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ABSTRACT

Credit card borrowing is an increasingly common way for first-time borrowers to access formal sector credit. We study new borrower behavior by combining detailed card-level data for a product that accounted for 15% of all first-time formal loans in Mexico, individual-level employment histories, and a large-scale randomized experiment. We find that although default rates are high, they are relatively unresponsive to large variations in interest rates and minimum payments; unemployment shocks have much larger effects on default (about two to seven times larger). An implication is that social protection policies aimed at moderating labor market shocks may be more effective at limiting credit market default than regulation of contract terms among new borrowers.

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1 Introduction

There is a growing body of work linking financial development to improved economic outcomes and some evidence that this relationship is causal.[1] At the same time, a substantial fraction of the world's poor lack access to financial services, including formal credit.[2] Credit card borrowing is becoming increasingly relevant as the first formal sector loan product in many developing countries. For instance, in Mexico it was the first loan type for 74% of all formal sector borrowers. The corresponding figures for Peru and Colombia are 83% and 51% respectively.[3] This growth has been accompanied by increasing concern amongst regulators and policymakers that card use among inexperienced borrowers leads to excessive default risk. In Mexico and Taiwan, such concerns led to legal mandates restricting contract terms (a floor on minimum payments). [4]

Despite its increasing role in expanding credit access, greater policy scrutiny and regulation, credit card borrowing in developing countries remains relatively understudied—particularly relative to other recent approaches 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 (henceforth the study card), targeted at borrowers with non-existent or limited credit histories, alone had 1.3 million customers at the time (Pedroza, 2010, and authors' calculations). Thus, relative to their potential importance as an engine of credit expansion and the increased policy attention, credit cards have received relatively little attention from development economists.

Perhaps as a result, policy discussions on the issue lack both a coherent theoretical underpinning and credible empirical evidence. In this paper, we address both issues. First, we provide a simple clarifying framework that makes explicit the assumptions needed for commonly proposed policies to have the effect intended by policymakers. Second, we provide empirical evidence on the effectiveness of two of the most commonly proposed policy proposals to limit default. Finally, we show that inexperienced borrowers have much more tenuous labor market attachments and

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

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

[3]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. Even in the United States the figure is 50% (Haughwout et al., 2020). There does not 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.

[4]Other countries have also considered regulation. 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. Financial Conduct Authority (2015); Nelson (2020).

that plausibly exogenous unemployment shocks have a substantially larger effect on credit card default than the contract term changes proposed by regulators.

We address these issues by examining credit card use and default among inexperienced borrowers (borrowers with limited or no formal credit histories) in Mexico. Default among this population has been a persistent concern among regulators. For instance in our sample, newer borrowers, those who had been with the bank for only 6-11 months, defaulted at almost twice the rate as those with longer tenure ($\geq$ 24 months or more).[5] Conversations with bank officials as well as policymakers make clear that these default rates are an important policy concern as they increase lender risk and limit borrowers' future access to formal credit. In fact, the randomized experiment studied in this paper arose directly from central bank concerns about default among new borrowers.

An important reason for policy and research interest in new borrowers is their economic vulnerability. Improving our understanding of new borrowers in this context, thus, appears to be a first-order issue. In this paper, we examine the repayment behavior of new borrowers in response to changes in contract terms as well as unemployment shocks using a large representative sample from one of Mexico's largest banks for a product explicitly aimed at new borrowers.

We begin by providing a simple optimizing framework that helps readers interpret the subsequent estimated effects (of contract terms and unemployment). We then estimate the causal effect of contract terms on card default using a randomized experiment by our partner bank (henceforth Bank A) on a credit card targeted at new borrowers that accounted for approximately 15% of all first-time formal sector loan products in the country (see Figure 1). The experiment allocated a stratified random sample of $144,000$ *pre-existing* study card borrowers to 8 treatment arms that varied annual interest rates (between 15%, 25%, 35% and 45%) and monthly minimum payments (between 5% and 10%) for 26 months (from March 2007 to May 2009). To our knowledge, this is the first paper examining experimental variation in both the minimum payment and the interest rate in credit card contracts. Furthermore, the magnitude of experimental variation as well as the sample sizes are substantial, allowing for precise estimation of treatment effect heterogeneity over a range of contract terms and population strata. In addition, the sampling scheme ensures that the experimental results are representative of the bank's national population of study card customers (about 1.3 million at the start of the study).

We examine the effect of unemployment shocks by matching our experimental sample to their monthly employment histories in the Mexican Social Security database (the Instituto Mexicano del Seguro Social or IMSS) and using firm downsizing as a measure of involuntary separation.[6] We also repeat the exercise using a much larger sample of one million borrowers that are representative of the Mexican population. To our knowledge, these are the first estimates of the effects of formal

[5]By contrast, default is much lower for micro-finance lenders. For instance, Karlan and Zinman (2019) note that default is less than 1% for Compartamos, a prominent for-profit Mexican micro-lender.

[6]We follow what is now a standard methodology and was first outlined in Jacobson et al. (1993).

sector job loss on default for new borrowers in a developing country.

Reducing the annual interest rate from 45% to 15% has *no* effect on default for newer borrowers, and reduces default by 2.6 percentage points (on a base rate of 19 percent) for all borrowers over the 26 -month experiment. The average implied elasticity is $+0.20$ (and those for newer borrowers is $+0.05$) and considerably smaller than those in the literature (e.g Adams et al., 2009; Karlan and Zinman, 2019, See Table OA-2 for comparisons). The result for newer borrowers is somewhat surprising and we provide a rationale based on a stylized model.

The estimated elasticities suggest that prices might not be an effective lever in limiting default among new borrowers. Quantity restrictions (as implied by raising minimum payments) are another potential policy tool. In fact, policymakers in many countries, worried that low minimum payments for new and inexperienced borrowers could increase default, have long advocated raising minimum payments.[7] Higher minimum payments, however, could have two opposing effects and it is not *apriori* clear which one will dominate. On the one hand, higher minimum payments, ceteris paribus, tend to reduce future debt and decrease default in the longer-run. On the other hand, higher payments tighten short run liquidity constraints which may increase current default. We formalize these ideas by viewing them through the lens of the stylized model which we develop in Section 5. Liquidity constraints may be particularly relevant as, at the start of the experiment, 73% of card holders' monthly payments were less than 10 percent (of the amount due). Strikingly, not only does doubling the minimum payment not reduce default, the point estimate is a .8 percentage point (pp) increase (the corresponding elasticity is $+.04$).

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

In contrast to the above results, we find that default is very responsive to formal job loss. We match the IMSS employment data with our experimental sample and find that job loss is common: of those employed at least one month in the formal sector between January 2004 and March 2007, 42% experienced at least one month out of formal employment. Second, newer borrowers are more likely to experience unemployment: those who had the study card for less than a year before the

[7]See e.g. Bar-Gill (2003); Financial Conduct Authority (2015); Kim (2005); Rushton (2003); Warren (2007) and this circular from the Mexican Central Bank (`https://goo.gl/MkYbVO`). As noted earlier, Mexico and Taiwan mandate minimum payment requirements prompted by such arguments. Such prescriptions find some support in models of time-inconsistent or unaware agents (DellaVigna and Malmendier, 2004; Gabaix and Laibson, 2006; Heidhues and Kőszegi, 2016; Heidhues and Kőszegi, 2010). There is some evidence that time inconsistent preferences play a role in credit card debt accumulation (Laibson et al., 2003; Meier and Sprenger, 2010; Shui and Ausubel, 2005) and that minimum payments serve as an anchoring device (Stewart, 2009).

[8]Our results are consistent with those in Karlan and Zinman (2019) and Angelucci et al. (2015) for the micro-lender Compartamos Banco (in Mexico).

experiment are 1.34 times more likely to be unemployed than those who had the card for more than two years. We estimate that displacement (defined as job loss that occurs as part of a downsizing) leads to a 6.1 percentage point increase in the probability of default on the study card in the next eighteen months. This corresponds to about a third of the mean default rate and it dwarfs the experimental effects discussed above. These magnitudes are substantial and consistent with the hypothesis that new borrowers are vulnerable to large shocks that precipitate default.[9]

We draw three lessons from these results. First, despite the regulatory emphasis on increasing minimum payments to protect inexperienced borrowers, they are ineffective at reducing default, at least in our setting. Second, ex-post (i.e. conditional on selection) variations in interest rates do little to mitigate default among our sample of pre-existing new borrowers (in contrast to models of interest-rate driven moral hazard that predict lower default). This is unfortunate since ex-ante screening through credit scoring methods is difficult for new borrowers given their limited credit histories.[10] In fact, since default elasticities are increasing in bank tenure, interest rate changes are least effective in mitigating default for precisely those borrowers for whom the asymmetric information problem is likely the most acute (i.e. the newest borrowers). Third, the substantial effects of employment shocks on default for the same experimental sample during the same study period suggests that their mitigation may be an important factor determining continued access to formal credit in populations such as the ones we study.

In addition to policymakers, we believe our work should be also of direct interest to economists more broadly. We benchmark the relative importance of contract terms and interest-rate driven moral hazard to labor market shocks in causing default. The comparison highlights the link between credit and labor markets, suggesting that unemployment insurance or social protection policies in developing countries more generally may improve credit market functioning and facilitate lending to inexperienced borrowers.

This paper connects with several strands in the literature on credit markets. A recent literature identifies lack of access to formal financial services as a general problem in developing countries (Dabla-Norris et al., 2015; Demirgüç-Kunt and Klapper, 2012; Dupas et al., 2018) and advocates supply-side interventions aimed at increasing financial inclusion. We provide a detailed empirical analysis using a widely-used, popular product specifically targeted at those with limited credit histories. 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). Compared to this literature, instead of taking the limited formal private sector engagement with poor borrowers as *prima facie* evidence for inviability, we provide detailed evidence on a private sector bank's attempts to use ex-post contract terms to limit default among its lower income borrowers.

[9]Since the IMSS only captures 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.

[10]See e.g. Liberman et al. (2018) on the importance of credit histories for borrowing outcomes in Chile.

Research on credit cards among inexperienced populations in development economics is scarce despite their increasingly important role as the source of entry into the formal credit sector.[11] Ponce et al. (2017) and De Giorgi et al. (2021) examine credit cards in Mexico, but do not focus on new borrower populations and products targeted specifically at them. To the best of our knowledge, this is the first paper to experimentally estimate the causal effects of changing minimum payments in credit cards. Keys and Wang (2019) study anchoring on minimum payments using an event-study design while d'Astous and Shore (2017) use a difference-in-differences type approach on a non-experimental change in minimum payments.

Finally, our matched worker-borrower administrative data allows us to compare the effects of contract term variations to those of negative employment shocks on default on a common sample of new borrowers in a developing country. The closest paper to us in this aspect is Keys (2018) which analyzes the effects of employment loss on bankruptcy filing in the United States using a selection on observables assumption. Gerardi et al. (2018) use an instrumental variable approach to estimate the effect of income and housing equity on mortgage default using the PSID. Our contribution is to use individual level administrative employment data matched with our experimental sample to estimate an event study design using downsizing as a source of exogenous job separation (see e.g. Couch and Placzek, 2010; Flaaen et al., 2019; Jacobson et al., 1993). Our paper complements research studying the connections between labor and credit markets. Herkenhoff (2019) studies the effect of credit markets on the labor market in the United States, while we study the reverse causal relationship. Hsu et al. (2018) show that more generous unemployment insurance generates lower mortgage default (in the United States). We establish a relationship between unemployment and default in a country without unemployment insurance, and benchmark the effect size against large changes in interest rates and minimum payments.

The paper proceeds as follows: Section 2 outlines the various data sets we use and provides basic summary statistics. Section 3 provides some context for the rapid increase in credit cards in Mexico and characterizes new card holders. Section 4 describes the experiment. Section 5 provides a simple model to frame the interpretation of the experimental results. Section 6 reports the experimental results and Section 7 estimates the effect of job displacement on default. Section 8 concludes. Due to space constraints some robustness analyses and secondary figures and tables are reported in the Online Appendices (OA).

[11] There is an active literature examining credit cards in the US, e.g. Agarwal et al. (2010, 2015, 2017); Ausubel (1999). This literature typically focuses on a distinct set of issues (e.g. pass through, card fees, complexity) in the context of a well-developed credit card sector with sophisticated risk scoring and complex product offerings (balance transfers, reward programs and bundled services). See Grodzicki (2022) for a useful institutional overview.

2 Data and Summary Statistics

The main focus of the paper is on borrowers using the study card—Mexico's main financial inclusion product in our sample period—for which Bank A implemented a large experiment on existing borrowers.[12] We obtained and matched the study card borrowers to two relevant data sources. The first is the credit bureau, in which we observe every (formal) loan held by the study card borrowers, to study spillovers. The second source is employer-employee data from the IMSS, to compare the experimental results to the effect of job loss on loan default.

In addition to matching with the study card sample, we obtained several cross-sectional snapshots (of one million borrowers each) from the credit bureau. These enable us to compare our study card borrowers to borrowers in Mexico in general. We also match our credit bureau snapshots to the employer-employee IMSS data to examine whether our unemployment results generalize to the universe of formal workers. Figure 3 depicts when we observe information from the different data sources. We now describe the six data sets in more detail.

Study Card and Bank Data (Experimental Sample): We use detailed data from one of Mexico's largest commercial banks (Bank A) and a product (the study card) that accounted for 15% of first-time loans nation-wide in 2010 (Figure 1). The study card is a credit card that can be used at a large set of supermarkets as well as other stores (e.g., see Figure OA-8). In 2011, these stores accounted for 43% of all household expenditures at supermarkets and 16% of all household expenditures in Mexico.[13]

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). Consistent with this, the study card was the first formal loan product for 47% of our study sample, and for 57% it was their first credit card. Customers for the study card were acquired at bank kiosks in supermarkets (located all over Mexico) and completed a brief paper application. The card had an initial credit limit of approximately 7,000 pesos, an annual interest rate of 55 basis points over the base rate and a monthly minimum payment of 4% of the total amount outstanding. 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 sampling frame consisted of all study card holders who had paid at least the minimum amount due in each of the last six months through January 2007. The frame was partitioned into nine strata based on tenure with the bank and payment behavior (each taking on three values), both of which the bank uses internally as predictors of default. The bank then selected a random sample of 18,000 clients per stratum. We use stratum weights (see Table OA-3) in all of our analysis

[12]The study card was the product that most borrowers with no credit history got as their first formal loan in credit bureau data by June 2010.

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

to ensure our results are representative of the sampling frame. We examine the external validity of the sample for the national population of new borrowers in Table 1.

Variables: We have monthly data on purchases, payments, debt, credit limits, and cancelations from March 2007 to May 2009. We observe default from March 2007 to December 2014 but at different frequencies: monthly for the duration of the experiment (March 2007 to May 2009), every two months from June 2010 through December 2011, and then monthly again through December 2014. Throughout the paper we focus on default because it is the focus of a significant literature on asymmetric information problems in credit markets and because we observe default for many years during and after the experiment.[14] Since default is a key outcome for the analysis we describe it in some detail here. In keeping with the legal definition, default is defined as three consecutive monthly payments that are each less than the minimum payment due. In such instances, it is Bank A's policy to revoke the study card automatically (there is no appeal procedure). Our measure of default at time t for a borrower i is a binary variable equal to zero if i has not defaulted until period t (including t) and equal to one in the period in which i defaults and also for all subsequent periods post-default. Finally, we also observe some basic demographic variables—age, gender, marital status and residential zip code.

Credit Bureau Data (Matched to Experimental Sample): A borrower appears in the credit bureau if she or he has had a loan with a formal financial intermediary.[15] For each loan we observe the date of initiation and closing, the source and type of loan, monthly delinquency and default history. We observe the credit score, but we do not observe interest rates, debt, or contract terms, except for credit limits. We were able to match the experimental sample to the credit bureau (Buró de Crédito) data once each year from June 2007 to June 2012. This enables us to observe all other formal sector loans and their default status for these borrowers, allowing us to measure effects on non-Bank A related outcomes. We will refer to this data as the *matched* CB data.

Credit Bureau Data (Representative Cross-Sections): We use six representative random cross sections of one million borrowers from the Mexican credit bureau to describe the population of new borrowers in the country: June 2010, June 2011, June 2012, June 2013, December 2013, and March 2014. Unlike the matched CB data, we do not observe credit scores for the borrowers in these snapshots. In addition to the borrowing data outlined above we also observe some demographics—age, gender, marital status and zip code. We will refer to this as the *population representative* CB data.

[14] Furthermore, it allows us to circumvent statistical challenges related to attrition which are present with variables like debt, payments and purchases. We examine these variables and their link to default in the appendix.

[15] 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).

IMSS Employment Data (Matched to Experimental Sample): An individual appears in Mexico's social security data if they have held a formal sector job for at least one month. Presence in the IMSS is, by definition, employment in the formal sector. Absence from the IMSS data can be thus interpreted as absence from the formal sector. We observe monthly data from January 2004 to December 2012. For each worker and for each month they are employed, we observe their salary, a firm identifier (anonymized), and a geographical identifier. We match our experimental sample to the social security data using individual identifiers (known as *CURPS* in Mexico), which are 18-digit strings designed to identify individuals and eliminate duplicate entries. We observe CURPS for 144,320 (89%) of our experimental borrowers and match 84,679 of them.

IMSS Employment Data (Matched to the CB): We obtained additional monthly Mexican social security data from October 2011 to March 2014. The only difference between the matched IMSS data and this one (besides the time period) is that instead of the population identifier, we observe tax identifiers (known as *RFC*), which are a 13-digit strings identified tax-payers. Our matched CB sample includes 600,339 individuals with both credit information and employment histories. This data allows us to estimate the effect of formal job loss on loan default for a representative sample for Mexican borrowers.

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

2.1 Summary Statistics

Table 1 presents summary statistics for the experimental sample in columns 1–2 and comparisons with samples representative of Mexican borrowers in columns 3–5. Column 3 is a nationally representative sample of the population of borrowers with at least one credit card in 2010. Column 4 finds a set of borrowers in the CB data that matches the tenure of the experimental sample in the formal credit market (measured by the year of their first loan of the experimental sample; see Online Appendix B.2 for details). Finally, for comparison, Column 5 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, 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 IMSS, average monthly income in the experimental sample is 13,855 pesos compared to an average of 14,759 for recent borrowers and 22,641 for experienced borrowers.[16] The proportion of study card borrowers who we could match in the IMSS data (i.e. those that held a formal sector job for at least one month between January 2004 and December 2012) is 56%. 41% of the study card borrowers were employed in the formal sector in March 2007—the month when the experiment started.

3 Context

In this section we provide some context for the intervention (the rapid increase of credit card lending to low income individuals in Mexico) and some basic characteristics of borrowers new to the formal sector.

Rapid credit card expansion among low income individuals: The number of credit cards in Mexico 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 OA-9(a) and Banco de México, 2016). The study card played an important role in this expansion, accounting for 15% of all first-time formal sector loans in 2010 in Mexico. This pattern is common throughout Latin America, as a significant fraction of borrowers use only credit cards in their formal loan portfolio (see Figure OA-10).

This desire to pursue low-income clients appears to have been in part inspired by the success of Banco Compartamos and Banco Azteca.[17] However, both Compartamos and Azteca pursue markedly different strategies than those pursued by Bank A, our partner bank. Compartamos primarily uses joint liability via group lending, while Azteca requires collateral, typically household durables. Both lenders expend considerable resources on face-to-face interactions and home visits for loan collection.[18] In contrast, Bank A relies on traditional bank credit card approval and monitoring methods based on individual uncollateralized lending, distance monitoring, credit scoring methods for screening, and standard bank debt collection mechanisms. These traditional methods are cheaper compared to Compartamos and Azteca, with operating expenses relative to assets being an order of magnitude smaller (see Figure OA-11). Whether these lower cost traditional methods work for new-to-banking borrowers is an open one. The concern is that default may be substantially higher, although at least in theory contract terms could be used to mitigate it.

[16] 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.

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

[18] Azteca uses "crude collection and repossession mechanisms" (Ruiz, 2013). Ruiz attributes Banco Azteca's 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."

New Borrowers have low credit scores and high utilization of credit limit increases: The subjects in our experiment, borrowers with limited or no credit histories, unsurprisingly, tend to have low credit ratings. The mean credit score for our primary sample (645) is low in absolute terms—borrowers with scores below 670 are typically ineligible for standard credit card products.[19] They also have low credit limits. In our study sample the credit limit for the study card was relatively low at 7,879 pesos. For comparison, in 2010 the mean card limit was 49,604 pesos for those with at least one active card in the credit bureau. In Appendix D we use credit limit changes to test for credit constraints (using the definitions and approach of Gross and Souleles, 2002) and find that, by our definitions, the experimental sample, particularly the sub-sample of newer borrowers, is substantially credit constrained.

Default is high for New Borrowers and declines with tenure: 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. As a further point of comparison, default rates for the micro-lender Compartamos are less than 1% (Karlan and Zinman, 2019). Figure 2 shows that newer borrowers in the study card are indeed riskier: default rates are 36% during the experiment in the control group for the newest borrowers (those who had been with the bank for 6-11 months when the study began) and 18% for oldest borrowers (those with a tenure greater than 2 years at the start of the experiment).

Figure 2 also shows that patterns of default and tenure hold not only for the study card. It plots default rates for three different groups: the study card (red diamonds), all credit cards offered by Bank A (blue squares), and all cards in the credit bureau (green circles).[20] Default on the study card is twice as high as that on Bank A's other cards—consistent with the study card being a "financial inclusion" product targeted at those with lower incomes and limited credit histories. Default rates for Bank A's other cards are similar to those at other banks.

4 Experiment Overview

In this section, we describe the design of the large nation-wide experiment implemented by Bank A on new borrowers to better understand the causes of loan default. The experiment arose from regulator concerns that high interest rates and low minimum payments were causing inexperienced borrowers to default and subsequently limiting their future access to credit. Bank A sought to answer this question by running a strongly powered stratified experiment on their main financial inclusion product.

[19]See Drenik et al. (2018).

[20]To be comparable with the experiment, we condition on cards that had not been delinquent in the 6 months previous to January 2007, and use the same time-period as our experiment.

To run the experiment, the bank partitioned its sample frame of eligible study card clients into nine different strata based on the length of tenure with the bank, and repayment history over the past 12 months (both measured in January 2007).[21] Each borrower was classified into one of three categories of tenure with the bank: (a) a long term customer who had been with the bank for more than 2 years, (b) a medium term customer who had been with the bank for more than one but less than two years, and (c) a new customer who had been with the bank for more than six months but less than a year. Each borrower was also classified into one of three categories based on their repayment behavior over the past 12 months: (i) a "full payer" who had paid their bill in full in each of the previous 12 months and hence accrued no debt, (ii) a "partial payer" whose average monthly 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 (ii) a "poor payer" whose average monthly payment over the past 12 months was less than 1.5 times the average of the minimum payments required from her during this time. These two variables were used to define 9 strata, and 18,000 borrowers were randomly selected from each of these 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.

Experimental Design: Within each stratum, the bank randomly allocated 2,000 members to each of the 8 intervention arms and one hold out arm. Each treatment arm is a combination of two contract terms: (i) a required minimum monthly payment which is expressed as a fraction of amount outstanding (debt) on the card, and (ii) the interest rate on the amount outstanding.

The minimum payment was set at either 5% or 10%. This is a large and significant change since 73% of borrowers paid less than 10% of the amount due before the experiment began (see Figure OA-12). The minimum payment prior to the study was 4%. The interest rate (expressed as the annual percentage rate or APR) 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 was applied to all new debt incurred going forward as well as to debt outstanding, it therefore includes a forward-looking component as well as a current component (in contrast with Karlan and Zinman, 2009, who vary both components independently).

The two different minimum payments and four different interest rates yield 8 unique contract terms. This makes the experiment one of the most comprehensive in terms of variation in this literature. In addition, 2,000 customers within each stratum also served as a hold-out group whose contract terms were unchanged (see Table OA-4). We were informed that the minimum payment for the hold-out arm was 4% but the interest rate varied across clients and, unfortunately, we do not observe this rate in our data.[22] Consequently, we do not use the hold-out group as a contrast. This

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

[22] We were also told that marketing efforts for this group may have been different than for the 8 experimental groups,

leaves us with 8 experimental arms with a sample size of 144,000 individuals; a three-fold variation range in interest rates and two-fold variation in minimum payment. In all 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-5 in the Online Appendix tests the randomization procedure and shows that treatment assignment is uncorrelated with baseline observables for the initial sample (as well as for the sample that did not attrit for the entire duration of the experiment).

Figure 3 shows the timeline of the experiment as well as measurement dates. Each study client was sent a letter in March 2007 stating the new contract terms that would be in force starting in April 2007. Clients were not informed that this was a study or of any time-lines for when the new contract terms would change. The measurement of experimental outcomes began in March 2007 and lasted through May 2009. During this period the interest rate and the minimum payment were kept fixed at their experimentally assigned levels. Internally, 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-6 and Figure OA-13). 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%.

5 A Simple Framework

The goal of this section is to formulate a simple framework providing comparative statics for default with respect to minimum payments, interest rates, and income shocks. A vast theoretical literature studies the effect of asymmetric information in credit markets (e.g. Bizer and DiMarzo, 1992; Stiglitz and Weiss, 1981) and a recent empirical literature tests some implications of these models (e.g Adams et al., 2009; DeFusco et al., 2021; Karlan and Zinman, 2009, 2019).[23] In this section, we outline a basic framework (that closely follows Einav et al., 2013) to interpret our experimental results.

As is the case in our experiment, we model a borrower who already has a credit card (i.e. we do not model selection) and is observed for two periods. In the first period, she decides how much to

which received virtually no marketing. The fact that both minimum payment and interest rates are simultaneously different in the holdout group, and that marketing and other policies may be different as well, means that we cannot attribute differences in behavior separately to either interest rates or minimum payments.

[23] The literature distinguishes between at least three channels in understanding the effect of varying interest rates on default: (a) the "debt burden" channel describes the idea that higher interest rates increase debt mechanically, and this makes repayment harder; (b) the "pure current incentive effect" or "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). In our case, interest rate changes apply on all current debt as well as on future debt for the foreseeable future. Therefore, a muted default response implies that the contributions from all three channels are correspondingly small. Of course, there are types of moral hazard that are not related to interest rates.

purchase on the card, C, and in the second period she decides her payment to the bank, P. Income in each period y_t is realized before making decisions in that period. We interpret unemployment as exogenous low income realizations—this exogeneity is consistent with our use of firm downsizing events to identify unemployment shocks exogenous to the worker in Section 7.

The borrower derives utility, V, from a usable credit card in period 2.[24] This can be interpreted as a reduced form parameter capturing the flow of card benefits, a warm glow from card ownership, or the option value of having a credit card in the future. Consistent with our context, V is only experienced if the card is not in default, as defaulted cards are closed by the bank. The card will be in default in period 2 if the payment does not cover the minimum payment due which equals a fraction, m, of the period 2 debt. Period 2 debt is last period's purchases C along with the interest accrued, rC, where r is the one-period interest rate. The borrower must thus pay at least $m(1+r)C$ to avoid default in period 2. In this framework, the experiment can be interpreted as manipulating m and r and the primary purpose of this section is to derive comparative statics with respect to each of (m, r) as well as income y_2.

We model first period utility as $U_1(C, y_1) \equiv \ln(y_1 + C)$. In period 2, if the borrower pays $P > m(1+r)C$, they derive utility $V + \ln(y_2 - P)$. If the agent pays less than the minimum payment, the card is in default and the agent's utility is $\ln(y_2)$. Given the assumptions made so far, the agent's optimal decision in period 2 then reduces to a binary choice: pay the minimum amount (i.e. $P = m(1+r)C$) and remain in good standing or default without making any payments. We assume that the agent always defaults if the second period income realization is less than the minimum payment due (i.e. when $x \equiv \mathbb{I}\{y_2 < m(1+r)C\} = 1$).

The agent may default even if $x = 0$, i.e. even when the second period income realization is high enough. In particular, the agent will default in this case if $D \equiv \mathbb{I}(\ln(y_2) > \ln(y_2 - m(1+r)C) + V)$. To simplify the analysis, we assume that the utility from holding the card V can be decomposed as $V = v + \epsilon$ where v is a constant and ϵ is a random variable which we interpret as a period-specific preference shock unobserved by the agent in period 1. This along with a distributional assumption on ϵ, allows us to derive closed form solutions for both the choice probabilities in period 2 as well as the expected maximized utility needed in period 1's maximization problem.

With these assumptions, utility in period 2 is given by

$$U_2(D; C, x, y_2) = x\ln(y_2) + (1-x)\left[D\ln(y_2) + (1-D)\left(\ln(y_2 - m(1+r)C) + v + \epsilon\right)\right].$$

In the following, we denote choice variables (default and debt) using capital letters to aid exposition and suppress dependence on the exogenous variables (r, m, v, δ, y_2) unless it is useful.

In Appendix C, we solve the model and derive a set of comparative statics results that we now

[24] We do not model utility from the card in period 1 since it does not affect optimal debt choices (since it appears additively) and is also inessential for our comparative statics exercises. Adding a first period V would introduce additional notation without any modeling advantage in our context.

summarize. We consider two scenarios. First, we characterize default responses to changes in interest rates and minimum payments when agents cannot re-optimize their debt choices. This is intended to capture unannounced changes in contract terms after debt choices have been made and is perhaps more relevant for considering the initial months of the experiment following Bank A's unannounced changes in contract terms. Unsurprisingly, increases in both interest rates and minimum payments unambiguously increase default in this setting. Second, we characterize default responses when agents can also adjust their debt decisions. This corresponds to changes in contract terms made e.g. at the start of period 1, before any choices have been made and is more relevant for considering responses towards the latter part of the experiment once agents have had an opportunity to respond to the contract terms. Default responses are now typically ambiguous, with the sign of the response depending upon the magnitude of the elasticity of debt with respect to the interest rate and the minimum payment respectively.

Prediction 1 Holding Debt (C) fixed, the probability of default, $P_D(r, m, C)$, is increasing in both the interest rate and minimum payments.

Prediction 2 When debt is allowed to adjust to changes in contract terms (denote the optimal choice by $C^*(r, m)$), the effect of interest rate (minimum payment) changes on the probability of default $P_D(r, m, C^*(r, m))$ depends upon the elasticity of debt with respect to the interest rate (minimum payment). We discuss these effects in turn (see Appendix C for the formal derivations).

Implication 2a When debt is allowed to adjust to changes in minimum payments, the probability of default is decreasing in the minimum payment if and only if debt is sufficiently elastic with respect to the minimum payment ($\epsilon_{Cm} < -1$) .

Implication 2b If debt is allowed to adjust in response to interest rate changes, the probability of default is increasing in the interest rate if and only if $\epsilon_{Cr} > -r/1 + r$, where ϵ_{Cr} is the elasticity of debt with respect to the interest rate. In the appendix, we outline conditions under which the lower bound condition requires that debt be sufficiently inelastic with respect to the interest rate.

These conclusions provide a useful framework through which to view the policy prescriptions outlined in Section 1. In particular, policies advocating for higher minimum payments as a means to limit default must implicitly assume (within the terms of this model) that the debt response to changes in minimum payments is sufficiently large. Similarly, lower interest rates as a device for limiting default must assume (within the framework of this model) that that the debt response to interest rate changes be sufficiently small.[25] Finally, we examine the effect of income on default probabilities.

Prediction 3 Holding debt responses fixed, lower income in period 2 increases default probabilities (see Appendix C.1.5). The thought experiment here is replacing the distribution of second period income F by another distribution G that is first-order stochastically dominated by F (holding first-period debt fixed).[26]

[25] The contract terms r and m enter symmetrically in the model. This is adequate for our purposes but a richer model (e.g. one with explicit liquidity constraints and a longer horizon) may be useful to differentiate between the terms.

[26] Within our framework this thought experiment corresponds most closely to modeling *unexpected* unemployment shocks in the context of our empirical application. We discuss incorporating debt responses to changing period 2 income

6 Does Changing Contract Terms Reduce Default for New Borrowers?

Main specification For ease of exposition, our primary specification is

$$Y_{it} = \alpha_t + \beta_t \cdot \mathbb{1}\{MP_i = 10\%\} + \gamma_t \cdot (45\% - r_i)/30\% + \varepsilon_{it} \tag{1}$$

estimated on the sample of 144,000 individuals in the eight treatment arms using stratum weights (as defined in Table OA-3). Y_{it} is the dependent variable for borrower i in month t, $\mathbb{1}(MP_i = 10\%)$ indicates assignment to the 10% minimum payment arms, and r_i is the experimentally assigned interest rate (this specification is equivalent to running month-by-month regressions).

We interpret α_t as the mean value of Y_{it} in month t for the excluded group (i.e., the $r = 45\%$ and $MP = 5\%$ treatment arm), β_t as the average treatment effect of increasing the minimum payment to 10%, and γ_t as the effect of decreasing interest rates to 15%. We estimate Equation (1) both with and without stratum-by-month fixed effects and find almost identical results for β_t and γ_t. Equation (1) is restrictive in that it assumes that the effects of minimum payments and interest rates are separable and that the effect of interest rate changes has a specific linear form. We relax both assumptions and estimate fully saturated specifications in Table OA-7 which yield similar estimates. We also test both assumptions and cannot statistically reject them.[27] For ease of interpretability we only discuss estimates from Equation (1).

Given the large number of estimated monthly treatment effects $\{\beta_t, \gamma_t\}_t$ over seven years, we present the results succinctly, in two ways. First, we present the estimates graphically in Figure 4, plotting monthly means and treatment effects starting in March 2007 through December 2014. The estimated means and treatment effects for the minimum payment arms are in pink (left side) while those for the interest rate arms are in blue (right side). Second, we present point estimates in tabular form at a set of (nine) time-points in Table OA-7.

6.1 Default on the Study Card

Increasing Minimum Payments Does Not Reduce Default during the Experiment: The experiment doubled the minimum payment from 5% to 10% from April 2007 through May 2009. Figure 4(b) plots the evolution of default in response to this intervention over the course of the next

distributions in Appendix C.1.6. The elasticity of debt with respect to second period income plays a key role in the analysis. Unlike the case with interest rates and minimum payments where we can use experimental variation and the study data to discipline the relevant elasticities, we do not have a credible measure of the income elasticity of debt in our context.

[27] For example, we use the fully saturated model to test whether the minimum payment effect is different across interest rate treatment arms and cannot reject the null that they are equal. Similarly, we test whether the interest rate effect is different in the low and high minimum payment groups and cannot reject the null of no differences either. The full details are in Table OA-7.

several years.[28] We see from Figure 4(d) that default rose starting about six months into the intervention and treatment effects hovered between a half a percentage point and a one percentage point increase in default through the rest of the experimental period. This response rebuts arguments advanced by regulators and policymakers (referenced in Section 1) advocating for (and legislating) minimum payment increases as a device for *decreasing* default. In fact, at the end of the intervention in May 2009 the increase in minimum payment had increased default by 0.8 percentage points (see col (3) in Table OA-7). The implied 26 month elasticity is $+0.04$ and the confidence intervals rule out negative values. Thus, we find no experimental evidence that increases in minimum payments (of policy relevant magnitudes) decrease default, even over relatively long horizons.

Recall that the model in Section 5 identifies the debt response to minimum payment changes as a key parameter driving default in the long run (i.e. when debt is allowed to adjust). In particular, Implication 2a stated that the probability of default is decreasing in the minimum payment if and only if the minimum payment elasticity of debt ϵ_{Cm} is smaller than -1. Our preferred estimates for the elasticity are $\epsilon_{Cm} \in [-0.31, +0.04]$ (see Figure OA-23) which in the model implies that default should be increasing in minimum payments as is indeed the case.[29]

To our knowledge, these are the first experimental results on the effects of minimum payments. Comparing our findings to other results in the literature in Figure 4(f) and Table OA-2 we find that our elasticities for minimum payments are of the same order of magnitude as documented in earlier, albeit non-experimental, work.

Long Run Effects of Increasing Minimum Payments: In May 2009, all study cards were returned to their pre-experiment minimum payment level (of 4%) and interest rates were likewise returned to their pre-experiment levels. There is evidence, however, that the two year experimental increase in minimum payments had persistent long-term effects. Figure 4(b) plots treatment effects for five and a half years after the intervention ended. In contrast to the effects during the intervention, the point estimates are now consistently negative hovering around a one percentage point decline in default for the higher minimum payment arm. The estimates are typically only borderline statistically significant (at the 5% level) so it is difficult to draw firm conclusions. Nevertheless, it seems clear that while during the intervention, default did not decrease, it likely decreased after the higher minimum payments were withdrawn.

All cards were returned to a 4% minimum payment in May 2009, so the 10% MP group experienced a $6pp$ decrease while the 5% group experienced a $1pp$ decrease. Our simple framework would predict that, for each arm, default would decrease under the same conditions on the debt

[28] For reference, 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 credit card to be in default.

[29] Studying debt outcomes requires taking a stance on the econometric treatment of card exit. We discuss the issues involved and our estimates of debt responses in Appendix E.

elasticities that led to an increase during the experiment. Indeed, Figure 4(b) shows that the slope of the cumulative default curves for both arms flattened after the intervention ended—default continued to rise in each group but at a slower rate. We also observe that the slope flattened more for the (previously) 10% MP arm. However, interpreting these slope differences across arms would require a richer model as the groups may differ along a range of characteristics after two years of the intervention. In addition to debt levels, behavioral responses (such as persistent changes in payment or purchase patterns) may differ across arms and more generally the samples are no longer comparable.[30] Consequently, it is difficult to provide a convincing rationale for why the sign of the treatment effect changes after the intervention ends.

Interest Rate Changes Have Limited Effects on Default: Figure 4(a) plots the evolution of default for the $(45, 5)$ and $(15, 5)$ arms using estimated coefficients from eq. (1) and Figure 4(c) plots the corresponding treatment effects. The lower interest rate arm experienced gradually declining default rates so that by the end of the intervention, default rates were approximately 2.5 percentage points lower in the $(15, 5)$ arm relative to a default rate of 19% for the $(45, 5)$ arm and the estimates are statistically significant at the 0.001 level (see also Table OA-7). The implied 26 -month interest rate elasticity of default is $+0.20$. We view this as a modest effect as it is considerably lower than the elasticities estimated in in e.g. Adams et al. (2009); Karlan and Zinman (2019) and in the same range as the default elasticities in DeFusco et al. (2021); Karlan and Zinman (2009).[31]

Implication 2b from our model states that default will be increasing in the interest rate if and only if the interest rate elasticity of debt ϵ_{Cr} is greater than a negative lower bound. Our preferred estimates are that $\epsilon_{Cr} \in [+0.18, +0.54]$ which suggest that default should be increasing in the interest rate, as is indeed the case. [32]

Long Run Effects of Interest Rate Changes: As noted above, all study borrowers were returned to the same set of contract terms after the end of the experiment with interest rates being returned to 55 percent. Figure 4(c) displays (monthly) effects on default for five and a half years after the end of the intervention. The lower-interest rate arm continues to experience lower default for

[30]Table OA-14 in the Appendix measures the effect of having been subjected to the 10% MP in the past on current behavior (conditional on debt). It finds no effect, which we interpret as evidence against habit formation. We observe debt declines in the 10%MP arm, though the Lee bounds are wide and include zero.

[31]See Figure 4(e) and Table OA-2 for a comparison to other estimates in the literature. Note that although default decreased by 2.5 percentage points, this was from a 30 percentage point reduction in interest rates (typical changes in interest rates are substantially smaller).

[32]The positive elasticity range implies that lower interest rates *decreased* the stock of debt during the experiment (see also Figure OA-23(e) to see this graphically). This appears to arise because interest compounding has a larger effect than client purchase and payment responses. Roughly $debt_{t+1} = debt_t \times r + purchases_t - payments_t + fees_t$ (Table OA-9 verifies this is the case in our data). Figures OA-24 and OA-25 show that purchases are (at best) modestly higher and payments (at best) modestly lower at the lower interest rates but that debt is lower in the 15% arm. Given these behavioral responses and the lower interest rate, the overall stock of debt accumulates at a lower rate in the low interest rate arm (see Appendix E.1. The positive elasticity thus suggests that any behavioral responses are swamped by the lower compounding effect.

about three years after the intervention ended with the estimates gradually declining to about 1% by May 2011 after which they become statistically indistinguishable from zero. Thus, the 26 -month reduction in interest rates decreased default for nearly three years after the intervention ended, with the elasticities ranging between $.1 - .2$ during this time. Since we lack debt data after the end of the experiment, we cannot directly link these changes in default rates to debt changes.

No Interaction between Minimum Payments and Interest Rate Interventions. We see no evidence of interactions between the two interventions—we cannot reject the null hypothesis that the effect of the minimum payment intervention is constant across the various interest rate arms when the experiment ended in May 2009 ($p = .44$) and three years after ($p = .09$). Similarly, we cannot reject the null that the effect of a 30 pp decrease in interest rates is constant across both minimum payment arms ($p = .54$ in May 2009 and $p = .22$ in 2012).

Taken together, these results imply that two of the standard tools routinely used by large financial institutions to control default have smaller effects on new borrower behavior than typically presumed in policy discussions. Most strikingly, these tools are least effective for the newest borrowers, i.e. precisely for those borrowers for which the bank has the least information. From this perspective, it is perhaps unsurprising that Bank A subsequently reduced its interactions with new borrowers. Figure OA-9(b) shows the trend in both the current stock of and new issues of the study card. After issuing the study card in substantial numbers for several years, the bank ceased issuing new cards in 2009. In personal conversations, bank officials claimed that the card had not achieved its profitability objectives and that high default appeared to have played a role. The closing of the card appears to have had large effects on overall borrowing by new borrowers: Figure OA-14 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 new borrowers in Mexico.

6.2 Spillover Effects

The large variation in contract terms could also have affected behavior with other lenders. For instance, higher minimum payments could have driven borrowers to other lenders, and lower interest rates may have had the opposite effect. We use the matched credit bureau data to examine whether the experimental changes in the study card contract terms affected behavior with other lenders.

We first examine default on other loan products in Figures 5, OA-15 and OA-16. The dependent variable in Panels (a) to (d) of Figure 5 is equal to one if a cardholder has defaulted on at least one loan with *any* lender in the credit bureau at the given date. The dependent variable in Panels (e) to (h) is a cumulative measure of new loans equal to one if a cardholder has opened a new loan with *any* bank from the beginning of the experiment to the given month. Similarly, Figures OA-15 and OA-16 decompose spillovers by examining default on other loans from Bank A and loans from any other bank respectively. We find that default on other loan products is largely unresponsive to interest rate and minimum payment changes, both during the experiment and a half-decade after the experiment ended.[33]

In addition, we do not find any changes in cancelations with other lenders in response to contract term changes in the study card. We also do not find evidence of crowd-out or crowd-in from other lenders (see Figures 5(g), 5(h), OA-15 and OA-16). These results hold both during the experiment and five years after it ended. Angelucci et al. (2015); Karlan and Zinman (2019) similarly find no spillovers in the number of loans or lenders in a micro-finance context.

6.3 Heterogeneity in Treatment Effects

While the default elasticities are very modest on average, they could mask considerable heterogeneity. The explicit stratified design and the large sample size imply we are well positioned to examine heterogeneity in treatment effects. We consider two important stratifying variables—the extent of borrower tenure with Bank A (i.e. newer vs. older borrowers) and borrower repayment behavior prior to the experiment (minimum payers vs. full payers) as well as one variable that was not explicitly used for pre-experiment stratification (labor force attachment).

Treatment effects for newer borrowers are of direct policy interest given regulator concern over default risk for inexperienced clients. Further, since newer borrowers have the highest default rates and lenders have the least information about them, their responsiveness to contract term changes is of particular interest for lenders as a potential mechanism for limiting default. Finally, examining and documenting heterogeneity by credit market experience is of interest to researchers as well since such differences may motivate particular models (or interpretations of existing models, as we show below in our simple context).

We find that for newer borrowers, i.e. those who had been with Bank A for 6-11 months as of January 2007, the interest rate elasticity of default is $+0.05$ while the corresponding elasticity for borrowers who had been with the bank for more than two years is five times larger at $+0.25$. Figure OA-17 graphs both treatment effects over time and shows that the treatment effects for new borrowers are consistently smaller (in absolute terms) than those for the oldest ones. The new borrower elasticity of $+.05$ is considerably smaller than those documented in the literature.[34] The lower elasticity is striking because it suggests that interest rate declines are much less effective at reducing default for newer borrowers for whom the asymmetric information is likely most severe and baseline default rate is higher (relative to older borrowers).

However, newer borrowers may also vary in other important dimensions from older borrow-

[33]The only exception is a small decrease in default (3%, or 2 pp. out of a 61 pp. basis) among other Bank A loans in the high minimum payment group.

[34]It is an order of magnitude smaller than those documented in Adams et al. (2009); Karlan and Zinman (2019) and about a fifth of the elasticities documented in DeFusco et al. (2021); Karlan and Zinman (2009)

ers. We therefore, re-estimate treatment effects after including a range of baseline covariates (as well as interacting the covariates with treatment indicators) and find that the differential treatment effect between older and newer borrowers remains (as seen in Table OA-8).[35] While not dispositive, these results suggest that the observed treatment effects for newer borrowers are not driven by age, labor force attachment or earnings (or more broadly the set of observables controlled for).

Within the framework of Section 5, the difference between these elasticities can be rationalized by differences in the value of having the card for newer versus older borrowers. *Caeteris paribus* (specifically, holding debt C and the second-period income distribution $dF(y_2)$ constant), newer borrowers may value the card more than older borrowers because they have fewer outside credit options in the formal sector due to their limited credit histories. This can be interpreted as a higher continuation value, v, in the model and thus a lower response to interest rate changes. Consistent with this, we find that newer borrowers on average are less likely to have another card with another bank at baseline. Using credit bureau data, 64% of the 6-11 month strata cardholders have a card with another bank, while the corresponding figure for those in the 24+M strata is 78%.[36]

Figure OA-18 shows that full payers (before the experiment) have null responses to the treatment, whereas minimum payers tend to have larger responses than the average. This is not unexpected, as contract terms are only relevant when clients use it to incur debt.

Finally, we calculate treatment effects separately for borrowers that have strong versus weak formal baseline labor market attachment. We restrict our sample to borrowers that were employed in the formal sector for at least one month between January 2004 and February 2007. We define borrowers as having a strong attachment if they were continuously employed from January 2004 to February 2007, and those that have lost employment at least once as having weak labor market attachment.[37] Figure OA-19(a) shows that default rates for borrowers with weaker labor force attachment are much higher than those for borrowers with stronger attachments. However, as was the case with newer borrowers, Figure OA-19(c) shows that borrowers with weak labor force attachments are less responsive to changes in interest rates than those with a stronger attachment, although the difference is not statistically significant at the experiment endline ($p = .283$) or 5.5 years after the experiment ended ($p = .181$).[38]

[35]The covariates (interactions between covariates and treatment indicators are also included) included are: strata indicators, age, earnings, labor force attachment, study card utilization, gender, age and card ownership.

[36]Caeteris paribus, a higher continuation value for newer borrowers implies lower default in general (and not just in response to interest rate changes). However, this is counteracted by the extent to which newer borrowers have lower incomes. For instance, holding v and C fixed (or equivalently considering a change in the second period income distribution after first period choices have been made), if $dF(y_2)$ for newer borrowers is first-order stochastically dominated by the $dF(y_2)$ distribution for older borrowers, then overall default will be higher for newer borrowers. In support of this, we find that average monthly income for newer borrowers is lower than that for older borrowers (measured in 2007)—the numbers are $8,315$ pesos as against $10,459$ pesos. Under these configurations, the model can qualitatively reconcile both higher default among newer borrowers (relative to older borrowers) and a lower response to changes in interest rates.

[37]Of those employed for at least one month between January 2004 and February 2007 (50% of our individuals with CURPs), 42% have low labor market attachment in this definition.

[38]Note that baseline labor force attachment and its interaction with treatment are included as covariates in Table OA-8.

6.4 How Costly is Formal Sector Default ?

In principle, high default rates could reflect borrowers lack of value for the study card or a low cost of default. In this section we document that default can have serious consequences for new borrowers.

Default Reduces Access to Formal Credit: Default is associated with large declines in subsequent formal sector borrowing. We explore this using the experimental sample and estimating a a cross-sectional regression where the primary explanatory variable is an indicator equal to one if a borrower defaulted on the study card in the six months after the start of the experiment (i.e. between March and September 2007) and the dependent variable is an indicator for whether the borrower obtained a new formal loan of any kind (including a card) six, twelve, or forty-eight months after September 2007. Default (see Panel A of Table OA-15) on the study card is associated with a 26 percentage point decrease in the likelihood of obtaining any new formal sector loans in the next 6 months. This is a large magnitude, given that he mean for non-defaulters is 29 percent. The negative consequences of default are also persistent. We continue to find substantial effects four years after default. Restricting attention to credit cards we find even starker results: default on the study card is associated with an absence of any subsequent credit card up to four years later.[39] Defaulters are then presumably forced to rely on informal lenders and this is not an enticing prospect as we see next.

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.[40] We find that informal loan terms are significantly worse than formal loan terms. Table OA-17 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 informal loan interest rate is 291% while the corresponding rate for formal loans is 94 points lower (col. 1). The average informal loan amount is 3658 pesos and 9842 pesos for formal loans (col. 4), and the average term for informal loans is 0.52 years against 1.07 years for formal loans (col. 9). Figure OA-27 shows that the distribution of interest rates for informal loans first-order stochastically dominates the distribution for formal loan rates while the

[39]This result is only descriptive, however we think the claim that default recorded in the credit bureau reduces credit access is not controversial. One concern with the regression above is that omitted variables may drive both default and future loan demand. We address this by adding borrower and time fixed effects and continue to find a negative relationship, in this case between delinquency (not covering one minimum payment in the study card) and subsequent borrowing. Borrowers cease to obtain any subsequent additional credit from Bank A following the first delinquency (see Table OA-16 for details). We focus on delinquency here in order to allow for borrower fixed effects as we can observe borrowers being delinquent many times but after any default the study card is closed.

[40]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.

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.[41] While clearly not dispositive, these results suggest that informal loan terms are quite onerous.

New Borrowers Respond Strongly to Credit Line Changes in the Study Card: If borrowers are credit constrained, standard models would predict an increase in borrowing following credit line increases. We find this emphatically to be the case for our experimental sample, thereby providing additional evidence on the value of our study card. Using the methodology proposed in Gross and Souleles (2002) to address endogeneity concerns, we use exogenous credit limit changes (given by the timing of the last credit limit change) to estimate debt responses to credit line changes. We find that a credit limit increase of 100 pesos on the study card translates into 32 pesos of subsequent additional debt (see Appendix D for details). This increased propensity to borrow from increases in the credit limit is about thrice as large as the figure for the United States in Gross and Souleles (2002). The use of the increased line is much higher for newer borrowers suggesting credit constraints are more binding for them (than for older borrowers).

Based on these results we conclude that it is costly to be excluded from the formal loan market, particularly for newer borrowers. The muted default response to relatively large contract term changes is consistent with the dire outside options outlined above.

7 The Effect of Job Loss on Default

Despite dire outside options, default rates among new borrowers remain high. Section 6 showed that contract terms changes do little to mitigate default at least over the (substantial) range of variation in the experiment and do not provide much evidence for default being driven by higher debt burden or by interest-rate-driven moral hazard.[42] In this section, we argue that new borrowers are vulnerable to frequent, large shocks that precipitate default. This is a simple but relatively unexplored hypothesis in the financial development literature which has typically focused on asymmetric information and high fixed lending costs (see e.g. the survey in Banerjee and Duflo, 2010). We focus on one particular shock—job separation in the formal sector—which we observe using our matched social security data.

Job loss is an appealing candidate shock for several reasons. First, job loss is common in our experimental sample: of those employed at least one month in the formal sector between Jan-

[41]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.

[42]See footnote 23. Banerjee and Duflo (2010) argue that giving loans to under-served populations is problematic because "borrowers with little wealth must get small loans, the fixed administrative cost has to be covered by the interest payment, which pushes the interest rate up. *But high interest rates exacerbate the problem of getting borrowers to repay."* (our italics). We don't find evidence for this in our sample of new borrowers.

uary 2004 and March 2007, 42% experienced at least one month out of formal sector employment. Second, newer borrowers are more likely to experience unemployment: those in the 6-11 month stratum are 1.34 times more likely (i.e., 54% vs. 40%) to experience formal sector unemployment than those in the 24+ month stratum. Third, a large literature—for developed countries with near universal formal sector employment—has shown that job loss results in both short- and long-term earnings losses (Couch and Placzek, 2010; Flaaen et al., 2019; Jacobson et al., 1993) as well as increasing the likelihood of other adverse events such as bankruptcy (Keys, 2018; Sullivan et al., 1999) and mortality (Sullivan and von Wachter, 2009). It is thus reasonable to conjecture that job loss is a significant negative shock. Its precise magnitude is less clear, particularly in developing countries with a large informal sector and frequent self-employment. For instance, Meza et al. (2022) document that formal sector job loss pushes workers into lower paid informal labor markets.

In our experimental sample we find that default for borrowers with a stronger pre-experimental attachment to the labor force was substantially lower than those for borrowers with weaker attachments. Figure 6(a) plots monthly default rates for the two groups from the beginning of the experiment through December 2014 and finds that default is systematically higher in the group with weaker labor force attachment throughout this entire period. The average gap in May 2009 (the experiment endline) is 8 pp. and by December 2014 (93 months after the experiment started), the gap is 7 pp.[43]

In this section we go further and ask whether formal sector job loss causes loan default and contrast its magnitude to that documented above for contract term changes. It is straight-forward to generate models where job loss causes default. For instance, in our framework of Section 5, if job loss is viewed as a reduction in period 2 income—e.g if the distribution for second period income is replaced by a first-order stochastically dominated distribution—then job loss increases default (Prediction 3).[44] However, while the sign of the effect is not controversial, the magnitude remains largely an open empirical question and bedeviled by endogeneity concerns.

Given the difficulty of any explicit randomization, work on the effects of job loss has focused on natural experiments and quasi-experimental methods. The most common approach to deal with the endogeneity of job loss uses mass layoffs (large net contractions in the number of workers at a firm) as a source of exogenous variation under the assumption that a given individual's job separation during a mass layoff is less likely to be subject to endogeneity concerns (relative to job loss in general). The approach was developed by Jacobson et al. (1993) and subsequently applied successfully, inter alia, by Couch and Placzek (2010); Flaaen et al. (2019) and Sullivan and von Wachter (2009). The causal effects on such workers (termed displaced workers) are interpreted as the effect of involuntary separations.[45]

[43] See Figure OA-19 for the estimated group-specific treatment effects.

[44] In the context of Appendix C, we view employment shocks as unexpected changes in future income (y_2). From Equation (4) in Appendix C, the probability of default is strictly decreasing in y_2.

[45] Couch and Placzek write that "these large-scale events [are viewed] as natural experiments that allow researchers [. . .] to accurately gauge the wage losses that result from breaking ties to a specific firm."

We focus on firms with more than 50 employees and use the universe of formal employment data (from the IMSS) to define a mass layoff month as the first month in which the year-on-year employment decrease at a firm exceeds 30 percent of average employment in the 12 months prior to the experiment. The size and layoff definitions are standard in the literature (see e.g. Davis and Von Wachter, 2011; Flaaen et al., 2019) and yield 872 mass layoff events for our experimental sample over the time-period of the experiment (Mar/07-May/09). At the firm level, mass layoffs decrease employment by 60 employees on average (about 27% of the average number of employees in a firm), and the wage bill by $424,000 MXN (about 20% of the average wage bill).

We define an individual as displaced if they lost employment in the same quarter as the mass layoff event at their firm (i.e. in the month of the layoff and the preceding and succeeding month). At the individual level, mass layoffs represent on average a decrease of $8,900 MXN in worker earnings a year after being laid off (12% of median yearly earnings). Figure OA-21 shows event study graphs for total employees, wage bills and individual earnings using the estimation approach in de Chaisemartin and D'Haultfoeuille (2022) which confirm the large effects of mass layoffs.

We interpret mass layoffs as large, sudden, plausibly exogenous shocks with respect to default on the study card (conditional on a set of time dummies and individual fixed effects). The identification assumption is that the exact timing of the mass layoff is uncorrelated with the displaced worker's potential loan default outcomes. The main threat to this empirical strategy is that the downsizing event coincides with credit market shocks that trigger default.[46] However, mass layoffs occur in every period in our data and it is unlikely that they coincide with particular credit market shocks. Further, the inclusion of time indicators controls for common time-varying shocks—so that common macroeconomic trends are not a threat as long as non-displaced workers are also exposed to the same trends. Finally, we will show that the default pre-trends for displaced and non-displaced workers are statistically indistiguishable.

We believe our evidence on the effects of mass layoffs on default is well identified relative to the literature and the only one, to our knowledge, for developing countries.[47] Relative to earlier work (in the U.S. context) our estimates reveal a more stable pre-trend, are more precise and survive estimation using recent advances in Differences-in-Difference best practices (e.g., de Chaisemartin and D'Haultfoeuille, 2022).

[46]We do not view reverse causality as an important source of endogeneity in our context especially in the light of results in the previous section.

[47]The closest paper to us is Keys (2018) which uses U.S. household survey data to examine the effects of the receipt of unemployment insurance on bankruptcy filing in a standard TWFE framework. Our approach uses administrative data to define both default as well as unemployment spells and mass layoffs, we focus only on the effect of unemployment during a mass layoff (rather than unemployment in general) to isolate exogenous variation, and can allow for individual fixed effects. Gerardi et al. (2018) is also tangentially related in that their main focus is on examining whether default arises from an unwillingness or inability to pay. Their examination of the effect of unemployment on default relies either on a selection on observables assumption or the construction of Bartik-type instruments for individual residual income which require strong assumptions for validity.

We examine the effect of being separated as part of a mass layoff in an event-study design. Denote τ_i as the month in which individual i was displaced (i.e., lost their job due to a mass layoff). For borrower i in month t, we specify the following estimating equation for default on the study card:

$$\text{default}_{it} = \alpha_i + \gamma_t + \sum_{k \neq 0} \beta_k \times \mathbb{1}\left\{t - \tau_i = k\right\} + \varepsilon_{it} \tag{2}$$

where α_i and γ_t are individual and month fixed-effects. With this specification we can compare borrower behavior before and after job loss due to a mass layoff. We also include non-displaced borrowers to serve as a "pure control" group and include dummies for leads and lags and so can then provide suggestive evidence for parallel trends. In addition to the standard two-way fixed effects model, we use the staggered difference-in-difference methodology developed by de Chaisemartin and D'Haultfoeuille (2022) which remains valid even with heterogenous and dynamic effects of displacement on default.

Figure 7(a) shows the effect of job separation as part of a mass layoff on default for our experimental sample during the experimental time period. The dependent variable is cumulative default in the experimental card—the same outcome as in the previous sections. We estimate no differential pre-trends in default between displaced and non-displaced workers before separation, suggesting that their behavior in the credit market was similar prior to separation. We find that one year after separation, borrowers are 4.8 percentage points more likely to default on the study card and increasing to 6.1 pp. after eighteen months. The effect is 2.4 times the size of the interest rate treatment effect at the end of the experiment (i.e. the effect of decreasing interest rates from 45% to 15% for 26 months), and 7.6 times larger than the size of the minimum payment treatment effect at the end of the experiment (increasing minimum payments from 5% to 10% for 26 months).

Figure 7(b) repeats our estimation exercise using the universe of Mexican formal workers (rather than formal workers in the experimental sample) and default in any loan in the credit bureau (rather than default on the credit card). The larger and more representative sample (of all formally employed borrowers with formal credit between October 2011 and March 2014) yields substantially more mass layoff events (8,723) and displacements but finds quantitatively similar results to those above, thereby providing a measure of external validity to our study estimates.

We offer two conclusions from this exercise. First, even if the informal labor market serves as a buffer for formal job loss (as may be the case in Mexico), losing formal employment has substantial and persistent negative effects on agents' credit market outcomes. The fact that newer borrowers are more subject to these shocks is consistent with them defaulting at higher rates. Second, job separation is a far more significant determinant of default than substantial increases in contract terms, at least for the new borrower population we study.

A narrative consistent with this evidence is that new borrowers attempt to minimize default, since they have fewer formal borrowing options beyond the study card. That is, the punishment from default—in this case losing access to their card, what we called v in the model—limits interest-driven moral hazard. Consequently, even large (30pp) increases in interest rates do not increase default substantially despite increasing debt burden or the exacerbation of interest-rate-driven moral hazard (Banerjee and Duflo, 2010; Bizer and DiMarzo, 1992; Karlan and Zinman, 2009; Stiglitz and Weiss, 1981). However, the consequences of unemployment and the large income loss it entails are harder mitigate and, as in the model, borrowers default. This is consistent with Hsu et al. (2018), who argue that declines in mortgage defaults attributable to unemployment insurance expansions primarily worked through increasing loan affordability.[48]

8 Conclusion

Credit card borrowing is an increasingly common way for borrowers to first access formal sector credit in many developing countries and has received increased attention from policymakers and regulators. In this paper we examine a large-scale effort by a commercial Mexican bank to expand credit via credit cards to poor and financially inexperienced new borrowers. We combine detailed card-level data for a bank product that in 2009 accounted for 15% of all first-time formal loans in Mexico with individual employment histories and a nation-wide randomized experiment with 144,000 borrowers.

We find that default rates are high and higher for newer borrowers. Next, we use an RCT to assess default sensitivity to key contract terms. We find that doubling the minimum payment increased default, contrary to the beliefs underpinning recent regulation; but consistent with our modeling framework. This yields sobering evidence on the effectiveness of the commonly proposed policy of increasing minimum payments to limit default. We also find that a 30pp higher interest rate has modest effects on default, and zero effect for new borrowers.

In stark contrast to the above results, we find that default in our experimental sample is very responsive to plausibly exogenous job separation. These findings speak to the fragility of economic conditions for new borrowers. A formal sector job separation leads to a substantial and persistent increase in default on the study card. Default in turn usually leads to exclusion from the formal credit market with its more attractive terms. Combining these findings with the limited effects of experimental interest rate reductions in reducing default, we conjecture that, in our context, default is less likely to be driven by interest-rate-driven moral hazard than by large, frequent shocks experienced by new poor borrowers. Such shocks could also explain the puzzle of why lower income borrowers recurrently fall into debt (see e.g. the discussion in Karlan and Zinman, 2019).

Bank A stopped issuing the study card in 2009. We speculate that the combination of the ineffectiveness of contract terms in limiting default documented here combined with the limited information available for screening borrowers contributed to the bank's decision. Given the difficulty of

[48]Where affordability is defined as the mortgage payment to income ratio.

modifying default behavior, improving screening of loan applicants would appear to be key. This is clearly challenging for borrowers with limited histories, but there has been progress with mobile (see e.g. Björkegren and Grissen, 2019) and other kinds of data. A broader question 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 limited credit histories. Finally, given the prevalence of shocks as documented here, it would be interesting to study whether some form of insurance or social protection policies would be useful in improving credit market outcomes.

Tables

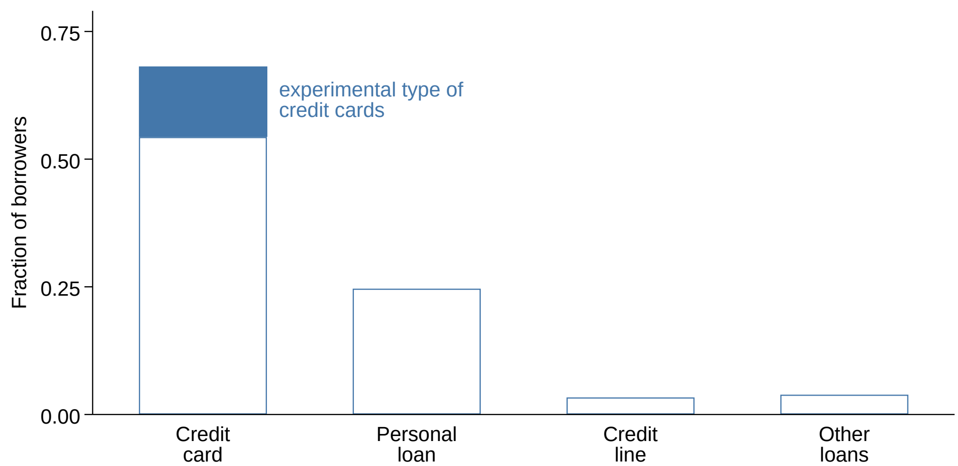
Table 1: Summary Statistics and Baseline Characteristics

	Experimental sample	Experimental sample	Credit bureau sample		
			$\geq$ 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)			
Credit score	645	-	-	-	-
	(52)				
(%) Consumers for whom experiment is their first card	57	-	-	-	-
(%) Consumers who default between Mar/07 - May/09	17	-	-	-	-
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 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. (‡) 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% of the CB sub-sample were matched with the IMSS via Tax IDs (RFCs).

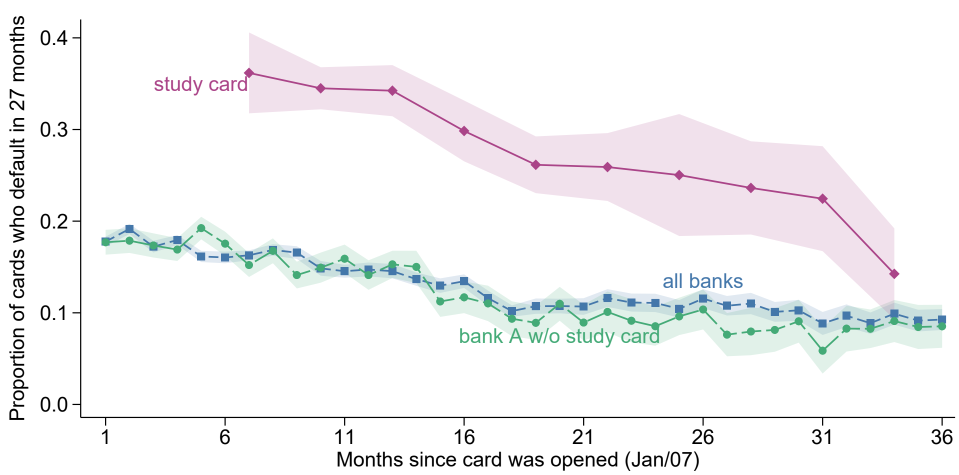
Figures

Figure 1: First Time Loans, by Type



Notes: This figure is constructed using a representative sample of one 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. credit cards, personal loans, credit lines, auto loans, real estate loans). We then plot the fraction of first loans by type. The blue area represents the type of card (described in Section 2) we study.

Figure 2: Default, by Months with the Credit Card



Notes: This figure is constructed using a representative sample of one million borrowers in the credit bureau in 2010 (blue squares and green dots), and with the control group from our study credit card (red diamonds). The figure plots the probability that a credit card defaults on or before May 2009 (y-axis) against card tenure as of January 2007 (x-axis). The red diamonds show, for the control group of our study card, the proportion of cardholders that default by the months since the card was opened (binned into quarters). The blue squares and green circles repeat the sampling exercise in the credit bureau data. The blue squares use all cards, whereas the green circles restrict attention to Bank A cards that are not the same type as the card we study.

Figure 3: Timeline for the Datasets

1. **Bank data:**

   Monthly card-level data of the study card from Mar/07 to May/09, bimonthly from Jun/10 to Dec/11 and monthly from Jan/12 to Dec/15.

2. **Credit Bureau data:**

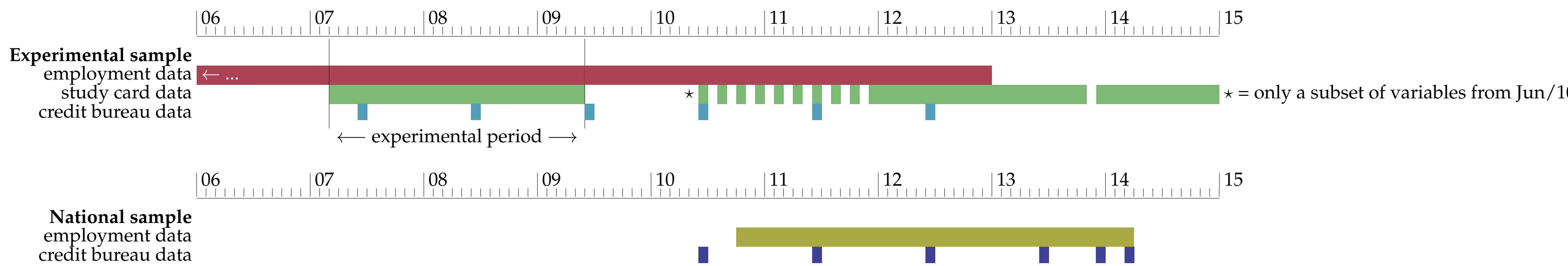
   Loan-level data matched to the experimental sample for Jun/07 to Jun/12, annually.

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

3. **Social security employment data:**

   Individual-level data matched to the experimental sample, monthly information from Jan/04 to Dec/12.

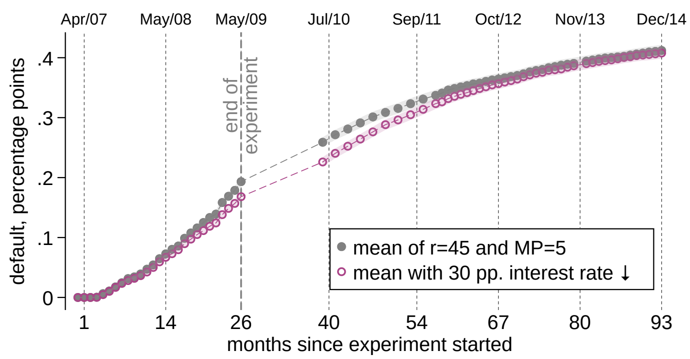
   Individual-level, monthly information from Oct/10 to Mar/14.



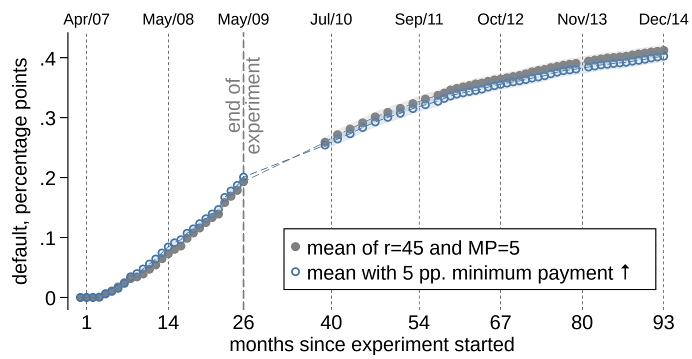
Notes: This figure presents a timeline for the experiment. The data for the 9 experimental strata was recorded in January 2007. Data from the experiment is provided monthly for each card from March 2007 to May 2009, bimonthly from June 2010 to December 2011, and monthly from January 2012 to December 2014 (with the exception of November 2013). Starting in June 2010, we only observe a limited set of variables that includes default and payments. We use CB information for the experimental sample, which is provided to us in 6 snapshots: June 2007-2012. The remaining datasets are the random sample credit bureau data, and the social security data.

Figure 4: Treatment Effect of Contract Terms on Default
(Share of Cardholders that Default)

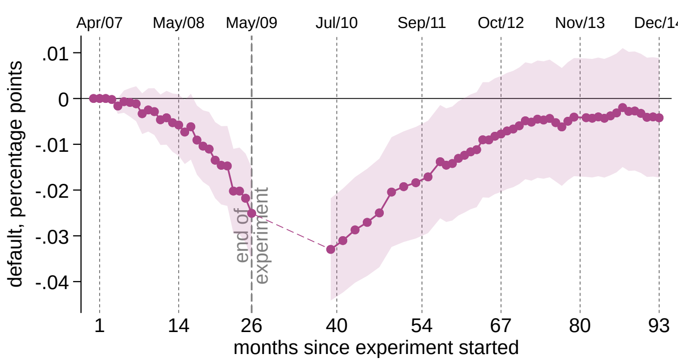
(a) Comparison of Means
(Different Interest Rates)



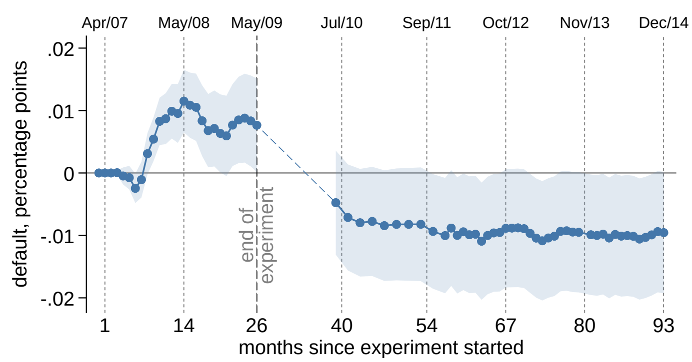
(b) Comparison of Means
(Different Minimum Payments)



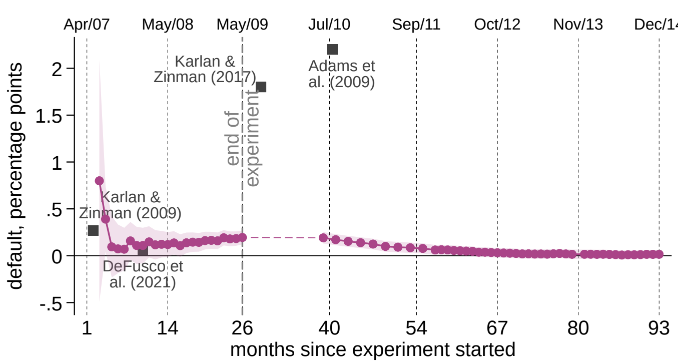
(c) ATE of 30 pp. Interest Rate Decrease



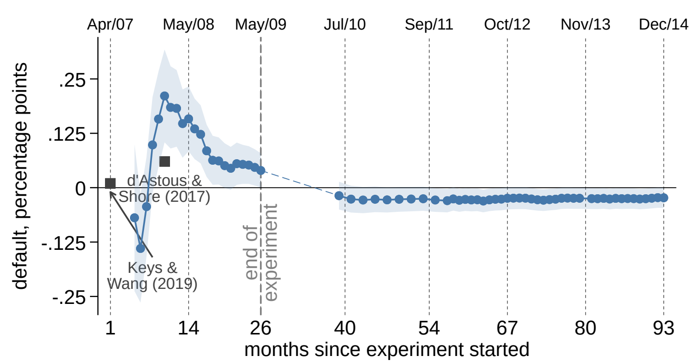
(d) ATE of 5 pp. Minimum Payment Increase



(e) Comparison to the Literature
Interest Rate ATE in elasticity terms

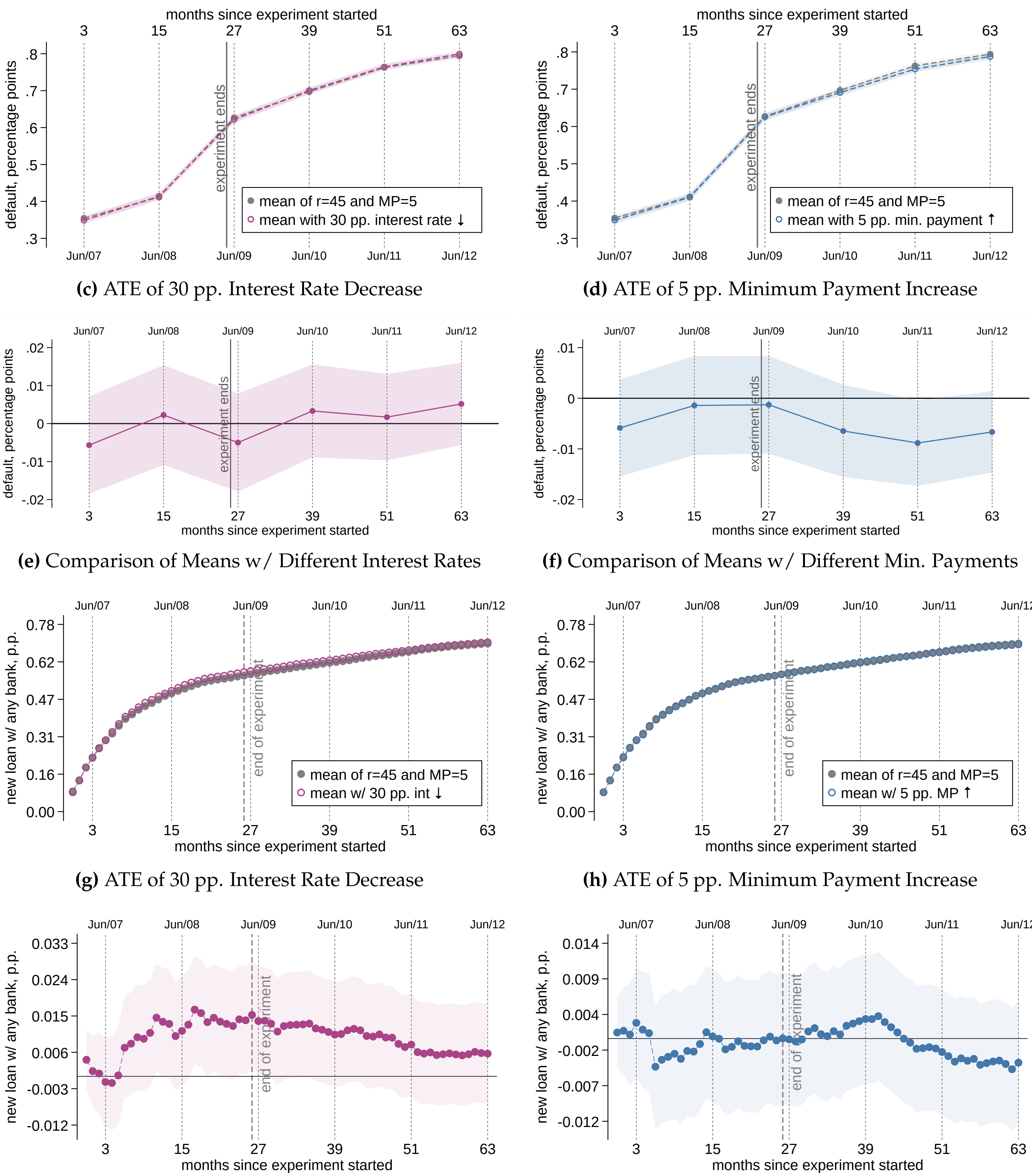


(f) Comparison to the Literature
Minimum Payment ATE in elasticity terms



Notes: These figures plot the causal effect of interest rates and minimum payment changes on default in the experiment credit card. Figures on the left examine interest rate changes. Figures on the right examine minimum payment changes. The grey dots in Panels (a) and (b) plot the share of cardholders that default over time in the $(r = 45\%, MP = 5\%)$ group. The red dotted line in Panel (a) plots the share of cardholders that default over time when interest rate is decreased by 30 pp. from 45% to 15%. The difference between the two lines in Panel (a) is is plotted in Panel (c) and corresponds to the average treatment effect of a 30 pp. interest rate decrease from 45% to 15%. Panel (e) computes the elasticity of default by computing the average treatment effect in percent terms(i.e., β_t/α_t in Equation (1)) and dividing it by $(45-15)/45$. Similarly, Panels (b) plots the comparison of the share of cardholders that default when the minimum payment increases by 5 pp. relative to the $(r = 45\%, MP = 5\%)$ group; Panel (d) computes the average treatment effect of a 5 pp. minimum payment increase and Panel (f) computes the elasticity of default (i.e., β_t/α_t in Equation 1 divided by $(10-5)/5)$ with respect to a minimum payment increase from 5% to 10%.

Figure 5: Effect of Contract Terms on Default in Any Other Loan (top) & New Loans Issuance (bottom)



(a) Comparison of Means w/ Different Interest Rates

(b) Comparison of Means w/ Different Min. Payments

(c) ATE of 30 pp. Interest Rate Decrease

(d) ATE of 5 pp. Minimum Payment Increase

(e) Comparison of Means w/ Different Interest Rates

(f) Comparison of Means w/ Different Min. Payments

(g) ATE of 30 pp. Interest Rate Decrease

(h) ATE of 5 pp. Minimum Payment Increase

Notes: These figures plot the causal effect of interest rates and minimum payment changes on default in other loans and on new bank loan issuances. The dependent variable is default in any loan in the credit bureau except for the experiment credit card [Panels (a) to (d)] and a cumulative categorical variable on new loans from March 2007 to the given date [Panels (e) to (h)]. The data source for the dependent variables is the credit bureau. The figures on the left examine interest rate changes. The figures on the right examine minimum payment changes. The dots in Panels (a) and (b) [(e) and (f)] plot the share of cardholders that default over time [share of cardholders that obtain a new loan] in the $(r = 45\%, MP = 5\%)$ group. The difference between the two lines in Panel (a) [Panel (e)] is plotted in Panel (c) [Panel (g)] and corresponds to the average treatment effect of a 30 pp. interest rate decrease from 45% to 15%. Similarly, Panel(d) [Panel (h)] computes the average treatment effect of a 5 pp. minimum payment increase from 5% to 10%.

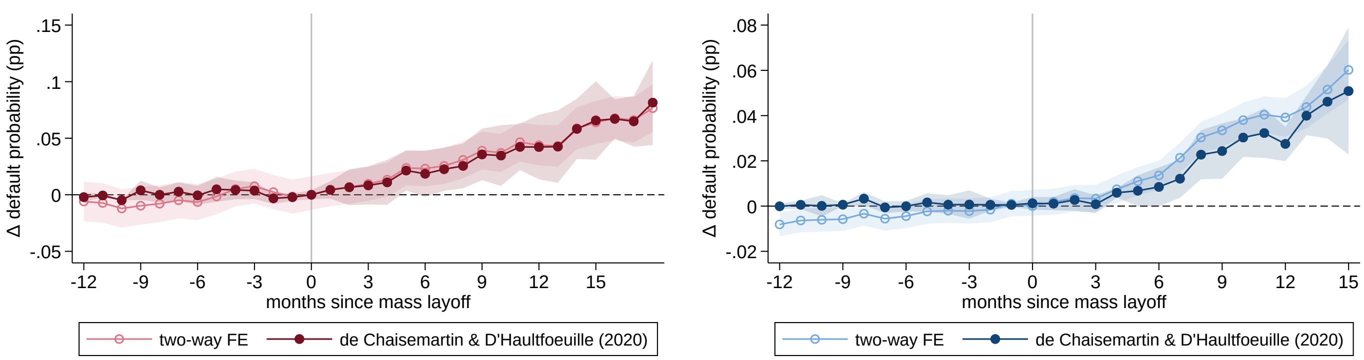
Figure 6: Default in Experiment Credit Card by Job Status
(Comparison of Means in the $r = 45, MP = 5$ Group)

(a) Lost Job Prior To The Experiment **(b)** Lost Job During The Experiment

Notes: These figures plot the difference in default between those who lost their job and those who did not in the $r = 45, MP = 5$ treatment group. The dependent variable is (cumulative) default in the study card. Panel (a) focuses on individuals who were employed continuously from January 2004 to Feb 2007 (in dark green) vs those employed formally for at least one month in the same period but not in all months (in light green). Panel (b) compares those who were employed continuously in the formal sector from March 2007 to May 2009 (in dark green) vs. those who were employed for at least one month but not all the time (in light green).

Figure 7: Job Displacement and Default

(a) Experimental Sample & Default in Experiment Card **(b)** Full Credit Bureau & Default in Any Loan



Notes: These figures plot the effect of being displaced from the formal labor market on default. Panel (a) plots the effect for displaced workers in the experimental sample and the dependent variable is default in the experiment credit card. Panel (b) uses all credit holders in Mexico and plots the effect for default in any loan in the credit bureau. The x-axis measures time since displacement (i.e., the downsizing event). The light-colored hollow circles in both panels are the regression coefficients of months since displacement with individual and month fixed effects. The dark-colored circles use the methodology developed by de Chaisemartin and D'Haultfoeuille (2022). For the months after displacement, the $l - th$ coefficeint compares displaced individuals and those not-yet displaced, from the displacement month until month l. For the months before displacement, the $l - th$ coefficient compares displaced individuals and those not yet displaced, l months before displacement.

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Contract Terms, Employment Shocks, and Default in Credit Cards

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Appendix – For Online Publication

Contents

A Additional Tables and Figures

A.1 Additional Tables

Table OA-2: 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)
Adams et al. (2009)	Default Hazard	4 (p.28)	1.022 (.002)	+2.2 (0.2)
d'Astous and Shore (2017)	Default	p.3	.04	+0.06
Keys and Wang (2019)	Delinquency	2 (p.542)	.4 (.3)	+0.01 (.12)
Karlan and Zinman (2019)	Delinquency	5 (p.42)	-1.96 (1.45)	+1.80
DeFusco et al. (2021)	Charge-Off	(p.2)	.1	.01

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 (2019) we use the results from Table 5 (col (4), Panel B) that show 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 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). The figures for DeFusco et al. (2021) are taken from the introduction. Standard errors for elasticities are computed using the delta method.

Table OA-3: Sampling weights

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

Notes: The table shows the sampling weights used throughout our analysis. Each cell shows the share of individuals in the population from which the experimental sample was drawn.

Table OA-4: 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
Hold out group	2,000	

Notes: The table shows the experimental design. Panel A shows the sample composition. Our 162,000 individuals are composed by 9 cells, each of which is a combination of the months with the credit card and the January 2007 payment behavior. Panel B shows, for each of the 18,000 individuals within each of the strata cells, how they were assigned to each of the 8 treatment arms and the control group.

Table OA-5: 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.

Table OA-6: 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 parentheses. One, two and three stars denote statistical significance at the 5, 1 and 0.1 percent level respectively.

Table OA-7: Experimental Effects of Contract Terms on Default

	Experimental period			Post-experimental period					
Months since experiment started:	5 Aug/07 (1)	16 Jul/08 (2)	26 May/09 (3)	39 Jun/10 (4)	49 Apr/11 (5)	60 Mar/12 (6)	71 Feb/13 (7)	82 Jan/14 (8)	93 Dec/14 (9)
Panel A. Main specification									
$(45\% - r_i)/30\%$	-0.001	-0.006	-0.025***	-0.033***	-0.020***	-0.013*	-0.005	-0.004	-0.004
	(0.001)	(0.004)	(0.005)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)
$\mathbb{1}\{MP_i = 10\%\}$	-0.001	0.011***	0.008*	-0.005	-0.008	-0.009*	-0.010*	-0.010*	-0.010
	(0.001)	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Constant	0.011***	0.086***	0.193***	0.259***	0.309***	0.349***	0.373***	0.396***	0.412***
	(0.001)	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Panel B. Fully saturated model									
$\mathbb{1}\{r = 15, MP = 5\}$	0.001	-0.002	-0.026***	-0.038***	-0.021*	-0.014	-0.004	-0.004	0.001
	(0.002)	(0.005)	(0.008)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)
$\mathbb{1}\{r = 15, MP = 10\}$	-0.001	0.007	-0.014	-0.039***	-0.028**	-0.024**	-0.016	-0.015	-0.014
	(0.002)	(0.005)	(0.008)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)
$\mathbb{1}\{r = 25, MP = 5\}$	0.001	-0.003	-0.022**	-0.036***	-0.022*	-0.017	-0.013	-0.017	-0.014
	(0.002)	(0.005)	(0.008)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)
$\mathbb{1}\{r = 25, MP = 10\}$	-0.000	0.007	-0.008	-0.028**	-0.020*	-0.012	-0.012	-0.015	-0.014
	(0.002)	(0.005)	(0.008)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)
$\mathbb{1}\{r = 35, MP = 5\}$	0.003	0.006	0.000	-0.007	0.003	0.005	0.005	0.005	0.006
	(0.002)	(0.006)	(0.008)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)
$\mathbb{1}\{r = 35, MP = 10\}$	0.002	0.016**	-0.001	-0.018*	-0.010	-0.009	-0.004	-0.007	-0.001
	(0.002)	(0.006)	(0.008)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)
$\mathbb{1}\{r = 45, MP = 10\}$	0.001	0.013*	0.005	-0.016	-0.015	-0.018	-0.019	-0.019*	-0.015
	(0.002)	(0.006)	(0.008)	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)
Constant ($r = 45, MP = 5$)	0.009***	0.083***	0.193***	0.263***	0.309***	0.348***	0.374***	0.398***	0.412***
	(0.001)	(0.004)	(0.006)	(0.006)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Panel C. Hypothesis testing with fully saturated model (p-values)									
r ATEs are linear	0.560	0.066	0.547	0.371	0.490	0.479	0.605	0.294	0.305
MP ATE is separable from r	0.816	0.976	0.442	0.194	0.571	0.344	0.542	0.477	0.658
r ATEs are separable from MP	0.684	0.021	0.088	0.468	0.489	0.289	0.481	0.437	0.411
no ATEs	0.661	0.003	0.000	0.000	0.006	0.042	0.178	0.119	0.139
Observations	144,000	144,000	144,000	144,000	144,000	144,000	144,000	144,000	144,000

Notes: All regressions use sample weights. Each column (within each panel) is a different regression. The dependent variable is default in the study card measured at different points in time, each denoted above the column numbers. Panel A shows the coefficients of Equation 1. Panel B shows the coefficients of a regression of default on treatment arm categorial variables (excluding the $r = 45, MP = 5$ treatment group). Panel C shows the p-values of several hypothesis tests performed on the fully saturated model that validates our preferred specification. To test that the interest rate ATEs are linear, we (jointly) test whether $\mathbb{1}\{r = 15, MP = x\} = 1.5 \cdot \mathbb{1}\{r = 25, MP = x\} = 3 \cdot \mathbb{1}\{r = 35, MP = x\}$ for $x = 5, 10$. To test that the minimum payment ATE is separable from the interest rate, we test that $\mathbb{1}\{r = 45, MP = 10\} = \mathbb{1}\{r = x, MP = 10\} - \mathbb{1}\{r = x, MP = 5\}$ for $x = 15, 25, 35$. To test that the interest rate ATEs are separable from the minmum payment, we test that $\mathbb{1}\{r = x, MP = 5\} = \mathbb{1}\{r = x, MP = 10\}$ for $x = 15, 25, 35$. To test that there are no treatment effects, we test that the seven treatment arms are equal to zero. 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.

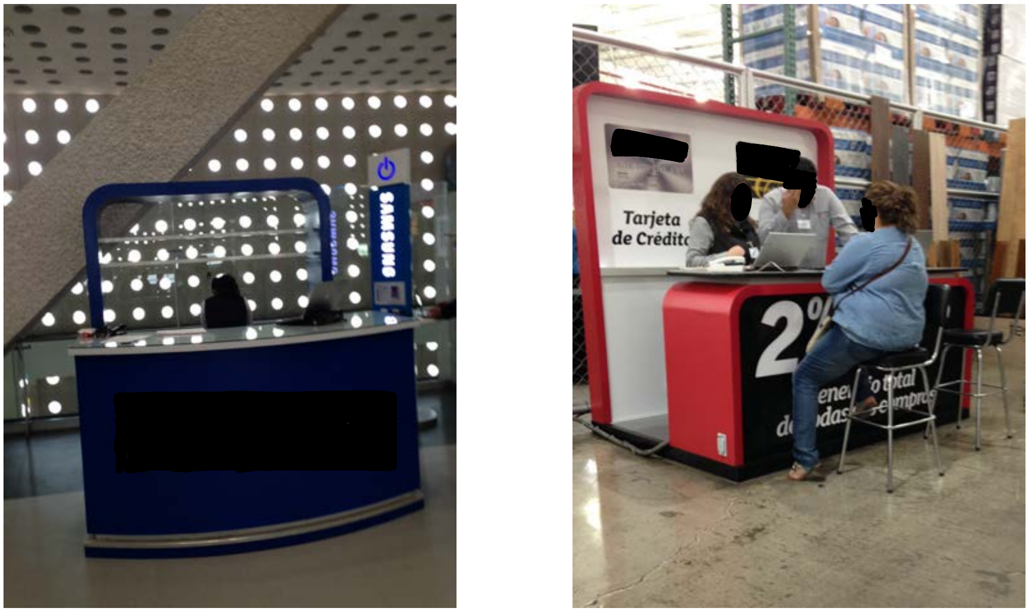
Table OA-8: Difference in Average Treatment Effect Across Months With Card Strata (Cumulative Default by May 2009 - Experiment Endline)

	(1)	(2)	(3)	(4)
	b/se/p	b/se/p	b/se/p	b/se/p
(45-r)/30 × 24+ M with card	-0.019	-0.022*	-0.021*	-0.021
	(0.011)	(0.011)	(0.011)	(0.011)
	[0.079]	[0.035]	[0.050]	[0.056]
1(MP = 10) × 24+ M with card	-0.005	-0.003	-0.004	-0.003
	(0.008)	(0.008)	(0.008)	(0.008)
	[0.558]	[0.709]	[0.637]	[0.665]
Interaction of treatment with:				
age terciles	No	Yes	Yes	Yes
gender	No	Yes	Yes	Yes
baseline utilization	No	Yes	Yes	Yes
other cards at baseline	No	Yes	Yes	Yes
always informal pre-experiment	No	No	Yes	Yes
ever unemployed pre-experiment	No	No	Yes	Yes
pre-experimental earnings terciles	No	No	No	Yes
Observations	144,000	142,693	142,693	142,693

Notes: This table documents the treatment effect difference across the months with credit card strata. The dependent variable is cumulative default measured in May 2009 (the end of the experiment). All regressions use strata weights. The specification of Column (1) includes the two treatment variables (i.e., $(45 - r_i)/30$ for interest rates and $1(MP_i = 10)$ for minimum payments, not reported for brevity), the months since credit card strata, and the interaction of these two treatment variables with our months to credit card strata. We use the 6-11M with card strata as the omitted group. Column (2) includes for (in addition to strata-specific treatment effects) other baseline covariates and their interaction with treatments. The covariates include age (terciles), gender, credit utilization (as a continuous variable), and a categorical variable on whether individuals have another card at baseline. Column (3) adds labor-market heterogeneity (income and labor force attachment). Robust standard errors are shown in parentheses. Column (4) adds pre-experimental earnings terciles (the sum of all formal sector earnings from January 2004 until February 2007). One, two and three stars denote statistical significance at the $.05$, $.01$ and $.001$ level, respectively. Squared brackets report two-sided test p-values.

A.2 Additional Figures

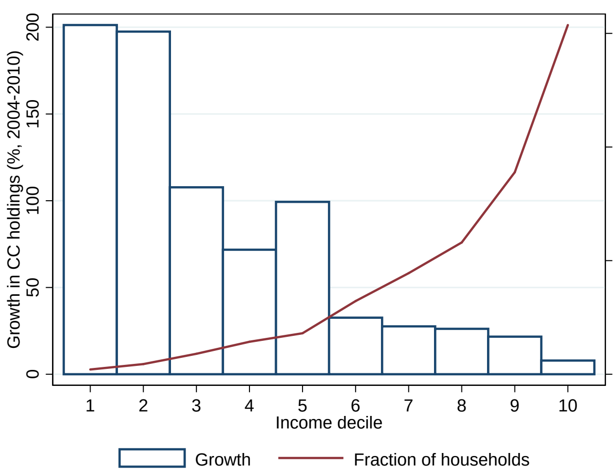
Figure OA-8: Example of Promotional Kiosks



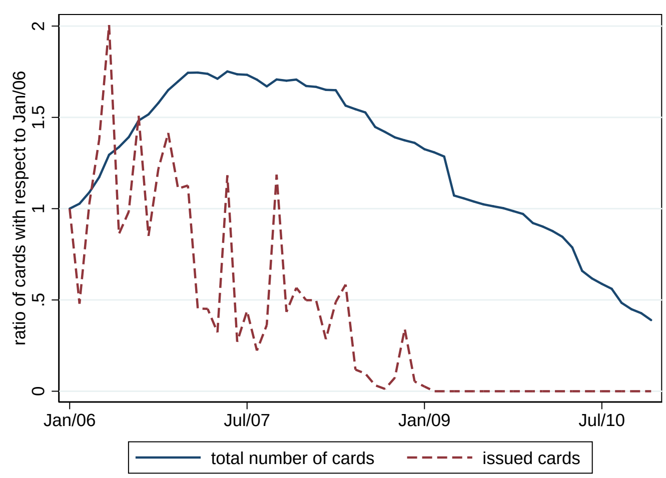
Notes: These kiosks do not necessarily correspond to those for our study card (for confidentiality reasons). They are similar to the ones Bank A used to sign up individuals for the study card.

Figure OA-9: Overall credit card growth, and study card's share and evolution.

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

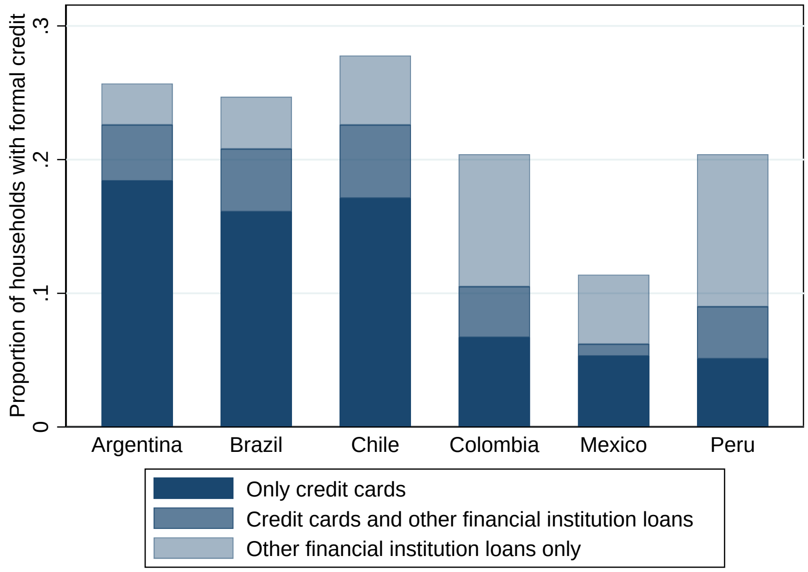


(b) Study Card Stocks and Flows



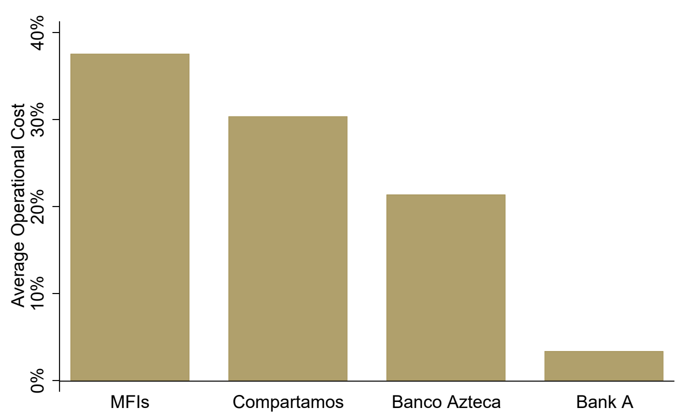
Notes: Panel (a) is constructed using data from the 2004 National Income Expenditure Survey (ENIGH). The X-axis represents (household) income deciles (the 10th decile is the richest decile). The left Y-axis— corresponding to the hollow bars—shows the percentage growth in the number of households that 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 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 issued in a given month.

Figure OA-10: 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. Credit holders 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.

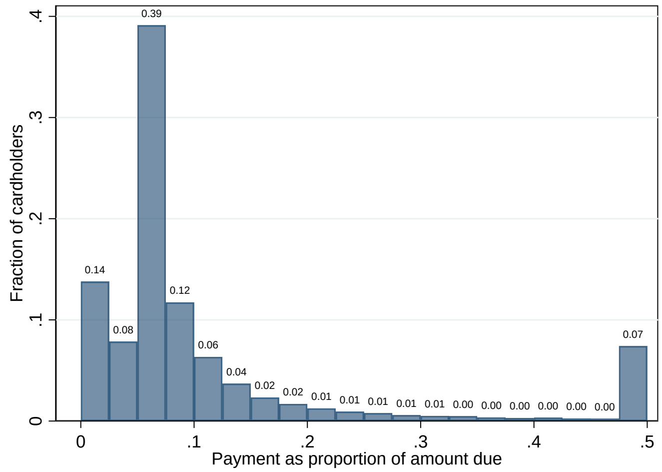
Figure OA-11: Operational Costs (relative to Assets): Compartamos, Azteca, and Bank A



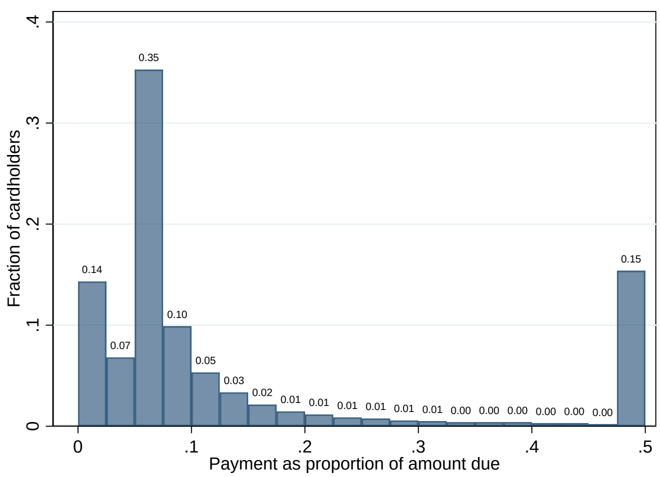
Notes: The cost ratio is defined as the ratio of administrative and promotion spending to total assets. Data is taken from the Mexican Banking Comission (CNBV) at `https://portafoliodeinformacion.cnbv.gob.mx/bm1/Paginas/infosituacion.aspx` (under 040-5Z-R6, indicadores financieros). We average annual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

Figure OA-12: Payment as a Fraction of Debt Before the Experiment

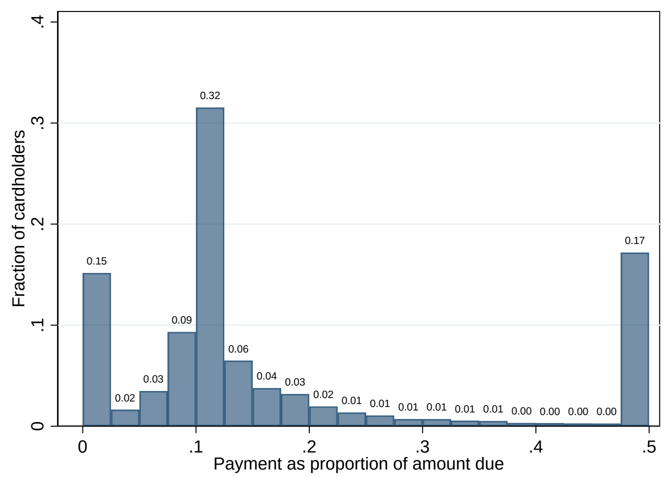
(a) Mar/07 (start of experiment) - all treatment arms



(b) Oct/07 (6 months into the experiment)
treatment arms with mp = 5%

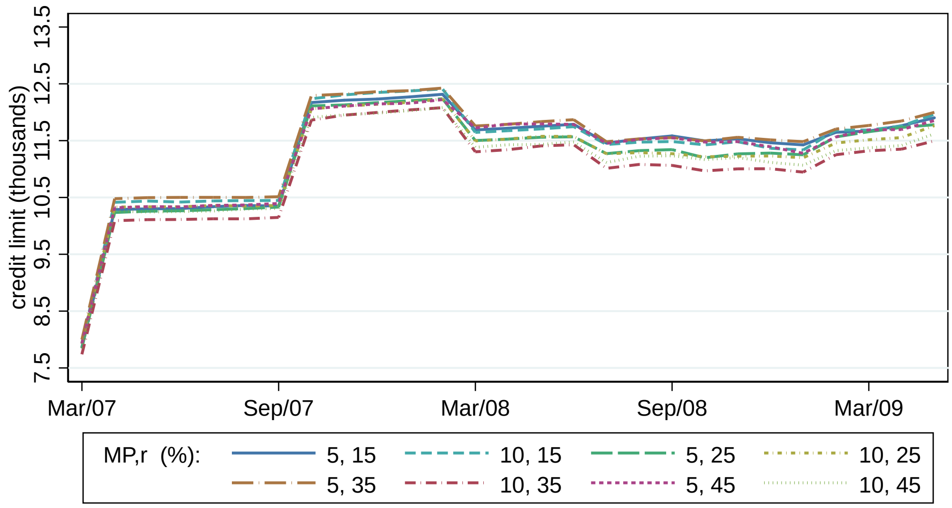


(c) Oct/07 (6 months into the experiment)
treatment arms with mp = 10%



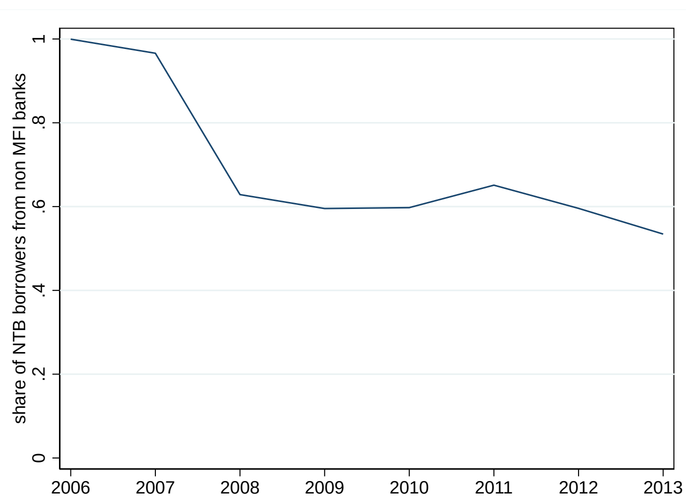
Notes: We plot monthly payment divided by the amount due. In Figure (a) this is the ratio of monthly payments in April 2007 and the amount due in the March 2007 statement. In Panels (b) and (c) we examine the ratio of monthly payments in October 2007 to the amount due in the September 2007 statement. 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.

Figure OA-13: Credit Limits by Month by Treatment Arms



Notes: This figure plots the monthly credit limit against treatment arms to show that credit limits are orthogonal to randomization. Table OA-6 formally tests this hypothesis.

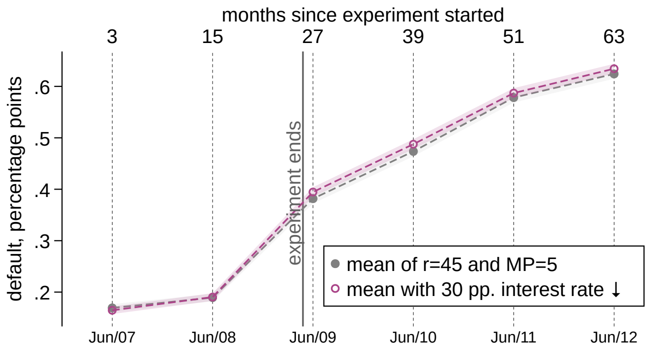
Figure OA-14: Study Card Demise Coincides With Smaller Share Of Loans Going To New Borrowers (Share of New Loans Going to New Borrowers in Mexico, All Lenders)



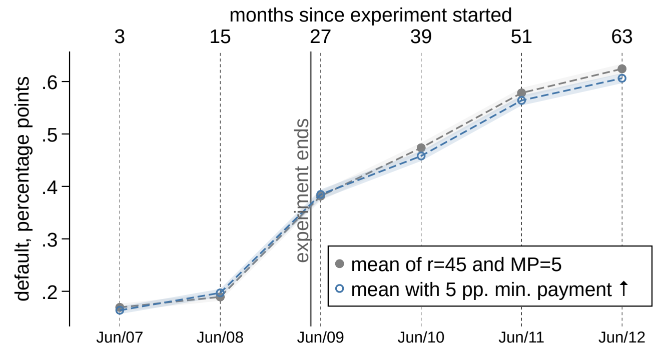
Notes: This figure plots the fraction of total newly originated loans (for the whole of Mexico) going to borrowers with no previous formal credit history each year, from 2006 when our study card was in its peak throughout the period in which Bank A reduced its rate of issuance (2007-2009), and when Bank A stopped issuing it altogether in 2010. We normalize 2006 to 1, so that changes in the share of new loans awarded to new borrowers can be easily read. In 2008, when Bank A reduced issuance of the Study Card, the share of loans going to new 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 does not necessarily reflects a causal relationship between the closing of the Study Card and financial inclusion and is only intended to be suggestive. There was no recovery of the share of loans going to new borrowers afterwards.

Figure OA-15: Spillovers to Bank A: Default on Other Bank A Loans (top) & New Bank A Loans (bottom)

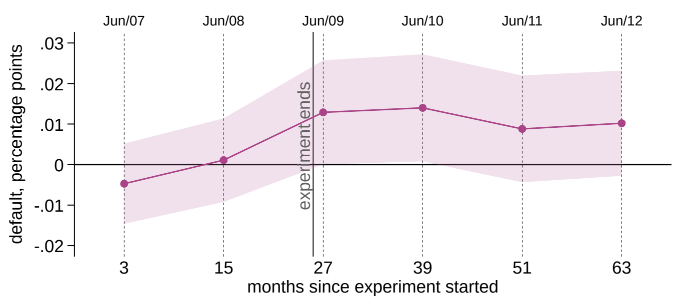
(a) Comparison of Means w/ Different Interest Rates



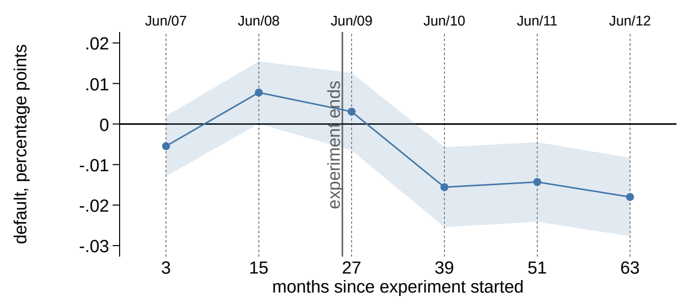
(b) Comparison of Means w/ Different Min. Payments



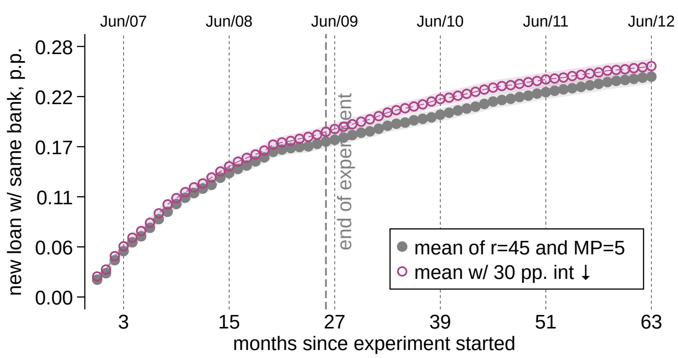
(c) ATE of 30 pp. Interest Rate Decrease



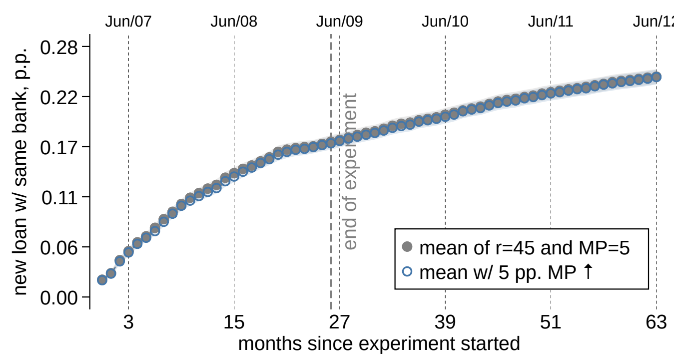
(d) ATE of 5 pp. Minimum Payment Increase



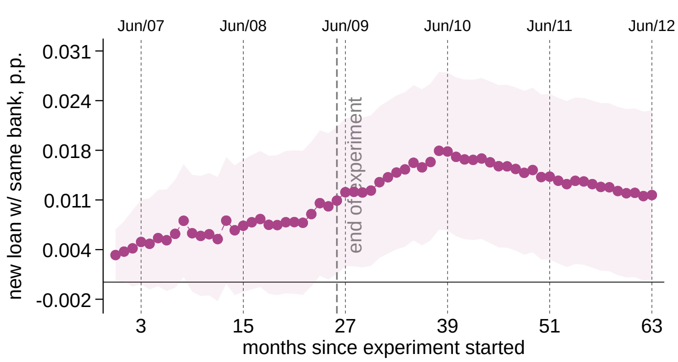
(e) Comparison of Means w/ Different Interest Rates



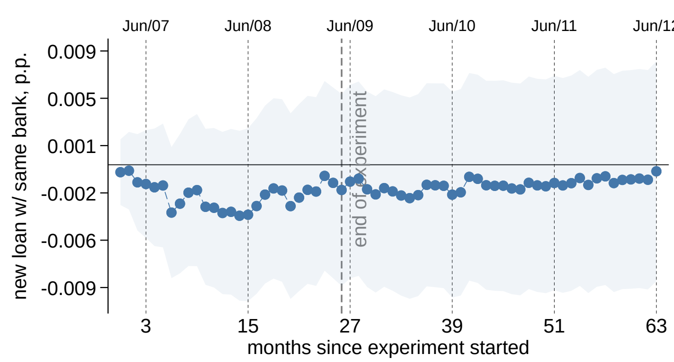
(f) Comparison of Means w/ Different Min. Payments



(g) ATE of 30 pp. Interest Rate Decrease



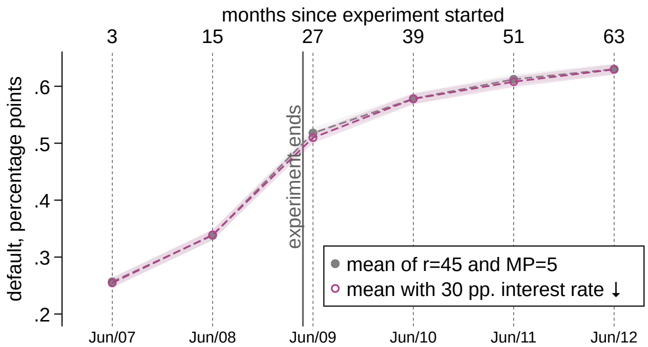
(h) ATE of 5 pp. Minimum Payment Increase



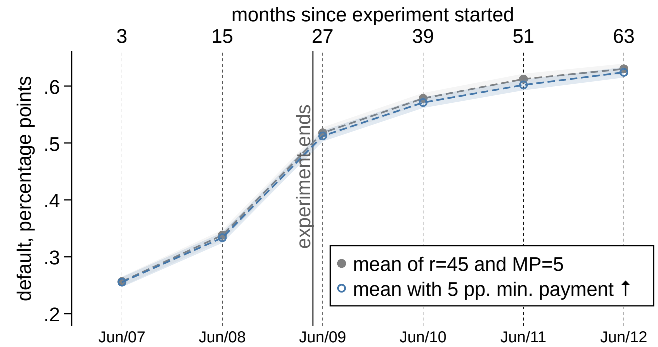
Notes: These figures plot the causal effect of interest rates and minimum payment changes on default in other loans in the same bank and on new bank A loan issuances. The dependent variable is default in any loan issued by Bank A except for the experiment credit card (a to d) and a cumulative categorical variable on new loans from Bank A from March 2007 to the given date. The data source for the dependent variables is the credit bureau. The figures on the left examine interest rate changes. The figures on the right examine minimum payment changes. The dots in Panels (a) and (b) [(e) and (f)] plot the share of cardholders that default over time [share of cardholders that obtain a new loan] in the $(r = 45\%, MP = 5\%)$ group. The difference between the two lines in Panel (a) [Panel (e)] is plotted in Panel (c) [Panel (g)] and corresponds to the average treatment effect of a 30 pp. interest rate decrease from 45% to 15%. Similarly, Panel (d) [Panel (h)] computes the average treatment effect of a 5 pp. minimum payment increase from 5% to 10%.

Figure OA-16: Spillover to Other Banks: Default on Other Bank Loans (top) & New Bank Loans (bottom)

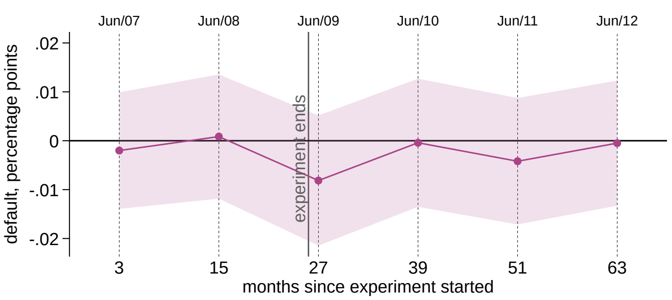
(a) Comparison of Means w/ Different Interest Rates



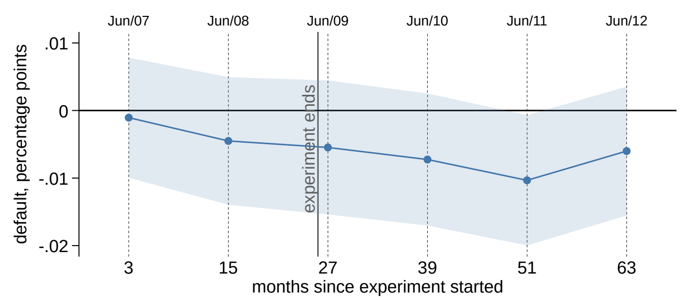
(b) Comparison of Means w/ Different Min. Payments



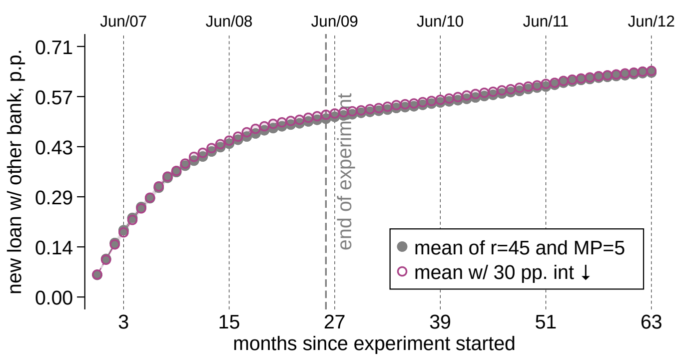
(c) ATE of 30 pp. Interest Rate Decrease



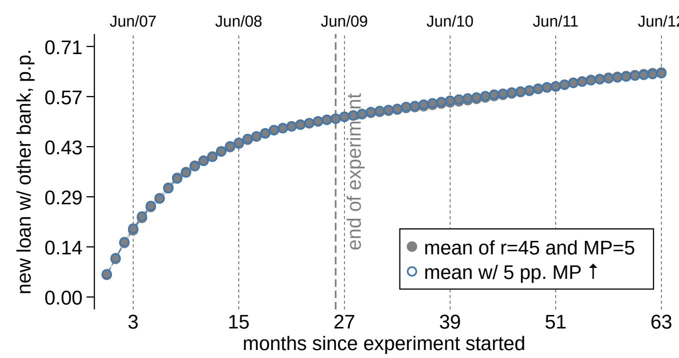
(d) ATE of 5 pp. Minimum Payment Increase



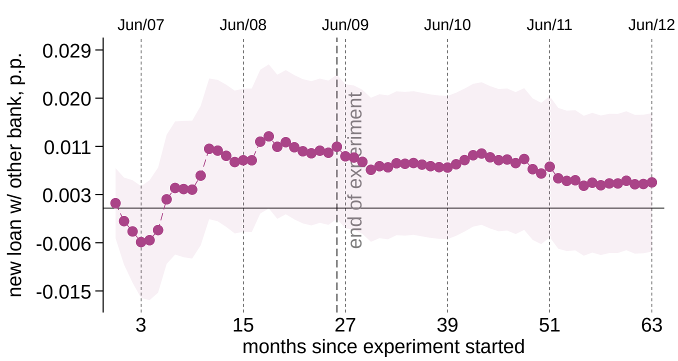
(e) Comparison of Means w/ Different Interest Rates



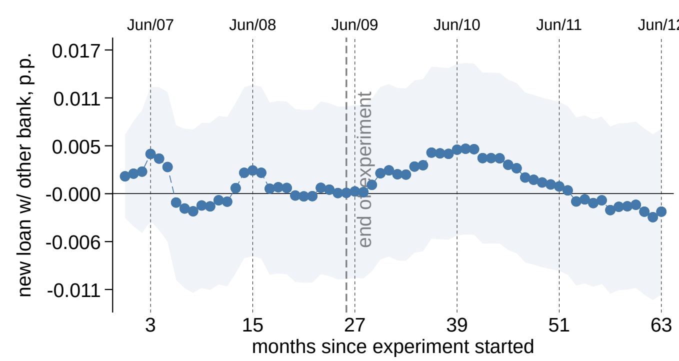
(f) Comparison of Means w/ Different Min. Payments



(g) ATE of 30 pp. Interest Rate Decrease

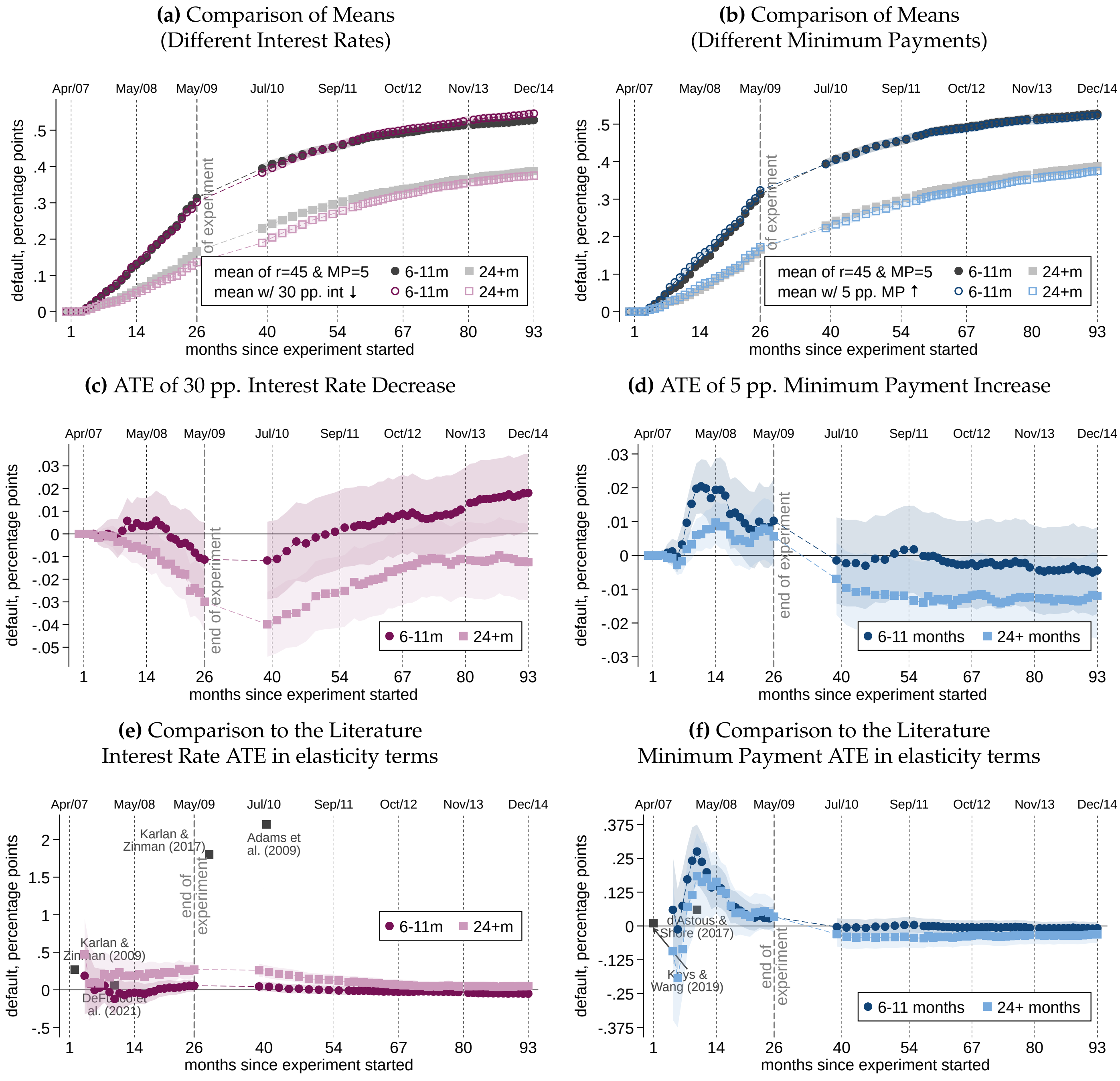


(h) ATE of 5 pp. Minimum Payment Increase



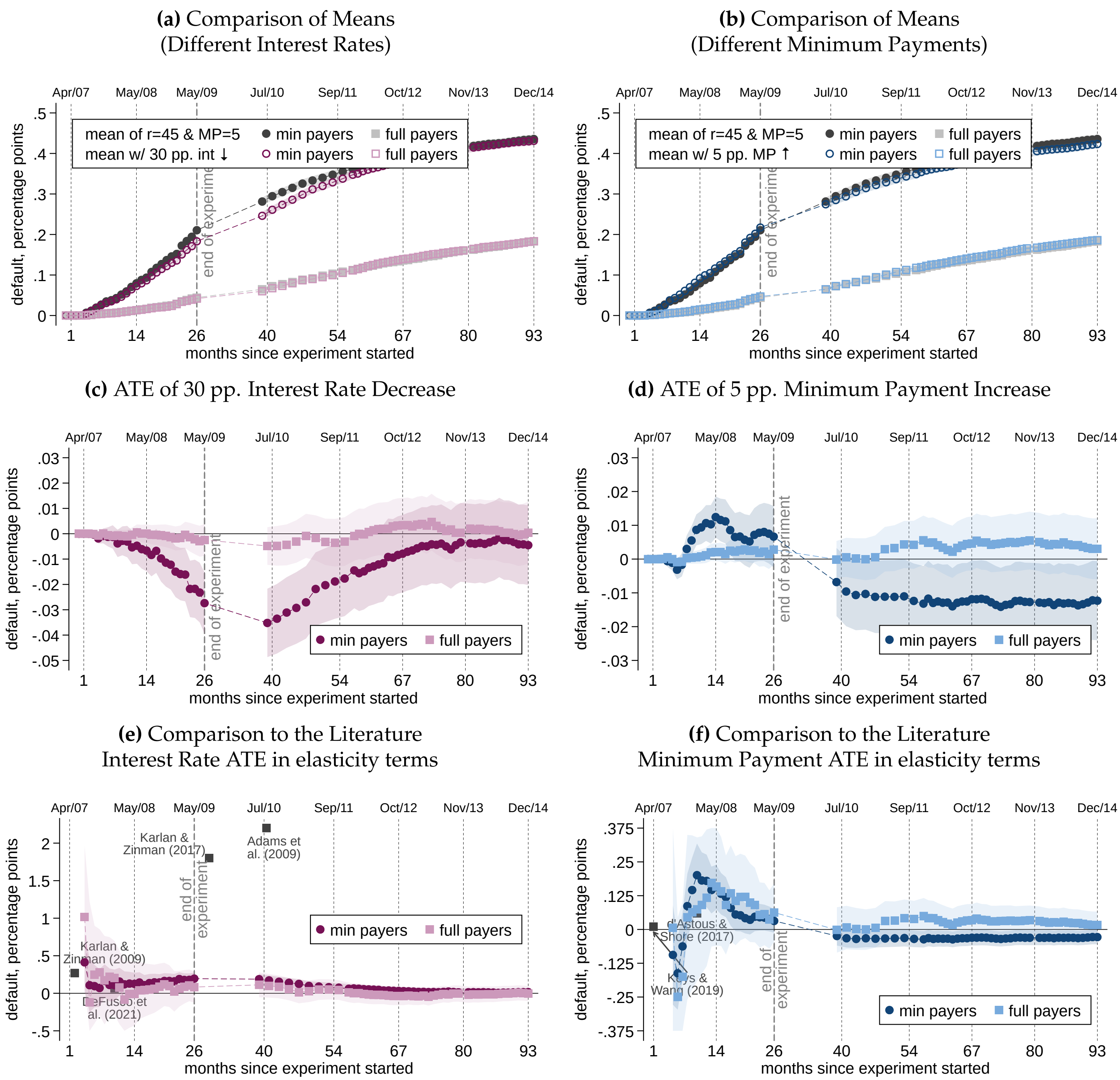
Notes: These figures plot the causal effect of interest rates and minimum payment changes on default in other loans in any bank (except for Bank A) and on new loan issuances (in any loan except for Bank A). The dependent variable is default in any loan issued by any bank (except for Bank A) (a to d) and a cumulative categorical variable on new loans from other banks from March 2007 to the given date. The data source for the dependent variables is the credit bureau. The figures on the left examine interest rate changes. The figures on the right examine minimum payment changes. The dots in Panels(a) and (b) [(e) and (f)] plot the share of cardholders that default over time [share of cardholders that obtain a new loan] in the $(r = 45\%, MP = 5\%)$ group. The difference between the two lines in Panel (a) [Panel (e)] is plotted in Panel(c) [Panel (g)] and corresponds to the average treatment effect of a 30 pp. interest rate decrease from 45% to 15%. Similarly, Panel (d) [Panel(h)] computes the average treatment effect of a 5 pp. minimum payment increase from 5% to 10%.

Figure OA-17: Treatment Effect of Contract Terms on Default by Months with Credit Card
(Share of Cardholders that Default)



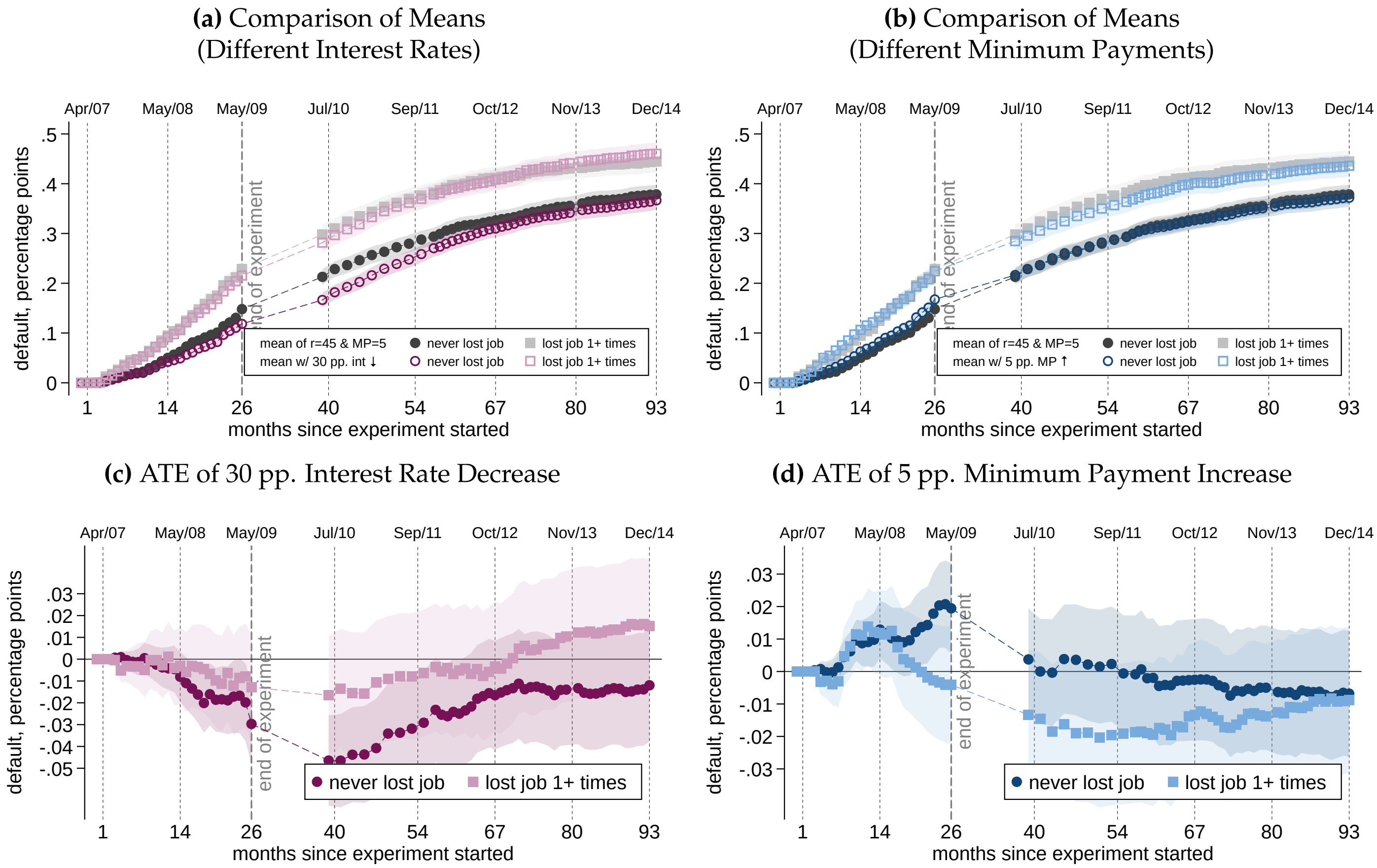
Notes: These figures plot the causal effect of interest rates and minimum payment changes on default in the experiment credit card. We separate borrowers using the months since credit card was opened strata, and restrict to the 6-11 months and the 24+ month strata. Figures on the left examine interest rate changes. Figures on the right examine minimum payment changes. The grey dots in Panels (a) and (b) plot the share of cardholders that default over time in the $(r = 45\%, MP = 5\%)$ group. The red dotted line in Panel (a) plots the share of cardholders that default over time when interest rate is decreased by 30 pp. from 45% to 15%. The difference between the two lines in Panel (a) is plotted in Panel (c) and corresponds to the average treatment effect of a 30 pp. interest rate decrease from 45% to 15%. Panel (e) computes the elasticity of default by computing the average treatment effect in percent terms(i.e., β_t/α_t in Equation 1) and dividing it by $(45 - 15)/45$. Similarly, Panels (b) plots the comparison of the share of cardholders that default when the minimum payment increases by 5 pp. relative to the $(r = 45\%, MP = 5\%)$ group; Panel (d) computes the average treatment effect of a 5 pp. minimum payment increase and Panel (e) computes the elasticity of default (i.e., β_t/α_t in Equation 1 divided by $(10 - 5)/5$) with respect to a minimum payment increase from 5 to 10%.

Figure OA-18: Treatment Effect of Contract Terms on Default by Payment Behavior (Share of Cardholders that Default)



Notes: These figures plot the causal effect of interest rates and minimum payment changes on default in the experiment credit card. We separate borrowers using the payment behavior strata, and restrict to borrowers who pay close to the minimum payment, and those classified as full payers. Figures on the left examine interest rate changes. Figures on the right examine minimum payment changes. The grey dots in Panels (a) and (b) plot the share of cardholders that default over time in the ($r = 45\%, MP = 5\%$) group. The red dotted line in Panel (a) plots the share of cardholders that default over time when interest rate is decreased by 30 pp. from 45% to 15%. The difference between the two lines in Panel (a) is plotted in Panel (c) and corresponds to the average treatment effect of a 30 pp. interest rate decrease from 45% to 15%. Panel (e) computes the elasticity of default by computing the average treatment effect in percent terms(i.e., β_t/α_t in Equation 1) and dividing it by $(45 - 15)/45$. Similarly, Panels (b) plots the comparison of the share of cardholders that default when the minimum payment increases by 5 pp. relative to the ($r = 45\%, MP = 5\%$) group; Panel (d) computes the average treatment effect of a 5 pp. minimum payment increase and Panel (e) computes the elasticity of default (i.e., β_t/α_t in Equation 1 divided by $(10 - 5)/5$) with respect to a minimum payment increase from 5 to 10%.

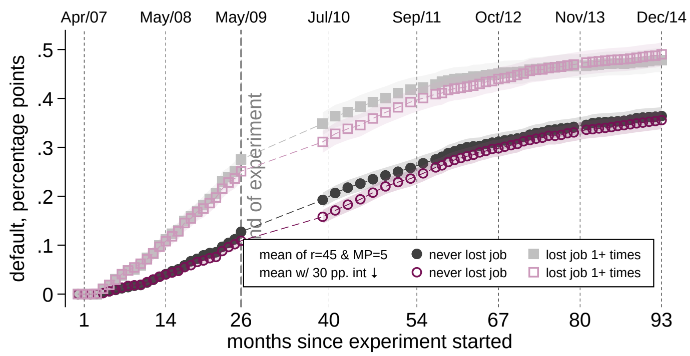
Figure OA-19: Treatment Effects of Contract Terms on Default by Pre-Experiment Formal Labor Attachment (Share of Cardholders that Default)



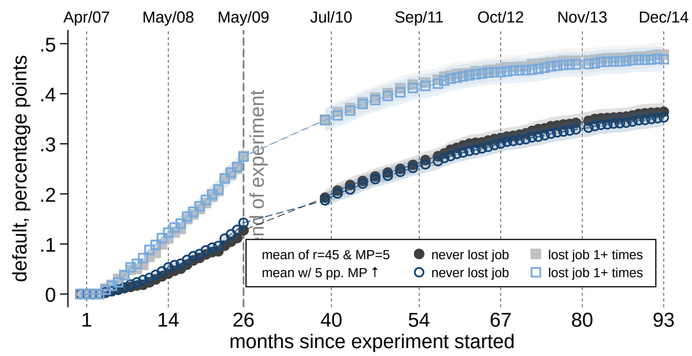
Notes: These figures plot the causal effect of interest rates and minimum payment changes on default in the experiment credit card. The figures restrict to borrowers that were formally employed for at least one month before the experiment (Jan/04-Feb/07), and separates borrowers based on whether they were continuously employed (i.e., the 'never lost job' group) or not (lost job 1+ times). Figures on the left examine interest rate changes. Figures on the right examine minimum payment changes. The grey dots in Panels (a) and (b) plot the share of cardholders that default over time in the $(r = 45\%, MP = 5\%)$ group. The red dotted line in Panel (a) plots the share of cardholders that default over time when interest rate is decreased by 30 pp. from 45% to 15%. The difference between the two lines in Panel (a) is plotted in Panel (c) and corresponds to the average treatment effect of a 30 pp. interest rate decrease from 45% to 15%. Similarly, Panels (b) plots the comparison of the share of cardholders that default when the minimum payment increases by 5 pp. relative to the $(r = 45\%, MP = 5\%)$ group; and Panel (d) computes the average treatment effect of a 5 pp. minimum payment increase.

Figure OA-20: Treatment Effects of Contract Terms on Default by Formal Sector Labor Attachment During Experiment
(Share of Cardholders that Default)

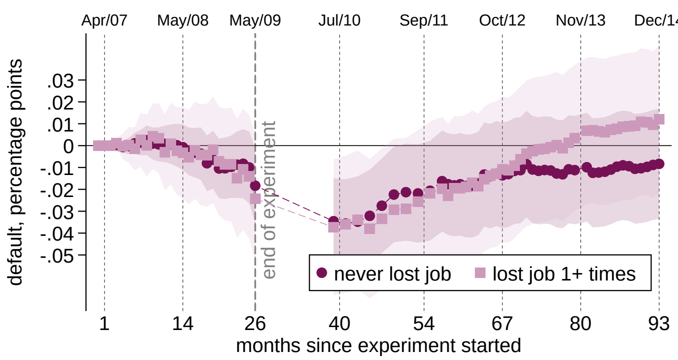
(a) Comparison of Means
(Different Interest Rates)



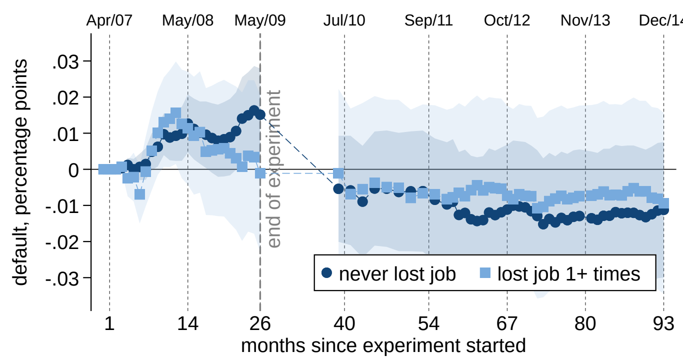
(b) Comparison of Means
(Different Minimum Payments)



(c) ATE of 30 pp. Interest Rate Decrease



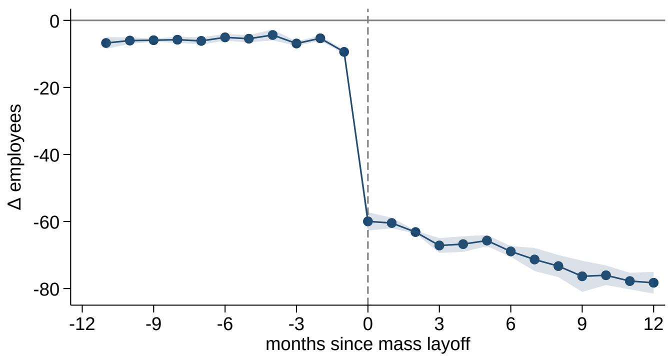
(d) ATE of 5 pp. Minimum Payment Increase



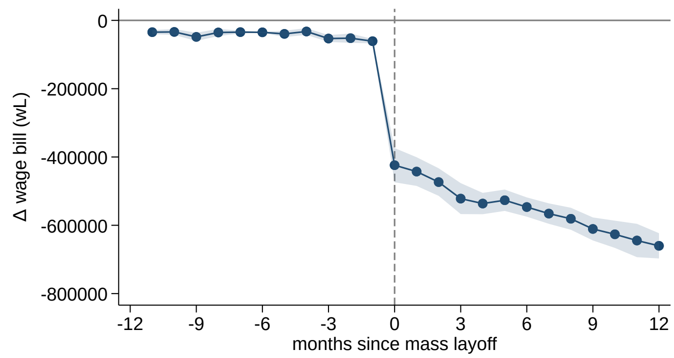
Notes: These figures plot the causal effect of interest rates and minimum payment changes on default in the experiment credit card. The figures restrict to borrowers that were formally employed for at least one month during the experiment (March/07-May/09), and separates borrowers based on whether they were continuously employed (i.e., the 'never lost job' group) or not (lost job 1+ times). Figures on the left examine interest rate changes. Figures on the right examine minimum payment changes. The grey dots in Panels (a) and (b) plot the share of cardholders that default over time in the $(r = 45\%, MP = 5\%)$ group. The red dotted line in Panel (a) plots the share of cardholders that default over time when interest rate is decreased by 30 pp. from 45% to 15%. The difference between the two lines in Panel (a) is plotted in Panel (c) and corresponds to the average treatment effect of a 30 pp. interest rate decrease from 45% to 15%. Similarly, Panels (b) plots the comparison of the share of cardholders that default when the minimum payment increases by 5 pp. relative to the $(r = 45\%, MP = 5\%)$ group; and Panel (d) computes the average treatment effect of a 5 pp. minimum payment increase.

Figure OA-21: Effect of Mass Layoffs on Employees, Wage Bill and Monthly Wages

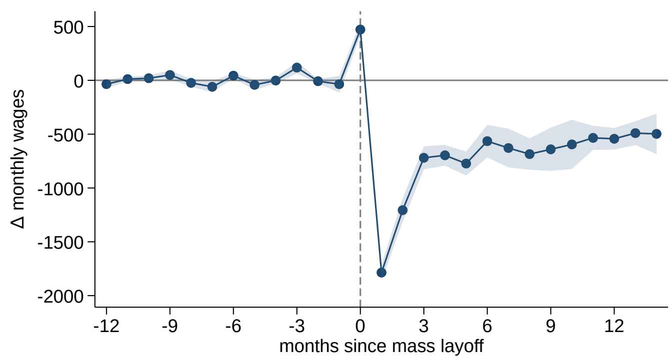
(a) Effect on Employee Headcount
(Firm Level)



(b) Effect on Wage Bill (wL)
(Firm Level)



(c) Effect on Wages
(Individual Level)



Notes: This figure plots the effect of mass downsizing events on the number of employees in a given firm, the total wage bill in pesos, and the individual level change in monthly wages in pesos. An observation in Panels (a) and (b) is a firm-month. An observation in Panel (c) is an individual-month. We use the methodology developed by de Chaisemartin and D'Haultfoeuille (2022) for these event studies.

B Data

B.1 Data Check

We argue the following relation holds in our data:

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

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-9 summarizes our results. We find that inferred interest rates match closely with experimental interest rates. This suggests that the debt transition equation (3) 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-9: 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 (3) by OLS on months with positive debt. That is we estimate the β's in the following equation: $Amount\ due_{it} = \beta_0 + \beta_1 Amount\ due_{it-1} + \beta_2 Payments_{it} + \beta_3 Purchases_{it} + \sum_k \gamma_k\ Debt_{it} \times I(r = k) + \beta_5 Fees_{it} + \epsilon_{it}$, where $k \in \{15, 25, 35, 45\}$. The coefficients are unconstrained, so a coefficient of payments equal to -1 for instance is a result and not an imposed constraint. The same is true of interest rates: the coefficient on $I(r = 25\%)$, i.e. γ_{25} =0.27 being close to 0.25 is a result as well. One, two and three stars denote statistical significance at the 5, 1 and 0.1 percent level respectively.

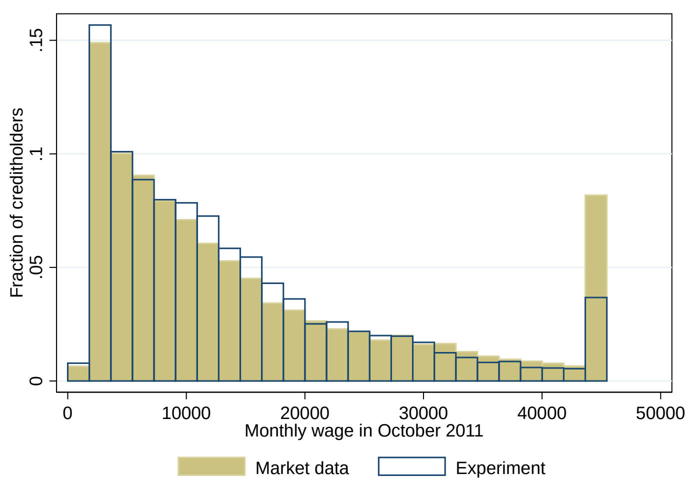
B.2 Details of "Matched" Sample for Table 1

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) 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. 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-22: Creditholders by income in October 2011



Notes: The histogram in shaded bars is the income distribution of a random sample of consumers in the credit bureau with at least one credit card. The hollow bars show 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.

C Model

The purpose of the model is to derive comparative statics for two endogenous (choice) variables: Credit card debt (denoted by C) and Default (denoted by D) as functions of the following exogenous variables: the one period interest rate (r), the minimum payment (m), the one period discount rate (δ), the continuation value of card ownership (v), first period income (y_1) and the distribution for period two income $f(y_2)$ from which y_2 is drawn.

In period 1 the agent observes their income y_1 and chooses credit card purchases C (subject to an upper limit U). In period 2, the agent realizes income y_2 and must decide whether to make the minimum payment on their card or to default. Given the model assumptions, there is no reason to pay more than the minimum amount. We analyze the model using backward induction and begin by specifying period 2 utility as well as the state and action space.

In order to do so we define a state variable that captures whether period two income is less than the amount owed in period 2: $x \equiv \mathbb{I}(y_2 > m(1+r)C)$. If $x = 0$, we assume that the agent defaults with probability one and earns a utility of $\ln(y_2)$. If $x = 1$, then the agent can either pay the minimum amount to remain in good standing and earn a utility of $\ln(y_2 - m(1+r)C) + v + \epsilon$ or default and earn a utility of $\ln(y_2)$. The ϵ term is a preference shock realized in period 2 and allows agents with identical debt, income and continuation levels to make different default decisions. We assume $\epsilon \equiv \epsilon_1 - \epsilon_0$ where (ϵ_1, ϵ_0) are i.i.d. Generalized Extreme Value Type I distributed random variables. This simplifies the calculation of both the period 2 choice probabilities as well as the expected maximized period 2 utility (the latter is required when solving the period 1 problem).

C.1 Period 2 Problem

We consider the problem separately for each value of the state variable. In what follows, we suppress the dependence of functions on the exogenous variables (m, r, v, δ) unless it is relevant. First, when $x = 0$, we assume the agent always defaults so that

$$\mathbb{P}(D = 0|C, x = 0, y_2) = 1$$

Next, when $x = 1$, that is when income $y_2 > m(1+r)C$,

$$D = 1 \iff \ln(y_2) + \epsilon_0 > \ln(y_2 - m(1+r)C) + v + \epsilon_1.$$

So that, the probability of default (integrating over the distribution of (ϵ_1, ϵ_0)) is

$$\mathbb{P}(D = 1|C, y_2, x = 1) = \frac{1}{1 + \exp\Bigg(\ln(y_2 - m(1+r)C) + v - \ln(y_2)\Bigg)}$$

Since the default probability is a key object of interest and is referred to many times below we define the function $P_D(C, y_2, x, m, r, v, \delta) \equiv \mathbb{P}(D = 1|C, y_2, x, m, r, v, \delta)$. This probability is directly decreasing in y_2,

$$\frac{\partial P_D}{\partial y_2} = -\frac{L(1-L)m(1+r)C}{y_2(y_2 - m(1+r)C)} < 0 \tag{4}$$

where L is the logistic function $L(x) = \frac{\exp(x)}{1+\exp(x)}$ and

$$L(C, y_2, m, r, v, \delta) = \frac{\exp\Bigg(\ln(y_2 - m(1+r)C) + v - \ln(y_2)\Bigg)}{1 + \exp\Bigg(\ln(y_2 - m(1+r)C) + v - \ln(y_2)\Bigg)}$$

Next, we consider the effect of changes in (r, m) on the default probability. Changes in (r, m) affect default in two ways: (a) by changing the distribution of x and (b) by changing utility (from repayment) when $x = 1$. First, write

$$D = \mathbb{I}\left(y_2 < m(1+r)C\right) + \mathbb{I}\left(y_2 > m(1+r)C\right)\left(1 - L(C, y_2, m, r, v\delta)\right) \tag{5}$$

Next, we derive the default probability after integrating over the distribution of second period income as

$$\begin{aligned} P_D(C, m, r, v, \delta) &\equiv \mathbb{P}(x=0) + \int_{m(1+r)C} (1 - L(C, y_2, m, r, v, \delta)))f(y_2|x=1)dy_2\mathbb{P}(x=1) \\ &= \mathbb{P}(x=0) + \int_{m(1+r)C} \left(1 - L(C, y_2, m, r, v, \delta)\right) f(y_2)dy_2 \end{aligned}$$

Note that in this calculation we have assumed that the conditional distribution of y_2 given (x, C) does not depend upon C (and in the sequel it is also assumed independent of first period income). A more detailed analysis (particularly one that seeks to incorporate effort explicitly) could relax these assumptions at the expense of greater complexity in the resulting comparative statics.

C.1.1 Effect of Interest Rate Changes Holding Debt Fixed

We first consider how default changes when interest rates change *after* debt has been chosen. After some simplifications, this is given by

$$\begin{aligned} \frac{\partial P_D}{\partial r} &= \int_{m(1+r)C} -\frac{\partial L(C, y_2, m, r, v)}{\partial r} f(y_2)dy_2 \\ &= Cm \int_{m(1+r)C} \frac{L(1-L)}{(y_2 - m(1+r)C)} f(y_2)dy_2 > 0 \end{aligned} \tag{6}$$

where the strict inequality follows since the integrand is everywhere strictly positive and $Cm > 0$ (we assume throughout that $C > 0$).

C.1.2 Effect of Interest Rate Changes Incorporating Debt Responses

In the above analysis we held C fixed as would be the case if the interest rate changes were announced after decisions about C had been made. If however, agents were allowed to choose C after the interest rate change, we will need to incorporate that into the analysis. We denote the optimal choice of C by $C^*(r, m)$ emphasizing its dependence on the key intervention parameters. We then compute the effect of interest rate changes after allowing for C to adjust by the following derivative (with some abuse of notation):

$$\frac{\mathrm{dP}_D}{\mathrm{dr}} = \frac{\partial P_D}{\partial C}\frac{\partial C^*}{\partial r} + \frac{\partial P_D}{\partial r} \tag{7}$$

where the last derivative was computed above in eq. (6). Next, using Liebniz's rule,

$$\frac{\partial P_D}{\partial C} = \int_{m(1+r)C} \frac{L(1-L)}{(y_2 - m(1+r)C)} m(1+r)f(y_2)dy_2 \tag{8}$$

Next, substituting the expression above and eq. (6) into eq. (7) we obtain

$$\frac{\mathrm{dP}_D}{\mathrm{dr}} = \left(C^* m \int_{m(1+r)C^*} \frac{L(1-L)}{(y_2 - m(1+r)C^*)} f(y_2) dy_2 \right) \left(\left(\frac{1+r}{r} \right) \epsilon_{Cr} + 1 \right)$$

where ϵ_{Cr} is the elasticity of debt with respect to the interest rate. The term in the first set of parentheses is strictly positive (under our assumptions), therefore the sign of the derivative depends on the sign of the last term. In particular,

$$\frac{\mathrm{dP}_D}{\mathrm{dr}} > 0 \iff \epsilon_{Cr} > \frac{-r}{1+r} \tag{9}$$

In the sequel we will consider conditions under which $\epsilon_{Cr} < 0$ so that the restriction above requires that the elasticity of debt with respect to the interest rate be sufficiently small in absolute value or in other words the elasticity has to be sufficiently small (i.e. $0 > \epsilon_{Cr} > -r/1+r$). In Appendix E.1 we show that our preferred bounds for debt responses to changes to interest rates are $[+0.18, +0.54]$. Taken literally, these imply that the inequality on the right-hand side of eq. (9) holds in our experiment.

C.1.3 Effect of Minimum Payment Changes Holding Debt Fixed

Formally, the effect of minimum payments is quite akin to that of interest rates since they both enter the agent problem in a symmetric fashion. In particular,

$$\frac{\partial P_D}{\partial m} = C(1+r) \int_{m(1+r)C} \frac{L(1-L)}{(y_2 - m(1+r)C)} f(y_2) dy_2 > 0 \tag{10}$$

So that increases in minimum payments (holding debt fixed) increase default probabilities.

C.1.4 Effect of Minimum Payment Changes Incorporating Debt Responses

We next consider the effect of minimum payments if agents are allowed to choose C after the change in minimum payments. As before, we denote the optimal choice of C by $C^*(r, m)$ emphasizing its dependence on the key intervention parameters. We then compute the effect of minimum payment changes after allowing for C to adjust by the following derivative (with some abuse of notation):

$$\frac{\mathrm{dP}_D}{\mathrm{dm}} = \frac{\partial P_D}{\partial C} \frac{\partial C^*}{\partial m} + \frac{\partial P_D}{\partial m} \tag{11}$$

Substituting eq. (8) and eq. (10) into eq. (11) above,

$$\frac{\mathrm{dP}_D}{\mathrm{dm}} = \left(C^*(1+r) \int_{m(1+r)C^*} \frac{L(1-L)}{(y_2 - m(1+r)C^*)} f(y_2) dy_2 \right) (\epsilon_{Cm} + 1)$$

where ϵ_{Cm} is the elasticity of debt with respect to the minimum payment. The term in the first set of parentheses is strictly positive (under our assumptions), therefore the sign of the derivative depends on the sign of the last term. In particular,

$$\frac{\mathrm{dP}_D}{\mathrm{dm}} < 0 \iff \epsilon_{Cm} < -1 \tag{12}$$

In the sequel we will consider conditions under which $\epsilon_{Cm} < 0$ so that the restriction above then requires that the elasticity of debt with respect to the minimum payment be sufficiently elastic in order for defaults to decrease in response to increased minimum payments. We consider a decrease in defaults here to accord with the policy position that increases in minimum payments will decrease default (see footnote 7 for details). In Appendix E.2 we show that our preferred bounds for debt responses to changes in interest rates are $[-0.31, +0.04]$. Taken literally, these estimates imply that the inequality on the right-hand of eq. (12) is reversed in our experiment so that default will be increasing in minimum payments even after accounting for debt responses.

Both the comparative statics results above depend upon the elasticity of optimal debt responses to changes in the contract terms. We next turn to the debt choice which is make in period 1.

C.1.5 Effect of Income Changes Holding Debt Fixed

Note that D defined in Equation (5) is decreasing in y_2. Therefore, for any two distributions for y_2, say F and G where F first-order stochastically dominates G we have that the probability of default under F will be lower than the probability of default under G (holding C fixed).[49] Thus, if the income distribution F in period 2 is replaced by G after debt choices have been realized, then default probabilities will be higher under G than under F.

C.1.6 Effect of Income Changes Incorporating Debt Responses

We examine debt responses to changes in second period income by positing a specific income distribution (a log-normal with parameters (μ, σ^2)) and then evaluate the responsiveness of debt to changes in σ^2.[50] Following the same logic as above

$$\frac{\mathrm{dP}_D}{\partial\sigma^2} = \frac{\partial P_D}{\partial C}\frac{\partial C^*}{\partial\sigma^2} + \frac{\partial P_D}{\partial\sigma^2} \tag{13}$$

$$= \left(\frac{C^*\partial P_D}{\sigma^2\partial C}\right)\epsilon_{C\sigma} + \frac{\partial P_D}{\partial\sigma^2} \tag{14}$$

From the analysis above we know that $\frac{\partial P_D}{\partial\sigma^2} > 0$ since $\sigma_F^2 < \sigma_G^2$ in the log-normal case implies that F first-order stochastically dominates G. In addition, the term in parentheses in the second display is also strictly positive. Therefore, the sign of the total derivative depends upon the sign of the income elasticity of debt $\epsilon_{C\sigma}$. If debt is sufficiently elastic to changes in period 2 income, then increasing σ^2 may not increase default. However, unlike in the case for (r, m) we do not have credible estimates of for $\epsilon_{C\sigma}$ to discipline the model's predictions.

[49]That is, $\int D(y_2)\,\mathrm{dF}(y_2) \leq \int D(y_2)\,\mathrm{dG}(y_2)$.

[50]Consider an initial distribution with parameters (μ, σ^2). Increases in σ^2 (keeping μ fixed) correspond to distributions that are first-order stochastically dominated by the initial distribution.

C.2 Period 1 Responses

In this section we characterize the optimal debt choice as a function of (r, m) in order to provide more insight into the elasticities above. We set the value of the card V to be 0 in period 1 (since it does not affect the choice of debt).[51] The period 1 maximization problem is

$$\max_{C \in [0,U]} \left\{ \ln(y_1 + C) + \delta \big[g(0)\mathbb{P}(x = 0) + g(1)\mathbb{P}(x = 1) \big] \right\} \tag{15}$$

where U is an exogenously set credit limit and $g(x)$ is the expected maximized utility ("Emax" in the dynamic choice literature) given by,

$$\begin{aligned} g(0) &= \mathbb{E}(\ln(y_2)|x = 0) \\ g(1) &= \int_{\epsilon, y_2} \max\left\{ \ln(y_2) + \epsilon_0, \ln(y_2 - m(1+r)C) + v + \epsilon_1 \right\} \mathrm{dF}(\epsilon)\, \mathrm{dF}(y_2|x = 1) \\ &= \mathbb{E}\left(\ln\left[y_2 + e^v (y_2 - (m(1+r)C)) \right] \middle|\, y_2 > m(1+r)C \right) \end{aligned}$$

where the last line follows from the previous one by the properties of the GEV-I distribution.

Substituting this into eq. (15), we can define the objective function

$$\begin{aligned} Q(C) &\equiv \ln(y_1 + C) + \delta\big[g(0)\mathbb{P}(x = 0) + g(1)\mathbb{P}(x = 1) \big] \\ &= \ln(y_1 + C) + \delta \left[\int^{m(1+r)C} \ln(y_2) f(y_2) dy_2 + \int_{m(1+r)C} \left(\ln\left[y_2 + e^v (y_2 - (m(1+r)C)) \right] \right) f(y_2) dy_2 \right] \end{aligned}$$

which we can differentiate to obtain

$$h(C, r, m) \equiv \frac{1}{y_1 + C} - e^v m(1+r)\delta \int_{m(1+r)C} \frac{f(y_2)}{y_2 + e^v (y_2 - m(1+r)C)} dy_2$$

Assuming an interior solution in $[0, U]$, the optimal debt level C^* satisfies the first-order condition $h(C^*, r, m) = 0$. We can characterize optimal debt responses to changes in (r, m) by applying the Implicit Function Theorem to the first-order conditions. Specifically,

$$\begin{aligned} \frac{\partial C^*(r, m)}{\partial r} &= -\frac{\partial h(C^*, r, m)/\partial r}{\partial h(C^*, r, m)/\partial C} \\ \frac{\partial C^*(r, m)}{\partial m} &= -\frac{\partial h(C^*, r, m)/\partial m}{\partial h(C^*, r, m)/\partial C} \end{aligned}$$

Next, $\frac{\partial h(C^*,r,m)}{\partial C} = \frac{\partial^2 Q(C^*,r,m)}{\partial C^2}$ and if C^* is an interior local maximum, then $\frac{\partial h(C^*,r,m)}{\partial C} < 0$.

Next,

$$\begin{aligned} &\frac{\partial h(C^*, r, m)}{\partial r} \\ &= -me^v \delta \left\{ m(1+r)e^v C^* \int_{m(1+r)C^*} \left(\frac{f(y_2)}{(y_2 + e^v\,(y_2 - m(1+r)C^*)^2} \right) dy_2 + \int_{m(1+r)C^*} \left(\frac{f(y_2)}{(y_2 + e^v\,(y_2 - m(1+r)C^*)} \right) dy_2 \right\} + e^v m\delta f(m(1+r)C^*) \end{aligned} \tag{16}$$

[51] Formally, this means we that the value of the card differs over the two periods with an unknown component in period 2 (from the point of view of period 1). This complicates the notation without adding any new insight so we do not index V in our discussion.

The first term on the right hand side is strictly negative while the second term is strictly positive. Therefore, the sign of the derivative is ambiguous. Similarly,

$$\frac{\partial h(C^*,r,m)}{\partial m}$$
$$= -(1+r)e^v\delta\left\{m(1+r)e^vC^*\int_{m(1+r)C}\left(\frac{f(y_2)}{(y_2+e^v\,(y_2-m(1+r)C^*)^2}\right)dy_2+\int_{m(1+r)C^*}\left(\frac{f(y_2)}{(y_2+e^v\,(y_2-m(1+r)C^*)}\right)dy_2\right\}+e^v(1+r)\delta f(m(1+r)C^*) \tag{17}$$

Here again, the first term on the right hand side is strictly negative while the second term is strictly positive. Therefore, the sign of the derivative is ambiguous. With these in hand,

$$\frac{\partial C^*(r,m)}{\partial r}<0 \iff \frac{\partial h(C^*,r,m)}{\partial r}<0$$
$$\frac{\partial C^*(r,m)}{\partial m}<0 \iff \frac{\partial h(C^*,r,m)}{\partial m}<0$$

If we consider settings where second period income is substantially higher than $m(1+r)C$ for all values of $C\in(0,U)$ (e.g. the support of $y_2>m(1+r)U$ for exogenous U, the last term in each of eqs. (16) and (17) will not be present (since the bounds of integration will not involve C). In this case then optimal debt will decrease in r and m. This in turn implies that the corresponding elasticities ϵ_{Cr} and ϵ_{Cm} will be negative.

D Are New Borrowers Credit Constrained?

Recent and limited participation in the formal credit sector raises the possibility that new clients continue to be credit constrained. Evidence of continuing credit constraints will provide the context for understanding the experimental treatment effects. 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.[52] Conversely, one can view debt (or more generally consumption) responses to changes in credit limits as evidence of credit constraints.[53] 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.[54] Let Debt_{it} be the amount of debt held by card i at the end of month t, let Limit_{it} denote the credit limit for account i at the beginning of month t and X_{it} denotes a set of controls. Following the main specification in Gross and Souleles (2002) we estimate

$$\Delta\text{Debt}_{i,t} = \delta_t + \sum_{j=0}^{T} \beta_j \Delta\text{Limit}_{i,t-j} + \gamma' X_{i,t} + \epsilon_{i,t} \tag{18}$$

where Δ is the first-difference operator and β_j represents the incremental increase in debt between month $t-1$ and t associated with a one peso change in credit limit in period $t-j$. The scalar parameter $\theta \equiv \sum_{j=0}^{T} \beta_j$ then provides us with a summary measure of the long-run (T month) total effect of credit limit on debt; we report $\hat{\theta} \equiv \sum_{j=0}^{T} \hat{\beta}_j$ for each regression.[55] Because the bank evaluates a card for credit-limit changes using pre-determined durations, cards that had received a credit limit change further back in the past will have a higher present probability of a credit limit change than otherwise identical cards that received a credit limit increase relatively recently. To address concerns that credit-limits change endogenously, we instrument limit changes by the time since the last limit increase, while controlling for the total number of increases in the sample period.[56]

The results are presented in Table OA-10. In all tables, we adopt the convention of three asterisks denoting significance at the .1% level, two asterisks at the 1% significance level and one asterisk at the 5% significance level. Panel A uses debit and limit data for just the study card while Panel B uses (changes in) total credit card debt (from the CB data) as the dependent variable.[57] For Panel B, since we only have annual data, we modify equation (18) 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.

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

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

[54] 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.

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

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

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

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.[58]

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 (18) 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.

[58] 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-10: 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 (18); 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 (18)) 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, eq. (18) 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 eq. (18) 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 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 parentheses. One, two and three stars denote statistical significance at the 5, 1 and 0.1 percent level respectively.

D.0.1 Variation Across Strata

A direct test of whether the strata vary systematically in terms of credit constraints is to estimate equation (18) separately for each stratum and compare the magnitudes of the estimates of θ across strata. The results are presented in Table (OA-10) 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).[59] This pattern is confirmed across the remaining seven strata: controlling for tenure with the bank, poorer repayment histories are correlated with higher estimates of θ and correspondingly, controlling for baseline repayment history, increased tenure with the bank is correlated with lower debt responses to credit limit changes.

E Effect of Interest Rate and Minimum Payment Changes on Debt

The framework outlined in Section 5 identifies the elasticities of debt (with respect to interest rates and minimum payments respectively) as key parameters governing the long-run effect of contract term interventions on default. In this section we estimate and discuss these elasticities.

One immediate concern is accounting for attrition—i.e., card exit (either via default or cancelation)—in estimation. In particular, since attrition is differential across treatment arms, estimates of debt responses using surviving borrowers without accounting for attrition will be biased. We address this concern in a number of ways. First, we implement Lee bounds (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 cancelations) 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 use Equation (1) as our estimating equation and plot the monthly means and treatment effects results graphically in Figure OA-23. We also present results in tabular form for treatment effects at two points in time (short-term results at 6 months and long-term results at 26 months) as well as for two different strata: newer borrowers (who had been with the bank for 6-11 months when the experiment began) and older borrowers (those who had been with the bank for more than two years when the experiment began) in Table OA-11.[60]

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. cancelations, revocations and the other category) as attrition. Second, we set all post-exit outcomes for card cancellers to zero and only consider the default as attrition. The latter strategy is arguably justified if

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

[60] Since we do not observe debt after the experiment ends, we cannot plot treatment effects on debt after May 2009.

we are willing to conflate treatment effects on the extensive and intensive margins. Moreover, 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.

E.1 Effect of Interest Rate Reductions on Debt

Figure OA-23 shows that interest rate declines lead to a reduction in debt. At the six-month mark, with relatively limited attrition, the implied elasticity bounds are relatively tight at $[0.22, 0.26]$ suggesting a modest reduction in debt. At the end of the experiment, with substantial attrition, the bounds widen to $[0.19, 0.92]$. However, if we impute a zero debt to all cancelers, the bounds narrow to $[0.18, 0.54]$. In all cases, these bounds suggest a relatively inelastic debt response to changes in interest rates.[61]

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. We find the following: First, interest rate declines have inconclusive effects on purchases with the Lee bounds for the long-term effect being a relatively wide $[-0.62, +0.55]$.[62] Second, interest rate declines have similarly inconclusive effects on payments with the long-term bounds estimated to be $[-0.11, +0.52]$.[63]. 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.[64] 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 relatively moderate, decline in total debt outstanding as a result of the interest rate decrease.

E.2 Effect of Minimum Payment Increases on Debt

Debt response to the minimum payment increase follows an interesting pattern. Figures OA-23(b) and OA-23(d) show that debt increases markedly in the third and fourth month of the experiment in response to the increase in minimum payments, increasing by almost 750 pesos by June 2007. However, there is a similarly precipitous decline soon after with the increase being wiped out by September so that the six-month effects are very small—the bounds for the implied elasticities are quite small at $[0.02, 0.05]$.The short term effect appears to arise from late payment fees.[65] 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

[61]Other papers examining debt responses to interest rate variation are Karlan and Zinman (2019), 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 (2019) is -2.9; Dehejia et al. (2012) provide estimates in the range of $[-0.73. - 1.04]$.

[62]The short-term effects have tighter bounds of $[-0.37, -0.35]$ that suggest modest increases in purchases.

[63]Bounds for the short-term are much tighter at $[+0.06, +0.12]$

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

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

declines (of 687 pesos or an elasticity of -0.31) or increases (461 pesos or an elasticity of +0.21).

In the case of debt, imputing a value of zero for all cancellers is a particularly reasonable approach if policymakers 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 somewhat tighter at $[-0.31, +0.04]$ but still include zero. Thus, our results are consistent with the conclusion that doubling the minimum payment had at, under the most generous interpretation (i.e. using the left hand side Lee bound), only a moderate effect on reducing debt.

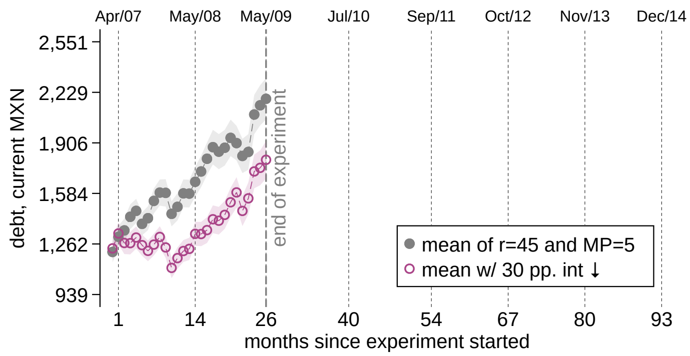
Table OA-11: Treatment Effects on Monthly Debt

	Standard Outcome		Inputting cancelers = 0		6-11M w/ Card Strata		24+M w/ Card Strata	
Months since experiment started	6	26	6	26	6	26	6	26
	Sep/07	May/09	Sep/07	May/09	Sep/07	May/09	Sept/07	May/09
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$(45\% - r_i)/30\%$	-208***	-389***	-202***	-246***	-251*	-548***	-206***	-360***
	(50)	(83)	(48)	(69)	(111)	(142)	(62)	(103)
$\mathbb{1}\{MP_i = 10\%\}$	33	-547***	25	-509***	52	-813***	47	-508***
	(37)	(62)	(36)	(52)	(82)	(106)	(46)	(78)
Constant	1,426***	2,187***	1,384***	1,807***	2,776***	3,442***	1,142***	1,989***
	(39)	(67)	(38)	(56)	(82)	(113)	(49)	(84)
Observations	134,385	87,093	139,043	105,237	44,878	27,610	44,887	31,027
R-squared	0.000	0.004	0.000	0.004	0.000	0.006	0.001	0.004
Lee bounds r	[-245, -213]	[-1342, -271]	[-235, -208]	[-650, -219]	[-273, -263]	[-1686, -376]	[-241, -213]	[-1353, -256]
Lee bounds MP	[32, 72]	[-686, 461]	[-13, 28]	[-560, 67]	[50, 74]	[-1050, 440]	[47, 61]	[-628, 480]
Lee bounds ε r	[0.22, 0.26]	[0.19, 0.92]	[0.23, 0.25]	[0.18, 0.54]	[0.14, 0.15]	[0.16, 0.73]	[0.28, 0.32]	[0.19, 1.02]
Lee bounds ε MP	[0.02, 0.05]	[-0.31, 0.21]	[-0.01, 0.02]	[-0.31, 0.04]	[0.02, 0.03]	[-0.31, 0.13]	[0.04, 0.05]	[-0.32, 0.24]

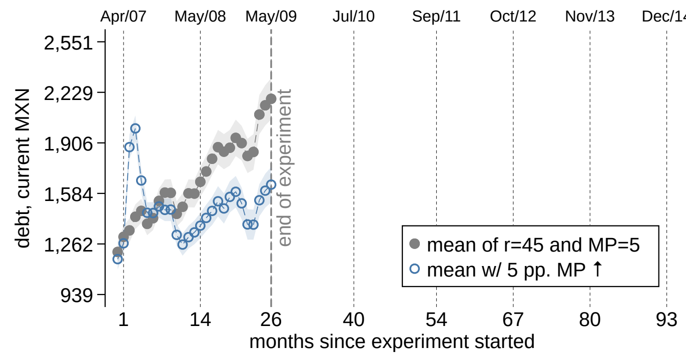
Notes: All regressions use sample weights. Each column is a different regression. The dependent variable is monthly purchases. Columns (1), (3), (5), and (7) are estimated using outcomes 6 months after the start of the intervention and the remainder are for outcomes at the end of the experiment. Columns (3) and (4) inpute a zero value for those who cancel their card, and the Lee (2009) bounds are more informative than the point-estimates for these columns. Columns (5) and (6) focus on the newest strata (pooling across payment behavior). Columns (7) and (8) focus on the oldest strata. The Lee bounds for interest rates compare the $r = 15$ treatment groups against the $r = 45$ treatment groups (pooling across MP). The bounds for minimum payments compare those in the $MP = 10$ treatment arms to those in the $MP = 5$ treatment arms (pooling across r). Bounds are tightened by strata and treatment arms whenever possible. Standard errors are shown in parentheses.

Figure OA-23: Treatment Effect of Contract Terms on Debt
(Debt in Current MXN Among Active Cards)

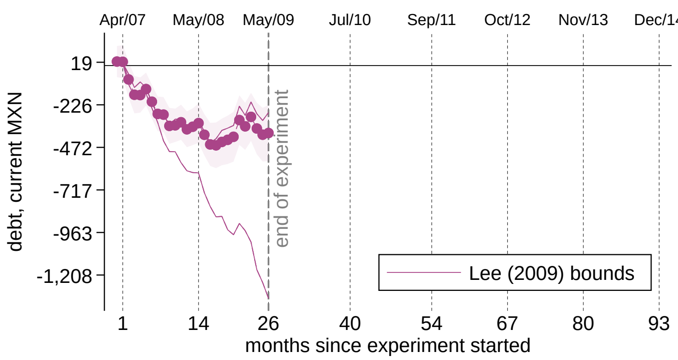
(a) Comparison of Means
(Different Interest Rates)



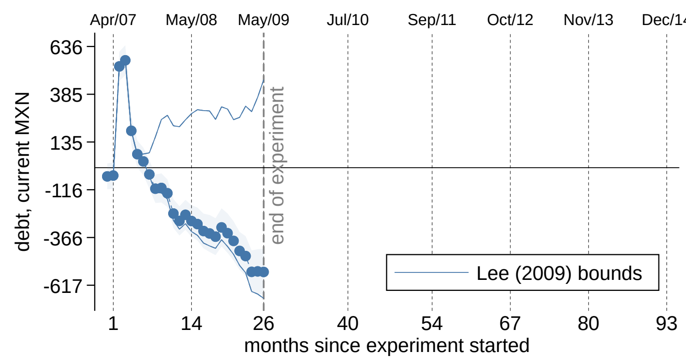
(b) Comparison of Means
(Different Minimum Payments)



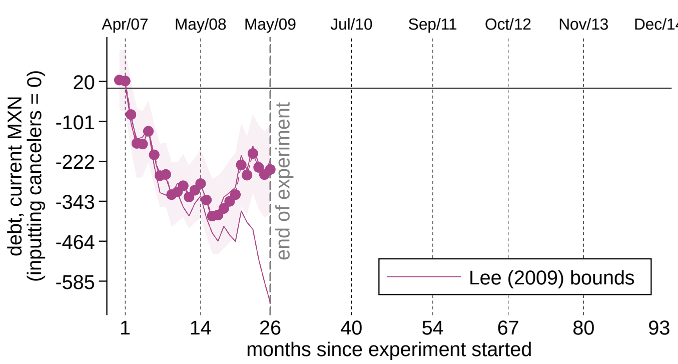
(c) ATE of 30 pp. Interest Rate Decrease



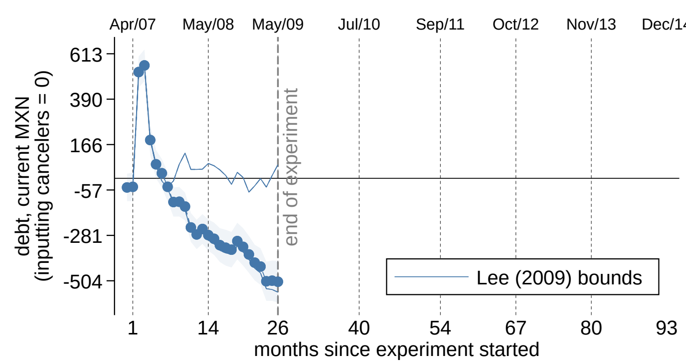
(d) ATE of 5 pp. Minimum Payment Increase



(e) ATE of 30 pp. Interest Rate Decrease
(Inputing Zero to Cancellers)



(f) ATE of 5 pp. Minimum Payment Increase
(Inputing Zero to Cancellers)



Notes: These figures plot the causal effect of interest rates and minimum payment changes on debt in the experiment credit card. We only observe debt in the experimental period. Debt is defined as average balances in the month. Interest is charged on average balances in the month. Figures on the left examine interest rate changes. Figures on the right examine minimum payment changes. The grey dots in Panels (a) and (b) plot the average amount owed over time in the $(r = 45\%, MP = 5\%)$ group. The red dotted line in Panel (a) plots the average debt over time when interest rate is decreased by 30 pp. from 45% to 15%. The difference between the two lines in Panel (a) is plotted in Panel (c) and corresponds to the average treatment effect of a 30 pp. interest rate decrease from 45% to 15%. Similarly, Panels (b) plots the comparison of the average debt when the minimum payment increases by 5 pp. relative to the $(r = 45\%, MP = 5\%)$ group; Panel (d) computes the average treatment effect of a 5 pp. minimum payment increase. Lee (2009) bounds, tightened by strata and treatment arms whenever possible. We were not able to obtain data for debt for the periods post-experiment.

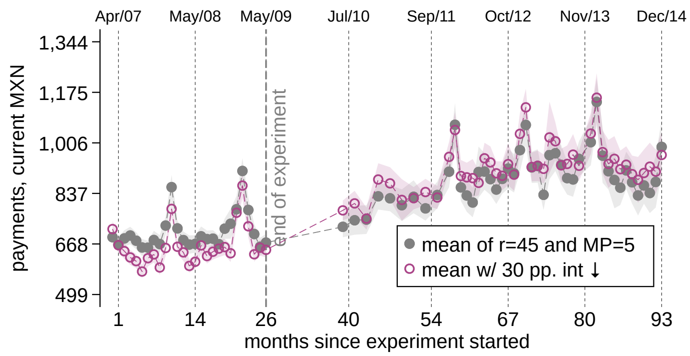
Table OA-12: Treatment Effects on Monthly Payments

	Standard Outcome		Inputting cancelers = 0		6-11M w/ Card Strata		24+M w/ Card Strata	
Months since experiment started	6 Sep/07 (1)	26 May/09 (2)	6 Sep/07 (3)	26 May/09 (4)	6 Sep/07 (5)	26 May/09 (6)	6 Sept/07 (7)	26 May/09 (8)
$(45\% - r_i)/30\%$	-35*	-24	-29	16	-44	-32	-31	-16
	(18)	(20)	(17)	(17)	(27)	(31)	(23)	(25)
$\mathbb{1}\{MP_i = 10\%\}$	153***	133***	147***	90***	206***	149***	145***	128***
	(13)	(15)	(12)	(13)	(20)	(22)	(17)	(19)
Constant	656***	673***	615***	545***	721***	638***	657***	691***
	(12)	(15)	(11)	(12)	(19)	(21)	(15)	(19)
Observations	134,385	87,093	139,043	105,237	44,878	27,610	44,887	31,027
R-squared	0.003	0.002	0.003	0.001	0.005	0.003	0.003	0.002
Lee bounds r	[-53, -25]	[-234, 51]	[-30, -20]	[-82, 44]	[-39, -33]	[-238, 26]	[-49, -21]	[-234, 67]
Lee bounds MP	[152, 177]	[87, 313]	[139, 149]	[73, 179]	[206, 221]	[102, 351]	[145, 153]	[82, 310]
Lee bounds ε r	[0.06, 0.12]	[-0.11, 0.52]	[0.05, 0.07]	[-0.12, 0.23]	[0.07, 0.08]	[-0.06, 0.56]	[0.05, 0.11]	[-0.15, 0.51]
Lee bounds ε MP	[0.23, 0.27]	[0.13, 0.47]	[0.23, 0.24]	[0.13, 0.33]	[0.29, 0.31]	[0.16, 0.55]	[0.22, 0.23]	[0.12, 0.45]

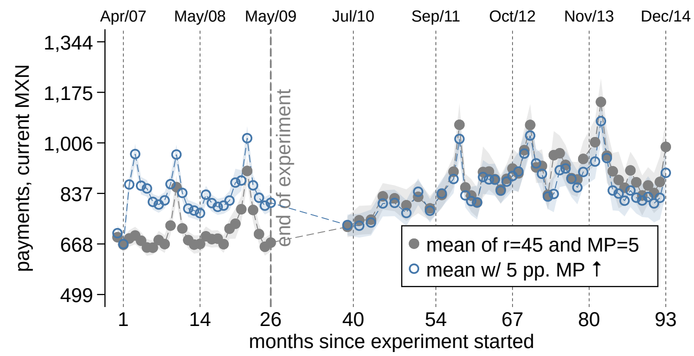
Notes: All regressions use sample weights. Each column is a different regression. The dependent variable is monthly payments. Columns (1), (3), (5), and (7) are estimated using outcomes 6 months after the start of the intervention and the remainder are for outcomes at the end of the experiment. Columns (3) and (4) inpute a zero value for those who cancel their card, and the Lee (2009) bounds are more informative than the point-estimates for these columns. Columns (5) and (6) focus on the newest strata (pooling across payment behavior). Columns (7) and (8) focus on the oldest strata. The Lee bounds for interest rates compare the $r = 15$ treatment groups against the $r = 45$ treatment groups (pooling across MP). The bounds for minimum payments compare those in the $MP = 10$ treatment arms to those in the $MP = 5$ treatment arms (pooling across r). Bounds are tightened by strata and treatment arms whenever possible. Standard errors are shown in parentheses.

Figure OA-24: Treatment Effect of Contract Terms on Payments
(Payments in Current MXN Among Active Cards)

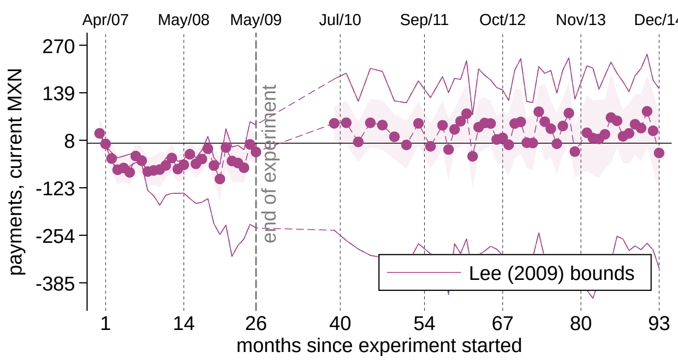
(a) Comparison of Means
(Different Interest Rates)



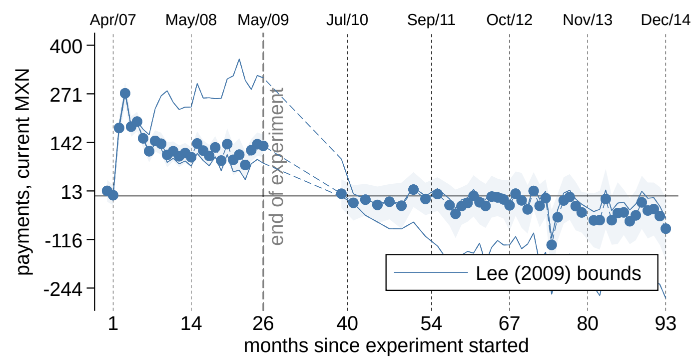
(b) Comparison of Means
(Different Minimum Payments)



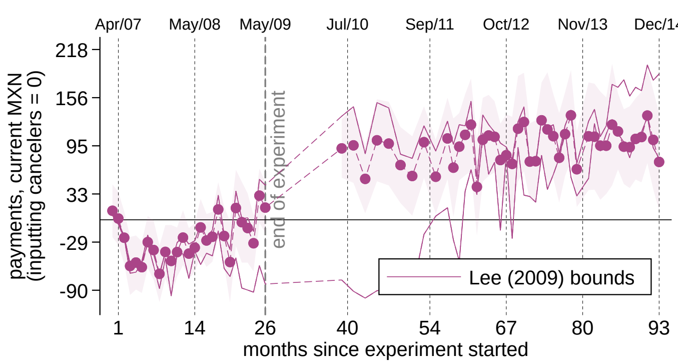
(c) ATE of 30 pp. Interest Rate Decrease



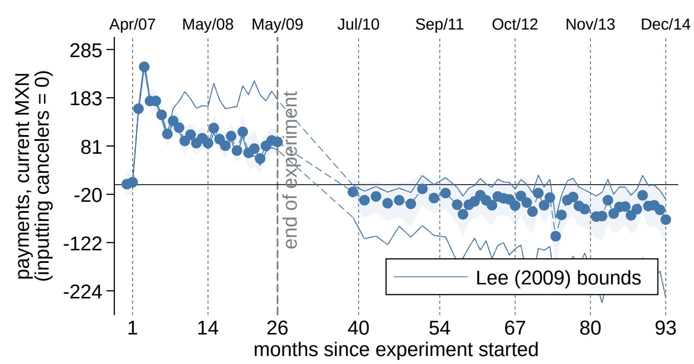
(d) ATE of 5 pp. Minimum Payment Increase



(e) ATE of 30 pp. Interest Rate Decrease
(Inputing Zero to Cancellers)



(f) ATE of 5 pp. Minimum Payment Increase
(Inputing Zero to Cancellers)



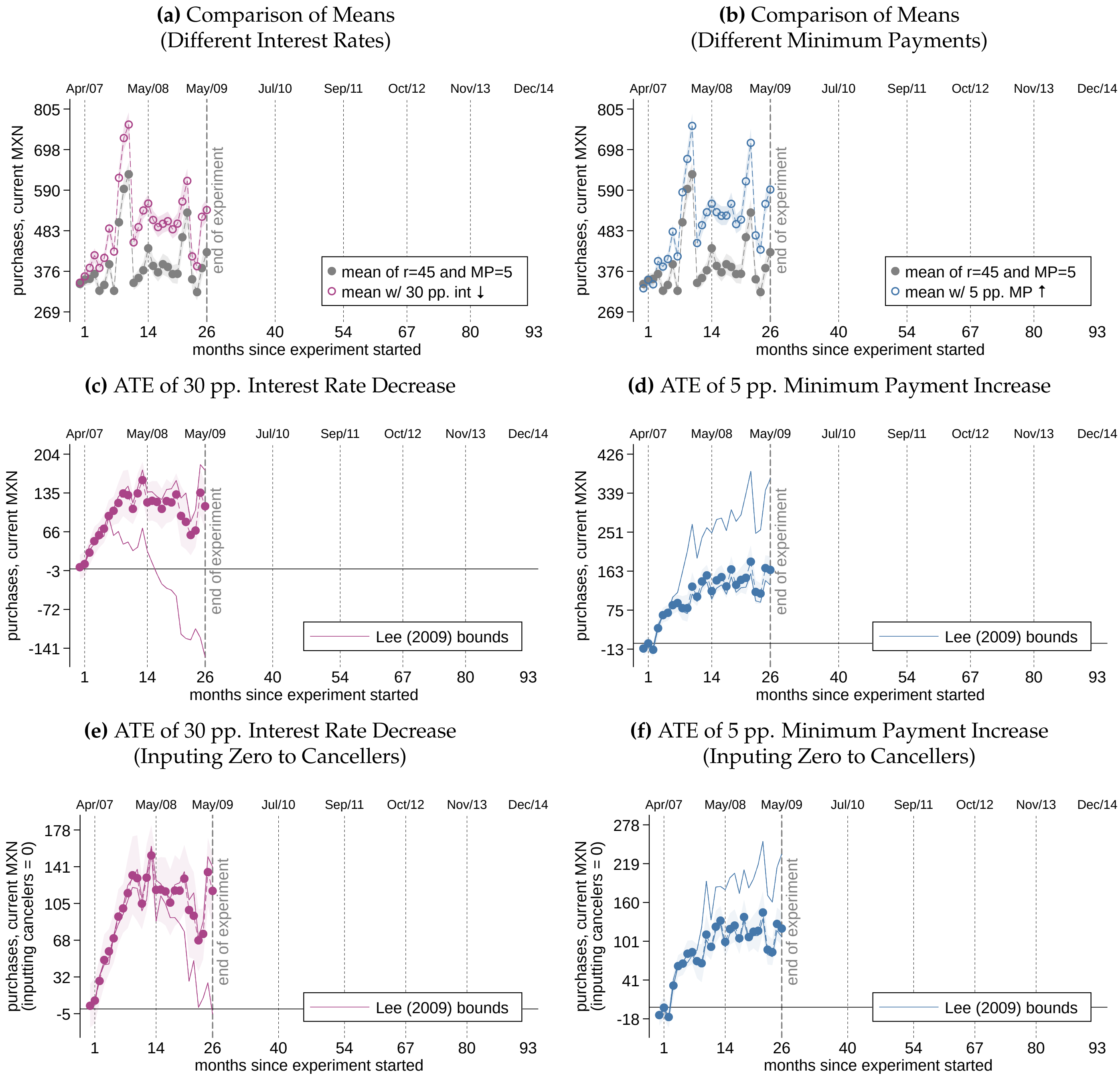
Notes: These figures plot the causal effect of interest rates and minimum payment changes on payments in the experiment credit card. Figures on the left examine interest rate changes. Figures on the right examine minimum payment changes. The grey dots in Panels (a) and (b) plot the average amount paid over time in the $(r = 45\%, MP = 5\%)$ group. The red dotted line in Panel (a) plots the average payment over time when interest rate is decreased by 30 pp. from 45% to 15%. The difference between the two lines in Panel (a) is plotted in Panel (c) and corresponds to the average treatment effect of a 30 pp. interest rate decrease from 45% to 15%. Similarly, Panels (b) plots the comparison of the average payments when the minimum payment increases by 5 pp. relative to the $(r = 45\%, MP = 5\%)$ group; Panel (d) computes the average treatment effect of a 5 pp. minimum payment increase. Lee (2009) bounds, tightened by strata and treatment arms whenever possible. We were able to obtain data for payments for the periods post-experiment.

Table OA-13: Treatment Effects on Monthly Purchases

	Standard Outcome		Inputting cancelers = 0		6-11M w/ Card Strata		24+M w/ Card Strata	
Months since experiment started	6 Sep/07 (1)	26 May/09 (2)	6 Sep/07 (3)	26 May/09 (4)	6 Sep/07 (5)	26 May/09 (6)	6 Sept/07 (7)	26 May/09 (8)
$(45\% - r_i)/30\%$	94***	112***	92***	117***	75***	76*	103***	119***
	(14)	(21)	(14)	(18)	(20)	(31)	(18)	(27)
$\mathbb{1}\{MP_i = 10\%\}$	86***	165***	82***	120***	122***	163***	78***	165***
	(10)	(16)	(10)	(13)	(14)	(22)	(14)	(20)
Constant	395***	427***	383***	350***	428***	414***	403***	442***
	(10)	(14)	(10)	(12)	(14)	(24)	(13)	(18)
Observations	134,385	87,093	139,043	105,237	44,878	27,610	44,887	31,027
R-squared	0.002	0.004	0.002	0.003	0.003	0.003	0.002	0.004
Lee bounds r	[92, 98]	[-157, 175]	[84, 94]	[-5, 141]	[75, 79]	[-168, 123]	[104, 106]	[-164, 186]
Lee bounds MP	[85, 104]	[131, 371]	[68, 83]	[107, 234]	[121, 132]	[129, 393]	[78, 85]	[130, 375]
Lee bounds ε r	[-0.37, -0.35]	[-0.62, 0.55]	[-0.37, -0.33]	[-0.60, 0.02]	[-0.28, -0.26]	[-0.45, 0.61]	[-0.39, -0.39]	[-0.63, 0.56]
Lee bounds ε MP	[0.22, 0.26]	[0.31, 0.87]	[0.18, 0.22]	[0.31, 0.67]	[0.28, 0.31]	[0.31, 0.95]	[0.19, 0.21]	[0.29, 0.85]

Notes: All regressions use sample weights. Each column is a different regression. The dependent variable is monthly purchases. Columns (1), (3), (5), and (7) are estimated using outcomes 6 months after the start of the intervention and the remainder are for outcomes at the end of the experiment. Columns (3) and (4) inpute a zero value for those who cancel their card, and the Lee (2009) bounds are more informative than the point-estimates for these columns. Columns (5) and (6) focus on the newest strata (pooling across payment behavior). Columns (7) and (8) focus on the oldest strata. The Lee bounds for interest rates compare the $r = 15$ treatment groups against the $r = 45$ treatment groups (pooling across MP). The bounds for minimum payments compare those in the $MP = 10$ treatment arms to those in the $MP = 5$ treatment arms (pooling across r). Bounds are tightened by strata and treatment arms whenever possible. Standard errors are shown in parentheses.

Figure OA-25: Treatment Effect of Contract Terms on Purchases
(Purchases in Current MXN Among Active Cards)



(a) Comparison of Means (Different Interest Rates)

(b) Comparison of Means (Different Minimum Payments)

(c) ATE of 30 pp. Interest Rate Decrease

(d) ATE of 5 pp. Minimum Payment Increase

(e) ATE of 30 pp. Interest Rate Decrease (Inputing Zero to Cancellers)

(f) ATE of 5 pp. Minimum Payment Increase (Inputing Zero to Cancellers)

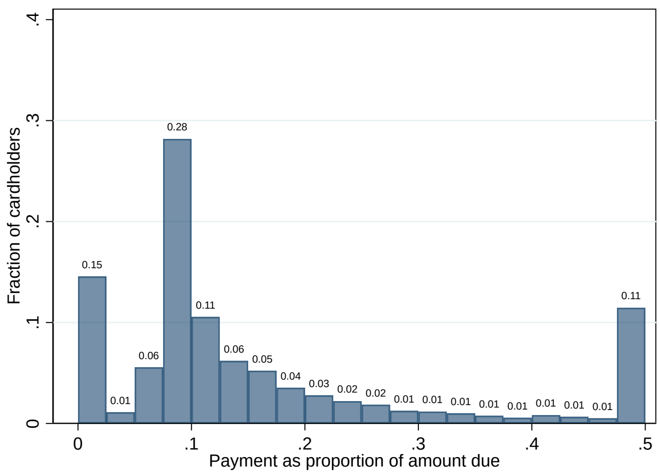
Notes: These figures plot the causal effect of interest rates and minimum payment changes on purchases in the experiment credit card. We only observe purchases in the experimental period. Figures on the left examine interest rate changes. Figures on the right examine minimum payment changes. The grey dots in Panels (a) and (b) plot the average amount purchased over time in the $(r = 45\%, MP = 5\%)$ group. The red dotted line in Panel (a) plots the average purchases over time when interest rate is decreased by 30 pp. from 45% to 15%. The difference between the two lines in Panel (a) is plotted in Panel (c) and corresponds to the average treatment effect of a 30 pp. interest rate decrease from 45% to 15%. Similarly, Panel (b) plots the comparison of the average purchases when the minimum payment increases by 5 pp. relative to the $(r = 45\%, MP = 5\%)$ group; Panel (d) computes the average treatment effect of a 5 pp. minimum payment increase. Lee (2009) bounds, tightened by strata and treatment arms whenever possible. We were not able to obtain data for purchases for the periods post-experiment.

F Habit formation

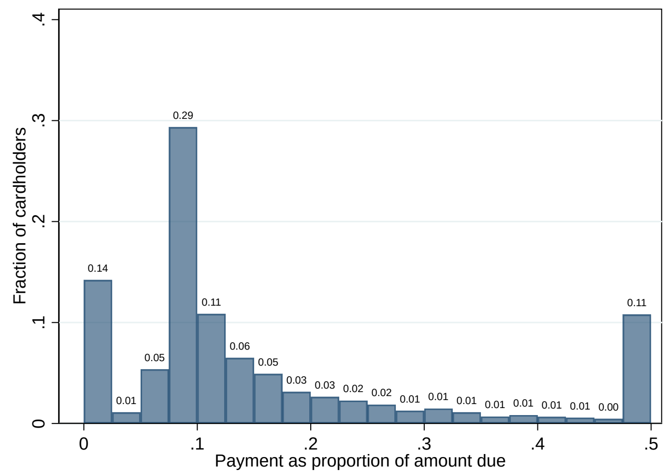
F.1 Comparison of min. payment across treatment arms 3 years after the experiment ended

Figure OA-26: 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.

F.2 Habit formation regressions

Table OA-14: Habit formation on payments

	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 parentheses. 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 (“Payments”) on June 2010, as a function of the minimum payment that was assigned during the experiment and debt (“Amount due”). We are most interested in the coefficient of MP = 10 in the even columns which measures the effect of having been subjected to higher MP in the past on payment amount in the future when the MP is no longer high, conditional on current debt. Since debt can be endogenous, we instrument for debt using the interest rate group cardholders were assigned to. Not instrumenting for debt leads to similar conclusions regarding the effect of MP10. 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.

G Severe Consequences of Default

Table OA-15: 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 card holder level. 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.

Table OA-16: Access to loans after the first delinquency

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

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

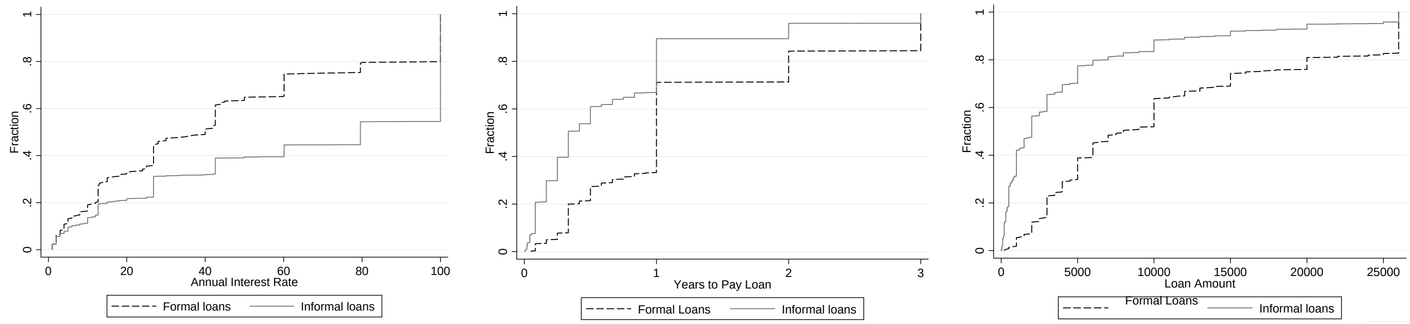
Table OA-17: Formal vs Informal Loan Terms

	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-27: 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.