given by $\alpha_i \equiv \phi_i \times 0.1 + (1 - \phi_i) \times 1$. Figure OA-11 in the OA shows that our measure of revenue is not particularly sensitive to the choice of $\alpha_i$.

## 2.3 Summary Statistics

Table 1 presents summary statistics of the experimental sample in columns 1-2 and comparisons with samples representative of Mexican borrowers in columns 3–5. The experimental sample is of broad interest in itself since the study card accounted for 15% of all first-time formal sector loan products nation-wide. In Column 3 we use the CB sub-sample that had at least one active credit card in June 2010, making it a nationally representative sample of the population of borrowers with at least one credit card (in 2010). Since our experimental sample is relatively new to formal credit, we attempt to find a comparable group in the CB data by constructing a sample whose credit history duration matches that of the experimental sample. We do this by matching the distribution of the oldest credit entry across the experimental and CB samples. This is the sub-sample for which summary statistics are reported in column 4 and we refer to it as the new (or recent) borrower sample (see Online Appendix Subsection B.2 for details). Finally, in column 5 we consider a sub-sample of experienced borrowers – those with a credit history of at least 8 years (the median) in the CB data.

The experimental sample is just over half male, with an average age of approximately forty, about three-fifths of whom were married at the start of the study (Panel C). Other than marriage rates (which are lower in the CB) the figures are roughly comparable to those of the three CB data sub-samples. Borrowers in the experimental sample are somewhat less well-off relative to the average CB member. For the borrowers that we could match to the social security, average monthly income (in 2011) in the experimental sample is 13,855 pesos compared to an average of 14,759 for recent borrowers and 22,641 for experienced borrowers.[15] Figure OA-7 shows that the distribution of income for the experimental sample is first-order stochastically dominated by corresponding distribution for the CB sub-sample from column 3. Since we were unable to match 82% of the experimental sample with the social security, we concluded that these individuals were in the informal sector with likely lower, less stable incomes.

Credit information also points towards our experimental sample being "marginal". First, the mean credit score of 645 is low in absolute terms – borrowers with scores below 670 are typically ineligible for standard credit card products.[16] Second, the card issued by Bank A was the first formal loan ever product for 47 percent of the sample, and for 57% it was their first card. Third, our study sample was, unsurprisingly, at the low-end of borrowing ability in the CB data. The credit limit for the study card was relatively low at 7,879 pesos and the overall card limit for the experimental sample (across all cards) was 15,776 pesos in 2007, rising to 18,475 pesos by June 2010. For comparison, in 2010 the mean card limit was 49,604 pesos for the CB sub-sample with at

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

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

least one active card, 22,082 pesos for the CB recent borrowers sub-sample and 56,187 pesos for the experienced sub-sample. Fourth, borrowers have high rates of default; 17% of the experimental sample as a whole defaulted on the study card over the course of the experiment (the figure is 19% for the sample we will use as our base comparison group in the experiment). Based on the figures presented above, we conclude that the experimental sample was indeed drawn from a financially fragile population.

# 3 Financial Inclusion with Credit Cards

## 3.1 Commercial Banks Increase Lending to Poorer Individuals

Formal credit penetration is low in Mexico,[17] but has been growing, primarily via large banks using credit cards to serve under-served populations. Figure 2(b) shows that more than 70 percent of first time loans are through a credit card. The corresponding figures for Peru, Colombia and the US are 83%, 51% and about 50% respectively – credit cards are increasingly the most common way for individuals to obtain formal credit. In Mexico, the number of credit cards nationwide grew from 10 million in the first quarter of 2004 to 24.6 million in the last quarter of 2011 with a substantial part of the growth being concentrated among lower income individuals (see Figure 2(a) and Banco de México, 2016) – the study card playing an important role in this expansion accounting for 15% of all first-time formal sector loans in 2010.

This desire to pursue low-income clients appears to have been in part inspired by the success of Banco Compartamos and Banco Azteca.[18] However, both Compartamos and Azteca pursue markedly different strategies than those pursued by Bank A. Compartamos uses joint liability (via group lending) while Azteca requires collateral (typically household durables) – in stark contrast, Bank A uses individual uncollateralized lending. In addition, both lenders expend considerable resources on face-to-face interactions and home visits while Bank A relies on credit scoring and distance monitoring. Finally, Bank A relies on standard debt collection mechanisms (declining to pursue loans defaults that are less than 50,000 pesos) while Azteca and Compartamos pursue debt collection much more vigorously.[19] This approach is costly – both Compartamos and Azteca have higher operating expenses (relative to assets, see Fig OA-8) than Bank A – and potentially limits the scale of financial inclusion.

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

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

[19]Azteca uses "crude collection and repossession mechanisms" (Ruiz, 2013). Ruiz attributes Banco Azteca success to its ability "to leverage its relationship with a large retail chain (Elektra) to reduce transaction costs, acquire effective information and enforce loan repayment."

### 3.2 Regulatory Concerns with Minimum Payments

Regulators in many countries have expressed concerns with credit card terms, particularly for financially inexperienced borrowers. In particular, minimum payments are often held to be too low – i.e. failing to cover even financing charges. Typical minimum payments have decreased from 5% of outstanding debt in the 1970s to 2% in the 2000s (Kim, 2005) – according to Keys and Wang (2019) this was partly a result of issuers lowering minimum payments in order to extend repayment periods. Lower minimum payments are thought to increase indebtedness and eventually default with potentially dire consequences for the financial sector (see e.g. this circular `https://goo.gl/MkYbVO` from the Mexican Central Bank). In addition, there is also evidence of behavioral biases such as borrowers anchoring payments at the minimum payment (Stewart, 2009; Keys and Wang, 2019) which is an additional concern.

Regulators worldwide have noticed and often criticized low minimum payments. In the U.S., the Office of the Comptroller of the Currency issued a guidance on minimum payment that "expect[ed] lenders to require minimum payments to amortize the current balance over a reasonable period of time" OCC (2003). Mexico and Taiwan have been the most forceful, instituting a regulatory floor for minimum payments. Other countries have mandated statement disclosures – that e.g. display the time required to pay off a debt while only making minimum payments (e.g. in the United States and South Korea). The United Kingdom's Financial Conduct Authority discusses the various measures undertaken by regulators in a set of nine countries.[20] Despite these persistent concerns, there is scarce evidence on the causal effects of minimum payments on debt and default.[21] This paper provides the first experimental evidence on this question.

### 3.3 Stylized Facts

We document that (a) NTB borrowers default at high rates and default decreases with formal credit sector tenure and (b) provide suggestive evidence that default and revenue are difficult to predict.

*A. High Default Rates*

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

[20] Financial Conduct Authority (2015). Economists have also shown concern. Keys and Wang (2019) write that "Increasing minimum payments could also help issuers mitigate default risk". They also recognize that mandating minimum payment increases has stark trade-offs: "it may be difficult to help anchoring [their minimum payments] consumers increase their payments without placing some restriction on the choices of liquidity constrained consumers.".

[21] d'Astous and Shore (2017) focus on examining delinquency and default employing using a quasi-experimental design on observational data from the United States.

Default risk, however, is not homogeneous. Figure 3(a) shows that newer borrowers are riskier (blue bars): default rates are $36\%$ (during the experiment for the control group) for borrowers who had been with the bank for less than a year (the "6-11m" stratum) when the study began. The corresponding figure for the borrowers who had been with the bank for more than 2 years when the experiment began are half that rate at 18 percent.[22]

The finding that newer borrowers are riskier also holds for a representative sample of NTB borrowers in the credit bureau. Mimicking the experimental sample, Figure 3(b) examines behavior on the first card obtained by Mexican borrowers during the same period as the experiment. To be comparable with the experiment, we condition on cards that had not been delinquent in the 6 months previous to January 2007. Figure 3(b) plots default in this card in the next 26 months as a function of how long borrowers have had the card. We plot default rates for three different groups: the study card (all cards, not just those in the experiment), all credit cards of Bank A (not just those targeted at NTB borrowers), and all cards of all banks. We see that the study card has default rates twice as high as those of Bank A's other cards – consistent with the study card being a "financial inclusion" product. Second, overall default rates for Bank A are similar to those at other banks, suggesting our partner bank is comparable to other banks in this respect. Finally, the negative slope shows that, across all banks, newer borrowers are more likely to default – for instance, borrowers with a card tenure of less than 3 months are twice as risky as borrowers with a card tenure of 3 years. Serving new borrowers – financial inclusion – is evidently risky.

### *B. Default and Revenue are Difficult to Predict for NTB Borrowers*

Large banks rely heavily on credit scoring and predictive modeling to screen consumers. In other contexts such methods have led to significant credit expansions.[23] It is unclear whether such an approach is suitable for financial inclusion as NTB borrowers by definition have little or no credit information with which to estimate such models. For instance, at the moment of application, 57 percent of the study card borrowers did not have any credit cards and 47 percent did not have any other formal sector loan.

We find that default is difficult to predict in our data even using state-of-the-art predictive methods.[24] We use the area under the receiver-operating characteristic curve (AUC–ROC) as our measure of predictive power for default. This measure summarizes the predictive ability of the model to separate defaulters (true positives) and non-defaulters (true negatives) and goes from 0.5 (worst) to 1 (best). We report an AUC of 0.79 for our best-performing model (see Appendix G.). For comparison, this is lower than those found for credit cards in the US (Khandani et al., 2010, report AUCs in the 0.89-0.95 range) and for other loans (Ala'Raj and Abbod, 2016, reports

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

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

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

AUCs of 0.80, 0.94, 0.93, 0.77 and 0.84 for loan data from Germany, Australia, Japan, Iran, and Poland, respectively), and lower than what Fuster et al. (2017) reports for US mortgage data. This comparison highlights the difficulty of predicting default among our NTB borrowers.

We, likewise, find that predicting revenue is extremely difficult in our NTB sample using credit bureau information available at the time of application. Table OA-6 show a wide array of predictive performance measures for a selected subset of prediction models with increasingly finer information sets. In terms of revenue, using the information available to the bank at the moment of application, the correlation ($\rho$) between the predicted (using random forests — our best model) and realized revenue is 0.28, with a substantial root mean square error of 7,204 pesos (mean revenue is 4,197 pesos). Using post-approval information, and even including the credit score does not yield much better results. Part of the difficulty in predicting revenue stems from the difficulty of predicting default ($\rho$=0.45), voluntary cancellations by clients ($\rho$=0.15), and interest accrual ($\rho$=0.44), all of which affect revenue.

# 4 Using Ex-Post Contract Terms to Limit Default

The previous section documented high rates of card exit and the difficulty of predicting default and revenue using borrower observables (even among a positively selected pool). This limited ability to screen borrowers ex-ante increases the importance of ex-post measures such as contract term adjustments – the most important being the minimum payment, the interest rate and the credit limit – in limiting default and maximizing profits.

Whether and to what extent such variation in contract terms can mitigate default for NTB borrowers, and its implications for bank profits are open empirical questions. Bank A's large experimental variation in interest rates and minimum payments for NTB borrowers (designed independently of the researchers) allows us to answer these questions.[25]

## 4.1 Experiment Description

*A. Sample Selection*

As outlined in Section 2.1, the sample frame consisted of all study card borrowers who had paid at least the minimum amount due in each of the six months prior to January 2007. The bank divided this sample frame of more than one million study card clients into nine different strata based on two pre-intervention characteristics which were used internally as default predictors: the length of tenure with the bank, and repayment history over the past 12 months (both measured in January 2007).[26] Each borrower was classified into one of three categories of tenure with the bank: (a) a long term customer who had been with the bank for more than 2 years, (b) a medium term customer

[25]Bank A did not experimentally vary credit limits. Aydin (2018) finds that (experimental) changes in credit limits have no effect on card default.

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

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

### *B. Experimental Design*

Within each stratum, the bank randomly allocated 2,000 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 monthly payment which is expressed as a fraction of amount outstanding (debt) on the card, and (b) the interest rate on the amount outstanding. The minimum payment was set at either $5\%$ or $10\%$. The interest rate could take on one of four values: $15\%, 25\%, 35\%$ or $45\%$. The interest rate for the study card prior to the study was approximately about $55\%$ so all the experimental interest rates are reductions relative to the status quo (as in Karlan and Zinman, 2009). The new interest rate applied to all new debt incurred going forward (borrowers were not informed about any duration for the contract changes, see below) as well as to debt outstanding. The variation thus includes a forward-looking component since it applied to all future debt as well as a current component since it applied to currently existing debt. Thus we are unable to isolate the dynamic repayment incentive aspect of moral hazard (unlike Karlan and Zinman, 2009, who can vary interest rate on future debt while holding constant the current debt burden).

The two different minimum payments and four different interest rates yield 8 unique contract terms. The experimental design thus identifies for each outcome and for each month $8$ treatment effects within each of $9$ different strata. In addition 2,000 customers within each stratum also served as a control group whose contract terms did not change during the period of the experiment. The minimum payment for the control arm was $4\%$ but the interest rate varied across clients and, unfortunately, we do not observe this rate in our data. Consequently, we do not use the control group as a contrast in most of the analysis below and are explicit in the sequel about which arm serves as the reference or comparison group. In most cases we use the $5\%$ minimum payment and the $45\%$ interest rate group (abbreviated to $(45, 5)$) as the comparison group and we often refer to it as the base arm or base group. Panel A of Table OA-10 in the Online Appendix tests the randomization procedure and shows that treatment assignment is uncorrelated with baseline observables for the initial sample and for the sample of non-attriters.

Figure 4 shows the time-line of the experiment as well as measurement dates. Each study client was sent a letter in March 2007 stating the new set of contract terms that would be in force starting in April 2007. Clients were not informed about the study or of any time-line for when the new contract terms would change. The measurement of experimental outcomes began in March 2007 and lasted until May 2009. During this period the interest rate and the minimum payment were kept fixed at their experimentally assigned levels. The experimental terms were not revealed to the risk department in charge of deciding credit limits. We cannot reject the null of no differences in credit limits across treatment arms at baseline and end-line (Table OA-11 and Figure OA-17). The experiment ended in May 2009 at which point all treatment arm participants received a letter setting out their new contract terms. These terms were the standard conditions with an interest rate of approximately $55\%$ and a minimum payment of $4\%$.

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

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

where $Y_i$ the outcome of interest (default, cancellation or bank revenues) for borrower $i$ and $\{T_{ji}\}_{j=1}^{7}$ are a set of treatment indicators – the excluded group is the (45,5) treatment arm. The strata fixed effects, $\delta_s$, are included in a way that allows us to interpret $\beta_0$ as the base group mean.[27] We use stratum weights (see Table OA-9) in all regressions.

## 5 Experimental Effects on Default, Cancellations and Revenues

### 5.1 Default

*A. Minimum Payment Increases Do Not Reduce Default*

Default is defined as a binary variable equal to one if a borrower defaults at some point during the 26 month experiment and zero otherwise. In contrast to the literature on interest rates and default, there is relatively limited empirical work on the relationship between minimum payments and default. However, minimum payments have received substantial attention in policy circles as a regulatory tool to protect consumers from default driven by over-indebtedness. We view minimum payments as having two primary effects: (a) a tightening of liquidity constraints and (b) a reduction in debt over the longer term. These will be a useful lens through which to interpret our experimental findings.

The experiment doubled the minimum payment from $5\%$ to $10\%$. This was a large and significant change since $73\%$ of borrowers paid less than $10\%$ of the amount due before the experiment began (see figure OA-16). Table 2 shows that, perhaps surprisingly in the light of this figure, the

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

minimum payment increase had no effect on default measured at the end of the 26 month experiment – the point estimate is a statistically insignificant increase of $0.5$ percentage points on a base default rate of $19\%$ for an elasticity of $+0.02$.[28] Figure OA-18 examines the evolution of treatment effects by month finds muted dynamics. To our knowledge this is the first experimentally estimated effect of minimum payments on default. See Table OA-15 for a comparison table.

The effects in the other arms are all broadly comparable, with the estimated elasticities ranging from $-0.01$ to $+0.08$. Interestingly, the elasticities are smaller for newer borrowers (a statistically insignificant $+0.03$) relative to older borrowers for whom the corresponding elasticity is substantially larger ($+0.10$) and also statistically significant. Thus, it is precisely those borrowers for whom the asymmetric information problem is most dire that are most unresponsive to contract term changes.

The overall effects are small, particularly relative to the policy attention paid to increasing minimum payments as a means of limiting default. A key, albeit implicit, component of the policy argument is that increasing minimum payments should decrease debt which in turn should reduce default. As we show in Appendix J.3, the first part of the argument is true – debt does decline, though it is imprecisely estimated. However, any such reductions did not translate into lower default. In Section 6.1 we present some evidence that the null effect is due to two countervailing forces (lower debt repayment burden versus tighter liquidity constraints) off-setting each other.

*B. Interest Rate Changes Have Small Effects on Default*

Table 2 shows that reducing the interest rate to a third of the base group rate (i.e. from $45\%$ to $15\%$) reduced default by approximately two and a half percentage points over 26 months (compared to the $19\%$ default rate for the base arm). The implied elasticity of default with respect to the interest rate is $+0.20$. This is considerably lower than the delinquency elasticity of $1.8$ in Karlan and Zinman (2017) and also somewhat lower than, though broadly in line with, the default elasticity of $0.27$ in Karlan and Zinman (2009) (see Table OA-15 for a comparison to other effects in the literature). The treatment effects for the other intermediate treatment arms are also similarly small. The results for the comparisons between the $(45, 10)$ and the $(r, 10)$ arms are even more stark with none of the estimated treatment effects being statistically different from zero. Figure OA-18 also shows zero effects in most periods.

Interestingly, again these effects are smaller for newer borrowers. For borrowers who had been with Bank A for less than one year (as of January 2007) the elasticity is $+0.08$ while the corresponding elasticity for borrowers who had been with the bank for more than two years (as of January 2007) is $+0.30$, nearly four times as large.

Our results on the unresponsiveness of new borrower default are important for at least two reasons. First, they show that the standard tools used by large banks have smaller effects on NTB

[28]The results are lower than those for delinquency in Keys and Wang (2019) but of the same order of magnitude as those for default documented by d'Astous and Shore (2017). The latter document that an increase in minimum payments of 2% on average over a base-rate of 3% increased default rates by $4\%$ over two years (which implies an elasticity of .06).

borrower behavior than typically assumed in policy discussions. Second, the inability to control default reduces the profitability of new borrowers, making financial inclusion harder.

### *C. A Moral Hazard Interpretation of Default Responses*

A substantial literature in credit markets infers moral hazard from the causal effect of interest rates on default, conditional on the selection of borrowers (see e.g. Karlan and Zinman, 2009; Adams et al., 2008). Standard models in this literature have borrowers deciding how much costly, but unobservable (to the lender), effort to exert (see e.g. Stiglitz and Weiss, 1981; Bizer and DiMarzo, 1992). Lower effort hurts the lender (through higher default). Holding debt constant, increases in interest rates decrease incentives to exert effort since the lender captures part of the returns. Moral hazard is important if effort is responsive to this incentive. A broad class of models predict that (randomized) increases in interest rates will causally increase default; therefore that absence of default changes (in response to exogenous interest rate changes) implies lack of moral hazard.[29] It could, however, be the case that moral hazard is not manifested in default but rather in other outcomes. However, default has been the main focus of this literature (partly because it is easily observable) and we follow the literature in this respect.

The literature distinguishes between at least three channels in understanding the effect of exogenously varying interest rates on default: (a) the "debt burden" channel describes the fact that higher interest rates increase debt mechanically, and this makes repayment harder; (b) the "pure current incentive effect", which we call concurrent moral hazard, viz. the incentive effect of higher current interest rates on default (holding debt constant); (c) the "pure future incentive effect", or dynamic moral hazard, arises if *future* interest rates from the lender are higher (while holding current debt and interest rates constant).[30] In our case, the interest rate changes apply on all current debt as well as on future debt for the foreseeable future (i.e. for all future debt). Therefore, a muted default response implies that the contributions from all three channels are correspondingly small.

The large experimental variation in interest rates, together with the fact that they applied to a sample of pre-existing clients (i.e. those that had already selected into the product) permits a clean test for the presence of moral hazard. Moreover the large sample sizes and stratified randomization ensure that the tests are well-powered to study borrowers new to formal credit. The setting complements that in Karlan and Zinman (2009) who focus on repeat borrowers, while we can also examine first-time borrowers for whom asymmetric information problems may be the most acute. We find that even for NTB borrowers, and even though baseline default rates are high, it appears that moral hazard is not an important determinant of default for the range of interest rate variation

[29] This is usually the key empirical test for the presence of moral hazard. For example Karlan and Zinman (2007a) write that they "identify any effect of repayment burden (which includes moral hazard) by ... comparing the repayment behavior of those who received the high contract interest rate with those who received the low contract interest rate. These borrowers selected in identically, but ultimately received randomly different interest rates on their contract. Any difference in default comes from the resulting repayment burden."

[30] Karlan and Zinman (2009) find evidence for channel (c) but not for (b): "Moral hazard appears to work in different directions on contemporaneous loan prices (where we find that lower interest rates do not generally improve repayment) and future loan prices (where we find the lower interest rates substantially improve repayment on current loans)".

considered here. Section 7 shows that, by contrast, job displacement is a stronger determinant of loan default.

### 5.2 Card Cancellations

Cancellation is a binary variable that equals one if a borrower voluntarily cancels her card at some point during the 26 month experiment and Table 2 shows that cancellations in the base arm $(45, 5)$ were $13.4\%$ over the 26 month period of the experiment. Cancelling a card is an active decision which the client can take after repaying the debt outstanding on the card. Cancellations are of direct interest because they provide direct evidence on the (change in) the study card's attractiveness as a result of contract term changes and because they typically reduce bank revenues, thereby making financial inclusion harder.

*A. Higher Minimum Payments Increase Cancellations*

Doubling the minimum payment increased cancellations (over the experiment duration) by a statistically significant $1.7$ percentage points, for an implied elasticity of $+0.12$ (Table 2). The estimated treatment effects for the other arms are all roughly comparable with elasticities ranging from $+0.12$ to $+0.24$. As noted previously, the increase in minimum payments tightens short run liquidity constraints – decreasing the card's attractiveness and likely then leading to increased cancellations.

*B. Lower Interest Rates Reduce Cancellations*

Reducing the interest rate to $15\%$ decreases cancellations by a statistically significant $3.5$ percentage points, for an implied elasticity of $+0.39$. The reduction in interest rates made the study card unambiguously more attractive and it is not surprising then that fewer borrowers chose to cancel. This behavior is consistent with consumers engaging in some search or at least being open to outside options.[31] Treatment effects from the other arms provide broadly comparable results as do the month-by-month treatment effects presented in Figure OA-18.

To summarize – the results from all the experimental contrasts show that interest rate changes have a robust moderate effect on card cancellations.[32] This larger response allows us to rule out inattention as a cause for the limited effects on default. Finally, from the bank's perspective, the benefits from these decreased cancellations need to be compared against the revenue losses from lowering interest rates, which we discuss below.

[31] We have some suggestive evidence that cancellations are followed by account openings elsewhere. Figure OA-15(a) plots a non-parametric regression of voluntary client cancellation between June 2008 and May 2009 against the change in credit scores in the preceding twelve months. Increases in credit scores are associated with higher borrower initiated cancellations. Virtually none of the borrowers that experience a decrease of 100 points in the credit score cancel, whereas more than 8 percent of those that experience a 50 point increase cancel the study card. Figure OA-15(b) plots non-parametric regressions of new card acquisitions between June 2008 and May 2009 against the same regressor as in Figure OA-15(a). We see that increases in credit scores are associated with increased card acquisition by NTB borrowers: 22% of borrowers with a 50 point increase in credit scores acquire at least one more card in the following year.

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

### 5.3 Effects on Bank Revenues

Changes in interest rates and minimum payments have implications for profits and it is importance to consider these consequences when evaluating the impact of regulations such as minimum payment floors or interest rate caps. We evaluate the effects of the interventions on the revenue proxy described above. We find that all departures from the $(45, 5)$ arm reduced bank revenues. Unsurprisingly, this suggests that changes in contract terms away from the standard terms – e.g. as a result of regulations – might reduce profits (although one would need to consider broader costs measures and market-level considerations in a fuller analysis).

*A. Lower Interest Rates Reduce Revenues*

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

In the Appendix (sections J.2,J.4, and J.6) we document the effects of the intervention on three proximate determinants of revenues – purchases, payments and debt. Surprisingly, debt *declined* in response to the interest rate decrease (the Lee bounds for the elasticity are $[+0.34, +0, 74]$).[33] Together with the small effects on borrower behavior (i.e. limited changes in purchases and payments) the result that debt is larger in higher interest arms suggest that NTB borrowers had limited substitution possibilities and that interest rate compounding is the dominant component of debt changes.

*B. Higher Minimum Payments Reduce Revenues*

Higher minimum payments also reduced bank revenues which are $469$ pesos lower in the $(45, 10)$ arm compared to the $(45, 5)$ arm, with an implied elasticity is $-0.16$ – the proximate mechanism appears to be lower debt and greater cancellations.[34] These results are consistent with the policy assumption that lower minimum payments increase bank revenues.

The revenue findings emphasizes another problem with using higher minimum payments as a policy lever – higher minimum payments reduce bank revenues. Thus, collating the results from the previous sections, we find that higher minimum payments increased cancellations (perhaps lowering consumer welfare), had no effects on default and lowered bank revenues.

### 5.4 Spillover Effects

The large variation in contract terms could also have affected behavior with other lenders. For instance, higher minimum payments could have led borrowers to substitute away towards

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

[34] The point estimate of effect on debt shows a decrease of about one-third though the the Lee bounds for the elasticity are quite wide – $[-0.46, +0.15]$. In the appendix (J.5, J.7 and J.3) we show that the minimum payment increase led to modest increases in purchases and payments (the Lee bounds are $[+0.18, +0.85]$ and $[+.01, +.48]$ respectively).

other lenders. In the Appendix (Table OA-16) we use the credit bureau data to examine whether the experimental changes in the study card's contract terms affected borrowing with other formal lenders. We find that neither default, cancellations, nor new borrowing, (all measured in June 2009) with all other formal lenders respond to the changes in minimum payments and interest rates in the study card. In addition, we find no evidence of crowd-out (or crowd-in) of borrowing, either along the extensive or intensive margin, from other lenders. This is true both during the experiment and three years after it ended. Studying group lending micro finance products, Karlan and Zinman (2017) and Angelucci et al. (2015) find similar results.

### 5.5 Bank A Discontinues the Study Card

In addition to the the limitations of ex-ante screening NTB borrowers documented in Section 3.3, the previous results document that even substantial changes in contract terms were relatively ineffective at reducing default among pre-existing borrowers. Viewed in this light, it is perhaps unsurprising that Bank A subsequently reduced its interactions with NTB borrowers. Figure 2(c) shows the trend in both the current stock of and new issues of the study card. After issuing the study card in substantial quantities for several years, the bank stopped issuing new cards in 2009 and by 2013 there were no borrowers with the study card in the credit bureau. The closing of the card appears to have had large effects on NTB borrowing: for instance Figure OA-23 shows that the closing of the study card coincided with a decrease of close to 25 percentage points in the fraction of new loans going to NTB borrowers.

## 6 What Explains The Limited Default Response?

In this section we consider some of the possible reasons for the limited response of default to the experimental changes in contract terms. For the case of minimum payments, we argue that the null default effect is consistent with the "cancelling out" of two opposing forces and we identify the debt repayment burden as a potentially important channel for explaining default.

### 6.1 Countervailing Debt Burden and Liquidity Constraint Effects

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

We can assess the reasonableness of this argument by studying default when one of the two effects is no longer operational. To do so we obtain data on default for the experimental sample for June 2012, three years after the experiment ended. After May 2009, all study borrowers were returned to the same set of contract terms. The minimum payment was set at the pre-experiment level of $4\%$ and the interest rates were likewise returned to their pre-experiment levels (we confirm this in Table 3, column (3) and in Figure OA-20). This means that for those previously in the $10\%$ minimum payment arms, the tightening of short-run liquidity constraints effect was no longer operational. On the other hand, since the higher minimum payments reduced debt (see Appendix J.3), clients in the $10\%$ minimum payment arm faced a lower debt repayment burden so the second effect was still present after the experiment ended.[35] Thus, of the two countervailing forces described above, only the debt burden effect was operational after the experiment ended. We hypothesize that after the experiment, default should should be lower for those previously in the $10\%$ minimum payment arms (relative to the $5\%$ arm).

Table 3 shows that this is the case. Higher minimum payments (between April 2007 and May 2009) decreased default through June 2012 by a statistically significant $4.6$ percentage points. The implied elasticity of $-0.11$ is much larger and opposite in sign from the statistically insignificant elasticity of $+0.02$ at the end of the experiment in May 2009.[36] The results are thus consistent with the hypothesis that during the experiment tightened short-run liquidity constraints counteracted the effect of a lower debt repayment burden yielding a zero net effect on default and that after the experiment, the reduced debt burden effect decreased default.

We can also directly examine the effect of the debt burden on default by using an instrumental variables strategy. The LATE of debt – measured in thousands of pesos in May 2009 – on default between June 2009 and June 2012, using the $10\%$ minimum payment arm as an instrument, is $0.06$, i.e. every 1000 peso reduction in debt due to increased minimum payments leads to a six percentage point reduction in default rates.[37] Finally, the (Spearman) rank correlation between the 7 ITT estimates of default and debt reduction in May 2009 (i.e. ITT for each of the 7 possible contrasts) is $0.96$ so that that the largest debt contrasts were those with the largest reductions in debt at the end of the experiment.

### 6.2 The Importance Debt Burden for Default

The previous section highlighted the importance of the debt repayment burden effect in determining default using the minimum payment intervention. In this section we provide further evidence on its importance using the interest rate variation generated by the experiment. We do by disentangling the debt burden effect from the "pure" moral hazard effect.

We again use outcomes three years after the experiment to learn about the relative magnitudes

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

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

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

of the two effects. During the experiment both effects should be operational. Since they both increase default and we found small default responses, we concluded that both effects were likely small. After the experiment, however, no differential "pure" moral hazard incentive effect was operational since borrowers across arms faced the same interest rates (see col. (3) of Table 3). However, Table OA-12 shows that the debt burden did vary systematically across the interest rate arms – the Lee bounds for the debt elasticity were $[+0.34, +0.74]$, a robust effect.[38] Consistent with the debt burden hypothesis, we find that default rates were lower in the lower interest rate arms three years after the experiment ended. Table 3 finds that five-year default rates in the $(15, 5)$ arm were $4$ percentage points lower relative to the base arm – the implied elasticity is $+0.15$.[39] This finding further emphasizes the importance of the debt-burden effect in our sample of NTB borrowers.

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

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

## 7 What explains the high baseline default rates?

In Appendix N.1 we argue that card default is costly for NTB borrowers.[40] If default is indeed costly, why are default rates so high? We conjecture that the answer is partly that NTB borrowers are vulnerable to frequent, large shocks that precipitate default. The importance of the debt repayment burden documented above is consistent with this hypothesis.

We focus on one particular shock — job separation in the formal sector — that we observe using social security data. Job separation is common, approximately $37\%$ of our sample (defined below) suffered a separation at least once during the three year study period. We attempt to isolate

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

[39] This is approximately the same as the $+0.20$ elasticity at the end of the experiment which is estimated off a reduction in default of $2.6$ percentage points over the two year experiment. Note also that the $4\%$ figure includes individuals who defaulted during the experiment. We do not limit attention to post-experiment defaults only to avoid sample selection issues, but note that such concerns might in fact lead us to *under*-estimate the debt-burden hypothesis if it is the case that the lowered interest rates led to more financially vulnerable borrowers surviving in the lower interest rate group post-experiment the $(15, 5)$ arm.

[40] In particular, we provide evidence that NTB borrowers are credit constrained in the formal sector, that default leads to virtual exclusion from formal credit and informal credit terms are much worse than formal credit terms.

the effect of job loss on default. We use a common approach in labor economics (see e.g. Jacobson et al., 1993; Couch and Placzek, 2010; Flaaen et al., 2019) to estimate the "treatment" effect of worker separations on default in an event study framework which compares workers who lost their job to a set of workers that did not. This literature focuses on effects for displaced workers – those that lost their job as part of a downsizing – and the effects are usually interpreted as the effect of involuntary separations. [41]

We assemble a dataset linking four repeated cross-sections (June 2012, June 2013, December 2013, March 2014) of one million borrowers each in the Credit Bureau to monthly data on the universe of formal workers in Mexico. The matched data yields a panel of 338,167 individuals (from 41,177 firms) with their full formal credit and employment history over a 31 month period.[42] The matched data is representative of all formal-sector borrowers in Mexico that have a credit card between October 2011 and March 2014 and held a formal sector position for at least one month in this time period. For each individual, we observe age, gender, and municipality of residence. For each individual-month observation, we observe formal sector employment status, firm and monthly wage (if formally employed) and default status for each formal sector loan product.

We focus on firms with more than 50 workers and define downsizing having occurred in firm $j$ as the first time $t_0$ such that the fall in employment between $t_0 - 1$ and $t_0$ was more than 10% (20%) of the firm's peak employment in the twelve month period 10/11–9/12. This definition captures a considerable reduction in a firm's workforce and there are only 31,581 (23,135) firms that experienced such a downsizing in our sample. Next, as in this literature, we define a worker as being displaced if at some point in the panel they suffered a separation, say in month $t$ and their employer also downsized in the same month $t$; we refer to the non-displaced workers as continuing workers (though they could subsequently be separated from the firm).[43] Our strategy is to compare default between displaced ("treated") workers and continuing ("control") workers within the same firm.[44]

[41] The argument is that "many view these large-scale (downsizing) events as natural experiments that allow researchers ... to accurately gauge the wage losses that result from breaking ties to a specific firm" (Couch and Placzek, 2010). See Flaaen et al. (2019) as well.

[42] The matching procedure is as follows. Of the four million borrowers across our credit bureau cross-sections, 2.03 million had both a tax identifier and a credit card at some point between October 2011 and March 2014. We then attempt to link each borrower to the monthly IMSS data (running from 10/11 to 5/14). We define a match as linking a borrower to one entry in the IMSS data using unique tax identifiers. We find 455,588 matches between our credit bureau snapshots and the IMSS formal sector labor datasets. Of those borrowers, we focus on 338,167 of them that work in firms of more than 50 workers. Since the IMSS is a census of all formal sector workers, a match indicates employment in the formal sector for that month and we assume that a lack of a match indicates no employment in the formal sector for that month. Since we do not observe employment in the informal sector, we cannot construct a more comprehensive indicator of employment, but this should if anything attenuate our results below.

[43] There are 6,070 displaced workers and 168,211 continuing workers using the $10\%$ cut-off and 5,179/106,403 workers using the $20\%$ cut-off. Other papers all using U.S. administrative data have longer panels and look at longer periods of downsizing (e.g. Couch and Placzek (2010) allow for 5 years), which in turn allows the use of larger downsizing thresholds (e.g. 30%) without losing too many firms. We have a shorter panel and measure downsizing relative to short window (that defines peak employment). This has the advantage of using sudden unemployment shocks for identification, but it forces us to use lower thresholds for defining downsizing.

[44] Appendix O. assesses robustness by looking at the universe of all firms (not just downsizing), using as controls employees who eventually lost their job (not just those in the same firm), as well as a different specification that exploits the size of unemployment spells. We obtain comparable results.

We estimate Equation (3) for borrower $i$ working in firm $j$, municipality $s$, in month $t$:

$$\text{default}_{ijt} = \alpha_i + \gamma_{s,t} + X'_{ijt}\delta + \sum_{k\geq -6}^{12} \beta_k \mathbb{1}(\text{months to } t_0 = k)_{i(j)} + \sum_{k\geq -6}^{12} \theta_k \mathbb{1}(\text{ months to } t_0 = k)_{i(j)} T_i + \epsilon_{ijt} \quad (3)$$

where the dependent variable $\text{default}_{ijt}$ is equal to 1 if any of borrower $i$'s credit cards are more than 90 days past due at time $t$; $\alpha_i$ is a borrower fixed effect, $\gamma_{s,t}$ are a full set of municipality by month indicators, $X_{ijt}$ include firm indicators[45] and worker age; $\mathbb{1}(\text{ months to } t_0^j = k)_{i(j)}$ is an indicator equal to one if the observation is $k$ months away from firm $j(i)$'s downsizing event $t_0^j$ ($k = 0$ corresponds to the month in which downsizing occurs). $T_i$ is an indicator equal to one if $i$ is a displaced worker and zero otherwise. The $\beta_k$ coefficients measure the trends in default for non-displaced workers before and after the downsizing event. The $\theta_k$ coefficients measure the default path of displaced workers *relative* to non-displaced workers at the same firm, that is the difference-in-difference coefficients.

The main identification challenge in this literature is that continuing and displaced workers may not be similar along unobservables, and that macroeconomic shocks may affect both downsizing and default. We are able to control for these flexibly with time indicators at the municipality level thus absorbing local macro shocks, and we compare workers within firms, which mitigates to some degree the unobservables problem. A potential concern is that continuing workers may also be affected when their firm downsizes – but we believe that such effects are likely negative, in which case we may in fact be underestimating the true displacement effect.[46]

The red line in Figure 5(a) plots the estimated $\theta_k$ coefficients in red, and the $\beta_k$ coefficients in gray from equation (3), using 10% as the downsizing threshold, while Figure 5(b) uses 20% as the threshold. Several things are noteworthy. First, continuing workers (gray line) do not show signs of distress when their firm downsizes. This suggests that we are adequately controlling for macroeconomic trends that may drive both default and unemployment. Second, all $\theta_k$ coefficients for $k < 0$ are statistically indistinguishable from zero, suggestive evidence in favor of the parallel trends assumption. Third, the effect of losing a formal job begins to affect default after about six months and then increases over time. This is consistent with gradual increased debt accumulation as earnings go down and because by definition default is defined as 90 days of balances past due. The estimates show that one year after displacement default increases by about 5-7 percentage points, or $28\%$ – $36\%$ of mean default rates among non-displaced workers.

The effect is quite large, particularly since we cannot rule out displaced workers finding jobs in the informal sector post-separation (and thereby dampening the separation shock). To our knowledge this is the first quantification of the effect of job displacement on loan default and the magnitudes suggest that displacement is an important contributor to default. Keys (2018) studies the effects of displacement on financial outcomes in the United States – using the NLSY to study the effect of unemployment on bankruptcy filings (rather than loan default) and finds considerably

[45] Including firm characteristics instead of fixed effects gives similar results.

[46] We find that earnings decrease immediately by 30% of mean non-displaced worker earnings and recover relatively slowly.

smaller (and more imprecisely estimated) estimates. We use larger administrative data (matching credit data with the universe of formal workers) which improves precision and mitigates measurement error (and recall issues associated with survey data), include better controls (worker and municipality-month fixed effects) and have plausibly greater exogeneity (using separations as part of a downsizing).

# 8 Conclusion

Borrowing via credit card is an increasingly common way for borrowers to enter the formal financial sector in many developing countries and has received increased attention from policy makers and regulators. However, it remains relatively under-studied. In this paper we examine one of the largest efforts to-date in expanding credit via credit cards to poor and financially inexperienced borrowers, carried out by one of Mexico's largest commercial banks. We use detailed individual-level data and a large-scale country-wide randomized experiment with a flagship financial inclusion product directed towards new-to-banking borrowers and establish some basic facts and answer several key questions.

We find that new to banking (NTB) borrowers default at high rates (over 19% of our sample defaulted over the study period) and that default appears to be difficult to predict. We then use an RCT to assess default sensitivity to substantial variation in key contract terms. We find that doubling the minimum payment did not affect default over the 26 month experiment. This is sharp evidence on the ineffectiveness of the commonly mooted policy of instituting minimum payment floors to limit default (and currently adopted by e.g Mexico and Taiwan). We provide evidence that the null effect on default during the experiment was likely the effect of two opposing effects canceling each other out – tightened liquidity constraints versus reduced debt burden; policy discussions have tended to focus primarily on the latter while ignoring the former. We also found that even large reductions in interest rates had small effects on default so that ex-post moral hazard appears to play a limited role in our context. Finally, given that contract terms do not explain default, we attempt to explain the high level of default in our population. We use a firm downsizing in difference-in-difference design and find that job separation has a significant effect on loan defaults.

Several avenues remain open for future research. A direct question is the role of the "outside" option in credit markets with NTB borrowers to better understand why moral hazard appears to be very important in some contexts (e.g Adams et al., 2008) and less so in others (e.g. Dobbie and Skiba, 2013). A broader one is the extent to which the distance lending model such as the one adopted by Bank A and other commercial banks (individual lending, credit-score based screening, remote monitoring and collection) can be used to expand credit to under-served populations with little or no credit history. Finally, it would be interesting to examine whether, given the large effects of idiosyncratic shocks on credit histories, some form of insurance is useful in improving credit expansion.

# Tables

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

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

*Notes:* This table presents means and standard deviations for selected variables from the experimental sample and three different credit bureau sub-samples. Column 1 shows statistics for the experimental sample at the beginning of the experiment – March 2007 (Panel A) and June 2007 (Panels B and C). Column 2 (Panel A) shows statistics for the experimental sample at the end of the experiment (May 2009) and June 2010 (Panels B and C). Column 3 presents summary statistics for the credit bureau sub-sample restricted to borrowers with at least one credit card in June 2010. Column 4 selects a sub-sample from the Column 3 sample that mimics the distribution of card tenure for the experimental sample (see the Appendix B.2 for details). Column 5 restricts the sample from Column 3 to individuals with at least eight years of credit history with the bureau. (†) The card revenue measure is constructed using monthly data on purchases, payments and debt and the procedure is described on p.7. (‡) Income is obtained by matching our data with social security data (IMSS) from October 2011. The IMSS contains firm reports of employee earnings. Approximately 18% and 13% of the experimental sample and the CB sub-samples were matched with the IMSS.

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

| | Default | | Cancellations | | Revenue | |
|---|---|---|---|---|---|---|
| Strata: | All | 6-11M | All | 6-11M | All | 6-11M |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| r = 15, MP = 5 | -0.026*** | -0.019 | -0.035*** | -0.033*** | -2,859*** | -3,139*** |
| | (0.008) | (0.012) | (0.007) | (0.007) | (105) | (184) |
| r = 15, MP = 10 | -0.015 | 0.012 | -0.011 | -0.027*** | -2,642*** | -2,893*** |
| | (0.008) | (0.013) | (0.007) | (0.007) | (105) | (184) |
| r = 25, MP = 5 | -0.023** | -0.014 | -0.024*** | -0.026*** | -1,889*** | -1,956*** |
| | (0.008) | (0.012) | (0.007) | (0.007) | (109) | (190) |
| r = 25, MP = 10 | -0.008 | -0.004 | -0.003 | -0.024*** | -1,893*** | -2,108*** |
| | (0.008) | (0.012) | (0.007) | (0.007) | (106) | (184) |
| r = 35, MP = 5 | -0.000 | 0.004 | -0.018** | -0.022** | -964*** | -1,008*** |
| | (0.008) | (0.012) | (0.007) | (0.007) | (118) | (198) |
| r = 35, MP = 10 | -0.002 | -0.010 | -0.004 | -0.003 | -1,167*** | -1,062*** |
| | (0.008) | (0.012) | (0.007) | (0.008) | (107) | (193) |
| r = 45, MP = 10 | 0.005 | 0.015 | 0.017* | 0.004 | -469*** | -394 |
| | (0.008) | (0.013) | (0.007) | (0.008) | (113) | (202) |
| Constant (r = 45, MP = 5) | 0.193*** | 0.315*** | 0.134*** | 0.099*** | 2,768*** | 1,615*** |
| | (0.006) | (0.009) | (0.005) | (0.005) | (86) | (143) |
| Observations | 143,916 | 47,959 | 143,916 | 47,959 | 143,916 | 47,959 |
| R-squared | 0.032 | 0.024 | 0.006 | 0.007 | 0.050 | 0.026 |

*Notes:* All regressions include strata dummies and use sample weights. The dependent variable for Columns (1) and (2) is default (bank-initiated revocations). The dependent variable for Columns (3) and (4) are (client-initiated) cancellations. The dependent variable for Column (5) and (6) is our measure of bank revenue from the study card. All outcomes measure the dependent variables at the end of the experiment (26 months). Columns (1), (3) and (5) include all cardholders in the experiment. Columns (2), and (4) and (6) restrict only the cardholders in the 6-11 months strata. Robust standard errors are shown in parenthesis. One, two and three stars denote statistical significance at the 5, 1 and 0.1 percent level respectively.

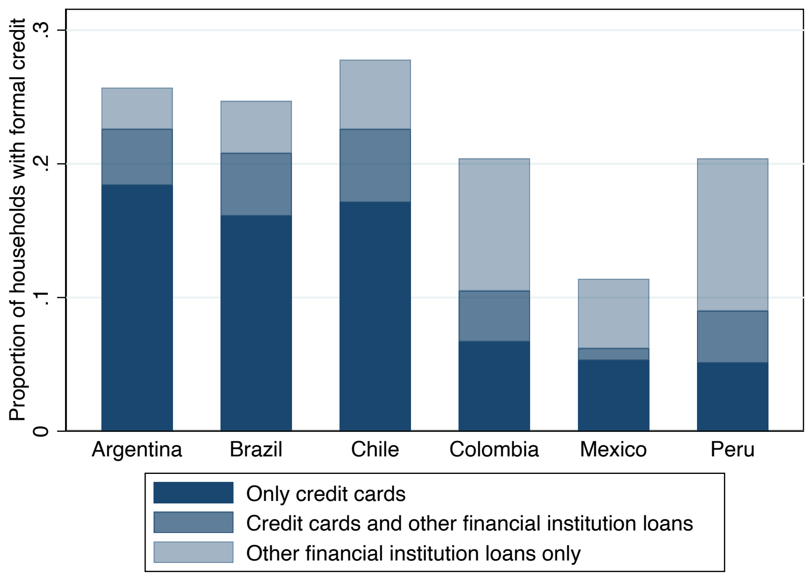
**Table 3:** Long-term Treatment Effects
(Outcomes measured in June 2012)

| | New Loan (any bank) Jun 09 - Jun 12 (1) | # Banks active Jun 12 (2) | Int Rate (Study Card) Jun 12 (3) | Cumulative Default Jun 12 (4) | Cumulative Cancellations Jun 12 (5) |
|---|---|---|---|---|---|
| r = 15, MP = 5 | 0.003 | 0.012 | 0.002 | -0.039*** | 0.008 |
| | (0.010) | (0.012) | (0.003) | (0.010) | (0.010) |
| r = 15, MP = 10 | 0.011 | 0.017 | 0.003 | -0.066*** | 0.027** |
| | (0.010) | (0.012) | (0.003) | (0.010) | (0.010) |
| r = 25, MP = 5 | -0.001 | 0.008 | -0.001 | -0.040*** | 0.017 |
| | (0.010) | (0.012) | (0.003) | (0.010) | (0.010) |
| r = 25, MP = 10 | -0.006 | 0.008 | 0.001 | -0.048*** | 0.012 |
| | (0.010) | (0.012) | (0.003) | (0.010) | (0.010) |
| r = 35, MP = 5 | 0.006 | 0.016 | -0.003 | -0.004 | -0.005 |
| | (0.010) | (0.012) | (0.003) | (0.010) | (0.010) |
| r = 35, MP = 10 | 0.015 | 0.002 | 0.005* | -0.038*** | 0.009 |
| | (0.010) | (0.012) | (0.002) | (0.010) | (0.010) |
| r = 45, MP = 10 | 0.012 | 0.016 | 0.002 | -0.043*** | 0.021* |
| | (0.010) | (0.012) | (0.003) | (0.010) | (0.010) |
| Constant (r = 45, MP = 5) | 0.342*** | 1.620*** | 0.530*** | 0.405*** | 0.428*** |
| | (0.007) | (0.008) | (0.002) | (0.007) | (0.007) |
| Observations | 139,197 | 139,197 | 42,463 | 139,197 | 139,197 |
| R-squared | 0.012 | 0.006 | 0.002 | 0.042 | 0.022 |

*Notes:* All regressions include strata dummies and use sample weights. Each column is a separate regression. The dependent variable in column (1) is a binary variable equal to 1 if client $i$ took out at least one loan between June 2009 and June 2012. The dependent variable in column (2) is the number of banks with whom client $i$ had an "active relationship" as of June 2012. An active relationship means having at least one active loan with a lender. Column (3) uses as dependent variable the interest rate (between 0 and 1) on the study card in June 2012 for those cards that remained open. Columns (4) and (5) use as dependent variables binary variables that are equal to 1 if the client defaulted, or cancelled, respectively, at any point between March 2007 and June 2012. Robust standard errors are shown in parenthesis. One, two and three stars denote statistical significance at the 5, 1 and 0.1 percent level respectively.

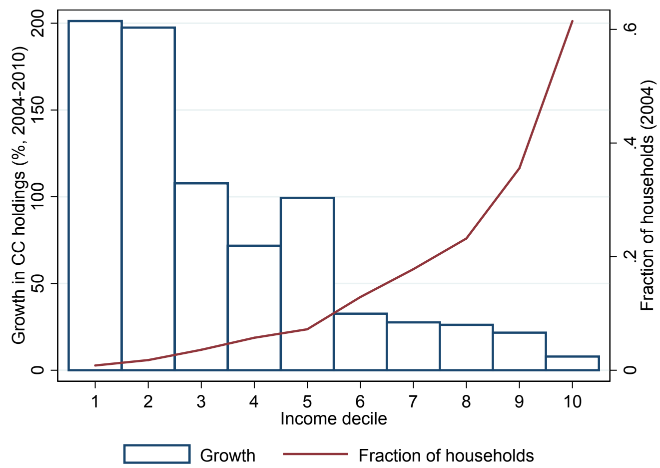
# Figures

**Figure 1:** Credit card and other borrowing across Latin American countries (2017)

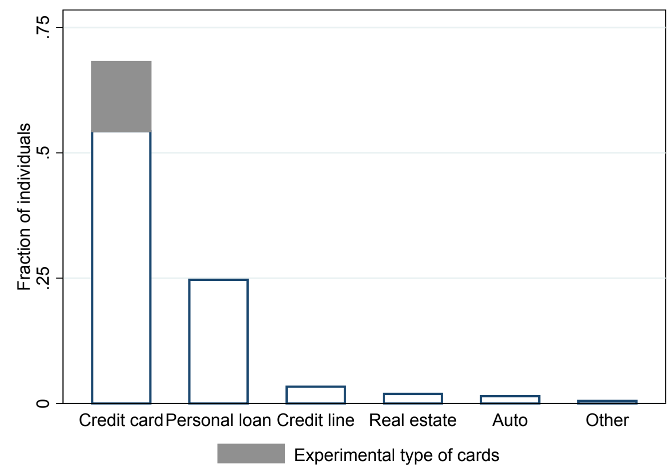



*Notes:* Source is the 2017 World Bank Global Findex database. The figure shows the proportion of adults who have had credit in the past 12 months for selected countries in Latin America. Formal credit is defined as credit issued by a bank or another type of financial institution. Creditholders are then separated into groups based on type of credit. The first group is those with credit from financial institutions but not using credit cards (light navy); the second is adults with credit from financial institutions and using credit cards (mid navy); the third is adults using only credit cards (dark navy). Note that the Global Findex database used for this figure presents data on the extent of formal credit held by respondents at a point in time, but does not record their first formal financial sector credit product.

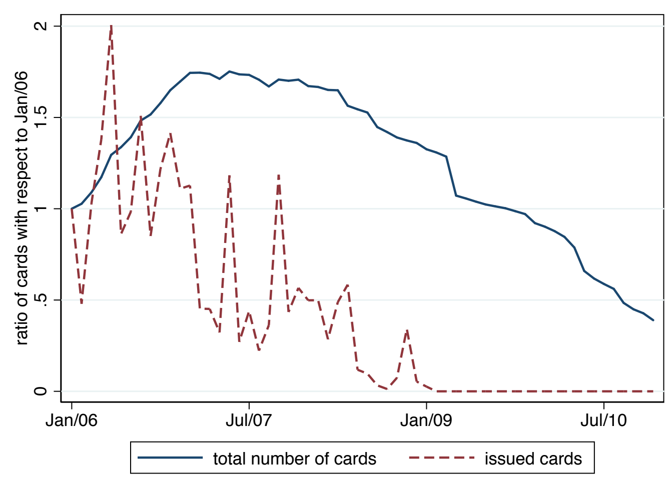
**Figure 2:** Overall credit card growth, and study card's share and evolution.




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



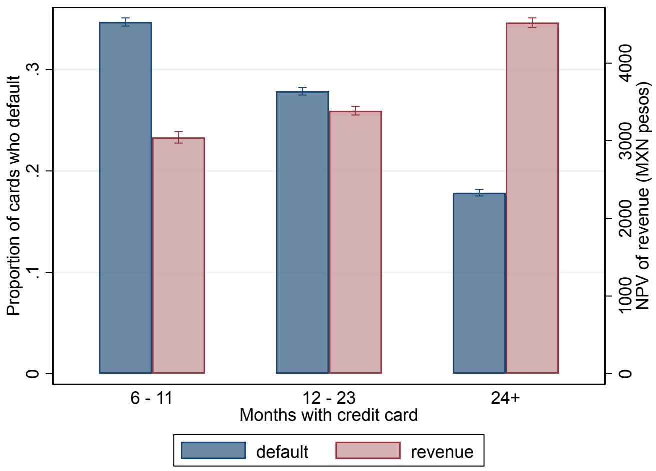

**(b)** First Time Loan, by type



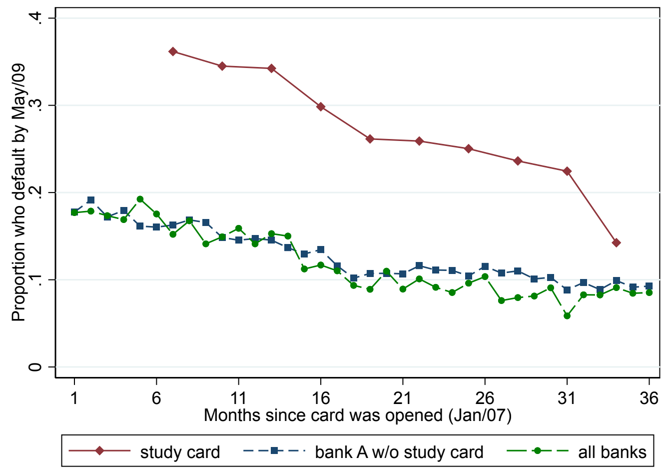

**(c)** Study Card Stocks and Flows

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

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




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




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

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

**Figure 4:** Timeline for the datasets

0. **Strata information:**

   Strata variables recorded.

1. **Bank data:**

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

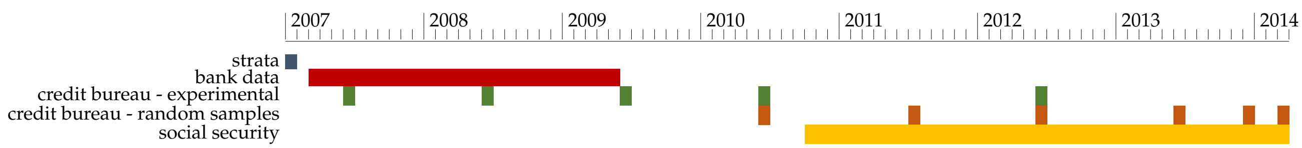
2. **Credit Bureau data:**

   Loan level data matched to experimental sample for 06/07 to 06/10, annually.

   Loan-level data representative of the entire credit bureau population (cross-sections).

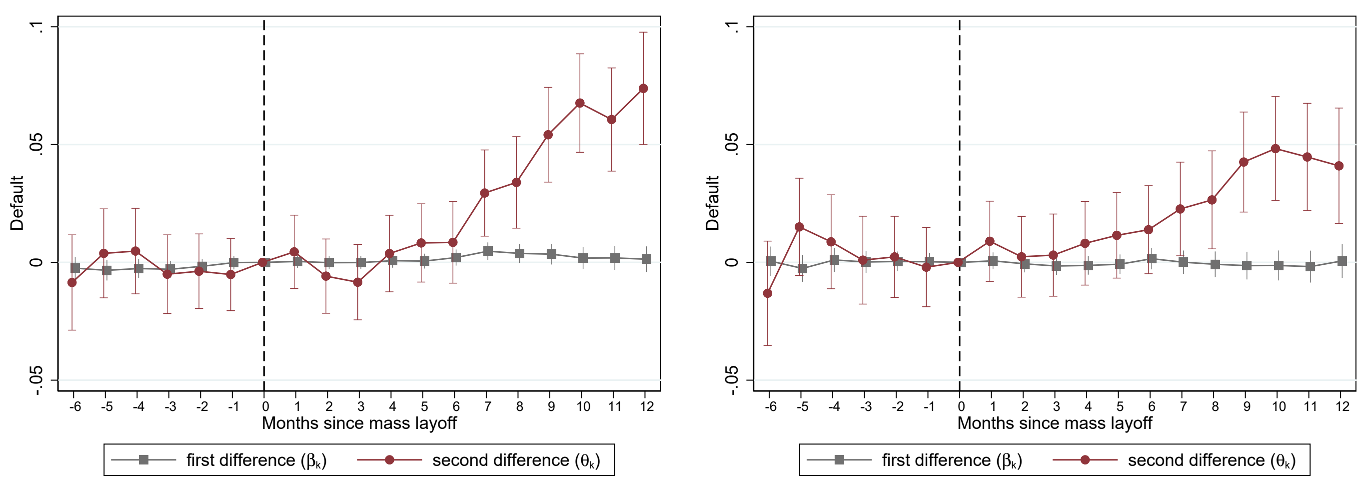
3. **Social security data:**

   Individual-level, monthly information from 10/10 to 03/14.




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

**Figure 5:** Job displacement and Loan Default




*Notes:* These figures plot the default trend for continuing workers ($\beta_k$ coefficients from equation (3) in gray color, and differences-in-differences effect for displaced workers ($\theta_k$ coefficients) in red. Panel (a) uses 10% as the downsizing threshold, while panel (b) uses 20% as the threshold. The x-axis measures time relative to the (firm specific) downsizing event.

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# Expanding Financial Access Via Credit Cards: Evidence from Mexico

## Appendix – For Online Publication

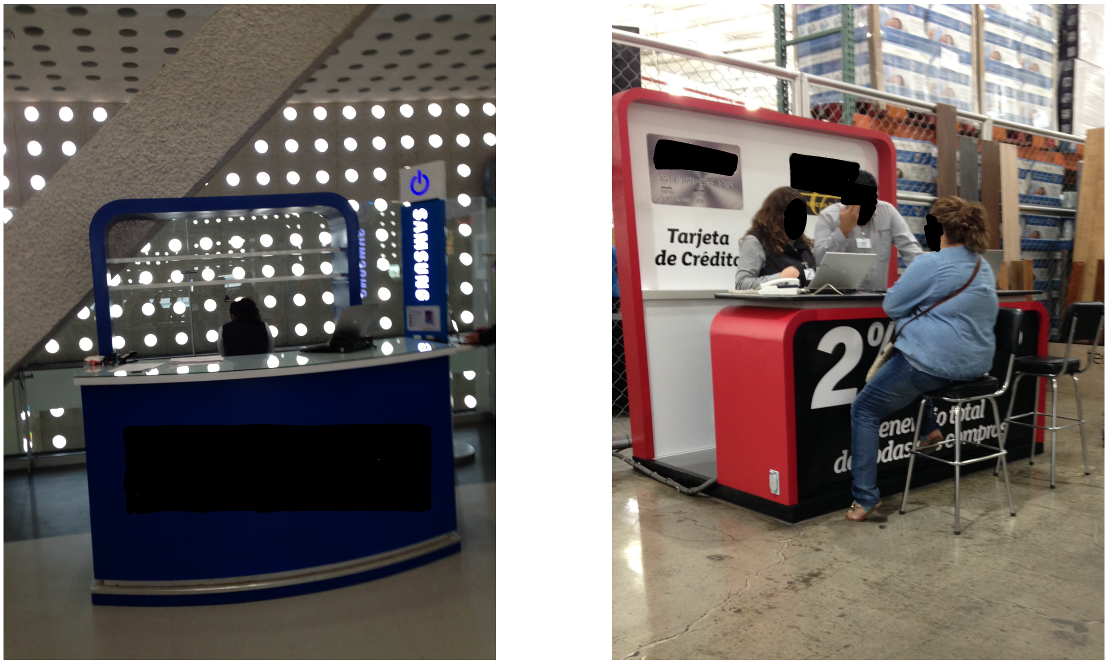
Sara G. Castellanos, Diego Jiménez-Hernández, Aprajit Mahajan, Enrique Seira

## Contents

# Appendix A. Background

## A.1 Types of kiosks used for find NTB

**Figure OA-6:** Example of Promotional Kiosks




*Notes:* This kiosks do not necessarily correspond to those for our study card (for confidentiality reasons). But they are similar to the ones Bank A used.

# Appendix B. Data

## B.1 Data Check

We argue the following relation holds in our data:

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

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

**Table OA-4:** Data check

| | (1) |
|---|---|
| Amount Due$_{i,t-1}$ | 0.996*** |
| | (0.000248) |
| Payments$_{i,t}$ | -1.000*** |
| | (0.000363) |
| Purchases$_{i,t}$ | 1.008*** |
| | (0.00102) |
| 15% x Debt$_{i,t}$ | 0.179*** |
| | (0.00343) |
| 25% x Debt$_{i,t}$ | 0.279*** |
| | (0.00356) |
| 35% x Debt$_{i,t}$ | 0.380*** |
| | (0.00370) |
| 45% x Debt$_{i,t}$ | 0.476*** |
| | (0.00474) |
| Fees$_{i,t}$ | 0.495*** |
| | (0.00178) |
| R-squared | 1.000 |
| Observations | 4,830,536 |

*Notes:* This table estimates equation (4) by OLS on months with positive debt. That is we estimate the $\beta$'s in the following equation: $Amount\ due_{it} = \beta_0 + \beta_1 Amount\ due_{it-1} + \beta_2 Payments_{it} + \beta_3 Purchases_{it} + \sum_k \gamma_k\ Debt_{it} \times I(r = k) + \beta_5 Fees_{it} + \epsilon_{it}$, where $k \in \{15, 25, 35, 45\}$ The coefficients are unconstrained, so a coefficient of payments =-1 for instance is a result and not an imposed constraint. The same is true of interest rates: the coefficient on $I(r = 25\%)$, i.e. $\gamma_{25}$ =0.27 being close to 0.25 is a result as well. One, two and three stars denote statistical significance at the 5, 1 and 0.1 percent level respectively.

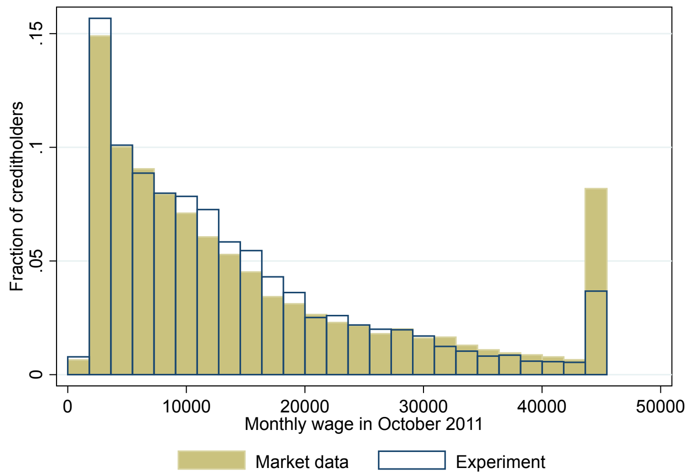
## B.2 Details of "Matched" Sample for Table 1

This subsection describes how we constructed the sample from Column 4 in Table 1. First, note that, for the experimental sample in March 2007 (Column 1), Panel B shows that the mean tenure is 68 months with a standard deviation of 54 months. Using the individuals from the experimental sample in (described in Section 2.1) and focusing in March 2007, we construct 50-quintiles for the tenure in months of the oldest credit. Doing so gives us values $r_1, \ldots, r_{49}$ where those cardholders whose loan tenure falls between $[r_i, r_{i+1})$ are in the $(i+1)-$th quintile, and we can define $r_0$ and $r_{50}$ as the min and max values for the tenure to have the first and last 50-quintile groups defined. By construction, we have the same amount of cardholders in each $[r_i, r_{i+1})$ region.

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

### B.3 Distribution of wages using social security data

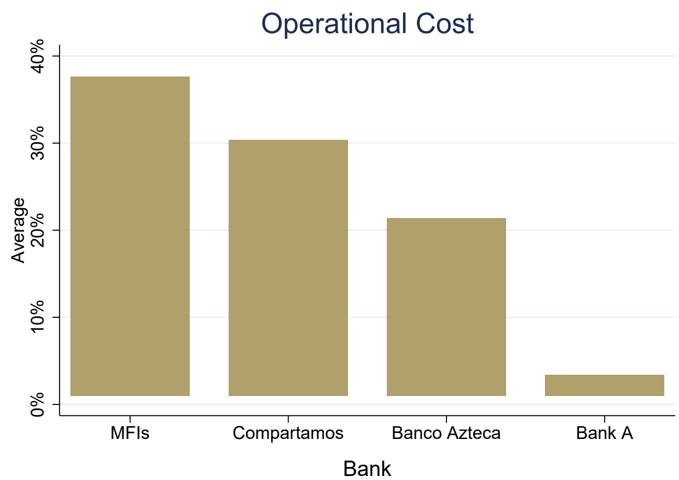
**Figure OA-7:** Creditholders by income in October 2011




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

## Appendix C. Operating costs: Compartamos, Azteca, and Bank A

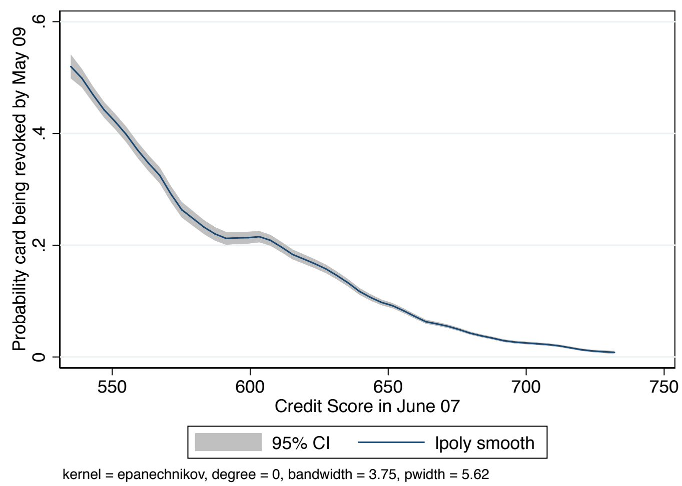
**Figure OA-8:** Operational Costs (relative to Assets)




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

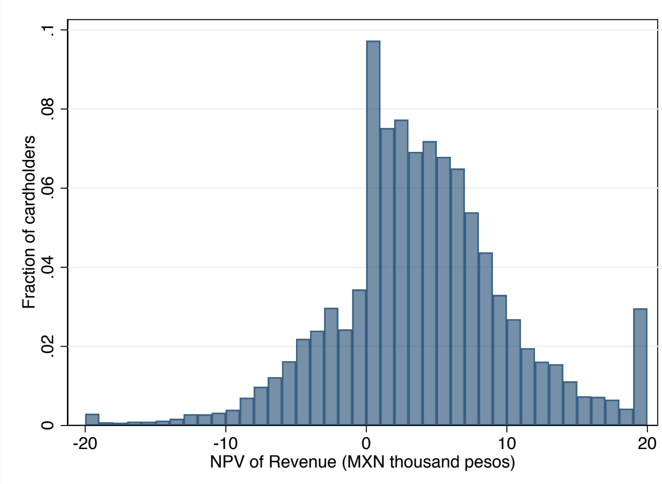
# Appendix D. Exploring our proxy of Revenue

**Figure OA-9:** Default and Credit Scores

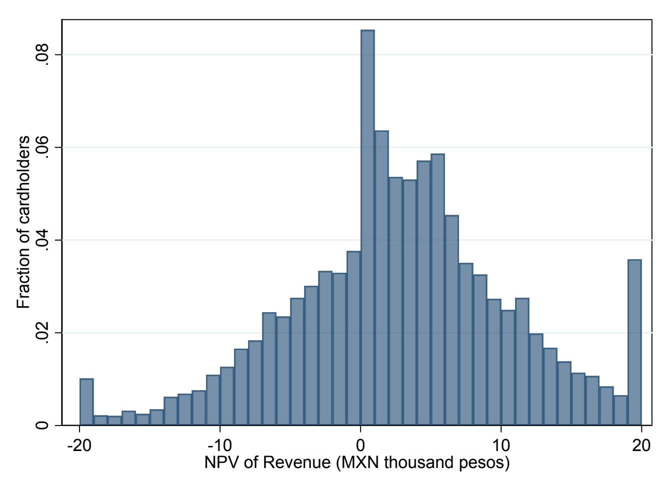



*Notes:* This figure plots a kernel regression of default (in May 2009) against credit scores (in June 2007). This forms basis of our estimate of the likelihood of default used in constructing our bank revenue measure.

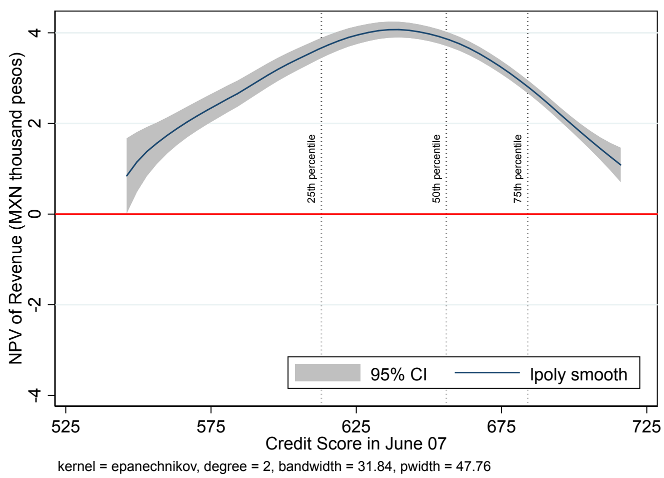
**Figure OA-10:** Distribution of Measured Revenue per Card and Relationship with Credit Scores



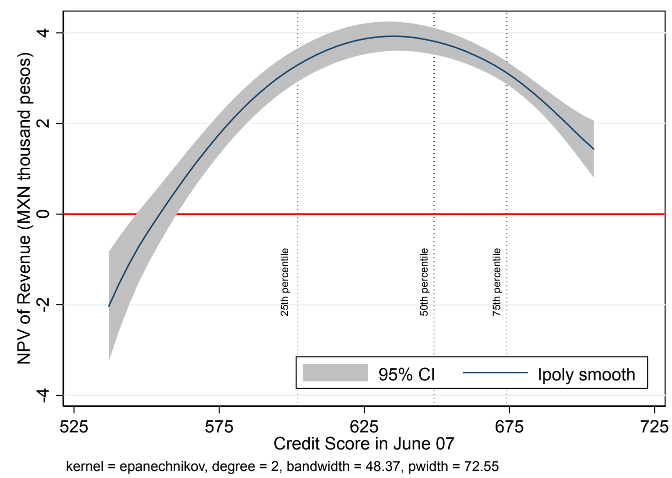

**(a)** Net Present Value of Revenue ('000 Pesos)
(control group borrowers)




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



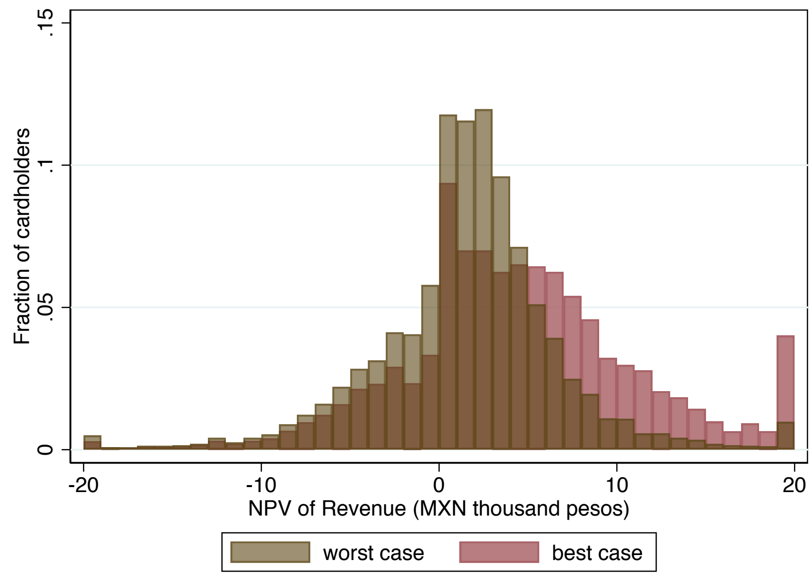

**(c)** Mean revenue by Credit Score
(control group borrowers)




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

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

**Figure OA-11:** Robustness: Best and Worse Case Bounds (for $\alpha_i$)

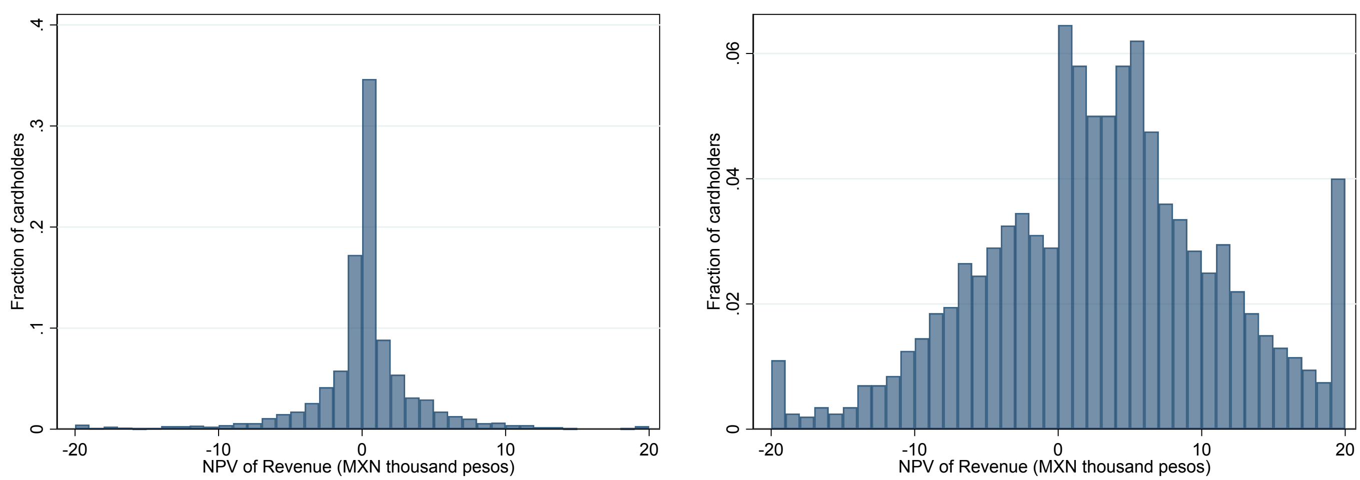



*Notes:* Histograms of the revenue measure computed with $\alpha_i = .05$ (low recovery rates) for all borrowers (in brown) and the best case scenario with $\alpha_i = 0.95$ (in pink). The resulting histograms do not vary significantly from the one constructed using estimated values of $\alpha_i$ for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

**Figure OA-12:** Revenue and Credit Scores: By Strata

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

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




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

Figure OA-10 (a) and (b) plot histograms of our revenue measure for different arms and strata. Figures OA-10(c) presents a kernel regression of revenue on 2007 credit scores at the borrower level for the control group, while OA-10(d) carries out the same exercise but for the stratum with the shortest tenure with the bank.

Strikingly, clients with low and high credit scores yield low revenues relative to clients with middling scores. Low score clients are more likely to default, thus yielding low revenue. On the other hand, high credit score clients generate little revenue because they accrue lower interest charges and fees (e.g. by paying off the amount outstanding each month).[47]

## Appendix E. Are NTB Borrowers Credit Constrained?

Recent and limited participation in the formal credit sector raises the possibility that NTB clients continue to be credit constrained. Evidence of continuing credit constraints will provide the context for understanding the experimental treatment effects in the sequel. We test for the existence of credit constraints by examining debt responses (in the experimental sample) to increases in credit limits for the study card. If borrowers are not liquidity or credit constrained, their debt should not respond to exogenous increases in credit limits.[48] Conversely, one can view debt (or more generally consumption) responses to changes in credit limits as evidence of credit constraints.[49] Note, however, increases in borrowing following credit limit expansions for a particular card could also be consistent with the *lack* of credit constraints if borrowers replace costlier debt with cheaper debt. We can partly address this problem by examining *all* (formal sector) debt responses (using the CB data) to credit limit changes. However, since we do not observe informal borrowing, we cannot rule out the possibility of substitution away from informal loans as a response to changing formal sector credit limits.

First, we use monthly data on debt and credit limits (using the bank data for the experimental sample) to regress one month changes in debt on 12 lagged one month changes in credit limits.[50] Let $\text{Debt}_{it}$ be the amount of debt held by card $i$ at the end of month $t$, let $\text{Limit}_{it}$ denote the credit limit for account $i$ at the beginning of month $t$ and $X_{it}$ denotes a set of controls. Following the main specification in Gross and Souleles (2002) we estimate

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

where $\Delta$ is the first-difference operator and $\beta_j$ represents the incremental increase in debt between month $t-1$ and $t$ associated with a one peso change in credit limit in period $t-j$. The scalar parameter $\theta \equiv \sum_{j=0}^{T} \beta_j$ then provides us with a summary measure of the long-run (T month) total effect of credit limit on debt; we report $\hat{\theta} \equiv \sum_{j=0}^{T} \hat{\beta}_j$ for each regression.[51] Because the bank evaluates a card for credit-limit changes using predetermined durations, cards that had received a credit limit change further back in the past will have a higher present probability of a credit limit change than otherwise identical cards that received a credit limit increase relatively recently. To address concerns that credit-limits change endogenously, we can therefore instrument limit changes by the time since the last limit increase, while controlling for the total number of increases in the sample period.[52]

The results are presented in Table OA-5. In all tables, we adopt the convention of three asterisks denoting

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

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

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

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

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

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

significance at the .1% level, two asterisks at the 1% significance level and one asterisk at the 5% significance level. Panel A uses debit and limit data for just the study card while Panel B uses (changes in ) total credit card debt (from the CB data) as the dependent variable.[53] For Panel B, since we only have annual data, we modify equation (5) and regress one year changes in debt on one year changes in credit limits (i.e $T = 2$). Column (1) presents results for the entire experimental sample while the subsequent columns estimate the model on the 9 different strata.

First, focusing on the entire sample we find that after 12 months a credit limit increase of 100 pesos for the study card translates into 32 pesos of additional debt (Row 1). This number remains essentially unchanged when we add controls (not reported) while the IV estimate is substantially larger (73 pesos). This propensity to consume out of increases in the credit limit is about thrice as large as the figure for the US and suggests that these Mexican borrowers are credit constrained and significantly more so than their US counterparts.[54]

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

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

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

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

**Table OA-5:** Suggestive Evidence for Credit Constraints: Cumulative Effect of Credit Limit Changes on Debt

| | | 6-11 months | | | 12-23 months | | | 24+ months | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | All | Minimum | Two + | Full | Minimum | Two + | Full | Minimum | Two + | Full |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| *Panel A. Bank A's debt (dependent variable) and Card A's credit limit (independent variable)* | | | | | | | | | | |
| Baseline estimate | 0.32*** | 0.69*** | 0.41*** | 0.23*** | 0.56*** | 0.47*** | 0.13*** | 0.33*** | 0.13*** | 0.03** |
| | (0.04) | (0.06) | (0.04) | (0.03) | (0.05) | (0.05) | (0.02) | (0.06) | (0.03) | (0.01) |
| IV estimate | 0.73*** | 2.14*** | 1.24*** | 0.47 | 1.60*** | 1.06** | 0.09 | 0.62** | 0.52 | -0.08 |
| | (0.14) | (0.32) | (0.28) | (0.37) | (0.28) | (0.39) | (0.09) | (0.19) | (0.27) | (0.14) |
| Observations | 1,366,035 | 118,687 | 143,397 | 170,791 | 125,859 | 145,077 | 174,305 | 14,6291 | 155,290 | 186,338 |
| Mean dependent variable | 70 | 184 | 102 | 59 | 100 | 55 | 23 | 95 | 43 | 23 |
| | (2292) | (3631) | (2771) | (1756) | (2639) | (2092) | (1163) | (2863) | (2174) | (1272) |
| Mean changes in limit | -104 | -141 | -115 | -105 | -97 | -90 | -77 | -100 | -97 | -120 |
| | (1460) | (1532) | (1452) | (1486) | (1149) | (1129) | (1177) | (1446) | (1487) | (1956) |
| Mean utilization | 0.52 | 0.72 | 0.59 | 0.39 | 0.68 | 0.58 | 0.4 | 0.64 | 0.53 | 0.3 |
| | (2.96) | (.34) | (3.07) | (.33) | (3) | (3.56) | (4.81) | (.35) | (3.6) | (2.82) |
| Median utilization | 0.5 | 0.81 | 0.58 | 0.33 | 0.78 | 0.58 | 0.3 | 0.71 | 0.51 | 0.2 |
| *Panel B. Total debt across all cards (dependent variable) and total credit limit across all cards (independent variable)* | | | | | | | | | | |
| Baseline estimate | 0.29*** | 0.37*** | 0.40*** | 0.32*** | 0.42*** | 0.35*** | 0.19*** | 0.29*** | 0.24*** | 0.15*** |
| | (0.01) | (0.03) | (0.02) | (0.02) | (0.03) | (0.02) | (0.02) | (0.02) | (0.02) | (0.01) |
| IV estimate | 0.45*** | 1.17*** | 0.76*** | 0.51*** | 0.84*** | 0.45*** | 0.37*** | 0.38*** | 0.34*** | 0.24*** |
| | (0.05) | (0.12) | (0.07) | (0.04) | (0.09) | (0.06) | (0.04) | (0.07) | (0.06) | (0.04) |
| Observations | 210,886 | 24,249 | 23,473 | 22,932 | 23,103 | 22,560 | 22,250 | 23,959 | 23,789 | 24,571 |
| Mean dependent variable | 598 | 1440 | 889 | 549 | 808 | 453 | 258 | 577 | 360 | 198 |
| | (4402) | (7023) | (5220) | (3342) | (5045) | (3886) | (2140) | (5095) | (3769) | (2257) |
| Mean changes in limit | 657 | 485 | 558 | 722 | 564 | 584 | 744 | 730 | 711 | 770 |
| | (2228) | (2058) | (2163) | (2438) | (1726) | (1807) | (2131) | (2246) | (2285) | (2820) |
| Mean utilization | 0.45 | 0.67 | 0.5 | 0.33 | 0.62 | 0.47 | 0.28 | 0.54 | 0.42 | 0.22 |
| | (.38) | (.42) | (.38) | (.31) | (.39) | (.37) | (.28) | (.37) | (.35) | (.24) |
| Median utilization | 0.38 | 0.65 | 0.45 | 0.24 | 0.59 | 0.41 | 0.2 | 0.51 | 0.35 | 0.14 |

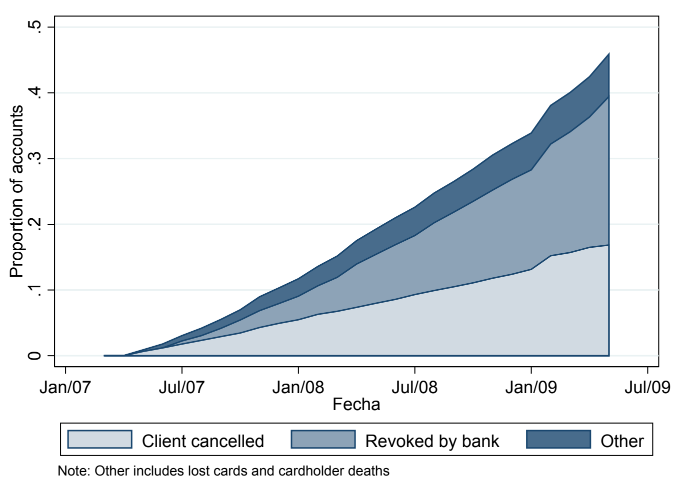
*Notes:* Each cell represents a separate regression and displays estimates of $\hat{\theta} \equiv \sum_{j=0}^{T} \hat{\beta}_j$ from Equation 5; all regressions include month dummies. The first row ("Baseline") in each panel displays estimates from regressions of current debt on past changes in credit limits (equation 5) estimated using OLS. The second row in each panel ("IV") displays results from estimating the equation using (dummies for the) months since the last credit limit change as instrumental variables. For the IV specification equation 5 controls directly for the total number of credit limit increases and decreases as well. Column (1) estimates include probability weights based on the size of each of the strata in the population. Columns (2)-(8) present stratum specific estimates. Both panels use the experimental sample albeit at different frequencies. Panel A presents results from estimating (5) at the monthly level with $T = 12$. The dependent variable is the total debt on the *study card* and the independent variable of interest is the credit limit for the study card. The dependent variable for Panel B is the total debt across *all cards* in the credit bureau for the experimental sample and the main independent variable is the total limit among across *all* cards. Since we only observe data at the annual level for the credit bureau, Panel B has $T = 2$. The instrument for both panels is months since last credit limit change in the study card only. Standard errors are shown for the baseline and IV estimates in parentheses and are clustered at the individual level. Standard deviations are shown for the mean of the dependent variable, the mean changes in limit, and the mean utilization in parenthesis. One, two and three stars denote statistical significance at the 5, 1 and 0.1 percent level respectively.

### E.0.1 Variation Across Strata

A direct test of whether the strata vary systematically in terms of credit constraints is to estimate equation (5) separately for each stratum and compare the magnitudes of the estimates of $\theta$ across strata. The results are presented in Table (OA-5) and show that by this metric the stratum with the newest borrowers and the poorest repayment history (i.e. the "6-11 Month ,Min Payer" stratum) is the most credit constrained and the stratum containing the oldest borrowers with the best ex-ante repayment history (the "24+Month, Full Payer" stratum) is the least constrained. For the former stratum, a 100 peso increase in the credit limit leads to debt increase of 69 pesos twelve months later, while the corresponding figure for the latter stratum is only 3 pesos (Panel A Row 1).[55] This pattern is confirmed across the remaining seven strata: controlling for tenure with the bank, poorer repayment histories are correlated with higher estimates of $\theta$ and correspondingly, controlling for baseline repayment history, increased tenure with the bank is correlated with lower debt responses to credit limit changes.

## Appendix F. Exit, study card closings and new cards

**Figure OA-13:** Card Exits: Experimental Data
Card Closings in Experiment, by Type of Closing

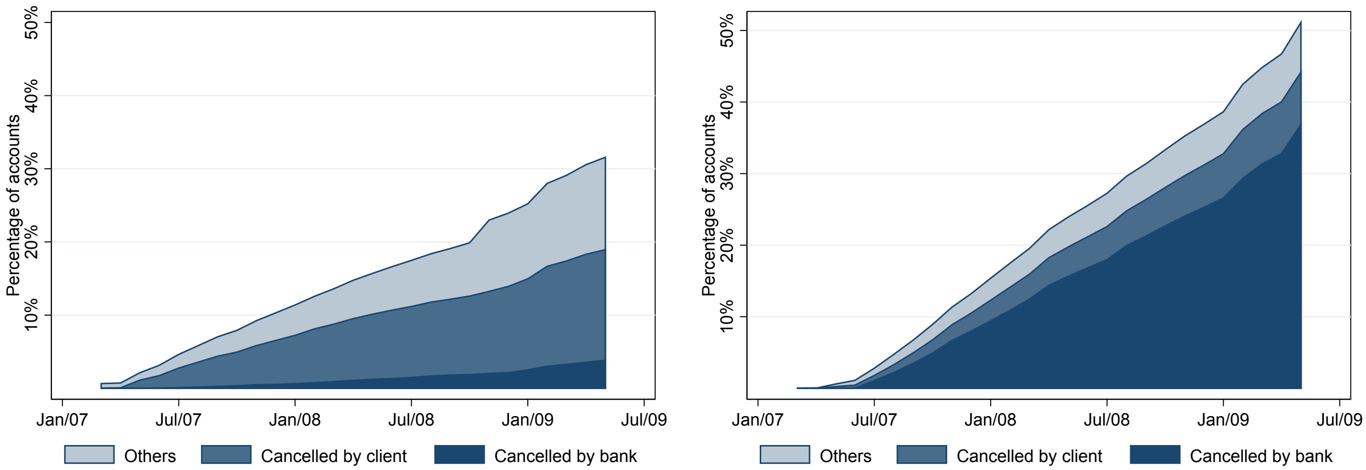



*Notes:* This figure plots card closing rates over the course of the experiment for the control group. Card closings are subdivided into (a) bank initiated revocations (i.e. default), (b) (borrower initiated) cancellations, and (c) other reasons (e.g. death of owner). For comparison, Figure OA-14 in the online appendix plots the analogous graphs for this figure for two different strata.

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

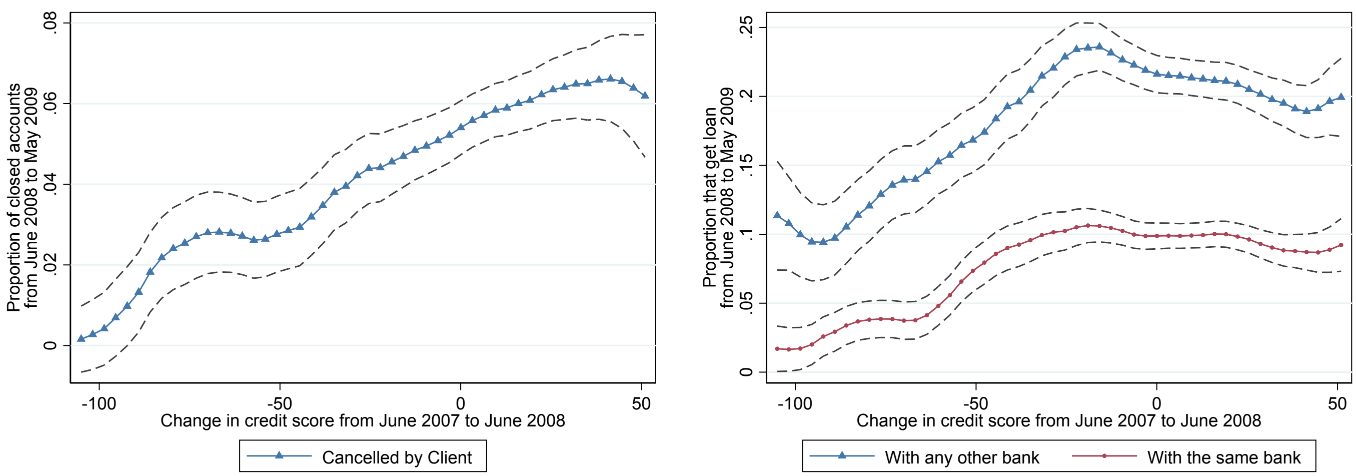
**Figure OA-14:** Card Exit during the Experiment: Selected Strata

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




*Notes:* These figures plot card exit rates over the course of the experiment for the control group and for two different strata. Card closure is subdivided into whether it was a (a) default/revocation, (b) cancellation or (c) closure for some other reason (primarily lost cards or death). The aggregate exit rates (for the entire sample) are in Figure OA-13 in the main paper.

**Figure OA-15:** Cancellations and opening of new cards




**(a)** Card Closings and Changes in Credit Scores **(b)** New Loans and Changes in Credit Scores

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

## Appendix G. Predicting Default, Cancellations, Revenues and Interest Paid

We show the results from predicting default, cancellations, revenues and which consumers end up paying interest at least once on Tables OA-6 and OA-7. The results in both Tables focus on the benchmark model, OLS and Random Forests. We use cross validation to fine tune the depth of each tree and the number of minimum samples within each leaf in the Random Forest model. For each model we predict the dependent variables (one at a time) using different sets of covariates. Each panel uses a different information set starting with a minimal set (most closely corresponding to the bank's information set when it issued the card) to progressively larger ones. Panel A includes variables measured at the time of application while Panel B uses the same variables but as observed in March 2007 (i.e. after the card was awarded) as well as the credit score in June 2007.[56] Panel C adds purchases, payments and total debt in March 2007 yielding the richest set of covariates. The most successful model, the Random Forest, has an out-of-sample $R^2$ of 0.06 in Panel A and 0.17 in Panel C. For revenues, the out-of-sample root MSE is 7,204 pesos for Panel A and 4,474 for Panel C, which are about the same as the intercept-only model. Performance improves somewhat in Panel C so that interactions with the bank (measured here in terms of payment, purchase and debt history) are useful indicators of revenue. We note, however, that even the best performing ML tool does not significantly out-perform the simplest intercept-only model on all measures. We also attempted to predict default and cancellations using the same covariates and strategy.

The general message from the differing information sets and methods is the same – it is quite difficult to predict which NTB borrowers will generate revenues for the bank and that adding a range of subsequent information (unavailable to the bank at the time of application such as payments, purchases and debt) does improve prediction, but only modestly.[57] A caveat is in order. We only observe successful applicants (rather than the entire applicant pool) and the prediction exercise is carried out on this (presumably positively) selected sample. This is clearly a limitation, but even this screened sample is by no means homogeneous or risk free and as we show above this risk is hard to predict. Even though the bank presumably screened as best it could, the result appears unsatisfactory – that the bank decided to shut down the study card provides further evidence of this.

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

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

**Table OA-6:** Predicting Revenue and Default with Different Information Sets

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

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

Regarding AUCs for default, they are lower than those documented in several studies. They are lower than those found for credit cards in the US[58], lower than those from loans in Australia, Japan, and Poland;[59] lower than those in the housing market in the US;[60] lower than those for credit default swaps in the US.[61] They are higher than those to predict repayment using cellphone data in an unnamed South American country;[62] and

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

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

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

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

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

higher than those for a micro-finance lender in Bosnia Herzegovina.[63]

**Table OA-7:** Predicting cancellations and paid interest

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

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

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

# Appendix H. Experiment Set Up

## H.1 Experiment Details and Randomization Check

**Table OA-8:** Experimental Design

| *Panel A: Stratification* | | | | |
|---|---|---|---|---|
| | Full-balance payer | Minimum payer | Part-balance payer | Total |
| 6 to 11 months | 18,000 | 18,000 | 18,000 | 54,000 |
| 12 to 23 months | 18,000 | 18,000 | 18,000 | 54,000 |
| 24+ months | 18,000 | 18,000 | 18,000 | 54,000 |
| Total | 54,000 | 54,000 | 54,000 | 162,000 |

| *Panel B: Sample Sizes for Arms Within Strata* | | |
|---|---|---|
| Interest Rate | Minimum payment | |
| | *10%* | *5%* |
| *15%* | 2000 | 2000 |
| *25%* | 2000 | 2000 |
| *35%* | 2000 | 2000 |
| *45%* | 2000 | 2000 |
| Control | 2,000 | |

**Table OA-9:** 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-10:** Randomization Check - Baseline statistics for March 2007

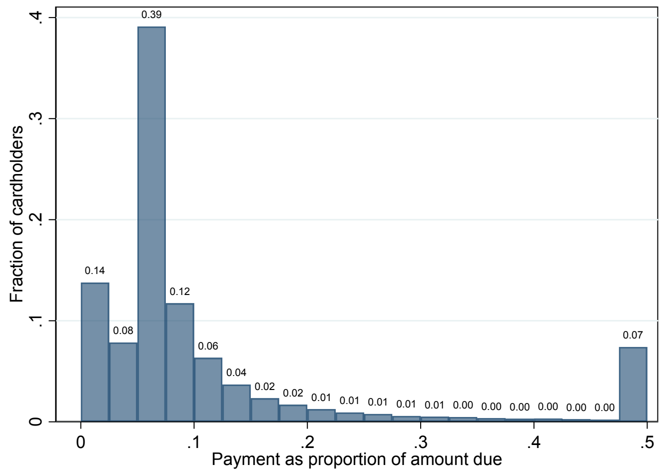
| | CTR | r = 15 % | | r = 25 % | | r = 35 % | | r = 45 % | | Total | P-value | Observations |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | mp = 5 % | mp = 10 % | mp = 5 % | mp = 10 % | mp = 5 % | mp = 10 % | mp = 5 % | mp = 10 % | | | |
| | (1) | (2) | (3) | (5) | (6) | (7) | (8) | (9)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

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

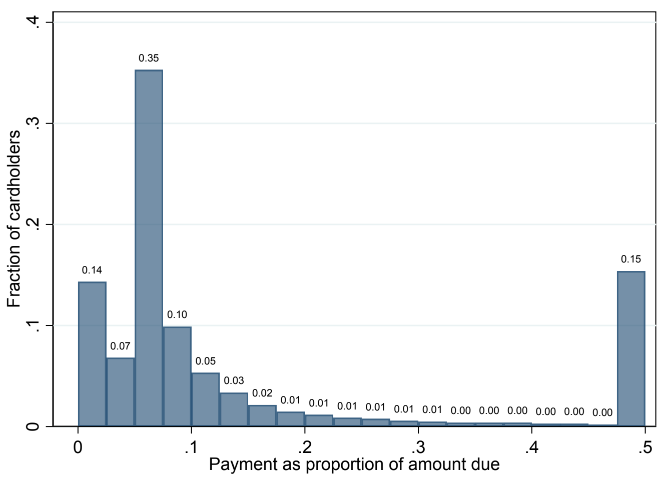
## H.2 Minimum Payments Bind for a Substantial Fraction of Borrowers

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

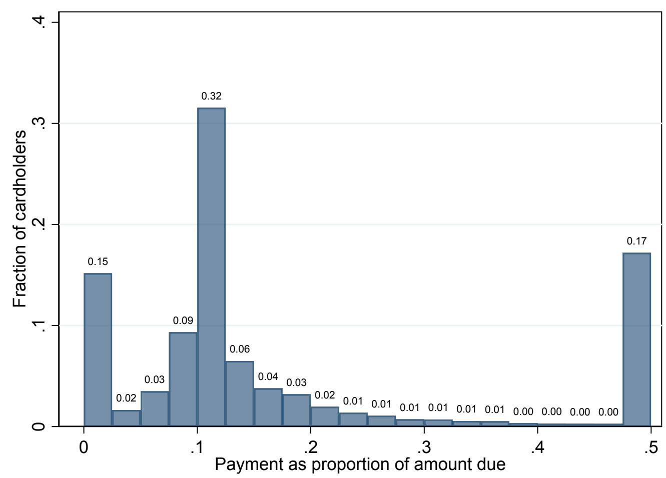
**(a)** Mar/07 - all treatment arms




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




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




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

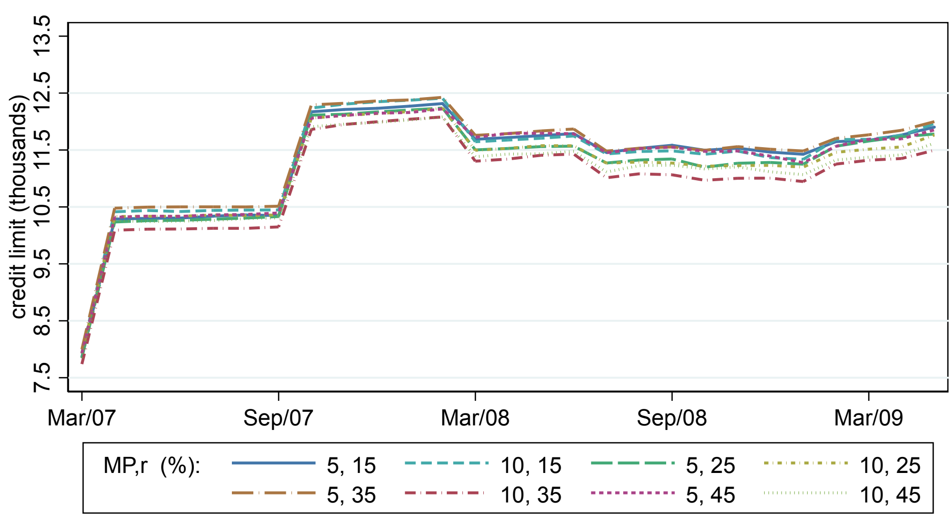
### H.3 Credit Limits Are Orthogonal to Randomization

**Table OA-11:** Credit Limits and Treatment Arms

| | Card Limit | |
|---|---|---|
| | (1) | (2) |
| r = 15, MP = 5 | 45 | 37 |
| | (210) | (210) |
| r = 15, MP = 10 | 41 | 43 |
| | (218) | (218) |
| r = 25, MP = 5 | -84 | -89 |
| | (209) | (209) |
| r = 25, MP = 10 | -108 | -103 |
| | (211) | (211) |
| r = 35, MP = 5 | 119 | 116 |
| | (220) | (220) |
| r = 35, MP = 10 | -312 | -305 |
| | (208) | (208) |
| r = 45, MP = 10 | -227 | -216 |
| | (209) | (209) |
| Constant (r = 45, MP = 5) | 11,778*** | 11,780*** |
| | (157) | (157) |
| Time fixed effects | No | Yes |
| Observations | 3,201,085 | 3,201,085 |
| p-value Treatments | 0.438 | 0.486 |
| p-value Strata | 0.000 | 0.000 |
| R-squared | 0.021 | 0.030 |
| Dependent Variable Mean | 11157 | 11157 |

*Notes:* Each column represents a different regression. The dependent variable is credit limit in month $t$ for individual $i$. Independent variables comprise treatment and strata indicators. Column (2) adds month fixed effects. Robust standard errors clustered at the individual level are shown in parenthesis. One, two and three stars denote statistical significance at the 5, 1 and 0.1 percent level respectively.

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

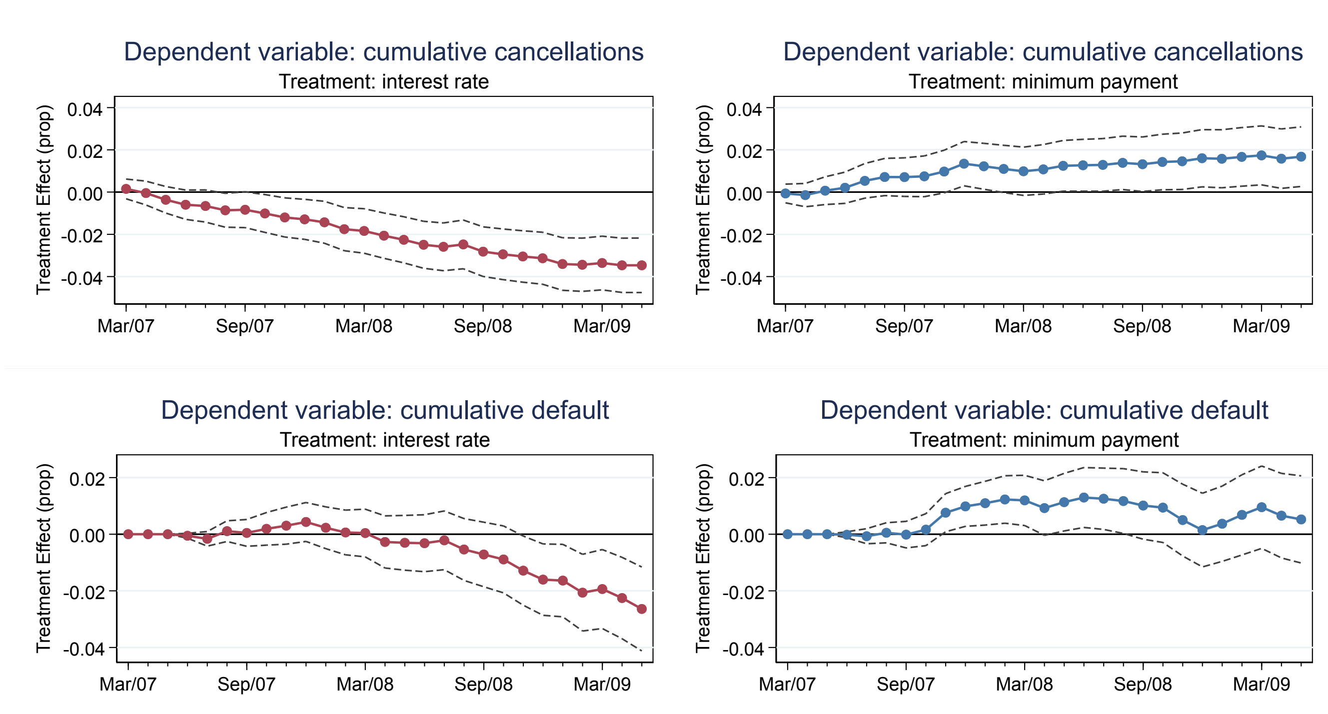


# Appendix I. Experiment Results: Dynamics in Treatment Effects

Figure OA-18 examines the evolution of treatment effects over the entire 26 month period by plotting the regression coefficients from estimating equation (2) month-by-month. The default measure at time $t$ is a cumulative measure: i.e. $Y_{it} = 1$ if $i$ has defaulted at any point up to $t$. Recall that it takes 3 *consecutive* months of delinquency for the loan to be classified in default. This plus the fact that the borrower may have some savings to cushion increases in minimum payments and interest rates means that it could take time for changes to manifest in default.

Overall we find zero or very small effects in the long run and in the short run for default, and somewhat larger — but still small— effects for cancellations.

**Figure OA-18:** Default/Revocation and Client Initiated Cancellations




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

# Appendix J. Experiment Results: Debt, Payment, Purchases

## J.1 Methodology

In Section 3.3 card exit was an outcome of interest in itself; here we view card exit as a threat to the internal validity. Specifically, we wish to account for card exit as we examine the effect of the experimental interventions on debt, purchases and payments. We attempt to address attrition in a number of ways: First, we implement Lee (2009) and present upper and lower bounds on treatment effects that account for attrition. These bounds are generally wide but for the most part still informative. Second, we present month-by-month treatment effects and because card-exit is low in the initial months, our short-term estimates are much less affected by attrition bias. Finally, in some cases (i.e. for card cancellations) it seems plausible to impute a value of zero to outcomes in the periods after card exit. Such a strategy is useful when we are interested in the effects of the treatment on the outcome without distinguishing between the extensive and intensive margins.

We present both short-term (at the six month horizon) as well as long-term effects (after 26 months at the end of the experiment). We also present month-by-month treatment effects for each of the 26 months of the experiment.[64] In addition, when useful, we also examine treatment effect heterogeneity by presenting stratum-specific treatment effects for three strata – (a) the "Full, 24M+" stratum comprising borrowers who had been with the bank for at least 24 months before January 2007 and had always paid their bills in full (4% of the population) (b) the "Min, 6-11M" stratum consisting of borrowers who had been with the bank for less than a year before January 2007 and had the poorest repayment history[65] (10% of the population) and(c) the "Min, 24M+" stratum comprising the longest term borrowers in the poorest repayment category (62% of the population and the largest stratum).

For each estimand we present point estimates and account for attrition using bounds. We view attrition in two distinct ways and thus provide two sets of bounds – first, we consider all card exits regardless of reason (i.e. cancellations, revocations and the other category) as attrition. Second, we set all post-exit outcomes for card cancellers to zero and only consider the defaulters and other category of card exits to be attriters. The latter strategy is arguably justified if we are willing to conflate treatment effects on the extensive and intensive margins. Further, since card cancellers have chosen to set purchases, payments and debt to zero by exiting the system one can plausibly set those outcomes to zero for cancellers rather than missing.[66]

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

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

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

where $Y_i$ is the outcome measured either six months after the experiment began or in the last month of the

[64] These are currently presented in graphical form. Tables available upon request.
[65] viz. their average payments prior to January 2007 were less than 1.5 times the average minimum payments during this period.
[66] A similar argument is harder to justify for defaulters.

experiment. The $\{T_{ji}\}_{j=1}^{7}$ are treatment dummies for each of 7 intervention arms. The omitted arm is the $(MP = 5\%, r = 45\%)$ arm since it is the group with terms closest to the status quo and we do not use the control group.[67] We include strata dummies $\{S_{ji}\}_{j=1}^{9}$ and probability weights in all specifications.[68]

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

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

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

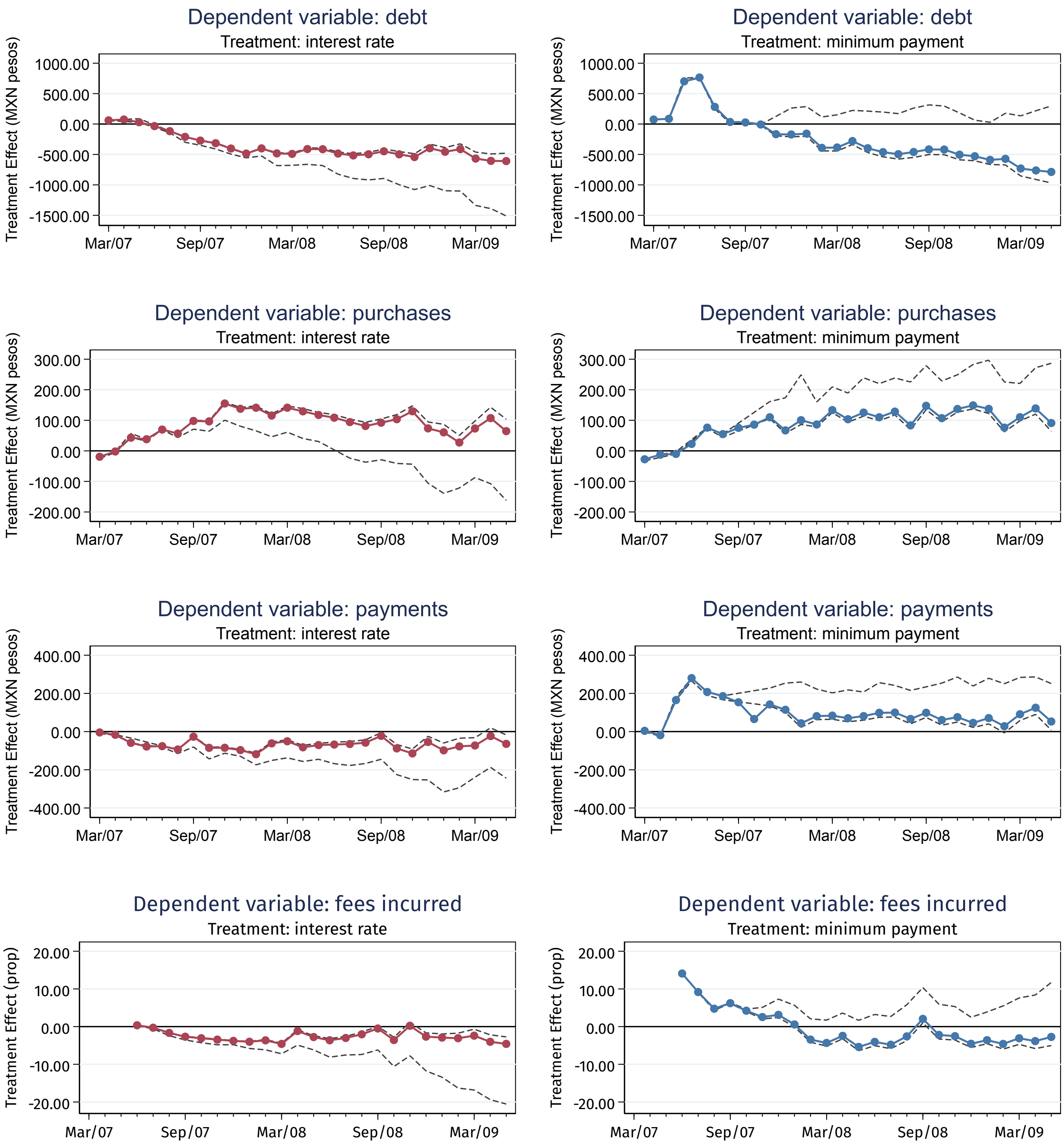
and in both cases the excluded arm is the $(45\%, 5\%)$ arm.[69] We then graph the estimates of $\beta_{1t}$ and $\beta_{2t}$ against time along with the corresponding Lee bounds in Figure OA-19. This is a parsimonious way of presenting the numerous treatment effects as well as allowing the reader to trace the evolution of the treatments over time. In most of the graphs, the bounds are typically tight for the first 6 months – reflecting limited attrition – and the point estimates at six months are of the same sign and typically the same order of magnitude as the long term (26 month) effects. Having described the general methodology we next turn to describing the effects of the interventions – first on debt and then on purchases, payments and fees.

[67]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.

[68]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.

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

**Figure OA-19:** Treatment effect estimates




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

## J.2 Debt: Effect of Interest Rate Decrease

Debt responses to the interest rate changes follow an interesting and, at first-glance, a somewhat counter-intuitive pattern. Figure OA-19 shows that interest rate increases result in a steady, gradual *decline* in debt (even after accounting for attrition). At the six-month mark, with relatively limited attrition, the implied elasticity bounds are relatively tight at $[0.28, 0.42]$.[70] The bounds begin to widen after the first year but remain consistently negative and even the upper bounds suggest reasonable sized treatment effects. At endline, the upper bound is a decline of 474 pesos and the lower bound is a decline of 1576 pesos. These final bounds imply a strictly positive elasticity ranging from $+0.34$ to $+1.12$ respectively. Replacing missing values with zeros for card cancellers provides similar results though the upper bound is now tighter at $+0.74$. These results suggest a robust, negative effect of interest rate reductions on total debt.[71]

The treatment effects for the other intermediate treatment arms are in line with these results. We compare debt for the $(45, 5)$ group to the $(r, 5)$ group where $r \in \{25, 35\}$ and debt in the $(45, 10)$ group to the $(r, 10)$ group where $r \in \{15, 25, 35\}$. The five ITT estimates are all comparable to the estimate above.[72] The implied elasticities of debt with respect to the interest rate from the five other ITT estimates thus are also in line with the elasticities from the primary contrast.

The negative effect of interest rate declines on debt seems counter-intuitive since borrowers appear to respond to price (interest rate) declines by decreasing quantities (debt). We explore this further by examining the effect of interest rates on purchases, payments and fees which together mechanically determine debt. In Appendix J.4 and J.6 we establish three facts about these outcomes. First, interest rate declines have inconclusive effects on purchases with the Lee bounds for the long-term effect being a relatively wide $[-0.38, +0.25]$.[73] Second, monthly payments declined modestly in response to the interest rate decreases with the long-term bounds estimated to be $[+0.04, +0.39]$.[74] Third, interest rate declines have a modest negative effect on fees (the Lee bounds for the implied elasticity are $[+0.15, +1.22]$).

Jointly, these facts suggest that the relatively large negative debt response to interest rate declines arises from the fact that lower interest rates result in debt outstanding being compounded at a correspondingly lower rate.[75] This decline more than offsets any increase in purchases as well as the decline in monthly payments observed earlier. To summarize, there is a fairly robust, though moderate, decline in total debt outstanding as a result of the interest rate decrease.

## J.3 Debt: Effect of Minimum Payments

Debt response to the minimum payment increase follows an interesting pattern. Figure OA-19 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

[70] 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.

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

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

[73] The short-term effects have tighter bounds of $[-0.38, -0.18]$ that suggest modest increases in purchases. More details are in Table OA-13.

[74] Bounds for the short-term are qualitatively similar at $[+0.06, +0.24]$. See Table OA-14 for more details.

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

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

Part of the increase in debt in the first months of the experiment appears to arise from late payment fees.[76] Following that, debt decreases gradually for the rest of the experiment though the Lee bounds become increasingly wide so that by the end of the experiment we cannot rule out declines (971 pesos or an elasticity of -0.46) or increases (326 pesos or an elasticity of +0.15). In the case of debt, imputing a value of zero for all cancellers is a particularly reasonable approach if policy makers are interested in the overall effect of minimum payments on debt, not distinguishing between borrowers who remain with the card and accumulate (or decumulate) debt or borrowers who cancel their card and cannot by definition accumulate any more debt with the card. This approach yields qualitatively similar results and the bounds for the implied elasticity tighten on the upper end so that the new bounds are $[-0.44, -0.01]$. These results suggest that doubling the minimum payment had a statistically significant, modest effect on overall debt.

[76]The late payment fee is 350 pesos for any payment less than the minimum required payment. We analyzed the long term effects of fees (results available upon request) and note that most of the increases in fees occurred in in the first few months of the experiment. Unfortunately, we do not have information on fees for the first three months of the experiment.

**Table OA-12:** Treatment Effects on Debt with Bank A

| | Standard Outcome | | | Deflated by Amount Due in $t-1$ | | Selected Strata (May/09) | | |
|---|---|---|---|---|---|---|---|---|
| | Sep/07 (1) | May/09 (2) | May/09 w/zeros (3) | Sep/07 (4) | May/09 (5) | Min.Pay,6-11M (6) | Full Pay,24+M (7) | Min.Pay, 24+ M (8) |
| r = 15, MP = 5 | -271.306* | -602.941*** | -419.136*** | -0.016* | -0.021** | -632.527* | -59.693 | -684.244*** |
| | (82.179) | (62.698) | (40.367) | (0.006) | (0.006) | (254.239) | (80.647) | (189.418) |
| r = 15, MP = 10 | -131.825** | -908.277*** | -726.710*** | 0.004 | -0.008** | -1.3e+03*** | -98.833 | -968.263*** |
| | (37.038) | (74.059) | (59.372) | (0.003) | (0.002) | (246.008) | (78.507) | (181.610) |
| r = 25, MP = 5 | -123.728*** | -318.241*** | -199.647*** | -0.002 | -0.012** | -160.146 | -33.820 | -326.770 |
| | (9.845) | (26.464) | (26.283) | (0.001) | (0.003) | (271.326) | (91.550) | (212.753) |
| r = 25, MP = 10 | -76.255*** | -860.327*** | -704.486*** | 0.008 | -0.001 | -1.1e+03*** | -179.102* | -924.682*** |
| | (13.474) | (60.496) | (49.118) | (0.003) | (0.002) | (251.472) | (71.428) | (181.226) |
| r = 35, MP = 5 | -14.085 | -332.818** | -228.272** | 0.010 | -0.011 | -98.670 | -70.836 | -444.117* |
| | (19.275) | (85.630) | (61.025) | (0.005) | (0.005) | (269.839) | (88.442) | (196.150) |
| r = 35, MP = 10 | -95.723** | -680.189*** | -556.369*** | 0.004* | -0.009* | -1.0e+03*** | 52.546 | -723.580*** |
| | (23.358) | (57.504) | (44.952) | (0.002) | (0.003) | (256.232) | (97.176) | (191.343) |
| r = 45, MP = 10 | 24.243 | -804.015*** | -699.266*** | 0.007* | -0.018* | -750.309** | -204.448** | -908.631*** |
| | (46.438) | (78.336) | (63.856) | (0.003) | (0.006) | (263.820) | (66.603) | (183.892) |
| Constant (r = 45, MP = 5) | 1408.794*** | 2117.133*** | 1735.354*** | 0.091*** | 0.091*** | 3432.694*** | 413.443*** | 2174.629*** |
| | (218.089) | (165.882) | (139.343) | (0.012) | (0.006) | (198.896) | (58.580) | (151.527) |
| Observations | 134,385 | 87,093 | 105,180 | 120,189 | 76,082 | 7,820 | 10,948 | 9,839 |
| R-squared | 0.001 | 0.005 | 0.004 | 0.001 | 0.001 | 0.008 | 0.001 | 0.005 |
| Lee Bounds IR | [-397.281, -266.049] | [-1.6e+03, -473.775] | [-851.598, -388.340] | [ -0.021, -0.016] | [ -0.091, -0.014] | [-1.8e+03, -385.538] | [-379.885, -45.816] | [-1.8e+03, -529.896] |
| Lee Bounds MP | [ 21.827, 106.293] | [-971.173, 326.368] | [-766.284, -0.595] | [ 0.005, 0.027] | [ -0.023, 0.027] | [-1.1e+03, 894.767] | [-205.675, -135.592] | [-1.1e+03, 244.803] |
| $\epsilon$ 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 (26 months). Columns (2) and (5) present OLS results on the non-attriters and account for attrition by presenting Lee bounds (bottom 4 rows). The Lee bounds compare the (r=15, MP=5) and (r=45,MP=10) arms against the (r=45, MP=5) arm. Columns (3) and (6) redo the analysis by assigning a zero to card cancellers post exit. Columns (7),(8) and (9) estimate the endline regressions for three different strata – (a) "Min, 6-11M" borrowers who were with the bank for less than a year in January 2007 and were in the lowest payment category ;(b) "Full,24M+" who had been with the bank for more than 2 years by January 2007 and had were in the highest payment category; (c) "Min,24M+" borrowers who had been with the bank for more than 2 years by January 2007 and were in the lowest payment category. Standard errors are shown in parentheses. One, two and three stars denote statistical significance at the 5, 1 and 0.1 percent level respectively.

### J.4 Purchases: Effect of Interest Rates

We begin by examining the effect of the experimental variation in interest rates on purchases in Figure OA-19 and Table OA-13. Figure OA-19 shows monthly treatment effects over the course of the experiment and Table OA-13 presents short- and long-term regression results accounting for attrition. We see in Figure OA-19 that purchases in the $15\%$ arm grew gradually (relative to the $45\%$ arm) over the first year or so of the experiment. The Lee bounds during the first six months of the intervention are quite tight and the bounds for the implied short-term elasticity (bottom of Table OA-13 col (1)) are $[-0.38, -0.18]$ indicating modest effectsc. The long-term results, however, are inconclusive. Attrition starts to widen the bounds particularly after the first year and by the end of the experiment we cannot rule out increases in monthly purchases of 104 pesos or *declines* of 192 pesos. These imply correspondingly wide bounds on the elasticity ranging from -0.38 to +0.69 respectively (bottom of Table OA-13 col (2)). Imputing zeros to purchases for all card cancellers reduces the upper bound, but it remains positive (bottom of Table OA-13 col (3)).

The long-term elasticity bounds are wide but even at the lower bound they are substantially smaller (in absolute value) than those found in other developing country studies that examine the effect of interest rate changes on total loan quantity.[77] For instance, Karlan and Zinman (2017) compute a two year elasticity of $-2.9$ of loan quantity with respect to interest rate in an experiment in Mexico with *Compartemos*. Gross and Souleles (2002) estimate a still high elasticity of $-1.3$ for credit-card holders in the United States using observational data. Dehejia et al. (2012) use plausibly exogenous geographic variation in interest rates to estimate slightly lower but still significant elasticities in the range of $(-1.04, -0.73)$ for micro-credit borrowers in Bangladesh. Our long-term lower-bound is close to the elasticity of $-0.32$ documented by Karlan and Zinman (2008) for short-term individual loans in South Africa and also the approximately zero elasticity for auto-loans documented in Attanasio et al. (2008).

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

**Table OA-13:** Treatment Effects on Monthly Purchases

| | Standard Outcome | | | Deflated by Amount Due in $t-1$ | | Selected Strata (May/09) | | |
|---|---|---|---|---|---|---|---|---|
| | Sep/07 | May/09 | May/09 w/zeros | Sep/07 | May/09 | Min.Pay,6-11M | Full Pay,24+M | Min.Pay, 24+ M |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (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 payments at the end of the experiment (26 months). Columns (2),(4)–(8) drop all card exits and the Lee Bounds are more informative than the point-estimates for these columns. The Lee bounds compare the (r=15, MP=5) and (r=45,MP=10) arms against the (r=45, MP=5) arm. Column (3) assigns a zero for all outcomes for card cancellers and the resulting Lee bounds are tighter than in Column (2). Columns (6)-(8) estimate endline regressions for three different strata: (a) "Min Payers, 6-11M" borrowers who were with the bank for more than six months but less than a year in January 2007 and were in the lowest payment category ;(b) "Full Payers,24M+" borrowers who had been with the bank for more than 2 years by January 2007 and were in the highest payment category; (c) "Min Payers,24M+" borrowers who had been with the bank for more than 2 years by January 2007 and were in the lowest payment category at baseline. Standard errors are shown in parentheses. One, two and three stars denote statistical significance at the 5, 1 and 0.1 percent level respectively.

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

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

### J.5 Purchases: Effects of Minimum Payments

Doubling the minimum payment led to an *increase* in monthly purchases. Figure OA-19 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.[78] Alternatively, since higher minimum payments imply, ceteris paribus, a decrease in debt, the increase in purchases may reflect changes in borrower behavior as a result of reduced debt. This argument implies that the effect of minimum payments on purchases should be higher for borrowers who see larger reductions in debt. We explore this implication by examining the changes in purchases across the various interest rate arms keeping the required payment fixed at $10\%$.

Finally, as expected, the "Full,24M+" stratum is largely unaffected by the minimum payment increase throughout the intervention while the effect is stronger for the "Min,12M–" stratum and the bounds for the implied elasticities are consistent with both modest ($0.24$) and substantive ($1.14$) effects. Finally, we also nor-

[78]Results available upon request.

malized monthly purchases by expressing purchases as a fraction of amount due (cols (4) and (5) of Table OA-13) 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.

### J.6 Payments: Effect of Interest Rates

Figure OA-19 presents the Lee bounds along with the point estimates from equation (8) for each month in the experiment. We see that there is a gradual decline in monthly payments during the first six months and the bounds at the six-month mark are $[-103, -24]$ pesos with implied elasticity bounds of $[.06, .24]$ suggesting relatively modest *declines* in payments.

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

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

### J.7 Payments: Effect of Minimum Payment

It is reasonable to expect that the most direct effect of the minimum payment intervention would be on monthly payments. Figure OA-19 documents a sharp increase in monthly payments in the treatment group in the third month of the experiment[79] (May 2007) and after a small increase in the next month there is a steady decline over the remainder of the experiment. The six month treatments effects are precisely estimated and the Lee Bounds for the implied elasticity are very tight at $[.24, .29]$ suggesting small, though robust, effects of the increase in required payments. The bounds then begin to widen considerably starting in the last months of 2007 and remain relatively wide throughout the remainder of the experiment. By the end of the experiment attrition widens the bounds considerably and the bounds for the implied elasticity, while still positive, range from $0.01$ to $0.48$. Imputing zero values to card cancellations provides qualitatively similar results with the upper bound tightened to $0.37$. These bounds indicate that the implied effects, even at the upper bound, are relatively small in substantive terms. We also consider the effect of the treatment on monthly payments measured as a fraction of the amount due in each month. The results suggest are broadly similar to the previous analysis with the short term bounds on the elasticity being $[0.24, 0.35]$ and the long-term bounds are somewhat wider at $[.16, .58]$. The patterns of heterogeneity in treatment effects are as expected with no effects on the "Full, 24M+" stratum and larger effects for the other strata particularly the "Min,12M–" stratum though even in that case the effects are not particularly large.

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

## **Table OA-14:** 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 (26 months). Columns (2),(5)–(8) drop all card exits and the Lee Bounds are more informative than the point-estimates for these columns. The Lee bounds compare the (r=15, MP=5) and (r=45,MP=10) arms against the (r=45, MP=5) arm. Column (3) assigns a zero for all outcomes for card cancellers and the resulting Lee bounds are tighter than in Column (2). Columns (7)-(9) estimate endline regressions for three different strata: (a) "Min Payers, 6-11M" borrowers who were with the bank for more than six months but less than a year in January 2007 and were in the lowest payment category ;(b) "Full Payers,24M+" borrowers who had been with the bank for more than 2 years by January 2007 and were in the highest payment category; (c) "Min Payers,24M+" borrowers who had been with the bank for more than 2 years by January 2007 and were in the lowest payment category at baseline. Standard errors are shown in parentheses. One, two and three stars denote statistical significance at the 5, 1 and 0.1 percent level respectively.

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

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

# Appendix K. Experiment Results: Comparison with other studies

**Table OA-15:** Comparisons with the Literature

| Paper | Outcome | Table (page) | Point Estimate (S.E.) | Elasticity (S.E.) |
|---|---|---|---|---|
| Karlan and Zinman (2009) | Account in Collection | 3 (p.37) | -1.60 (1.58) | +0.27 (0.14) |
| Karlan and Zinman (2017) | Delinquency | 5 (p.42) | -1.96 (1.45) | +1.80 |
| Adams et al. (2009) | Default Hazard | 4 (p.28) | 1.022 (.002) | +2.2 (0.2) |
| Keys and Wang (2019) | Delinquency | 2 (p.542) | .4 (.3) | +0.01 (.12) |
| d'Astous and Shore (2017) | Default | p.3 | .04 | +0.06 |

*Notes:* We use the working paper version of Karlan and Zinman (2009). Table 3 cols (4) and (5) for the "repayment burden effect." The table reports a decline from 13.9 to 12.3 in the percentage of accounts in collection status over a four month period. The difference between the high and the low interest rate was on average 350 basis points. We use the high risk category upper bound for the interest rate of 11.75 percent as the base rate and convert the monthly interest rates to APR to facilitate comparisons (the calculation is $(-1.6/13.9)(279/-120) = .27$). For Karlan and Zinman (2017) we use the results from Table 5 (col (4), Panel B) that shows delinquencies decline by 1.96 percentage points off of a control baseline of 10.5%. Low rate regions faced APRs of 80% while high rate regions faced APRs of 90%. The implied elasticity is $(-2/10)/(80-90/90) = 1.8$. We could not find the required information in the paper to compute standard errors for the implied elasticities. Adams et al. (2009) estimate a hazard model and the hazard rate suggests that a one percent increase in the APR leads to a 2.2 percent increase in the hazard rate of default. Keys and Wang (2019) find an insignificant increase in delinquency of .4 percent (relative to a base rate past due rate of 8 percent) due to a minimum payment change on average of 1% (off a base minimum payment average of 2%). d'Astous and Shore (2017) study changes in minimum payments while the remaining papers examine interest rate variation (standard errors not available). Standard errors for elasticities are computed using the delta method.

# Appendix L. Experiment Results: Spillovers (Treatment Effects on Other Loans)

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

Table OA-16 shows that this is not the case. Neither default, cancellations nor new borrowing, (all measured in June 2009) with all other formal lenders respond to the changes in minimum payments and interest rates. Although borrowers are more likely to cancel the study card in response to higher minimum payments, there is no substitution towards other formal credit sources.

We also attempt to test the hypothesis that lower interest rates during the experiment led to a greater engagement with formal credit. We were able to obtain information on the experimental sample from the credit bureau in June 2012, three years after the experiment ended. Columns 1 and 2 of Table 3 show the extensive margin treatment effects on other formal sector borrowing at that time. Consistent with the previous findings, we find that being exposed to lower interest rates or higher minimum payments on the study card did not (at a five year horizon) lead to a greater number of loans or interactions with a larger number of lenders.[80] Robust standard errors are shown in parenthesis.

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

**Table OA-16:** Treatment Effects on Other non-Card Loans
(Existing loans by March 2007, Outcomes measured in June 2009, Any Loan Type)

| | Default | | | Cancellations | | | New loan | | |
|---|---|---|---|---|---|---|---|---|---|
| | Any Bank (1) | Same Bank (2) | Other Bank (3) | Any Bank (4) | Same Bank (5) | Other Bank (6) | Any Bank (7) | Same Bank (8) | Other Bank (9) |
| r = 15, MP = 5 | 0.006 | -0.004 | 0.002 | 0.017* | 0.005 | 0.013 | 0.011 | 0.011 | 0.009 |
| | (0.010) | (0.009) | (0.010) | (0.007) | (0.003) | (0.007) | (0.010) | (0.007) | (0.010) |
| r = 15, MP = 10 | -0.006 | -0.017 | -0.001 | 0.016* | 0.006* | 0.011 | 0.018 | 0.009 | 0.011 |
| | (0.010) | (0.009) | (0.010) | (0.007) | (0.003) | (0.007) | (0.010) | (0.007) | (0.010) |
| r = 25, MP = 5 | -0.002 | -0.014 | 0.003 | 0.005 | 0.002 | 0.003 | 0.009 | 0.003 | 0.003 |
| | (0.010) | (0.009) | (0.010) | (0.007) | (0.003) | (0.007) | (0.010) | (0.007) | (0.010) |
| r = 25, MP = 10 | -0.001 | -0.008 | 0.002 | 0.003 | 0.006* | -0.002 | 0.013 | 0.004 | 0.006 |
| | (0.010) | (0.009) | (0.010) | (0.007) | (0.003) | (0.006) | (0.010) | (0.007) | (0.010) |
| r = 35, MP = 5 | -0.002 | -0.003 | 0.001 | 0.004 | 0.003 | 0.002 | 0.010 | 0.003 | 0.005 |
| | (0.010) | (0.009) | (0.010) | (0.007) | (0.003) | (0.006) | (0.010) | (0.007) | (0.010) |
| r = 35, MP = 10 | 0.007 | 0.003 | -0.001 | 0.006 | 0.002 | 0.005 | -0.003 | 0.002 | -0.007 |
| | (0.010) | (0.009) | (0.010) | (0.007) | (0.003) | (0.007) | (0.010) | (0.007) | (0.010) |
| r = 45, MP = 10 | -0.014 | -0.010 | -0.010 | 0.009 | 0.000 | 0.009 | -0.004 | -0.008 | 0.001 |
| | (0.010) | (0.009) | (0.010) | (0.007) | (0.003) | (0.007) | (0.010) | (0.007) | (0.010) |
| Constant (r = 45, MP = 5) | 0.560*** | 0.336*** | 0.449*** | 0.137*** | 0.021*** | 0.121*** | 0.541*** | 0.165*** | 0.490*** |
| | (0.007) | (0.007) | (0.007) | (0.005) | (0.002) | (0.005) | (0.007) | (0.005) | (0.007) |
| Observations | 143,916 | 143,916 | 143,916 | 143,916 | 143,916 | 143,916 | 143,916 | 143,916 | 143,916 |
| R-squared | 0.009 | 0.033 | 0.004 | 0.001 | 0.001 | 0.001 | 0.001 | 0.005 | 0.000 |

*Notes:* All regressions include strata dummies and use sample weights. The regressions include all loan types (mortgage, auto loan, credit card, etc). The dependent variable for Columns (1) to (3) is default (bank-initiated revocations). The dependent variable for Columns (4) to (6) are (client-initiated) cancellations. The dependent variable for Columns (7) to (9) are new loan originations after March 2007. All columns exclude the experimental card. Columns (1), (4) and (7) refer to loans issued by any bank. Columns (2), (5) and (8) refer to loans issued by the same bank as the experimental card (i.e. Bank A). Columns (3), (6) and (9) refer to loans issued by any bank except for Bank A. All dependent variables restrict to loans that were issued on or before by March 2007 that remained active by March 2007. All outcomes are measured in June 2009, one month after the experiment ended. Robust standard errors are shown in parenthesis. One, two and three stars denote statistical significance at the 5, 1 and 0.1 percent level respectively.

**Table OA-17:** Treatment Effects on Other Credit Cards
(Existing loans by March 2007, Outcomes measured in June 2009)

| | Default | | | Cancellations | | | New card | | |
|---|---|---|---|---|---|---|---|---|---|
| | Any Bank (1) | Same Bank (2) | Other Bank (3) | Any Bank (4) | Same Bank (5) | Other Bank (6) | Any Bank (7) | Same Bank (8) | Other Bank (9) |
| r = 15, MP = 5 | 0.004 | 0.007 | -0.001 | 0.011 | 0.002 | 0.010 | 0.011 | 0.008 | 0.012 |
| | (0.010) | (0.008) | (0.010) | (0.006) | (0.003) | (0.006) | (0.010) | (0.007) | (0.010) |
| r = 15, MP = 10 | -0.009 | -0.012 | -0.002 | 0.016** | 0.005 | 0.011* | 0.011 | 0.009 | 0.010 |
| | (0.010) | (0.008) | (0.010) | (0.006) | (0.003) | (0.006) | (0.010) | (0.007) | (0.010) |
| r = 25, MP = 5 | 0.001 | -0.002 | 0.002 | 0.004 | 0.001 | 0.002 | 0.003 | 0.001 | 0.003 |
| | (0.010) | (0.008) | (0.010) | (0.006) | (0.003) | (0.005) | (0.010) | (0.007) | (0.010) |
| r = 25, MP = 10 | -0.006 | -0.002 | -0.006 | 0.003 | 0.003 | -0.000 | 0.003 | 0.001 | 0.005 |
| | (0.010) | (0.008) | (0.010) | (0.006) | (0.003) | (0.005) | (0.010) | (0.007) | (0.010) |
| r = 35, MP = 5 | -0.003 | 0.000 | -0.002 | 0.002 | 0.000 | 0.002 | 0.006 | 0.001 | 0.005 |
| | (0.010) | (0.008) | (0.010) | (0.006) | (0.003) | (0.005) | (0.010) | (0.007) | (0.010) |
| r = 35, MP = 10 | -0.001 | 0.001 | -0.004 | 0.006 | 0.000 | 0.006 | -0.005 | -0.001 | -0.002 |
| | (0.010) | (0.008) | (0.010) | (0.006) | (0.003) | (0.005) | (0.010) | (0.007) | (0.010) |
| r = 45, MP = 10 | -0.026** | -0.016* | -0.017 | 0.007 | -0.000 | 0.006 | -0.017 | -0.008 | -0.008 |
| | (0.010) | (0.008) | (0.010) | (0.006) | (0.003) | (0.006) | (0.010) | (0.007) | (0.010) |
| Constant (r = 45, MP = 5) | 0.498*** | 0.222*** | 0.428*** | 0.096*** | 0.018*** | 0.082*** | 0.424*** | 0.137*** | 0.366*** |
| | (0.007) | (0.006) | (0.007) | (0.004) | (0.002) | (0.004) | (0.007) | (0.005) | (0.007) |
| Observations | 143,916 | 143,916 | 143,916 | 143,916 | 143,916 | 143,916 | 143,916 | 143,916 | 143,916 |
| R-squared | 0.007 | 0.033 | 0.004 | 0.002 | 0.001 | 0.002 | 0.002 | 0.005 | 0.001 |

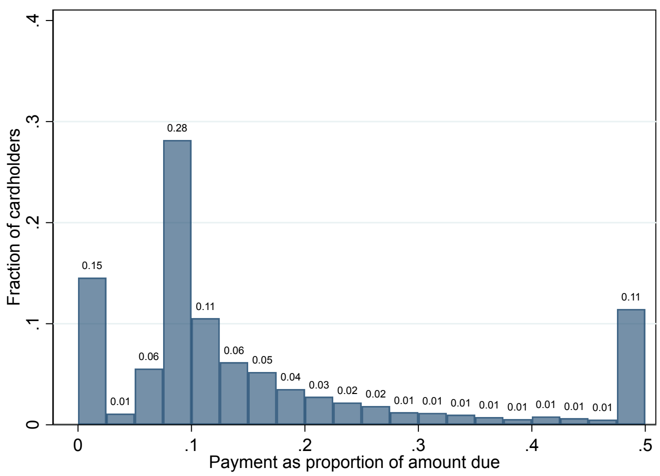
*Notes:* All regressions include strata dummies and use sample weights. This is the analogous table to Table OA-16 but restricted exclusively to credit cards. The dependent variable for Columns (1) to (3) is default (bank-initiated revocations). The dependent variable for Columns (4) to (6) are (client-initiated) cancellations. The dependent variable for Columns (7) to (9) are new loan originations. All columns exclude the experimental card. Columns (1), (4) and (7) refer to loans issued by any bank. Columns (2), (5) and (8) refer to loans issued by the same bank as the experimental card (i.e. Bank A). Columns (3), (6) and (9) refer to loans issued by any bank except for Bank A. All dependent variables restrict to loans that were issued on or before by March 2007 that remained active by March 2007. All outcomes are measured in June 2009, one month after the experiment ended. One, two and three stars denote statistical significance at the 5, 1 and 0.1 percent level respectively.

# Appendix M. Habit formation

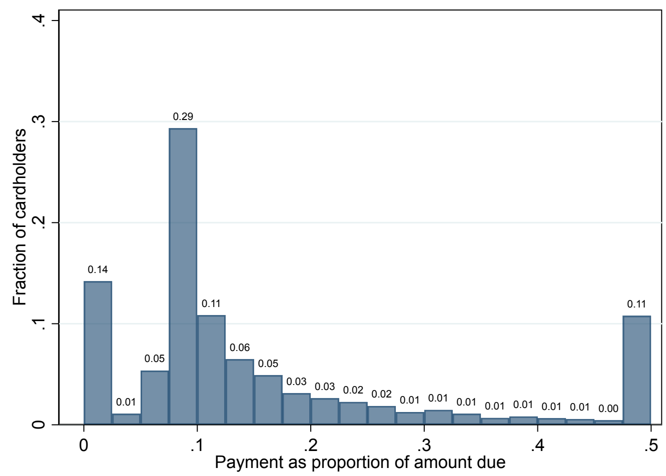
## M.1 Comparison of min. payment across treatment arms 3 years after the experiment ended

**Figure OA-20:** Payment as a fraction of debt 3 years after the experiment

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




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




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

## M.2 Habit formation regressions

**Table OA-18:** Habit formation regressions

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

*Notes:* Robust standard errors are shown in parenthesis. The sample is those cards that (i) participated in the experiment (ii) remained opened by 2010, and (iii) were assigned to either the highest or lowest interest rate groups (eg. [r = 15, MP = 5], [r = 15, MP = 10], [r = 45, MP = 5], and [r = 15, MP = 10]). Each column represents a different regression. Columns (2), (4) and (6) have as a dependent variable the amount paid on June 2010, as a function of the minimum payment that was assigned during the experiment and debt. Since debt can be endogenous, we instrument for debt using the interest rate group cardholders were assigned to. We also allow for a differential treatment effect for those in the "minimum-payment" strata. The dependent variable of Columns (1), (3) and (5) is the amount due on June 2010. Columns (1) and (2) show the regression equations without additional controls. Columns (3) and (4) add the months with credit cards strata dummies. Columns (5) and (6) add both the months with credit cards strata dummies as well as current contract terms, namely the interest rate and the required minimum payment in pesos in June 2010. One, two and three stars denote statistical significance at the 5, 1 and 0.1 percent level respectively.

## Appendix N. Mechanisms

### N.1 Severe Consequences of Default May Limit Moral Hazard

In principle, the high default rates could reflect the fact that borrowers do not value the study card (or have a low cost of default). In this section we document that default has serious consequences for NTB borrowers, which may explain why default responses were limited. We provide evidence for three claims: first, NTB borrowers are credit constrained in the formal sector and therefore presumably value formal sector credit. Second, default sharply reduces access to subsequent formal sector credit. Third, informal credit terms are substantially worse than formal credit terms. These claims together suggest that the range of variation in contract terms, even though substantial, was perhaps "infra-marginal" in the light of these concerns and so borrower responses were limited.

*A. NTB borrowers quickly use increases in lines*

One prediction of borrowers having a strong need for credit is that they would quickly use any increases in credit lines. We find that this is indeed the case. Using the methodology proposed in Gross and Souleles (2002), we use exogenous credit limit changes (given by the timing since last credit limit change) to estimate how debt changes when credit lines change. We find that a credit limit increase of 100 pesos on the study card translates into 32 pesos of subsequent additional debt (see Appendix E.). For comparison, this propensity to consume out of increases in the credit limit is about thrice as large as the figure for the United States.[81]

*B. Default Reduces Access to Formal Credit*

Default is associated with large declines in subsequent formal sector borrowing. Using the experimental sample we estimate a cross-sectional regression where the primary explanatory variable is an indicator if a borrower $i$ defaulted on the study card in the six months after the start of the experiment (i.e. between March and September 2007) and the dependent variable is an indicators for $i$ obtaining a new loan or card six,twelve, or forty eight months after September 2007. We include age, gender, and zip code indicators as controls, and restrict attention to the sub-sample for whom the study card was the first formal sector loan product and who had been with the bank for between 6 to 11 months at the start of the experiment. Panel A of Table OA-19 shows the results for all types of loans, while Panel B focuses only on credit cards. We further group columns by lender type (any lender, all lenders except Bank A, and Bank A).

[81] We observe both credit limit increases and decreases in the data so the parameter is estimated using both types of changes.

**Table OA-19:** Probability of getting a new loan or card against default

| | Any bank | | | Any bank except Bank A | | | Bank A | | |
|---|---|---|---|---|---|---|---|---|---|
| | September 07 up to | | | September 07 up to | | | September 07 up to | | |
| | Feb/08 | Aug/08 | Aug/11 | Feb/08 | Aug/08 | Aug/11 | Feb/08 | Aug/08 | Aug/11 |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) |
| *Panel A. Any loan* | | | | | | | | | |
| Default in Mar/07 - Aug/07 | -0.26*** | -0.33*** | -0.43*** | -0.21*** | -0.26*** | -0.37*** | -0.10*** | -0.15*** | -0.22*** |
| | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) | (0.03) | (0.01) | (0.02) | (0.02) |
| mean dep. var non-defaulters | 0.29 | 0.39 | 0.55 | 0.25 | 0.33 | 0.49 | 0.08 | 0.12 | 0.19 |
| Observations | 22,813 | 22,813 | 22,813 | 22,813 | 22,813 | 22,813 | 22,813 | 22,813 | 22,813 |
| R-squared | 0.363 | 0.366 | 0.370 | 0.363 | 0.361 | 0.369 | 0.346 | 0.359 | 0.365 |
| *Panel B. Credit cards only* | | | | | | | | | |
| Default in Mar/07 - Aug/07 | -0.24*** | -0.31*** | -0.43*** | -0.18*** | -0.24*** | -0.33*** | -0.09*** | -0.13*** | -0.20*** |
| | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) | (0.02) | (0.01) | (0.02) | (0.02) |
| mean dep. var non-defaulters | 0.23 | 0.30 | 0.42 | 0.19 | 0.25 | 0.35 | 0.07 | 0.11 | 0.18 |
| Observations | 22,813 | 22,813 | 22,813 | 22,813 | 22,813 | 22,813 | 22,813 | 22,813 | 22,813 |
| R-squared | 0.354 | 0.356 | 0.364 | 0.356 | 0.354 | 0.359 | 0.349 | 0.360 | 0.366 |

*Notes:* This table regresses measures of subsequent new card ownership against previous default on the study card. The sample consists of the set of borrowers with (a) the experimental card, that (b) belong to the 6-11 months strata, and (c) for whom the experimental card was their first formal loan. The observations are at the level of the card holder. Each column within each panel is a different regression. For all regressions the independent variable is equal to 1 if cardholder $i$ defaulted in the experimental card between the start of the experimental period and 6 months after the experiment started (March 2007 to August 2007). The dependent variable varies by column. For columns (1), (2) and (3) in Panel A, the dependent variable is an indicator variable equal to 1 if a borrower obtains a new loan (any kind of loan: mortgage, auto loan, credit card, etc) in any bank between the periods September 2007 and February 2007, August 2008, and August 2011 (6, 12, and 48 months). Columns (4), (5) and (6) repeat the exercise but restricting to loans with banks that are not Bank A, whereas Columns (7), (8) and (9) restrict to Bank A, exclusively. All regressions include postal code fixed effects, age, a male dummy, and a married dummy. Robust standard errors are shown in parentheses. One, two and three stars denote statistical significance at the 5, 1 and 0.1 percent level respectively.

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

One concern with the regression above is that omitted variables may drive both default and future loan demand. We attempt to address this by adding borrower and time fixed effects. This increase in flexibility forces us to restrict attention to delinquency as the primary outcome rather than default.[82] We continue to find a negative relationship between delinquency and subsequent borrowing. The rate at which borrowers get loans from any bank is 7 percentage points per month before being delinquent for the first time, but only 5 percentage points after the first delinquency. Borrowers cease to obtain any subsequent additional credit from Bank A following the first delinquency.

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

**Table OA-20:** Access to loans after the first delinquency

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

*Notes:* This table focuses on the sample of borrowers on the experimental sub-sample for whom the study card was the first formal sector loan product and who had been with Bank A between 6 to 11 months at the start of the experiment. We observe 55 months of data, from March/07 to Sept/11. We further restrict the sample to borrowers who defaulted in this period. This leaves us with 6,441 borrowers. For each of those borrowers we locate the first month they were delinquent (i.e. 30 days past due) on the experimental card, and create an indicator for any time period after this first delinquency I(After 1st Del for $i)_{it}$. We estimate by OLS the regression $y_{it} = \alpha_i + \gamma_t + \beta$ I(After 1st Del for $i)_{it} + \epsilon_{it}$, where $y_{it}$ is an indicator for borrower $i$ getting a new loan (any kind of loan) in period $t$ with any bank (column 1), non-Bank A (column 2), or Bank A (column 3). The table reports estimated $\beta$'s, as well as the mean of the dependent variable in the periods before default; $\beta$'s estimates the within borrower difference of the likelihood of get new loans in periods after delinquency compared to the likelihood of getting new loans before being delinquent, for the same borrower. One, two and three stars denote statistical significance at the 5, 1 and 0.1 percent level respectively.

We take the evidence above as being primarily suggestive, though the conclusion is unsurprising.[83] Decreased access to formal lending is precisely what one would expect from default on a formal loan given the Credit Bureau, though the magnitudes are striking. Defaulters are then forced to rely on informal lenders and this is not an enticing prospect as we see next.

### *C. Informal Terms are Worse Than Formal Terms*

We use the Mexican Family Life Survey (MxFLS) to compare interest rates, loan amounts, and loan duration for formal and informal loans.[84] We find that informal loan terms are significantly worse than formal loan terms. Table OA-21 shows the results from regressing contract terms on an indicator for a formal loan and controls. The first striking fact is that the average annual interest rate for informal loans is 291% while the corresponding rate for formal loans is 94 points lower (col. 1). The average loan amount is 3658 pesos for informal loans and 9842 pesos for formal ones (col. 4), and the term of the loan is 0.52 years for informal loans and 1.07 years for formal loans (col. 9). Figure OA-21 shows that the distribution of interest rates for informal loans first-order stochastically dominates the distribution for formal loan rates while the opposite is true for loan terms and loan amounts. These results are robust to controlling for income and wealth proxies (columns 2,4 and 7). The results on loan terms and duration also survive the addition of household fixed effects.[85]

Based on these results we conclude that it is costly to be excluded from the formal loan market. We conjecture that default responded only in a muted fashion to even relatively large changes in contract terms precisely because of the dire outside options outlined above.

[83] Bos et al. (2018); Dobbie et al. (2018) document similar magnitudes in the United States.

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

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

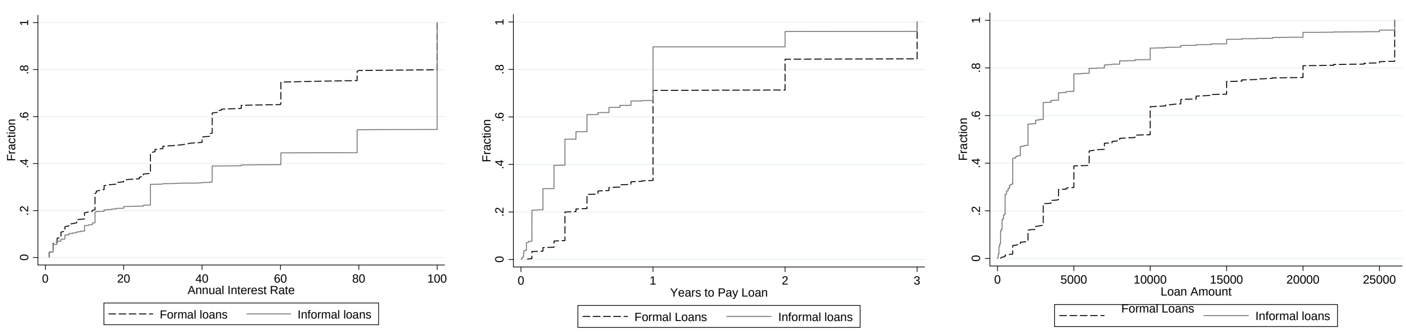
**Table OA-21:** Formal vs Informal Loan Terms

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

*Notes:* Data from National Survey of Household Living Standards (Rubalcava and Teruel, 2006) is used to construct the table. The table shows the difference between formal and informal interest rates (Columns (1)–(3)), peso loan amounts (Columns (4)–(6)) and the loan duration (Columns (7)–(9)). We consider a loan to be from a formal entity which we define as a banking institution and informal otherwise. The household controls include age, monthly expenditures, and dummy variables for car ownership, washing machines, and other household appliances. Standard errors are shown in parentheses. One, two and three stars denote statistical significance at the 5, 1 and 0.1 percent level respectively.

**Figure OA-21:** Comparison formal and informal loan market in Mexico

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




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

## Appendix O. Effects of unemployment: alternative method

This section provides some evidence that shocks indeed precipitate default using an alternative sample and an altenrative specification. We observe employment spells for the the subset of our CB sample employed in the formal sector by matching the CB data with Mexican social security data (the IMSS). The matching yields a panel of $86,363$ individuals with information on employment history in the formal sector as well as

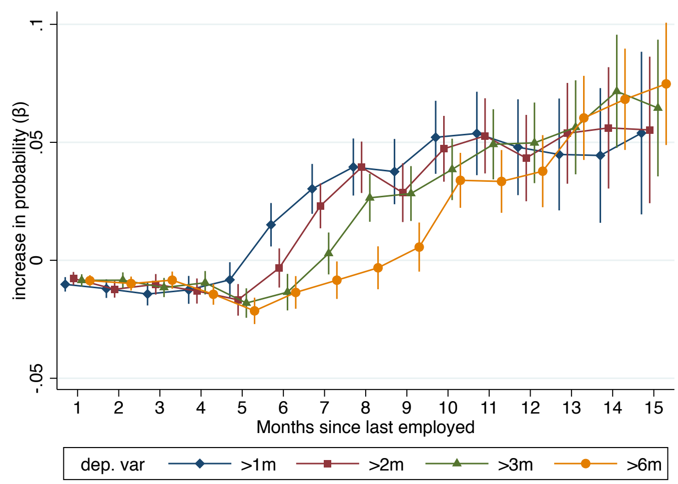
their formal credit records.[86]

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

$$\text{default}_{it}^{j} = \alpha_i^j + \gamma_{s,t}^j + \sum_{k\geq 1} \beta_k^j \times \mathbb{1}(\text{ months unemployed}_{it} = k) + \epsilon_{it}^j \tag{9}$$

where $\alpha_i$ is an individual fixed effect and $\gamma_{s,t}$ controls for trends at the state month level. The independent variables are a set of dummies $1(\text{ months unemployed}_{it} = k)$ that are equal to 1 if individual $i$ in month $t$ has been unemployed for $k$ months. For individuals who are employed $1(\text{ months unemployed}_{it} = k)$ is equal to zero for all $k$. The dependent variable, $default_{it}^j$ is equal to one if individual $i$ at month $t$ has a 'default code' of $j$ months, meaning that she has at least one loan in delinquency for $j$ or more months.

**Figure OA-22:** The "Effect" of Unemployment Spells on Delinquency




*Notes:* The figure presents $\widehat{\beta}_k^j$ estimates from (9) estimated using OLS. The data is matched CB-IMSS data at the borrower-month level. We observe employment for 86,363 borrowers from 10/11 to 05/14 (unbalanced panel). Each line corresponds to a regression with a different measure of delinquency – delinquency is defined as $j$-months past due where $j \in \{1, 2, 3, 6\}$. The $\beta$ coefficients are intended to capture the associational effect of unemployment spells (by duration of unemployment) on delinquency . For instance $\hat{\beta}_6^3$ is the correlation between having been unemployed for 6 consecutive months (relative to being employed during that time) and being 3 months delinquent in the current month.

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

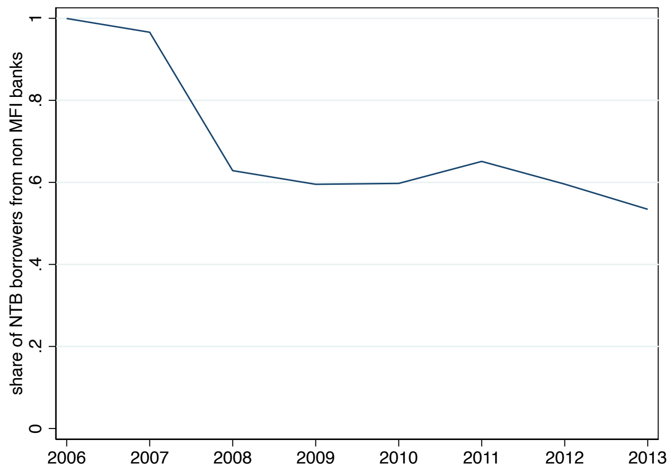
Figure OA-22 plots $\hat{\beta}_k^j$ for different values of $k$ and $j$. The likelihood of default is increasing in the length of the unemployment spell so that for instance, being unemployed for 10 months is associated with a 5 percentage point higher likelihood of being delinquent on at least one loan – the unconditional mean is is 12 pp, so this is a 41% increase.[87] These results demonstrate the severe effect of one particular shock (unemployment) on default (even after controlling for individual fixed effects). This is consistent with the view that large negative shocks, such as prolonged unemployment (or health shocks), could increase default markedly. At the same time, these results are only partial since unemployment in the formal sector is likely only one possible shock affecting NTB borrowers.[88]

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

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

# Appendix P. Closing of Study Card Not Taken up by Other Lenders

**Figure OA-23:** Study card demise coincides with smaller share of loans going to NTB borrowers




**(a)** Share of new loans going to NTB in Mexico, all lenders

This figure plots the fraction of total newly originated loans (for the whole of Mexico) going to borrowers with no previous formal credit history each year, from 2006 when our study card was in its peak throughout the period in which Bank A reduced its rate of issuance (2007-2009), and when Bank A stopped issuing it altogether in 2010. We normalize 2006 to 1, so that changes in the share of new loans awarded to NTB borrower can be easily read. In 2008, when the Bank A reduced issuance of the Study Card, the share of loans going to NTB borrowers declined by 40 percent. Note that the big decline comes before the Great Recession (Mexico grew at 1.1 percent in 2008). The graph not necessarily reflects a causal relationship between the closing of the Study Card and financial inclusion, but it is suggestive. There was no recovery of the share of loans going to NTB borrowers afterwards.