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CONTRACT TERMS, EMPLOYMENT SHOCKS, AND DEFAULT IN CREDIT CARDS

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ABSTRACT

Regulatory concerns over a tension between expanding financial access and limiting default have led to significant restrictions on contract terms in a number of countries, despite limited evidence on their effectiveness. We use a large nation-wide RCT to examine new borrower responses to changes in interest rates and minimum payments for a credit card that accounted for 15% of all first-time formal loans in Mexico. Default rates were 19% over the 26 month experiment and a 30 pp decrease in interest rates decreased default by 2.5 pp with no effects on the newest borrowers. Doubling minimum payments increased default by 0.8 pp during the experiment but reduced it by 1 pp afterwards, possibly by reducing debt. Matching the experimental sample to their formal employment histories we find that the effect of job separation—more common among new borrowers—on default is seven times larger than the effect of the 30 pp interest rate change. We provide a simple framework for interpreting the experimental results, and rationalize the smaller contract term effects by their limited effects on cash flow rather than by differences in per-peso impacts.

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1 Introduction

Policymakers in developing countries pursue two goals often perceived to be in tension with each other. Limiting credit market default is viewed as key to financial stability, while expanding formal credit to under-served populations is seen as critical for growth and welfare. The tension arises because new borrowers typically default at higher rates than established ones. This is particularly evident in policy discussions about credit-card borrowing—the most common way for new borrowers to access formal credit in many countries.[1] Concerns over new borrower card default have led several countries (e.g., Canada, Chile, Mexico, Taiwan, Turkey, and Indonesia) to mandate minimum payment floors or interest rate ceilings.[2]

Despite its role in expanding credit access and facing increased regulation, credit card borrowing in developing countries remains relatively understudied. Perhaps as a result, policy discussions lack a coherent theoretical underpinning and credible empirical evidence on the drivers of new borrower default and the effectiveness of policy alternatives. For instance, interest rate regulations to limit card default typically presume strategic considerations. However, adverse life events such as job loss are common in such settings and are potentially important default triggers, but remain under-explored. In part this reflects the difficulty of linking information on default, contract terms, and adverse life-events, and isolating exogenous variation for all key variables on a common sample.

We address these limitations by comparing the effects of contract terms and job loss on default within a large common sample of new card borrowers. First, by using a large nationwide experiment we find that substantial changes in contract terms—decreasing credit card interest rates by 30 pp (67% of baseline) and doubling minimum payments—had limited effects on default, far below expert predictions. Second, we document frequent job loss among new borrowers and that plausibly exogenous job separation events substantially increase default. Finally, a back-of-the-envelope calculation that normalizes the three "shocks" (contract term changes and job loss) finds that all three had similar *per-peso* effects on default, so that the smaller effects of contract terms can be rationalized by their smaller effects on total cash flow.

We examine a popular Mexican credit card (hereafter the "study card") that was at the center of national financial inclusion efforts and regulatory concerns. Issued by a large commercial bank ("Bank A") targeted specifically at borrowers with limited or no formal credit histories, the study card accounted for approximately 15% of all first-time formal sector loan products in the country by 2010. Default rates were higher for the newest borrowers—borrowers who had been with the bank for 6–11 months (at the start of the experiment) defaulted at twice the rate of those who had been with the bank for at least

[1] In Mexico, cards are the first loan type for 74% of all formal sector borrowers. The corresponding figures for Peru, Colombia, and the U.S. are 83%, 51%, and 50%, respectively. See Appendix B.1 for details on data sources.

[2] We emphasize perceptions as they matter for policy. In particular, even though high default rates may be optimal from a welfare perspective (see e.g., Garz et al., 2021), the regulatory measures we study proceed on the assumption that they are not. See, e.g., Financial Conduct Authority (2015) for minimum payment regulations in Mexico and Taiwan. See Breen (2019) for minimum payment floors in Quebec. Singapore mandates minimum income requirements and automatic credit suspension for any borrower not making their minimum payment for 60 days. In the United States, Office of the Controller of the Currency (2003) provided guidance to lenders to ensure minimum payments were set high enough to avoid negative amortization, with Citigroup, J.P. Morgan Chase, Bank of America, and others following the guidance (Kim, 2005). See also Cuesta and Sepulveda (2023); Nelson (2025); Williams (2005).

two years (36% versus 18%). Such default rates among financially inexperienced populations attracted regulator attention because of their implications both for systemic risk and defaulters' subsequent access to formal credit (see, for instance, the discussions in Banco de Mexico, 2008, 2009, 2010).[3] This attention led, most prominently, to national legislation restricting credit card contract terms in 2010.

The experiment allocated a large nationwide stratified random sample of 144,000 *pre-existing* study card borrowers (hereafter "the study sample") 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. The experimental variation is substantially larger than typical policy interventions, providing a strong test of contract terms' potential for limiting default.[4] The large sample size enables precise estimation of treatment effects across a range of contract terms and population strata (in our empirical results we use three asterisks to denote significance at the 0.001 level). The sampling scheme ensures the experimental results are representative of the bank's population of study card customers (about 1.3 million). We follow participants for five years after the intervention ended and examine their behavior across all formal financial institutions. In addition, we match the experimental sample to its monthly employment histories in the Mexican Social Security database (the Instituto Mexicano del Seguro Social or IMSS).

We document four main results. First, reducing the interest rate by 30 percentage points (pp) from 45% to 15% decreases default by 2.5 pp over the 26 month experiment (compared to a base default rate of 19%). The implied elasticity of +0.20 is considerably smaller than previous comparable estimates (e.g., Adams et al., 2009; Karlan and Zinman, 2019) and far below expert predictions—Mexican central bank regulators predicted a mean decrease of 8.6 pp and experts on the Social Science Prediction Platform predicted a mean decrease of 5 pp. The large stratified experiment allows us to precisely estimate effects for the newest borrowers, a population of considerable academic and policy interest, and our second result is that the interest rate changes have *no* effect on default for the newest borrowers over the 26 month experiment.

Higher minimum payments are another potential policy tool for limiting default.[5] Higher minimum payments, however, have two opposing effects, and it is not clear *a priori* which one will dominate. On the one hand, higher payments tighten short-run liquidity constraints by requiring higher payments immediately, which may increase current default. Liquidity constraints may be particularly relevant as, at the start of the experiment, 73% of cardholders' monthly payments were below 10% of the amount due. On the other hand, higher minimum payments, *ceteris paribus*, reduce debt and may decrease debt-burden-driven default in the longer run.

Our third result is that doubling the minimum payment requirements does not reduce default during

[3]Similar concerns have been raised elsewhere. See, e.g., Black and Morgan (1999); Livshits (2022) for the U.S.

[4]Policy changes have been much smaller—the mandated increases in minimum payments in Mexico was 1.5%, while interest rate caps considered in Turkey and Indonesia involved changes of no more than 5–10 pp. See, e.g., Web Archive Link for Turkey and Web Archive Link for Indonesia.

[5]See, e.g., Bar-Gill (2003); Financial Conduct Authority (2015); 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).

the experiment—the point estimate is a 0.8 pp increase in default, with an elasticity of +0.04. Default declines in the longer run (in the higher minimum payment arm) after the experiment ends and minimum payments are returned to the same pre-study levels in all arms. The ATE is a persistent decline of 1 pp over this period (relative to a base default rate of 41%).

We interpret these results using a simple optimizing framework that emphasizes debt, particularly previously accumulated debt, and liquidity constraints. The framework can parsimoniously explain the observed positive relationship between interest rates and default via debt as well as the short-term positive and longer-term negative relationship between higher minimum payments and default. We provide some evidence for these hypothesized mechanisms using information on debt, purchases, and payments.

Given the limited impact of even substantial contract term changes, what drives default in this population? One leading candidate is adverse life events such as illness or job loss. We explore the latter by matching study sample subjects to their monthly formal employment histories in the IMSS. While it is unsurprising that job loss increases card default, its precise magnitude is much less obvious for at least two reasons. First, cards are particularly valuable for smoothing consumption during unemployment spells, providing an incentive to avoid default. Second, card default may be lower than otherwise if informal employment (common in Mexico) substitutes for formal sector job loss (see, e.g., Donovan et al., 2023).[6]

Job loss is common in the study sample—43% of formally employed borrowers experienced job loss. Newer borrowers are more vulnerable: those who had the study card for less than a year before the experiment are 1.34 times more likely to lose a formal sector job than those who had the card for more than two years. Using comprehensive employment records, we use firm downsizing (Couch and Placzek, 2010a; Flaaen et al., 2019; Jacobson et al., 1993) to estimate the causal effect of involuntary job loss on study card default. The rarity of downsizing makes our large study sample particularly useful for the analysis.

Our fourth main result is that job loss leads to a 7.6 pp increase in the probability of default on the study card over the next eighteen months—a seven-fold larger effect than from the 30 pp change in interest rates on the same sample over the same 18-month period. A back-of-the-envelope calculation suggests that formal job loss alone can explain approximately 14% of total study card default during our study. These magnitudes are substantial and consistent with the hypothesis that new borrowers are vulnerable to large shocks that precipitate default. We replicate these findings with a representative sample of one million borrowers and find similar results.

What might explain the larger effect of job loss? One possibility is that unemployment shocks have a larger impact on cash flow relative to the debt-servicing requirements of a 30 pp increase in the interest rate or a doubling of the minimum payment. Another possibility is that unemployment has a larger effect on default on a per-peso basis. A back-of-the-envelope calculation suggests that all three shocks have similar per-peso effects—a shock-induced cash flow decrease of 1,000 MXN pesos (over 18 months) is associated with default increases of 0.36–0.51 pp for all three "shocks," and we cannot reject the null

[6]For instance, using Mexico's National Employment Survey (ENOE), we find that more than four-fifths of workers who lose formal employment are informally employed in the next quarter.

hypothesis that all three parameters are equal (p = 0.78). These results are consistent with cash flow impacts being an important determinant of default, regardless of source. The smaller effects of contract terms can thus be rationalized by their smaller effects on cash flow, despite the changes themselves being quite substantial.

We draw three lessons from these results. First, while higher minimum payments do not reduce default during the experiment (plausibly by tightening liquidity constraints), they decrease default in the long run (after the end of the experiment), possibly by reducing debt. Second, interest rate reductions do not reduce default for new borrowers. This is unfortunate, since ex-ante credit score screening is less useful for such borrowers given their limited credit histories (e.g., see Liberman et al., 2018) forcing banks to rely on default mitigation via contract terms. Interest rate changes are then least effective precisely where the asymmetric information problem is the most acute. These results strongly suggest the limited effectiveness of policies based on contract term changes to limit default. Third, the weaker labor force attachment of newer borrowers and the substantial effects of job separation on default suggest that job loss may play an important role in determining continued access to formal credit for populations such as those under study.

Related Literature. We connect with several strands in the literature on credit markets. First, a vibrant financial inclusion literature diagnoses low financial services penetration and advocates supply-side interventions to increase financial inclusion (Dabla-Norris et al., 2015; Demirgüç-Kunt and Klapper, 2012; Dupas et al., 2018). We examine Mexico's most popular financial inclusion loan product and provide evidence on the effectiveness of contract terms in limiting default. Second, we contribute to research on credit cards in developing countries—a relatively under-studied topic despite cards' increasingly important role as the source of entry into the formal credit sector for new borrowers (De Giorgi, Drenik, and Seira, 2023; Ponce, Seira, and Zamarripa, 2017). Third, we contribute to the literature evaluating the effect of changes in minimum payment terms—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 approach on a non-experimental change in minimum payments (both in the United States).[7] Fourth, a substantial literature has focused on the importance of contract terms and interest-rate-driven moral hazard (e.g., Banerjee and Duflo, 2010; Karlan and Zinman, 2009), though not focusing specifically on new borrower populations.

Fifth, we add to the fledgling literature analyzing the credit market consequences of job loss. Keys (2018) analyzes the effect of job loss and bankruptcy filing in the U.S. 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 Panel Study of Income Dynamics. Our contribution is to use individual-level administrative employment data matched with our experimental sample to estimate an event study design using mass layoffs as a source of exogenous job separation. More generally, we complement research studying the connections between labor and credit markets and social insurance (which is primarily U.S. focused). For instance, Herkenhoff (2019) studies the effect of

[7] There is an active literature examining credit cards in the U.S. (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, and complexity) in 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.

credit markets on the labor market in the U.S., while we study the reverse causal relationship. Hsu et al. (2018) and Bornstein and Indarte (2023) demonstrate the value of social protection programs (state-level expansions of unemployment insurance and Medicaid, respectively) in improving credit market outcomes. We instead establish the effect of individual-level unemployment shocks on default in a country with limited social protection and benchmark the credit market effects against (the upper bounds of) policy-relevant changes in interest rates and minimum payments. Finally, our work is also complementary to Ganong and Noel (2022), who examine the effect of "negative life events"—inferred through bank account data—on mortgage default in the U.S. In our context, we directly observe individual unemployment shocks from administrative data and can compare these effects to those of loan term changes on a common sample.

The paper proceeds as follows: Section 2 outlines our various data sets and provides basic summary statistics. Section 3 provides context about financial inclusion in Mexico, default rates, and borrower liquidity constraints. Section 4 describes the experiment. Section 5 provides a simple model to help interpret results. Section 6 reports the experimental effects of minimum payments and interest rates and provides some evidence on the mechanisms driving default. Section 7 estimates the effect of job displacement on default and compares it to the effects of the contract term changes on a per-peso basis. Section 8 concludes. Due to space constraints, some robustness analyses, secondary figures, and tables are reported in the Online Appendices (OA).

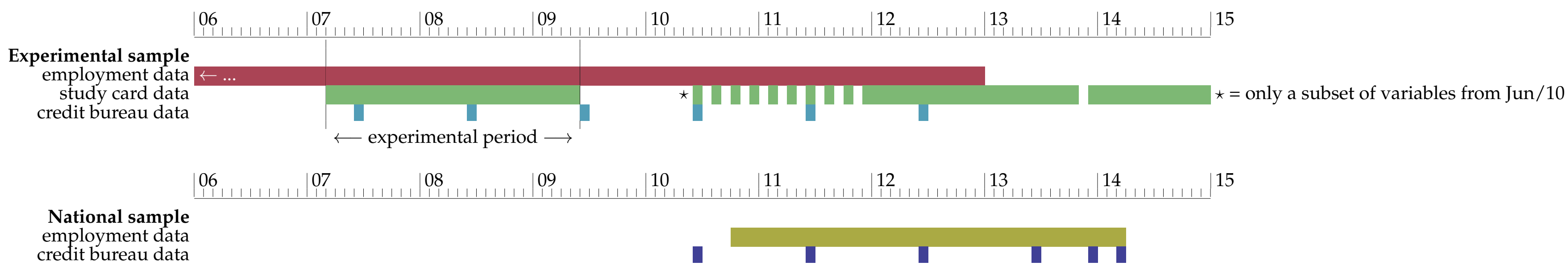
2 Data and Summary Statistics

We focus on study card borrowers in Bank A's contract terms experiment. In addition to obtaining Bank A data, we matched the experimental sample to two data sources. The first is employer-employee data from the social security administration (IMSS), which we use to study the effects of formal job loss. The second is credit bureau data, where we observe every (formal) loan held by the study card sample, which we use to gather additional information about our study sample and examine spillovers.

In addition, we obtained several representative cross-sectional random samples (of one million borrowers each) from the credit bureau. We use these snapshots to compare our study card borrowers to all formal sector borrowers in Mexico. We also match these snapshots to the IMSS data to examine whether our unemployment results generalize to the population of borrowers with a formal sector employment history. Figure 1 depicts when we observe information from the different data sources (see Appendix B.2 for more details). We now describe the data sets in more detail.

Figure 1: Timeline for the Datasets

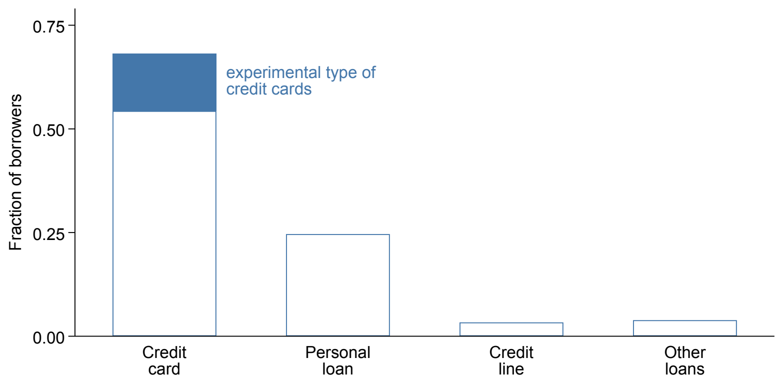
1. **Bank data:**
   - Monthly card-level data of the study card from Mar 2007 to May 2009, bimonthly from Jun 2010 to Dec 2011 and monthly from Jan 2012 to Dec 2014.
2. **Credit Bureau data:**
   - Loan-level data matched to the experimental sample for Jun 2007 to Jun 2012, annually.
   - Loan-level 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 2004 to Dec 2012.
   - Individual-level, monthly information from Oct 2010 to Mar 2014.



Notes: This figure presents a timeline for the experiment. The data used to define 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, every June from 2007 to 2012. National sample: we also have data unrelated to the experiment. This comprises random sample of 1 million borrowers from credit bureau data, and the universe of Mexico's social security data.

Study Card and Bank Data (Experimental Sample): We use detailed Bank A data for the study card, which accounted for 15% of first-time loans nationwide in 2010 (Figure 2). The study card is a credit card that can be used at a large set of supermarkets as well as other stores. In 2011, these stores accounted for 43% of all household expenditures at all supermarkets and 16% of all household expenditures in Mexico.[8]

Figure 2: 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 Study Card (described in Section 2) . The Study card represents 15% of all first time loans in Mexico.

Borrowers new to formal credit market: 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 was the first credit card for 57%. Customers for the study card approached bank kiosks in supermarkets nationwide 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 inter-bank interest rate (the TIIE), and a monthly minimum payment of 4% of the total amount outstanding. The card was initially offered in 2003, and by 2009 it had approximately 1.3 million users—–a substantial financial inclusion effort in a country with approximately 11 million cards at the time (Banco de Mexico, 2024).

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, and our results are representative of this population. Using data from the credit bureau, we find that the minimum payment eligibility restriction removed 6.7% of study card holders from the sampling frame. When we construct weights to attempt to make the experimental sample representative of the population without the eligibility criterion, we obtain treatment effect elasticities that are virtually the same as those reported here (results available upon request). 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

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

Table OA-1) in all of our analysis to ensure our results are representative of the sampling frame. Table 1 compares our sample with the national population of new borrowers in Mexico.

Variables: We have monthly data on purchases, debt, credit limits, and cancelations from March 2007 to May 2009. We observe default and payments from March 2007 to December 2014 but at different frequencies and with one gap (from June 2009 to June 2010): 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. We observe a limited set of demographic variables—age, gender, marital status and residential zip code.

Throughout the paper, we focus on default because it is the focus of a significant literature on credit markets, a key outcome of interest for lenders and regulators, and we observe it over a long horizon.[9] Since it is a key outcome, 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 default measure at time t is cumulative: i.e., $Y_{it} = 1$ if i defaulted in any month $s \leq t$ and 0 otherwise. This allows us to perform the default analysis on an unchanging sample. By contrast, defining $Y_{it} = 1$ if i defaults in t conditional on not defaulting in $t-1$ and dropping i from estimation for $t' > t$ implies that the sample changes from month to month, with attrition potentially driven by the treatment, making the estimands difficult to interpret.

Credit Bureau Data (Matched to Experimental Sample): A borrower appears in the credit bureau if they have had a loan with a formal financial intermediary.[10] 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 matched the study sample to the credit bureau (*Buró de Crédito*) data once each year from June 2007 to June 2012. This match 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 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 refer to this as the *population representative* CB data.

IMSS Employment Data (Matched to Study Sample): An individual appears in Mexico's social security database if they have held a formal sector job for at least one month. Presence in the IMSS is, by

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

[10]The credit bureau must maintain all records provided by reporting agencies for a fixed period. 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).

definition, employment in the formal sector.[11] Absence from the IMSS data can thus be interpreted as absence from the formal sector. We observe monthly data from January 2004 to December 2012. For each worker and each month they are formally employed, we observe their salary, a firm identifier (anonymized), and a geographical identifier. We match our experimental sample to the IMSS data using individual identifiers (known as CURP in Mexico). CURPs are stable 18-digit individual-level identifiers that are widely used in administrative as well as private-sector databases in Mexico. We observe CURPs for 89% (144,320/162,000) of the experimental sample and can locate 84,679 (59%) of these in the IMSS data. This is not unexpected since IMSS data only captures formal employment; estimates using labor force data suggest that about half of workers are not formally employed.

IMSS Employment Data (Matched to the population representative CB): We also obtained the universe of Mexican social security data from October 2011 to March 2014 which we matched to Credit Bureau Data representative cross-sections. Our matched CB sample includes 600,339 individuals with credit information and employment histories. Given the equivalence of presence in the IMSS with formal sector participation, the matched data allows us to estimate the effect of formal job loss on loan default for a representative sample of Mexican borrowers with a formal sector employment history (over the period of October 2011 to March 2014). We use this matched data for a robustness exercise to evaluate the generalizability of our results linking formal job loss and default.

Mexico's Official Employment Survey Data. We use data from the official (INEGI) Mexican Employment Survey (Encuesta Nacional de Ocupación y Empleo or ENOE) from 2005 to 2015. This is a rotating panel following individuals for 5 quarters allowing researchers to observe whether a person is employed, whether employment is formal (registered with IMSS) or informal, and wages. It has been used extensively (e.g. Donovan et al., 2023; Maloney, 1999) and we use it to estimate total earnings losses (i.e. combining formal and informal employment) as a consequence of formal job loss.

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 borrowers with at least one credit card in 2010. Column 4 is a sample of borrowers in the CB data that matches the tenure of the experimental sample in the formal credit market (measured by the year of the first loan of the experimental sample; see Online Appendix B.3 for details). Finally, for comparison, Column 5 considers a sub-sample of experienced borrowers—those with a credit history of

[11] The IMSS is responsible for social security provision in Mexico, and having social security coverage is typically the definition of formal employment in Mexico (see e.g., Duval-Hernández, 2022). Employers must register with the IMSS all employees with social security coverage (the latter is financed through a payroll tax, so the registration criterion is equivalently defined as all employees whose wages are subject to a payroll tax).

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	734	-	-	-
	(1,473)	(1,375)			
Purchases	338	550	-	-	-
	(1,023)	(1,438)			
Debt	1,198	1,799	-	-	-
	(3,521)	(4,804)			
Credit limit	7,879	11,823	-	-	-
	(6,117)	(10,101)			
Credit score	645	-	-	-	-
	(52)				
(%) Consumers for whom experiment is their first card	57	-	-	-	-
(%) Consumers who default between Mar/07 - May/09	19	-	-	-	-
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)	13,987	18,579	23,572	20,499	50,369
	(10,760)	(15,473)	(31,471)	(27,395)	(42,397)
Total credit line (all cards)	51,542	54,305	52,500	44,823	124,333
	(54,056)	(55,765)	(98,149)	(83,399)	(152,130)
Tenure of oldest credit	71	103	85	72	194
	(54)	(51)	(81)	(56)	(81)
Panel C. Demographic information					
Month of measurement	June 2007	June 2010	June 2010	June 2010	June 2010
(%) Male	53	-	50	49	56
(%) Married	65	-	52	52	51
Age (in years)	39	42	42	41	50
	(6)	(6)	(12)	(12)	(11)
Monthly income	10,065	11,951	-	-	-
	(8,345)	(10,143)			
Panel D. Comparable income estimates					
Month of measurement	October 2011	-	October 2011	October 2011	October 2011
Monthly Income‡	13,849	-	14,500	14,113	22,286
	(11,246)		(12,730)	(12,431)	(15,803)
Observations	162,000	97,248 (Panel A) 155,945 (Panel B & C)	415,793	379,310	110,904

Notes: This table presents means and standard deviations for selected variables from the experimental sample and three different credit bureau sub-samples. Panel A shows statistics for the experimental sample (what we called "Study Card and Bank Data (Experimental Sample)" in the data section). Panels B and C use different data sources. For columns 1 and 2 they use the "Study Card and Bank Data (Experimental Sample)". For columns 3,4,5 they use what we called "Credit Bureau Data (Representative Cross-Sections)". Panel C uses "Study Card and Bank Data (Experimental Sample)" in columns 1 and 2, and "Credit Bureau Data (Representative Cross-Sections)" for columns 3,4,5. Finally Panel D uses "IMSS Employment Data (Matched to Experimental Sample)" for columns 1 and 2, and "IMSS Employment Data (Matched to the CB)" for columns 3,4,5. Columns 1 and 2 are computed using strata weights. 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.3 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.

at least eight 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 the three CB data sub-samples. Borrowers in the experimental sample are somewhat less well-off than the average CB member. For the borrowers we could match to IMSS, the average monthly income in the experimental sample is 13,849 MXN pesos compared to 14,500 MXN pesos for recent and 22,286 MXN pesos for experienced borrowers.[12] The proportion of study card borrowers 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 59%, roughly similar to the fraction of the formal labor force in the country. 41% of study card borrowers were employed in the formal sector in March 2007, when the experiment began.

3 Context

In this section, we provide some context for the intervention and some basic characteristics of borrowers new to formal credit.

Rapid Card Expansion Among Low-Income Individuals: The number of credit card accounts in Mexico grew by 28% from 2006 to 2011 (Banco de Mexico, 2024), with a substantial part of the growth being concentrated among lower-income individuals (Figure OA-1). As noted above, the study card played a vital role in this expansion. This pattern is typical throughout Latin America, as many borrowers use only credit cards in their formal loan portfolio (see Figure OA-2).

Distance Lending and Default Mitigation: Bank A's initiatives, and those of other large traditional commercial banks, to pursue low-income clients with limited credit histories appear to have been partly inspired by the success of Banco Compartamos and Banco Azteca.[13] However, Compartamos and Azteca pursue markedly different strategies than those pursued by Bank A. Compartamos is a micro-finance lender, primarily using 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.[14] In contrast, Bank A relies on traditional 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 than those employed by Compartamos and Azteca, with operating expenses relative to assets being an order of magnitude smaller (see Figure OA-3). Whether these lower-cost distance-lending methods are sustainable with new-to-banking borrower populations remains an open question—the concern is that default may be substantially higher with such methods for these populations. In this context, it is important to understand the causes of default and the extent to which contract terms could mitigate default.

[12] For comparison, the average monthly per capita income in Mexico in 2007 was 4,984 MXN pesos. Our experimental sample's 25th and 75th percentiles of income are 2,860 and 19,535 MXN pesos, respectively. In comparison, they are 2,580 and 6,000 MXN pesos for the country as a whole.

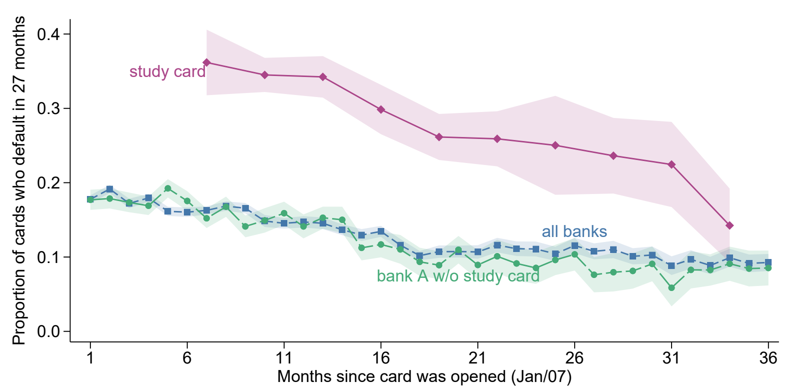
[13] See e.g., https://goo.gl/7HufqG; https://goo.gl/vi2EYK; https://goo.gl/sjgoAn.

[14] 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: Study subjects, who tend to have limited or no credit histories, have low credit scores. The strata-weighted mean credit score (645) is low in absolute terms. Borrowers with scores below 670 are typically ineligible for standard credit card products (De Giorgi et al., 2023). Borrowers also have low credit limits. In our study sample, the (weighted) mean credit limit for the study card was relatively low at 11,823 MXN pesos in May 2009. For comparison, in 2010, the mean card limit was 23,572 MXN pesos for those with at least one active card in the credit bureau.

Default is High for New Borrowers and Declines with Tenure: During our 26-month study, approximately 19% of the control group defaulted on their card (computed using stratum weights), compared to an average cumulative 26-month default rate of 12% 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 3 shows that newer borrowers in the study card sample 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 experiment began) and 18% for the oldest borrowers (those with tenure greater than two years).[15]

Figure 3: 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 in the 26-month period from March 2007 to 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 control group averages are constructed using stratum weights. The blue squares and green circles use the same sampling design used to generate the experimental sample (but 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 plots default rates for three different card groups: the study card (red diamonds), all credit cards offered by Bank A (blue squares), and all cards in the credit bureau (green circles).[16] 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

[15]Several mechanisms including selection and learning may help explain this tenure-default gradient (and we do not distinguish between them). However, it is worth noting that both of these candidate explanations (selection and learning) do not necessarily predict smaller responses from contract term changes for newer borrowers relative to older ones.

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

rates for Bank A's other cards are similar to those at other banks.

In principle, high default rates could reflect a low default cost or limited benefits from keeping the card current. We provide evidence against this below by (a) documenting that card default substantially reduces access to formal credit, (b) providing revealed preference estimates of the value of formal credit by showing that arguably exogenous increases in credit limits lead to sharp increases in borrowing, and (c) showing that formal credit has more attractive terms (lower interest rates, larger loan sizes, and longer repayment periods) than informal credit.

Default Reduces Access to Formal Credit: Default reduces subsequent formal sector borrowing. We document the magnitude of the effect using two complementary approaches, summarizing the results here with the details relegated to Appendix C. First, using an instrumental variables strategy that uses treatment assignment as an instrument for default, we find that the probability of having a new loan one year after default on the study card is 65 pp lower relative to the non-default counterfactual (p = 0.03). Second, using a selection on observables assumption, we show that default on the study card is associated with the complete absence of any subsequent credit card for at least four years. Card default thus severely limits subsequent formal credit. This is problematic since, as we now show, study borrowers appear to be credit constrained, and borrowing informally is much more costly.

New Borrowers are Liquidity Constrained: The ratio of debt to the credit limit is a commonly used measure of liquidity constraints (see e.g., Gross and Souleles, 2002). Following this approach, we (a) assess the responsiveness of debt to plausibly exogenous changes in credit limits as a measure of the extent of liquidity (or credit; we use the terms interchangeably in our setting) constraints, and (b) we examine the extent to which this responsiveness varies by baseline liquidity (i.e., the ratio of debt to the credit limit at baseline).

We carry out these exercises in Appendix D, where we begin by showing that debt is responsive to changes in credit limits for both study card debt and total card debt. A 100 MXN peso increase in the study card's credit limit translates into 32 MXN pesos of additional debt (the instrumental variable estimates are more than twice as large). These estimates are thrice as large as the comparable estimates from the U.S. and significantly larger than those documented by Aydin (2022). In addition, debt responsiveness is higher for sub-groups for whom we expect liquidity constraints to be more binding. In particular, this responsiveness is 59 MXN pesos for borrowers in the highest tercile of the debt-to-limit ratio at baseline (i.e., those most constrained at baseline by the measure), relative to 22 MXN pesos for borrowers in the lowest tercile. Similarly, borrowers paying close to the minimum had debt responses about three to ten times larger than those with the best repayment behavior.

Informal Terms are Worse Than Formal Terms: We use the Mexican Family Life Survey (MxFLS) to compare interest rates, loan amounts, and loan durations for formal and informal loans.[17] We find that informal loan terms are significantly worse than formal terms. Table OA-2 shows results from regressing contract terms on an indicator for a formal loan and controls. First, the average annual informal loan

[17] We define a loan as formal if the lender is a bank and informal otherwise. Informal loan sources comprise cooperatives (13%), money-lenders (8%), relatives (38%), acquaintances (20%), work (11%), pawnshops (5%), and others (5%). Consistent with 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. We do not observe informal sector loans in our bank data.

interest rate is 291%, while the corresponding rate for formal loans is 94 points lower (col. 1). The average loan size is 3,658 MXN pesos for informal loans and 9,842 MXN pesos for formal loans (col. 4). The average term for informal loans is 0.52 years versus 1.07 years for formal loans (col. 9). Figure OA-4 shows that the distribution of interest rates for informal loans first-order stochastically dominates that for formal loan rates, while the opposite is true for loan terms and amounts. These results are robust to controlling for income and wealth proxies (columns 2, 4, and 7), and the loan term and duration results hold even with household fixed effects.[18] While not dispositive, these results suggest that informal loan terms are onerous compared to formal terms, incentivizing borrowers to maintain access to formal credit.

The evidence thus suggests that formal credit is attractive to borrowers and that credit card default is consequential. This context will help in interpreting both default levels and experimental responses.

Formal Job Loss is Common and There is a Significant Informal Labor Market. Formal job loss is common in Mexico. In a recent analysis of labor force data from 49 countries including Mexico, Donovan et al. (2023) find that exit, job-finding, and transition rates are roughly twice as high in developing countries relative to developed ones. Job-finding rates are high partly because workers who lose (higher-paid) formal jobs often move quickly to (typically lower-paid) informal jobs (see also e.g., Maloney, 1999). In Appendix J, using the ENOE we estimate that approximately half the labor force is formally employed and that 82% of all workers who lose formal employment in a given quarter are informally employed in the immediate subsequent quarter (Figure OA-24).

4 Experiment Overview

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 year (both measured in January 2007).[19] 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 two 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 them during this time, and (iii) 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 them during this time. These two variables were used to define nine strata, and 18,000 borrowers were randomly selected from each stratum. The resulting sample is geographically widespread—covering all 32 states, 1,360 municipalities, and 12,233 zip codes.

Experimental Design: Within each stratum, the bank randomly allocated 2,000 members to each of the eight 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 the amount out-

[18] Only about 4.3% of households hold both formal and informal sector loans and are used in the fixed-effects specification.

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

standing (debt) on the card, and (ii) the interest rate on the amount outstanding.

The minimum payment was set at either 5% or 10%. For context, 73% of borrowers paid less than 10% of the amount due before the experiment began (see Figure OA-5). The minimum payment prior to the study was 4%. The interest rate (expressed as the annual percentage rate or APR) could take one of four values: 15%, 25%, 35%, or 45%. The interest rate for the study card prior to the study was approximately 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 and to debt outstanding. Thus, the rate changes include both a forward-looking component as well as a current component (in contrast with e.g. Karlan and Zinman, 2009, who vary both components independently).

These are substantial changes in contract terms. For instance, the caps on credit card interest rates considered by regulators (e.g., in Turkey and Indonesia) involved changes of no more than 5–10 pp (see, e.g., Web Archive Link for Turkey and Rossiana and Bisara (2016) for Indonesia). The experimental variation in interest rates is equivalent to moving from the 20th to the 80th percentile in Mexico's cross-sectional interest-rate distribution across lenders (Banco de México, 2009).[20] Similarly, the mandated increases in minimum payments (e.g., in Mexico and Quebec) are well below the 10% enforced in the experiment (at about 1.5% and 5%, respectively). Thus, the experimental contract terms changes lie on the upper end of the policy feasible changes contemplated by regulators.

The two different minimum payments and four different interest rates yield eight unique contract terms (see Table OA-3). We were informed that the minimum payment for the hold-out arm was 4%. However, the interest rate varied across clients, and, unfortunately, we do not observe this rate.[21] Consequently, we do not use the hold-out group as a contrast. We use the 45% interest rate and the 5% minimum payment group (abbreviated to $(45, 5)$ when useful) as the comparison group and refer to it as the base arm or excluded group. Panel A of Table OA-4 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 1 shows the experiment's timeline and measurement dates. The bank mailed each study client a letter in March 2007 stating the new contract terms in force starting in April 2007. Clients were not informed that they were part of a study or of any timelines 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 fixed at their experimentally assigned levels. Internally, the experimental terms were not revealed to the risk department in charge of determining credit limits. We cannot reject the null of no differences in credit limits across treatment arms at baseline and end-line (Table OA-5 and Figure OA-6).[22] The experiment ended in May 2009, when all participants received a letter stating their new contract terms. The new contract terms were the

[20] We conjecture that the experimental range of variation would cover an even larger range of within-lender (or within-borrower) variation as lenders typically specialize in different segments.

[21] We were also told that marketing efforts for this group may have been different than for the eight experimental groups, which received virtually no marketing. The fact that both minimum payment and interest rates are simultaneously different in the hold-out group and that marketing and other policies may also be different means that we cannot attribute differences in behavior separately to interest rates or minimum payments.

[22] Although not the focus of this paper, in an interesting and complementary paper Aydin (2022) analyzes the effects of randomized changes in credit limits.

standard conditions with an APR of approximately 55% and a minimum payment of 4%.

5 From Contract Terms and Income Shocks to Default: A Framework

This section outlines a model that provides comparative statics for the effect of key exogenous variables (contract terms and income) on key decision variables in the data (purchases, debt, and default). Our model is loosely based on Einav et al. (2013).

Given our setting, we do not model selection into the credit card and consider a borrower who already has a card and is observed for two periods. The agent begins period 1 with (exogenous) accumulated debt ($C_0 > 0$) on which they must make a minimum payment that equals a fraction m_1 of the amount due in period 1. We allow the minimum payment to differ across the two periods since it allows for useful comparative statics. R is the one-period gross interest rate ($R \equiv 1 + r$) and the amount due in period 1 is $m_1 RC_0$. We do not need R to vary across periods in order to rationalize our primary experimental findings (although we do explore the implications of doing so in Appendix E.1.5).

If the agent does not default, they make net purchases P (i.e., purchases minus any payments in excess of the minimum payment) on the card and therefore their total debt at the end of the period is given by $C_1 = P + (1 - m_1)RC_0$. Appendix B.4 verifies the multi-period analog of this identity for our experimental data. In principle, P can be negative so borrowers can choose to repay more than the minimum amount. We will assume that borrowers (if they pay) pay strictly less than what they owe, so that they carry positive debt into the second period (i.e., $C_1 > 0$). This is consistent with our setting where the vast majority of borrowers pay close to the minimum payment. Since interest is accumulated on previously accumulated debt C_0, interest rate increases will automatically increase one component of total debt. This feature will be useful in understanding the effect of interest rate changes on debt.

Borrower income in the first period is y_1. The static portion of the first period utility (i.e., without the continuation value) is given by:

$$\begin{aligned} u(y_1) + \epsilon_{11} \qquad & \text{if the borrower defaults} \\ u(y_1 + P - m_1 RC_0) + \epsilon_{10} \qquad & \text{if the borrower does not default,} \end{aligned}$$

where $u(\cdot)$ is the borrower's utility function and the random vector $\{(\epsilon_{t0}, \epsilon_{t1})\}_{t=1}^{2}$ captures underlying heterogeneity across borrowers which is independent of the model's other exogeneous variables.[23] If the agent defaults in period 1, they take no further actions, and their period 2 utility is $u(y_2)$.

In the second period the borrower realizes exogenous income $y_2 \in \{y_L, y_H\}$. If the income realization is low—y_L which occurs with probability q—the borrower defaults and earns utility $u(y_L)$. If the realization is high, the borrower chooses whether to default (and consume income y_H) or make the minimum payment $m_2 RC_1$ and continue using the card in the future. Thus, conditional on the high-income

[23] A number of the results do not require a particular functional form for $u(\cdot)$. In the appendix, we are explicit about which results require a specific (in our case, logarithmic) functional form.

realization, the second period utility is given by:

$$\begin{aligned} u(y_H) + \epsilon_{21} & \qquad \text{if the borrower defaults} \\ v + u(y_H - m_2 R C_1) + \epsilon_{20} & \qquad \text{if the borrower does not default,} \end{aligned}$$

where v is the additional utility derived from the continued access to credit, and which can be interpreted as a reduced form parameter capturing the future flow of card benefits, a warm glow from card ownership, or the option value of having a card in the future.[24] Consistent with our context, v is only experienced if the card is not in default (defaulted cards are closed by the bank). We assume that the high-income realization is high enough to cover the minimum amount due ($y_H > m_2 R C_1$). The agent will not choose to pay more than the minimum in period 2 when income is high since there is no benefit to doing so in the model. This reduces the agent's period 2 decision to either default or make the minimum payment (and remain in good standing).

In Appendix E, we solve the model and characterize three endogenous variables: (a) a binary default decision in period one, (b) a continuous debt (equivalently net purchases) decision in period 1, and (c) a binary default decision in period 2. These decisions are functions of the following exogenous variables: (i) the initial debt with which agents start period 1 (C_0), (ii) the one-period gross interest rate R, (iii) the required minimum payments in each period $(m_1, m_2) \in (0,1)^2$; (iv) the one-period discount factor, $\delta \in (0,1)$; (v) the continuation value of card ownership ($v > 0$); (vi) first-period income (y_1); and (vii) the distribution for period two income $y_2 \in \{y_L, y_H\}$ with $q \equiv P(y_2 = y_L)$. To ease notation, we define θ as the entire vector of exogenous variables $\theta \equiv (C_0, R, m_1, m_2, \delta, v, y_1, y_L, y_H, q)$.

Despite its simplicity, the model allows us to derive meaningful and testable comparative statics. We summarize these below and provide complete derivations in Appendix E. Our first prediction examines the effect of interest rate changes on default in period 1. Interest rates affect the choice problem in two ways. First, interest rate changes apply to previously accumulated debt C_0 (consistent with the experiment) and increases in interest rates will mechanically increase this component of debt. Second, changes in interest rates apply to new debt as well (i.e., to purchases made on the card in period 1). The overall effect of interest rate changes on default depends on both these effects. As long as the sum of period 1 debt C_1^* and previously accumulated debt with interest (RC_0) is positive, default in period 1 will be increasing in the interest rate. This condition holds in our setting since we do not allow $C_0 < 0$ or $C_1^* < 0$ (i.e., agents cannot lend to the bank).

Prediction 1: Lower interest rates decrease default. Period 1 default (when agents can adjust debt responses) is increasing in the interest rate R (as long as $C_1^* + RC_0 > 0$).

We next consider changes in minimum payments when borrowers assume the same minimum payments hold in both periods ($m_1 = m_2 = m$). Increases in m lead to an increase in default as long as optimal debt C_1^* is strictly positive. This is because although increases in m lead to a decline in optimal debt, this is insufficient to decrease default (see Appendix E.2.2 for the argument).

[24]We do not model direct utility from card ownership 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.

Prediction 2: Higher minimum payments increase short-run default. If borrowers assume minimum payments are set as $m_1 = m_2 = m$. Then, period 1 default is increasing in m as long as period 1 debt is strictly positive.

The baseline model assumes perfect foresight—agents correctly anticipate period 2 contract terms. However, it may be useful to consider a situation where borrowers make decisions based on beliefs about future contract terms that may differ from those actually implemented later on. In particular, the experiment changed contract terms with no notice and, likewise, provided no advance warning to borrowers about the end of the experiment. One way to model this is to assume that borrowers make period 1 debt decisions believing that minimum payments will be the same in both periods (denoted by m^e). We then evaluate changes in period 2 default in response to changes in m^e while holding fixed the actual minimum payment implemented in period 2. This is intended to capture the effect of the experimental changes in minimum payments on post-experimental default (see Appendix E.1.4 for a proof).

Prediction 3: Higher minimum payments reduce long-run default. If a borrower makes debt choice (C_1^*) assuming that the minimum payment in both periods is m^e, and m_2 is a surprise announcement after C_1^* is chosen, then, period 2 default ($P_2(\cdot)$) is decreasing in m^e, $\frac{\partial P_2(C_1^*(m^e); m_2)}{\partial m^e} < 0$.

These predictions provide a useful framework for analyzing the policy prescriptions outlined in Section 1. In particular, higher minimum payments can both increase or decrease default within the model. Finally, we record the effect of replacing the second-period income distribution by one that is first-order stochastically dominated by it. Within our framework this thought experiment corresponds most closely to modeling the unemployment shocks we examine in Section 7.

Prediction 4: Negative life events increase default. Default probabilities in period 2 (when debt is held fixed) as well as in period 1 (when debt is allowed to adjust) are increasing in the probability of the low-income draw (q).

We will use this framework (and particularly the role of debt) to interpret the effects of contract terms (Section 6) as well as the effects of formal job loss on default (Section 7).

6 The Effects of Contract Terms on Default

This section comprises three sub-sections. First, we present the three main experimental effects of interest rates and minimum payments on default. Next, we interpret and study the mechanisms behind our results by using our model and additional analyses of intermediate variables. In particular, we emphasize and provide evidence for the role of liquidity constraints (in the short run) and accumulated debt in explaining the observed treatment effects. In the final sub-section, we introduce a measure of the changes that each intervention had on borrowers' "free" cash flow and use it to benchmark the treatment effect estimates to each other.

For ease of exposition and to maximize statistical power, our primary specification is

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

estimated on the sample of 144,000 individuals in the eight treatment arms using stratum weights (as defined in Table OA-1). 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. The main dependent variable in the paper is cumulative default, as explained in Section 2, but we also use this specification to study additional variables such as debt and purchases.

We interpret α_t as the mean value of Y_{it} in month t for the excluded group (i.e., the $r = 45\%$ and $MP = 5\%$ 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) month-by-month with heteroscedasticity robust standard errors, which is equivalent to estimating a single equation, i.e., pooled OLS, that fully interacts the intervention variables with month dummies, along with month-specific intercepts with robust standard errors clustered at the borrower level. We estimate the equation with and without stratum-by-month fixed effects and find almost identical results for β_t and γ_t.

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 from March 2007 through December 2014, along with their corresponding confidence intervals. The estimated means and treatment effects for the interest rate arms are in pink (left side), while those for the minimum payment arms are in blue (right side). Second, we present point estimates in tabular form at a set of (nine) time points in Table OA-6.

Equation (1) is restrictive because 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-6, which yield similar estimates. We also use the fully saturated model to test the separability and linearity assumptions and cannot statistically reject them.[25] For these reasons and because of their interpretability, we only discuss estimates from Equation (1). Additionally, we assess the sensitivity of our results to using cumulative default as the main dependent variable by estimating duration models in Appendix F and find that they yield nearly identical treatment effects.

6.1 Main Results

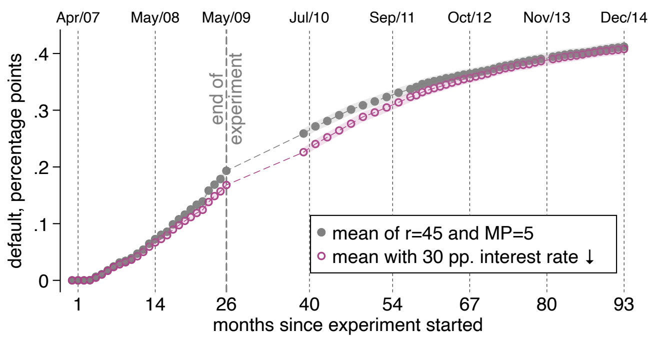
We begin by plotting the evolution of default for the excluded group (i.e., the $(45, 5)$ arm) with grey dots in Figures 4(a) and 4(b) using the estimates of $\{\alpha_t\}_t$ from March 2007 until December 2014 from Equation (1). 19% of the base arm had defaulted by the end of the 26 month experiment, and this figure rose to 41% by the end of the 93-month study period.

Result 1: Decreasing interest rates by 30 pp for 26 months causes a 2.5 pp decrease in default. Figure 4(c) plots the estimated treatment effects corresponding to a 30 pp decrease in the annual interest rate (i.e., plotting the estimates of $\{\gamma_t\}_t$ from eq. (1)). Default declines gradually in response to the interest rate decrease. By the end of the experiment, default fell by 2.5 pp (p < 0.001). Policy-relevant interest

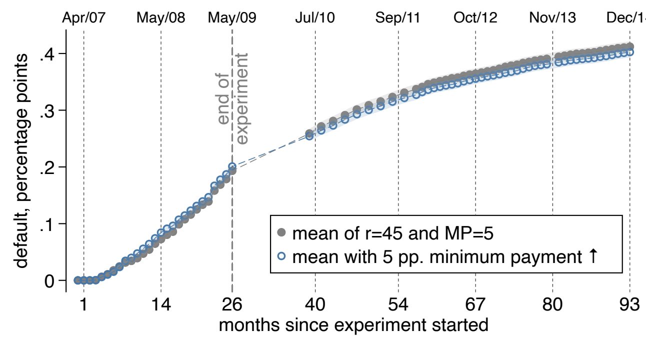
[25] For example, we use the fully saturated model to test whether the minimum payment effect is different across interest rate treatment arms and cannot reject the null that they are equal. Similarly, we test whether the interest rate effect differs in the low and high minimum payment groups and cannot reject the null of no differences. Full details are in Table OA-6.

Figure 4: Treatment Effects of Contract Terms on Default

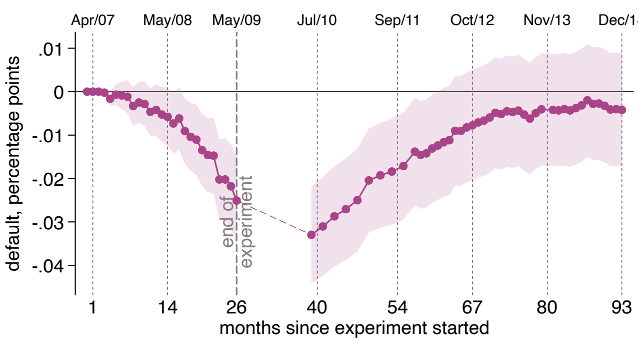
(a) Default Levels by Interest Rate



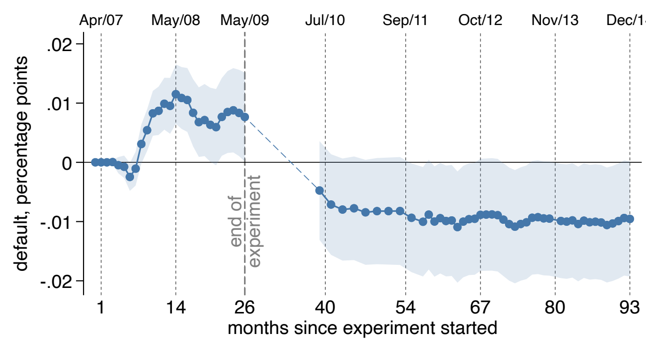
(b) Default Levels by Minimum Payment



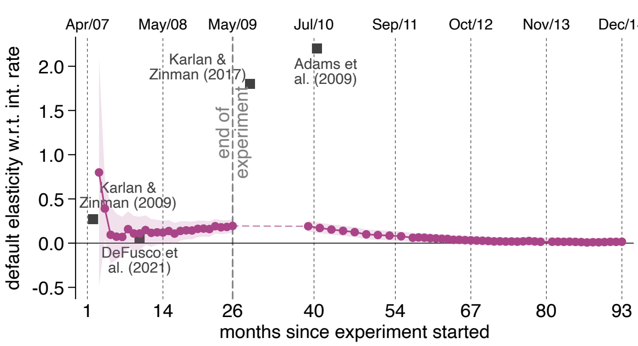
(c) ATE of 30 pp Interest Rate Decrease



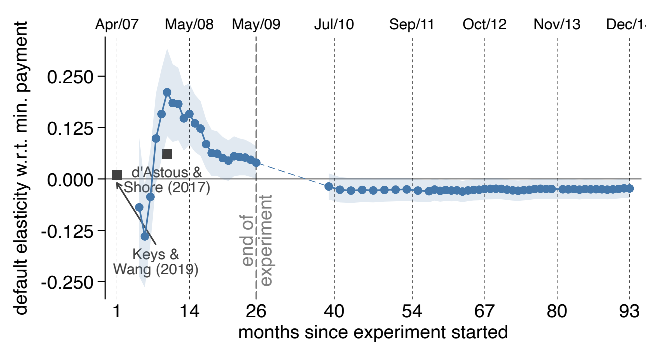
(d) ATE of 5 pp Minimum Payment Increase



(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 rate and minimum payment changes on default in the Study card. Figures on the left examine interest rate changes, and 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 pink dotted line in Panel (a) plots the share of cardholders that default over time when the 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., γ_t/α_t in Equation (1)) and dividing it by $(45-15)/45$. The first estimate is particularly high because the elasticity involves a term in the denominator very close to zero. Panel (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%.

rate changes (of, e.g., 10 pp) result in correspondingly smaller effects (0.84 pp).

We benchmark this result in two ways. First, our 26-month default elasticity of +0.20 is considerably lower than in Karlan and Zinman (2019) (1.8) and Adams et al. (2009) (2.2), though in the same range as Karlan and Zinman (2009) (0.27) and DeFusco et al. (2021) (0.01)—see Figure 4(e) for a graphical comparison accounting for intervention length. The variation in elasticities across studies could reflect variation in borrower tenure, and we explore this in Result 2 below.[26] Second, we compare the estimated effects with senior Mexican regulator predictions as well as 72 incentivized responses on the Social Science Prediction Platform (SSPP). Among regulators, the average predicted decline in default from a 30 pp decrease in interest rates was 8.6 pp (at the 18-month horizon), while the corresponding figure for SSPP respondents was 5 pp. Both estimates are considerably higher than the estimated ATE of 1.03 pp.[27]

All study borrowers were returned to the same contract terms after the end of the experiment in May 2009. Figure 4(c) displays the differences in default across study arms through December 2014. Default continues to be lower in the 15% rate arm for about three years after the experiment ends. The effects attenuate over time, reaching -1 pp by March 2012, after which they become statistically indistinguishable from zero. The 26-month reduction in interest rates thus decreased post-experiment default for nearly three years after the intervention ended, with elasticities ranging between 0.1 and 0.2.

Result 2: Decreasing interest rates by 30 pp has *no* effect on default for the newest borrowers. The large sample size and explicit stratification allow us to focus on newer borrowers—a population of interest since they most starkly reflect the challenges of financial inclusion. We estimate eq. (1) separately for the newest (6–11 months with the study card) and oldest strata (24+ months) and plot our results in Figure 5. Figure 5(a) shows that newer borrowers default at roughly twice the rate of older borrowers by the end of the experiment, with a level difference that persists through the study period. Figure 5(b) displays the ATEs separately for the newest and oldest strata, showing that new borrowers do not respond to a 30 pp decrease in interest rates, with point estimates remaining consistently small and statistically indistinguishable from zero (in contrast to older borrowers, who are much more responsive). This is in stark contrast to the elasticities reported in the literature above. The unresponsiveness of newer borrowers to large changes in interest rates is striking, as asymmetric information problems are likely most severe for this population.[28]

Result 3: Increasing minimum payments does not reduce default during the experiment, but reduces default by 1 pp in the long run. Figure 4(d) plots the estimated treatment effects corresponding to a doubling of the minimum payment (i.e., plotting the estimates of $\{\beta_t\}_t$ in Equation (1)) from the base rate of 5%. The treatment effect is indistinguishable from zero for the first eight months of the study. Although we observe a slight decline in months 7 and 8, the point estimates are small (about 0.001 or a tenth of 1 percent) and not statistically significant (the smallest p-value is 0.46). For these reasons, we view these declines as indistinguishable from zero. Default rose sharply starting at month 9 and peaked

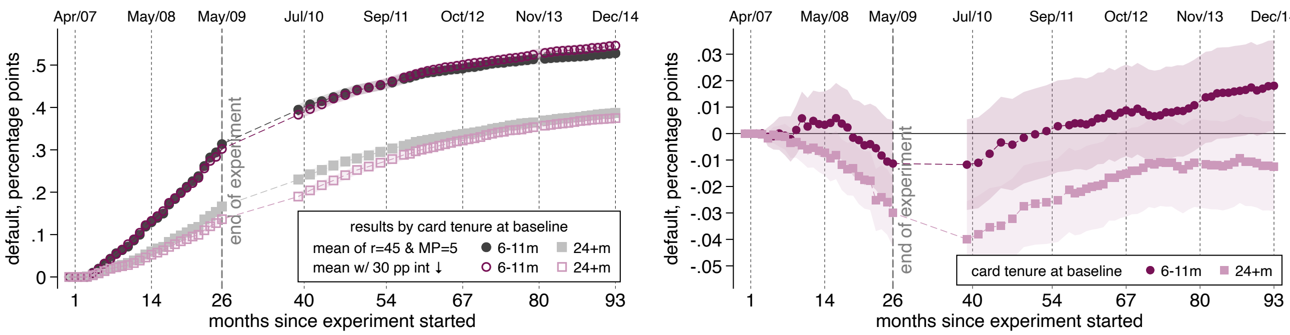
[26] Unfortunately, the cited papers do not report elasticities by borrower tenure.

[27] We elicited expectations at the 18-month horizon to allow for comparisons with our effects of unemployment, which, as we discuss in Section 7, are estimated at this horizon. See Appendix G for details.

[28] The null results are consistent with the findings in Nelson (2025) for the United States estimated using interest rate changes induced by a lender repricing campaign.

Figure 5: Effects of Interest Rates on Default by Tenure with Card

(a) Default Levels by Interest Rate **(b)** ATEs of 30 pp. Interest Rate Decrease



Notes: These figures plot the causal effect of interest rate changes on default in the study card by tenure with the card at baseline. We plot results for the newest (those who had the card for 6–11 months when the experiment began) and the oldest (those who had the card for 24+ months when the experiment began) strata. The grey solid dots/squares in Panel (a) represent the share of cardholders that default over time in the $(r = 45\%, MP = 5\%)$ group for each tenure group. The pink hollow dots/squares in Panel (a) plot the share of cardholders that default over time when the interest rate is decreased by 30 pp from 45% to 15% for each tenure group. The differences between the two lines for each tenure group in Panel (a) are plotted in Panel (b) and correspond to the average treatment effect of a 30 pp interest rate decrease from 45% to 15%. The dark pink circles represent the average treatment effect for the 6–11 month strata while the light pink squares represent the average treatment effect for the 24+ month strata.

at approximately 1 pp about 14 months into the intervention. The ATE then hovers around this point, and by the end of the experiment, the minimum payment increase had increased default by 0.8 pp (p = 0.016).

Given the cumulative nature of our default measure, this rough constancy of the ATE after month 14 implies very little subsequent differential default by treatment arm for the rest of the experimental period. Therefore, the increase in default due to higher minimum payments during the 26-month experiment predominantly arises from default in the first year. The implied 26-month elasticity is +0.04, and the confidence intervals rule out negative values. There are relatively few studies on the effect of minimum payments on default to benchmark our results. Our estimated elasticity is similar to those found in earlier non-experimental work (d'Astous and Shore, 2017; Keys and Wang, 2019) that documents estimates in the 0.01–0.06 range (see Figure 4(f) and Table OA-7 for more details), and considerably different from Mexican regulator and SSPP respondent predictions. On average, regulators predicted a decrease in default of 0.4 pp in response to an 18-month doubling of the minimum payment (from 5%) compared to the actual increase of 0.8 pp. Three-quarters of SSPP respondents predicted an increase in default from increasing minimum payments (consistent with our findings), but the mean predicted increase in default was substantially larger (6.4 pp).

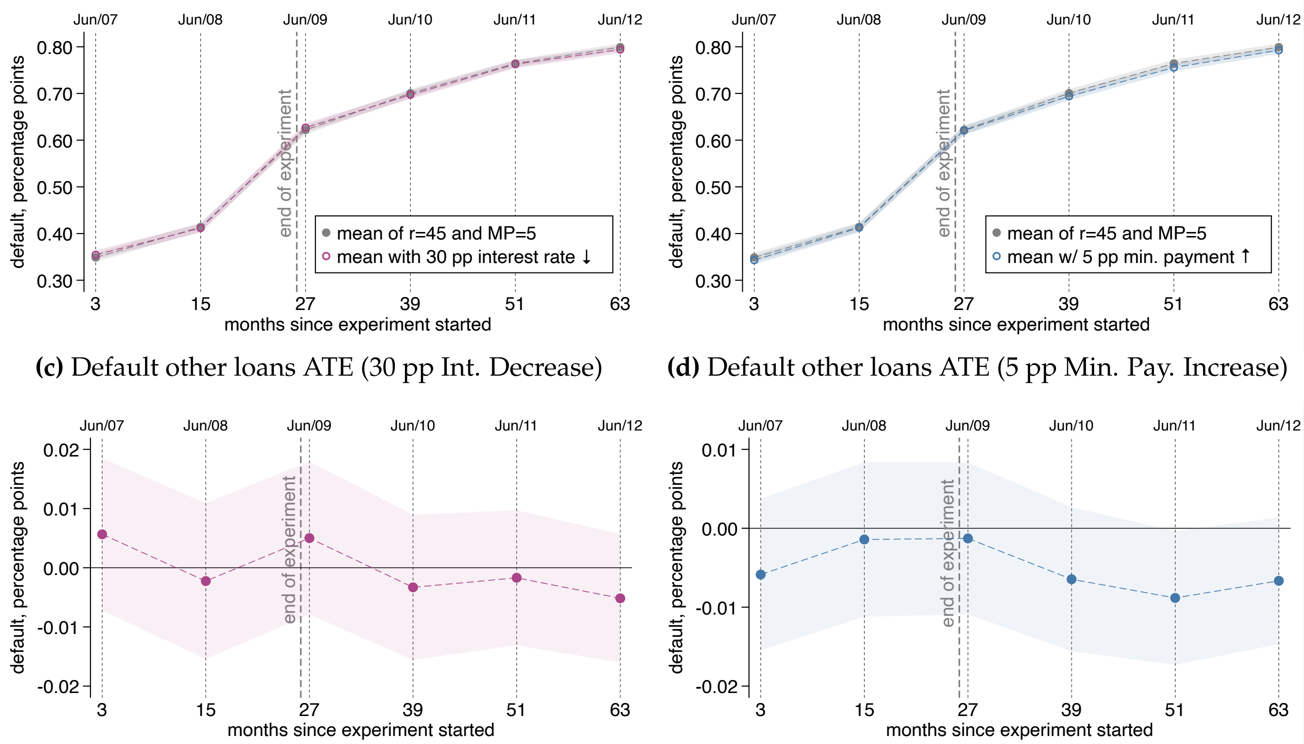
In contrast to the work cited above, the length of our panel and the experiment's timing allow us to estimate the post-intervention effects of the higher minimum payment. We find that the post-experimental long-run effects of the increase in minimum payments are opposite in sign to the effects in the short run. The post-intervention point estimates are consistently negative (see Figure 4(d)), showing a 1 pp *decline* in default for the (previously) higher minimum payment arm (p = 0.054 at the end of our sample period). In Section 6.2, we provide an interpretation for these findings based on the framework of Section 5.

6.1.1 Secondary Results

No Interactions of Treatment Effects: As shown in Table OA-6, we find no evidence of interactions between the two interventions. Despite the large sample size, 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 ($p = 0.44$) and five years after, by the end of our sample period in December 2014 ($p = 0.65$). Similarly, we cannot reject the null that the effect of a decrease in interest rates is constant across both minimum payment arms in May 2009 ($p = 0.08$) and five years after ($p = 0.411$).

Figure 6: Spillovers: Default on Other Loans

(a) Default on other loans vs Interest Rates (levels) **(b)** Default on other loans vs Min. Payment (levels)



(c) Default other loans ATE (30 pp Int. Decrease) **(d)** Default other loans ATE (5 pp Min. Pay. 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 issuance. The dependent variable is default on any loan in the credit bureau except for the experiment credit card. 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.

No Spillover Effects: The considerable variation in contract terms could also have affected behavior with other lenders, which we observe using the credit bureau data. For example, reductions in the study card interest rate could have decreased default on other loans. Additionally, higher minimum payments could have driven borrowers to other lenders, while lower interest rates may have had the opposite effect. We find no evidence of spillovers in terms of default, both during the experiment and after it ended (Figure 6).[29] We also find no evidence of crowd-out or crowd-in from other lenders in terms

[29] Figure OA-7 studies this separately for Bank A and other banks. We find that default on other loan products is largely unresponsive to interest rates and minimum payment changes, both during the experiment and after it ended. The only exception is a small decrease in default (3%, or 2 pp out of a 61 pp base) among other Bank A loans in the high minimum payment arm.

of new loans or loan closures (Figure OA-8). This aligns with Karlan and Zinman (2019) and Angelucci et al. (2015) who similarly find no spillovers on the number of loans or lenders in a micro-finance context.

6.2 Mechanisms and Interpretation

We now combine our basic framework from Section 5, additional estimation results from intermediate variables such as debt, payments, and purchases, and sub-population analyses to understand the contract-term induced changes in default. In particular, we emphasize and provide some evidence for the role of liquidity constraints in the short run and accumulated debt to explain the observed treatment effects. For simplicity, we describe the mechanisms behind each result separately and present supporting exhibits in Figure 7.[30]

Mechanisms for Result 1: The model predicts that decreasing interest rates will decrease default (Prediction 1) and Appendices E.1.3 and E.1.5 clarify the role played by debt. While default is increasing in debt, the model demonstrates that debt *can be* increasing in the interest rate. Equation (15) shows that a decline in the interest rate affects debt in two ways. First, debt increases as individuals purchase more in response to lower interest rates (the usual price effect). Second, debt declines since a lower interest rate is applied to the stock of previously accumulated debt. Thus, debt will *decline* in response to interest rate declines if the decrease in overall debt due to the latter exceeds the increases due to the former.

These patterns are indeed what we observe in the experiment. First, we document that purchases-net-of-payments in Figure 7(a) (and purchases in Figure OA-20(e)) increase in response to interest rate declines, consistent with downward-sloping demand. Despite these increases, overall debt *declines* as Figure 7(b) shows.[31] Appendix B.4 empirically verifies that current debt can be decomposed into past debt and current net purchases as in our model, and Equation (39) in the appendix shows that this decomposition implies that debt will increase with the interest rate, as we show empirically, if and only if the (mechanical) interest compounding effect exceeds the (behavioral) new purchase response. Finally, consistent with this line of argument, Figure 7(c) shows that the declines in default are concentrated among borrowers with the highest baseline debt utilization levels, i.e., those for whom the debt compounding effect is likely the strongest.

The debt channel can also rationalize the continued decline in default after the end of the experiment. The model predicts that agents with lower interest rates during the experiment default at lower rates after the experiment since they have lower debt at the end of the experiment. Appendix E.1.5 describes the theoretical argument in greater detail. As we do not observe debt after the experiment ends we cannot examine long-term debt responses, but as mentioned above Figure 7(b) shows that the lower interest rate arms had lower debt at the end of the experiment.

The literature distinguishes between at least three channels through which interest rates affect default: (a) the "debt burden" channel refers to the idea that higher interest rates increase debt mechani-

[30] Additionally, we present treatment heterogeneity results in Appendix H, and expand on the results on debt in Appendix I.

[31] Appendix I.1 examines the effect of interest rate declines on debt in more detail, concluding that the debt elasticity to the interest rate is indeed positive (our preferred estimates are $\epsilon \in [+0.18, +0.54]$). As noted in the relevant appendices, the results for debt, purchases, and payments are complicated by the fact that we do not observe these after default. In all cases, we attempt to adjust for this using Lee bounds.

cally, making repayment difficult; (b) the "pure current incentive effect" or "concurrent" moral hazard channel refers to the incentive effects of higher current interest rates on default (holding debt constant); (c) the "pure future incentive effect" or dynamic moral hazard channel refers to the incentive effects of anticipated changes to *future* interest rates (while holding current debt and interest rates constant) on current default. All three channels imply the same directional relationship—higher interest rates increase default. In our setting, all three channels are operative since interest rate changes apply to current and future debt for the foreseeable future. Therefore, a muted default response implies that the contributions from all three channels are correspondingly small.

Mechanisms for Result 2: We conjecture that the difference between new and old borrower default elasticities arises from differences in the value of the card—formalized in the Section 5 model by newer borrowers having a higher continuation value, v. Appendix E.3 shows that higher values of v imply more muted responses to interest rate changes.[32]

Several pieces of evidence are consistent with newer borrowers placing a higher value on the card than older ones. Newer borrowers, perhaps due to their limited credit histories, have fewer credit options in the formal sector. At baseline, 64% of the 6–11 month strata cardholders have a card with another bank. In contrast, the corresponding figure for those in the 24+ month strata is 78%. Newer borrowers also respond more strongly to credit line increases reflecting tighter liquidity constraints (see Section 3).

However, newer borrowers may also vary in other important dimensions from older borrowers. To study whether other baseline covariates may explain the difference between the treatment effect elasticities of new and older borrowers, we re-estimate the treatment effects at endline including a range of baseline covariates and interacting the covariates with treatment indicators. We find that the differential treatment effect between older and newer borrowers remains ($p = 0.05$, see Table OA-8).[33] While not dispositive, these results suggest that the observed treatment effects for newer borrowers are not driven by age, gender, baseline levels of card ownership, debt utilization, labor force attachment, or earnings.

Mechanisms for Result 3: The minimum payment-induced increase in default in the first year is consistent with tightened liquidity constraints. When interpreting the model's period 1 results as the short run, Prediction 2 states that increases in the minimum payment tighten liquidity constraints (in the sense of requiring a higher minimum payment holding period one income fixed), thereby increasing default among borrowers with sufficiently high levels of previously accumulated debt.

The empirical patterns we observe align with this interpretation. First, Figure 7(f) shows that the increase in default is entirely concentrated among borrowers with the highest debt utilization rates at baseline, who are arguably more liquidity-constrained than borrowers with lower utilization rates (see the discussion in Section 3). Second, the increase in default is preceded by a sharp rise in delinquencies (i.e., failure to make the minimum payment), particularly in months 3, 4, and 5 (Figure OA-9(b)). This

[32]More formally, *caeteris paribus*, a higher continuation value implies lower default in general, not just in response to interest rate decreases. However, this is counteracted for new borrowers by their lower incomes (e.g., 8,315 vs 10,459 MXN pesos) and their higher likelihood of job loss, as documented in Section 7. For instance, holding v and debt fixed, if the low-income probability q is higher for newer borrowers than for older borrowers (or, e.g., if y_H is lower for newer borrowers), then overall default will be higher. In these cases, the model can qualitatively reconcile higher default among newer borrowers relative to older borrowers and a lower response to changes in interest rates.

[33]Covariates included: strata, age, earnings terciles, labor force attachment, card utilization, gender, owns other card.

increase in the delinquency ATE only occurs among borrowers with the highest debt utilization rates (Figure 7(d)). Each such delinquency incurs a fee of 350 MXN pesos, further exacerbating repayment concerns. Indeed, we find a sharp rise in debt that mirrors the rise in delinquencies, suggesting that delinquency fees contributed to an increased repayment burden during this period and thus can also be attributed to liquidity constraints.[34]

Third, Figures OA-9(f) and OA-15(d) show that default and delinquency increases are likewise almost entirely concentrated among borrowers in the minimum-payer stratum, which is the most liquidity-constrained stratum (the baseline debt utilization rate for minimum payers is 85%, more than twice the rate for full-payers). Furthermore, the constancy of the ATE after the first year implies there was very little differential default by the treatment arm after the first year during the experiment.

The model can also help rationalize the post-treatment default effects. Bank A did not inform borrowers of the changed contract terms in advance of the experiment or their duration, so it seems reasonable to assume that borrowers expected minimum payment terms to continue. In the model, borrowers who anticipate a continuing higher minimum payment in period 2 (i.e., after the experiment) will choose lower debt levels in period 1 relative to those who anticipate a lower minimum payment, and the lower debt will translate into lower default in period 2. This is the content of Prediction 3, formally derived in Appendix E.1.4. Figure 7(e) shows that debt is indeed lower in the higher minimum payment arm at the end of the experiment. Borrowers with lower debt are then better placed to deal with negative shocks after the experiment ends and subsequently less likely to default.[35]

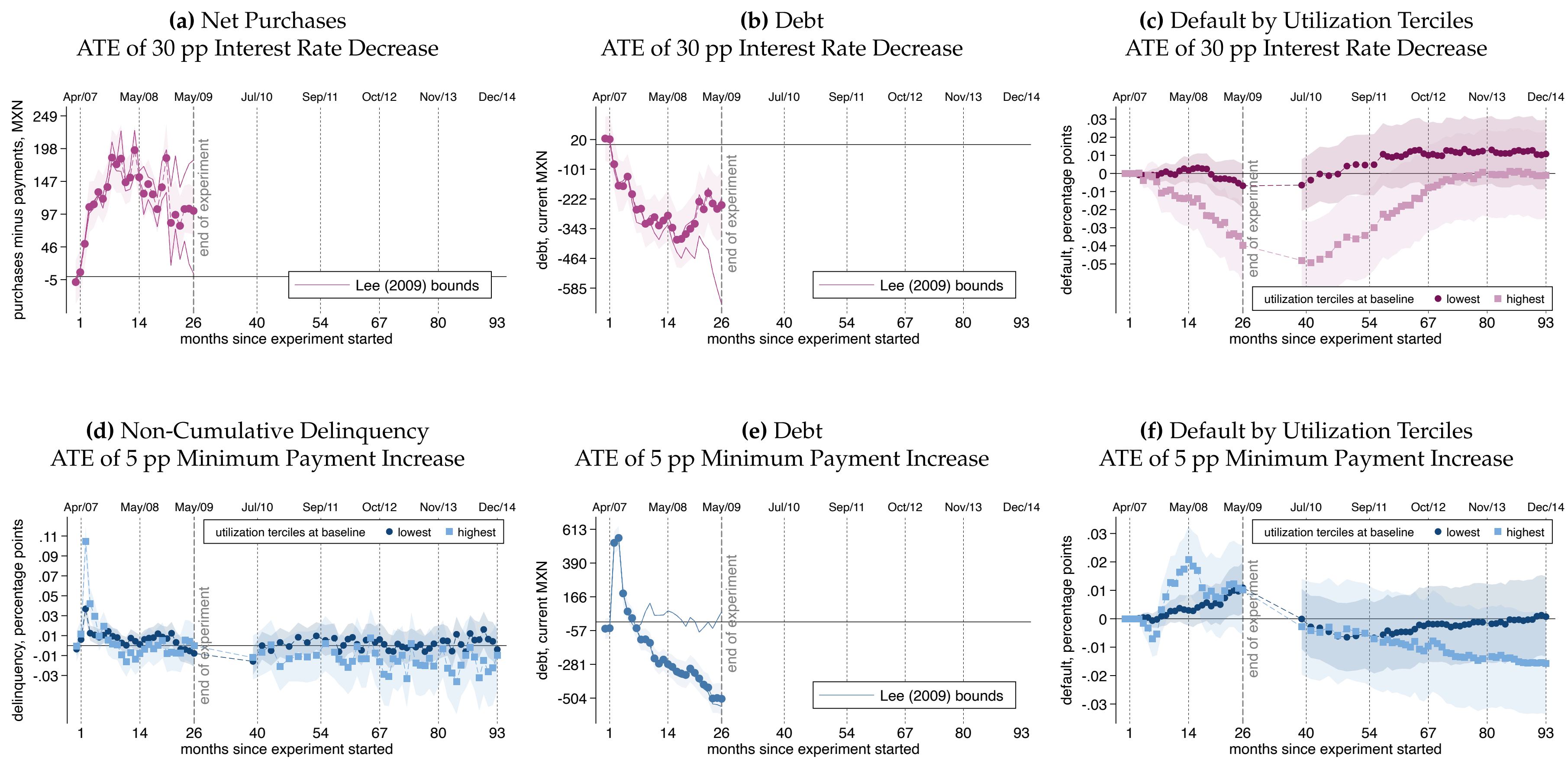
The long-run decrease in default from raising minimum payments during the experiment is consistent with regulators' safety-and-soundness concerns cited in the introduction (see fn.2). For instance, the Office of the Comptroller of the Currency in the United States has been concerned that minimum payments are too low, thus leading to negative amortization, debt accumulation, and elevated default risk.[36] Result 3 provides some evidence that higher minimum payments can decrease default over the longer run possibly by reducing debt (which may be a normative goal for paternalistic policy makers and/or in the presence of behavioral consumers, e.g., see fn.5).

[34]See Appendix I.2 for a discussion of the effect of increased minimum payments on debt (in particular during the first year). We show that the short-term rise in debt does not arise from reductions in net payments.

[35]One plausible alternative to this debt-driven explanation is that borrowers in the higher minimum payment arm changed their payment behavior permanently in response to the experimental intervention e.g. through habit formation. Table OA-9 measures the effect of having been subjected to the 10% MP in the past on post-experimental payment behavior and finds no effect, which we interpret as evidence against such habit formation.

[36]The OCC (Williams, 2005) wrote (see here) that "Finally, over the past several years, examiners observed declining minimum payment requirements for credit card accounts. During the same period, credit lines, account balances, and fees all have increased. As a result, borrowers who make only minimum payments have been unable to meaningfully reduce their credit card balances. From a safety-and-soundness standpoint, reductions in minimum payment requirements can enable borrowers to finance debts beyond their real ability to repay, thus increasing credit risk to the bank."

Figure 7: Mechanisms: Contract Term Effects on Intermediate Variables



Notes: These figures plot the contract term treatment effects on selected dependent variables during the study period. The first three panels (a)-(c) examine the ATE of a 30 pp interest rate decrease, and the bottom three (d)-(f) examine the ATE of a 5 pp increase in the minimum payment. The dependent variable for panel (a) is "net purchases," defined as purchases minus payments. The dependent variable for panels (b) and(e) is debt in the study card. We only have data on debt during the experiment. The dependent variable in panels (c) and (f) is default computed separately for the lowest and highest utilization terciles. The dependent variable for panel (d) is non-cumulative delinquency, defined as payments below the required minimum to stay current. For monetary variables (net purchases and debt), we impute a value of zero to cancellers after the cancellation month and compute Lee (2009) bounds tightened with the treatment-strata variables. For non-imputed variables and additional panels, see Figure OA-21 for net purchases, Figure OA-18 for debt, and Figure OA-9 for non-cumulative delinquencies.

6.3 Benchmarking Contract Terms Effects via Cash Flow Changes

In this section we place both the interest rate and the minimum payment interventions on an equal footing by normalizing their respective default ATEs by each intervention's effect on borrowers' "free" cash flow, thus obtaining an estimate of default on a *per-peso* basis.[37] We measure the changes in the free cash flow using the required minimum payment due to avoid delinquency (mpd). We focus on mpd for two reasons. First, it is a comprehensive measure of the monthly payment required to stay current on the card—including interest charges, fees, and borrower responses to the intervention. Second, both interventions affect mpd thus facilitating comparisons. *Ceteris paribus*, declines in the interest rate affect mpd by decreasing the monthly interest payments. Changes in the minimum payment affect mpd in two ways. In the short run, higher minimum payments mechanically increase mpd. In the longer run (again *ceteris paribus*) the increased minimum payments reduce total debt, thereby reducing the mpd. Of course, each intervention could also change borrower behavior (i.e., purchases and payments), which in turn could affect debt or fees, so that mpd reflects these changes as well.

We estimate the total reduced form effect of contract term changes on mpd using the same specification as Equation (1) and estimate:

$$mpd_{it} = \rho_t + \mu_t \cdot \mathbb{1}\{MP_i = 10\%\} + \kappa_t \cdot (45\% - r_i)/30\% + \nu_{it}. \tag{2}$$

We discuss estimation in Appendix J.1.1 and plot the results in Figure OA-22. Cumulatively over 18 months, the required minimum payment is 2,917 MXN pesos lower for the 15% interest arm relative to the 45% arm (i.e., $\sum_{t=1}^{18} \hat{\kappa}_t = -2917$). Turning to minimum payment interventions, we estimate that the minimum payment due is 1,325 MXN pesos higher in the 10% minimum payment arm relative to the 5% minimum payment arm (i.e., $\sum_{t=1}^{18} \hat{\mu}_t = 1325$). We use an 18-month horizon since this is the horizon for estimating the formal job loss effects to which we will compare these effects in the sequel (see Table OA-19 for the point estimates).

Normalized Default: We define the per-peso effect of each intervention on default at month t as $\lambda_t^{IR} \equiv \gamma_t / \sum_{j=1}^{t} \kappa_j$ and $\lambda_t^{MP} \equiv \beta_t / \sum_{j=1}^{t} \mu_j$.[38] We scale each estimate so that λ_t can be interpreted in terms of percentage points per 1,000 MXN pesos of additional required payments. We find $\hat{\lambda}_{18}^{IR} = 0.36$ and $\hat{\lambda}_{18}^{MP} = 0.51$. A 1,000 MXN peso increase in the minimum payments due from an increase in the minimum payment (interest rate) is associated with a 0.51 pp (0.36 pp) increase in default over 18 months. The point estimates suggest that minimum payments have a stronger effect on default per-peso increment to debt servicing, consistent with minimum payments lowering the denominator for λ via reduced debt in the longer run. However, the two estimates are statistically indistinguishable (Table OA-22), so based on this analysis, we conclude that both interventions had similar *per-peso* effects on default.

[37] We do not interpret this ratio as an instrumental variable estimate since we do not impose the exclusion restrictions or other assumptions on the evolution of treatment effects required for such an interpretation to hold.

[38] (γ_t, β_t) are defined in Equation (1). Recall that default is defined cumulatively when estimating Equation (1), so the numerator in each term captures the cumulative effect of the respective intervention on default while the denominator captures its cumulative effect on the minimum payment due.

7 The Effect of Job Loss on Default

Despite the value of formal credit and the high cost of default documented in Section 3, default among new borrowers is substantial. Section 6 documents that even significant contract term changes have limited effects on default. In this section, we provide evidence 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. We focus on one particular shock—job separation in the formal sector—which we observe using our matched borrower-employee data. We can think of job loss in the framework of Section 5 by viewing it as generating a first-order stochastically dominated period-two income distribution. Prediction 4 would then imply that job loss increases default.

Formal job loss as a "negative life event." Job loss is an appealing candidate to partly explain default levels for several reasons. First, job loss is common in our experimental sample: of those employed for at least one month in the formal sector between January 2004 and March 2007 (45% of the experimental sample), 43% experienced at least one month out of formal sector employment. Second, it has the potential to explain higher default for newer borrowers, as they are more likely to experience unemployment: those in the 6–11 month stratum are 1.34 times more likely (54% vs. 40%) to experience formal sector unemployment than those in the 24+ month stratum. Third, a large literature—in 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, 2010a; Flaaen et al., 2019; Jacobson et al., 1993), increases the likelihood of bankruptcy (Keys, 2018; Sullivan et al., 1999), mortality (Sullivan and von Wachter, 2009), and worsens mental health (Schaller and Stevens, 2015). Fourth, in Figure 8(a) and Figure 8(b), we find that by the end of the experiment, default is 8 pp lower for borrowers with a strong pre-experimental attachment to the formal labor force (26% of our sample) than for those with lower attachment.

7.1 Quantifying the Default Effect of Formal Job Loss

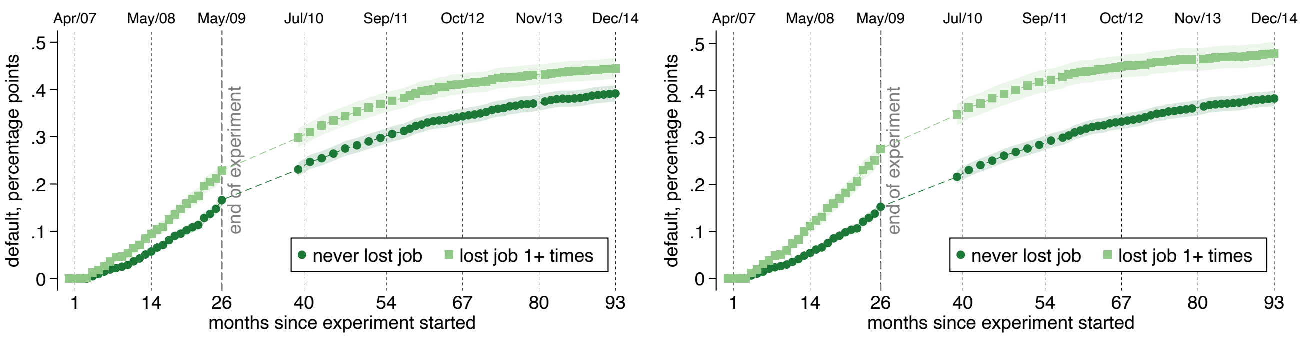
The magnitude of the effect of job loss on default is important yet understudied. On the one hand, limited unemployment insurance in Mexico suggests that formal unemployment shocks could strongly increase default. On the other hand, informal employment and informal insurance are common in Mexico (see Section 3 for the former and, e.g., Morduch, 2004; Ohnsorge and Yu, 2022, for the role of informal insurance as a buffer from economic disruptions) and could mitigate the effects of formal sector job loss on default. In addition, default may be further mitigated as access to credit may be particularly valuable during unemployment spells. Thus, even if the sign of the effect of job loss on default may not be controversial, its magnitude (and magnitude relative to, e.g., contract term effects) remains largely an open empirical question.

Given the difficulty of explicit randomization, work on the effects of job loss has focused on quasi-experimental methods. Jacobson et al. (1993) use mass layoff events, defined as significant net contractions in firm employment, to deal with the endogeneity of job loss. This approach has become increasingly common and has been used, inter alia, by Couch and Placzek (2010a); Flaaen et al. (2019); Sullivan

Figure 8: Default in Experiment Credit Card by Job Status
(Comparison of Default Levels 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).

and von Wachter (2009). The key idea is that job loss during mass layoff events—referred to as *displacement*—is more likely to be an involuntary separation and, thus, potentially orthogonal to displaced worker characteristics.

This approach compares the outcomes of displaced workers to those of undisplaced workers. The identification assumption is that, conditional on a set of time and worker indicators, the exact timing of the mass layoff is uncorrelated with the workers' potential default outcomes. This assumption would be violated if (conditional on time and worker indicators) unobservables driving study card default were correlated with mass layoff events. We consider the exogeneity assumption plausible in our context for three reasons. First, mass layoffs occur in every period in our data, making it unlikely that they exactly coincide with particular credit market shocks. Second, the inclusion of time indicators absorbs common trends. Third, the default pre-trends for displaced and non-displaced workers are statistically indistinguishable.

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. These definitions (for firm size and layoffs) are standard in the literature (see, e.g., Davis and Von Wachter, 2011; Flaaen et al., 2019) and yield 872 mass layoff events for the experimental sample during the experiment. 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 pesos (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 months). Figure OA-

10 shows event study graphs for total employees and the wage bill using the estimation approach in de Chaisemartin and D'Haultfoeuille (2024), which confirm the substantial effects of mass layoffs on firm outcomes.

Event studies: We examine the effect of job displacement using an event-study design. Let τ_i denote 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:

$$\text{default}_{it} = \alpha_i + \gamma_t + \sum_{k \neq 0} \beta_k \times \mathbb{1}\{t - \tau_i = k\} + \varepsilon_{it}, \tag{3}$$

where α_i and γ_t are individual and month fixed effects. With this specification, we can compare borrower behavior before and after a displacement event (i.e., job separation as part of a mass layoff). We also include dummies for leads and lags to provide suggestive evidence for parallel trends. In addition to the standard two-way fixed effects model, we use the staggered difference-in-differences methodology developed by de Chaisemartin and D'Haultfoeuille (2024), which remains valid even with heterogeneous and dynamic displacement effects on default. We estimate a "*fully dynamic*" model (in the language of Borusyak et al., 2024), including all months since mass layoff coefficients (except $k = 0$) for displaced individuals. Following current best practice, we include the never-treated units (i.e., those borrowers who were formally employed at baseline but were never part of a mass layoff) in the regression. Finally, we only present the coefficients for the periods over which the sample remains "unchanged," following the recommendations of Borusyak et al. (2024).[39]

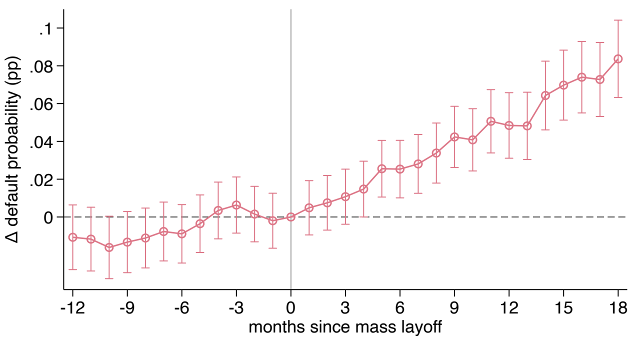
Result 4: One year after separation, borrowers are 4.8 pp more likely to default on the study card, and this figure increases to 7.6 pp after eighteen months. Figure 9(a) and Figure 9(c) show the effect of job displacement on default for our experimental sample during the experiment. The dependent variable is cumulative default on the study 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.

Figure 9(b) and Figure 9(d) repeat our estimation exercise using the intersection of the representative CB sample with the universe of formal employment from the IMSS. The larger sample is representative of the population of borrowers in the credit bureau who were formally employed during October 2011 and March 2014. This analysis expands our sample considerably and yields substantially more mass layoff events (8,723). The results are quite similar to those above, thereby providing a measure of external validity.

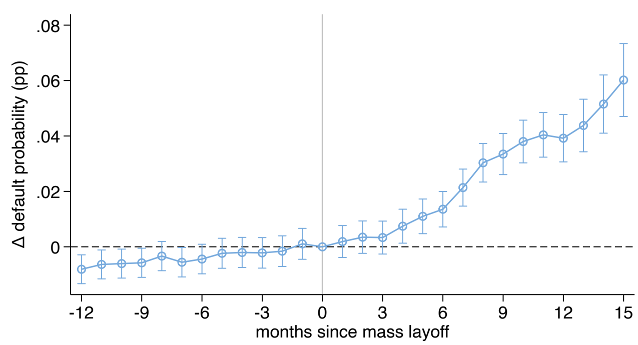
[39]There are two papers that examine similar outcomes (both in the U.S.). Keys (2018) uses U.S. household survey data to examine the effects of the receipt of unemployment insurance on bankruptcy filing in a standard TWFE framework. Our approach uses administrative data to define both default as well as mass layoffs and displacement; we focus only on the effect of unemployment during a mass layoff (rather than unemployment in general) to isolate exogenous variation. In addition, our specification includes individual fixed effects, and we implement improved difference-in-difference estimators. In terms of results, our estimates reveal a more stable pre-trend and are more precisely estimated. Gerardi et al. (2018) examines related outcomes though their main focus is 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 residual income.

Figure 9: Job Displacement and Default

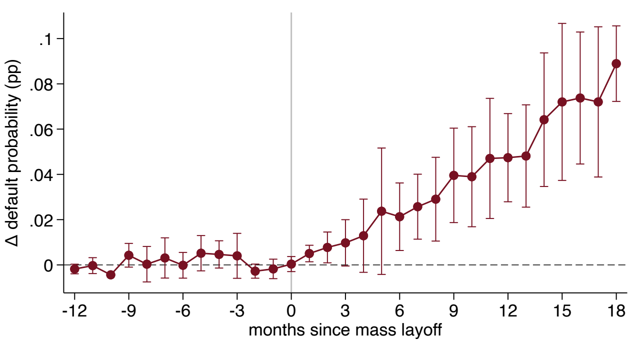
(a) Experimental Sample & Default in Experiment Card
(Two-Way Fixed Effects)



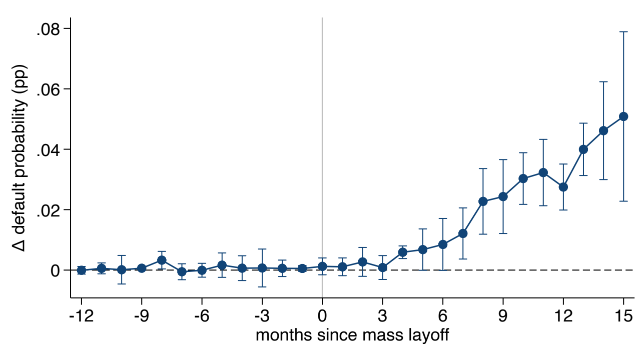
(b) Full Credit Bureau & Default in Any Loan
(Two-Way Fixed Effects)



(c) Experimental Sample & Default in Experiment Card
(de Chaisemartin and D'Haultfoeuille, 2024)



(d) Full Credit Bureau & Default in Any Loan
(de Chaisemartin and D'Haultfoeuille, 2024)



Notes: These figures plot the effect of being displaced from the formal labor market on default. Panels (a) and (c) plot the effect for displaced workers in the experimental sample, with the dependent variable as default on the study card. Panels (b) and (d) use the intersection of our CB sample with the IMSS database (i.e., it includes all formal sector workers with at least one bank loan in the credit bureau) and plot the effect on default for any loan in the credit bureau. The x-axis measures time since displacement (i.e., the downsizing event). The light-colored hollow circles in all panels represent 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 (2024). For months after displacement, the l-th coefficient compares displaced individuals with those not-yet displaced, from the displacement month until month l. For months before displacement, the l-th coefficient compares displaced individuals with those not yet displaced, l months before displacement.

7.2 Comparing the Default Effects of Displacement and Contract Terms

The estimated 18-month default effect of 7.6 pp is seven times larger than the effect of a 30 pp increase in interest rates (1.03 pp at the 18-month horizon) and nine times larger than the effect of doubling minimum payments over the same horizon (0.8 pp). Documenting the relative magnitudes of these effects over a common sample and timeframe is valuable both as a standalone exercise and for informing policy priorities.

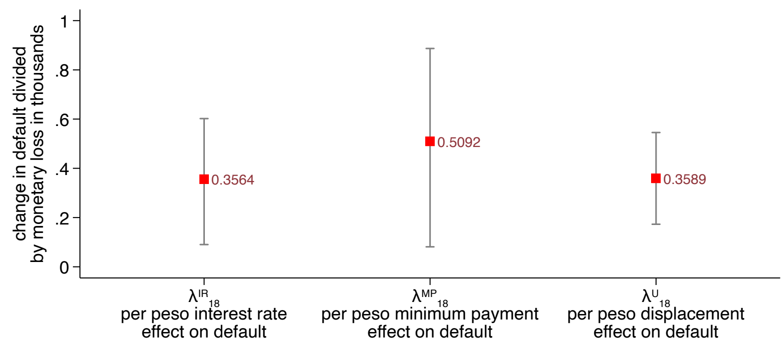
However, it is natural to seek the proximate sources for the larger size of the displacement effects. One possibility is that job loss has a greater impact on "free" cash flow than contract term changes; after all, job loss likely involves significant income losses. Another possibility is that job loss has larger effects on default even on a "per-peso" of cash flow affected basis. This could arise from the additional negative consequences of job loss beyond its effect on cash flow (e.g., from the mental and physical health consequences of losing a job). In this section, we perform an illustrative back-of-the-envelope calculation to compare the default effects of formal job loss against those of contract term changes, by normalizing the default treatment effects by their corresponding effects on cash flow. The calculations are described in Appendix J and we summarize the results here.

We proceed in two steps. First, we compute the loss in *formal sector earnings* arising from a displacement shock using the same econometric design as in Equation (3) but with formal sector monthly earnings (from the IMSS) as the dependent variable. We find that the change in formal sector earnings over the 18 months following formal job loss is 77,555 MXN pesos (Figure OA-23). Second, we adjust this amount by accounting for income replaced via informal employment (which is not recorded in the IMSS). Ignoring this could seriously overestimate income losses given the fluid transitions between formal and informal jobs (see Section 3). Using the Mexican labor force survey (the ENOE), we find that 82% of all workers who lose formal employment in a given quarter are informally employed in the following quarter. In Appendix J.1.2, we estimate that that fall in total labor earnings (after a formal unemployment event) is only 27.5% of the fall in formal earnings. We therefore calculate the total loss in income due to formal sector job loss as 21,328 MXN pesos ($\approx 0.275 \times$ 77,555). Encouragingly, accounting for informal earnings also brings our magnitudes closer to previous work documenting earnings losses due to job displacement in other settings (see Table OA-23). Finally, we normalize the default effect of job loss by dividing it by the estimated income loss:

$$\lambda_{18}^{U} \equiv \frac{\text{18-month default from formal job loss}}{\text{(18m formal income loss ('000 pesos))} \times \text{(\% formal income not replaced)}}.$$

We find $\hat{\lambda}_{18}^{U} = 7.6/21.328 \approx 0.36$. Thus, a 1,000 MXN peso decline in "free" cash flow due to formal job displacement is associated with a 0.36 pp increase in default. We plot all three $\lambda_{18}^{\cdot}$ estimates along with their bootstrapped confidence intervals in Figure 10. The three estimates are quite similar, and we cannot reject the null hypothesis that the per-peso effects of all three "shocks" are the same (p = 0.78). In the appendix, we examine the robustness of our results to alternative estimates of informal sector earnings (Appendix J.3), discounting (Appendix J.4), and attrition (Appendix J.5), and find remarkably similar results.

Figure 10: Effect Sizes After Normalizing Default by Cash Flow Shock



Notes: This figure compares the per-peso effect on default from three shocks. Standard errors were obtained via the bootstrap. To compare the per-peso unemployment effect estimate with those of contract terms we estimate the joint covariance matrix for $(\lambda^U_{18}, \lambda^{MP}_{18}, \lambda^{IR}_{18})$ using the bootstrap with 1,000 repetitions using stratified sampling at the strata $\times$ treatment level. We then use this to compute the Wald statistic.

7.3 Discussion

We offer three conclusions from our empirical analysis. First, despite the potential value of the study card during unemployment spells, job displacement has a substantial and persistent effect on study card default—even in a context with widespread informal employment and informal insurance. Second, the high frequency of unemployment overall, and particularly among newer borrowers, suggests that quantifying the role of "negative life-events" (to use Ganong and Noel's terminology) in credit market outcomes is an important area for research in developing countries. A rough calculation suggests that in our context, formal sector job loss alone can explain roughly 14% of total study card default during the experiment.[40]

Third, our calculations suggest that, on a per-peso cash flow basis, formal job loss has the same effect on default as the contract term interventions. This implies that the smaller effects of the (substantial) changes in contract rate terms can be rationalized by their relatively small effects on total cash flow. Equivalently, the larger effect of unemployment shocks can be explained by their larger effects on cash flow. The fact that both effects were estimated on the same sample and over the same time period is reassuring since it eliminates some obvious problems with such comparisons.

A common thread linking our findings, consistent with our framework, is that cash flow shocks and their impacts on debt are important determinants of default. A speculative narrative combining the stylized facts about the Mexican credit card sector and our estimates could be the following: new borrowers are credit-constrained with limited access to cheaper formal credit. This makes the study card valuable, and borrowers have an incentive to avoid default when cash flow permits (interest-rate-driven moral hazard appears to be less important in this context). Given initial debt burdens, even large

[40] 19.8% of study card holders employed in the formal sector lose formal employment at least once in the first 18 months of the study period. If each unemployment spell increases default probabilities by 7.6 pp, then the proportion who default because of formal job loss would be $1.5\% \approx 0.198 \times 0.076$. Unconditionally, 10.8% of study card borrowers default by the 18th month of the experiment. Thus, formal job loss explains $14\% \approx 1.5/10.8$ of borrower default.

contract term changes have relatively modest effects on free cash flow and, consequently, modest effects on default. However, the frequency and consequences of job loss on cash flow are harder to mitigate, resulting in default.

8 Conclusion

Borrowers increasingly use credit cards to first access formal credit in many developing countries. This has received considerable attention from policymakers concerned about high default rates among new borrowers. Such concerns have led to contract term regulations despite limited evidence on their role in limiting default. We examine a large-scale effort by a commercial Mexican bank to expand credit by issuing credit cards to financially inexperienced new borrowers. We combine detailed card-level data for a product that accounted for 15% of all first-time formal loans with individual employment histories and a large nationwide randomized experiment.

Default rates are high (19%) and substantially higher (36%) for newer borrowers. We document a default elasticity of 0.2 for the interest rate intervention and zero for newer borrowers, suggesting a limited role for interest-rate-induced strategic default. Doubling the minimum payment leads to a short-term increase in default, likely driven by tightened liquidity constraints, followed by a subsequent decline after the end of the experiment consistent with reduced debt. These findings suggest that varying contract terms by policy-relevant magnitudes may have small contemporaneous effects on default, although they may affect longer-term outcomes by changing debt.

A natural question, then, is what might drive default for new borrowers. Matching the experimental sample to their formal employment histories, we document that job loss is common and more common among newer borrowers. The effect of plausibly exogenous job loss on default is several-fold larger than the contract term effects, and formal sector job loss alone can explain about 14% of total default on the study card during the experiment. Using a simple back-of-the-envelope calculation, we find that both the contract term changes and job loss have similar per-peso effects on default. The smaller effects of contract terms (despite the substantial size of the contract term changes themselves) can thus be rationalized by their smaller effects on cash flow relative to that of unemployment.

An implication of our results is that improving our understanding of new borrower default may benefit from examining borrowers' economic environment more broadly beyond the tight focus on interest-rate-driven moral hazard. Our results highlight the economic vulnerability of new borrowers in developing countries where financial inclusion occurs against a backdrop of precarious employment with limited social protection. While we focus on job loss, illness and other negative shocks could also be important (see, e.g., Karlan et al., 2019, for discussion of the role of negative shocks and persistent indebtedness among the poor). Given the prevalence of such shocks, examining whether some form of insurance or social protection could improve credit market outcomes in developing countries (as documented in the United States by, e.g., Aaronson et al., 2012; Bornstein and Indarte, 2023; Hsu et al., 2018) is a crucial research and policy priority.

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Contract Terms, Employment Shocks, and Default in Credit Cards

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

Appendix – For Online Publication

Contents

A Additional Tables and Figures

A.1 Additional Tables

Table OA-1: Sampling weights

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

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-2: 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.

Table OA-3: 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-4: 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-5: Credit Limits and Treatment Arms

Months since experiment started:	0 Mar/07 (1)	4 Jul/07 (2)	7 Oct/07 (3)	11 Feb/08 (4)	15 Jun/08 (5)	19 Oct/08 (6)	22 Jan/09 (7)	26 May/09 (8)
$(45\% - r_i)/30\%$	20	78	204	188	102	123	194	187
	(87)	(129)	(157)	(165)	(154)	(161)	(166)	(180)
$\mathbb{1}\{MP_i = 10\%\}$	-48	-77	-144	-101	-198	-224	-225	-174
	(64)	(96)	(117)	(123)	(114)	(119)	(123)	(134)
Constant	7,901***	10,333***	12,058***	12,207***	11,703***	11,356***	11,264***	11,790***
	(65)	(96)	(116)	(122)	(115)	(120)	(122)	(134)
H_0 = no ATEs	0.725	0.583	0.184	0.352	0.164	0.115	0.086	0.225
Observations	143,626	138,564	132,234	123,443	114,779	107,155	99,986	87,093

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-6: 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 and are clustered at the borrower level. One, two and three stars denote statistical significance at the 5, 1 and 0.1 percent level respectively.

Table OA-7: 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 (0.002)	+2.2 (0.2)
d'Astous and Shore (2017)	Default	p. 3	0.04	+0.06
Keys and Wang (2019)	Delinquency	2 (p. 542)	0.4 (0.3)	+0.01 (0.12)
Karlan and Zinman (2019)	Delinquency	5 (p. 42)	-1.96 (1.45)	+1.80
DeFusco et al. (2021)	Charge-Off	(p. 2)	0.1	+0.01

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 low interest rates was on average 350 basis points. We use the high-risk category upper bound for the interest rate of 11.75% as the base rate and convert the monthly interest rates to APR to facilitate comparisons (the calculation is $(-1.6/13.9)(279/-120) = 0.27$). For Karlan and Zinman (2019), we use the results from Table 5 (col (4), Panel B), which show delinquencies decline by 1.96 pp 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 0.4 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-8: ATEs by Months With Card Strata
(Cumulative Default by May 2009 - Experiment Endline)

	(1) b/se/p	(2) b/se/p	(3) b/se/p	(4) 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.

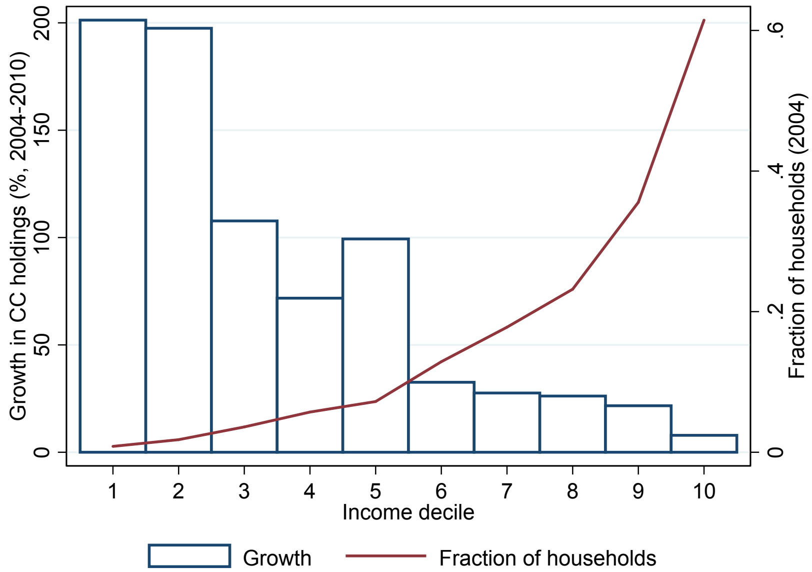
Table OA-9: Habit Formation Payments

	No controls		Months with CC strata		Months + Current Terms	
	First stage (1)	Second stage (2)	First stage (3)	Second stage (4)	First stage (5)	Second stage (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 includes those cards that (i) participated in the experiment, (ii) remained open by 2010, and (iii) were assigned to either the highest or lowest interest rate groups (e.g., $(r = 15, MP = 5)$, $(r = 15, MP = 10)$, $(r = 45, MP = 5)$, and $(r = 45, MP = 10)$). Each column represents a different regression. Columns (2), (4), and (6) use as the dependent variable the amount paid ("Payments") in June 2010, as a function of the minimum payment 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 a higher minimum payment in the past on payment amount in the future, when the minimum payment is no longer high, conditional on current debt. Since debt can be endogenous, we instrument for debt using the interest rate group to which cardholders were assigned. Not instrumenting for debt leads to similar conclusions regarding the effect of $MP = 10$. We also allow for a differential treatment effect for those in the "minimum-payment" strata. The dependent variable in Columns (1), (3), and (5) is the amount due in 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) include 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 levels, respectively.

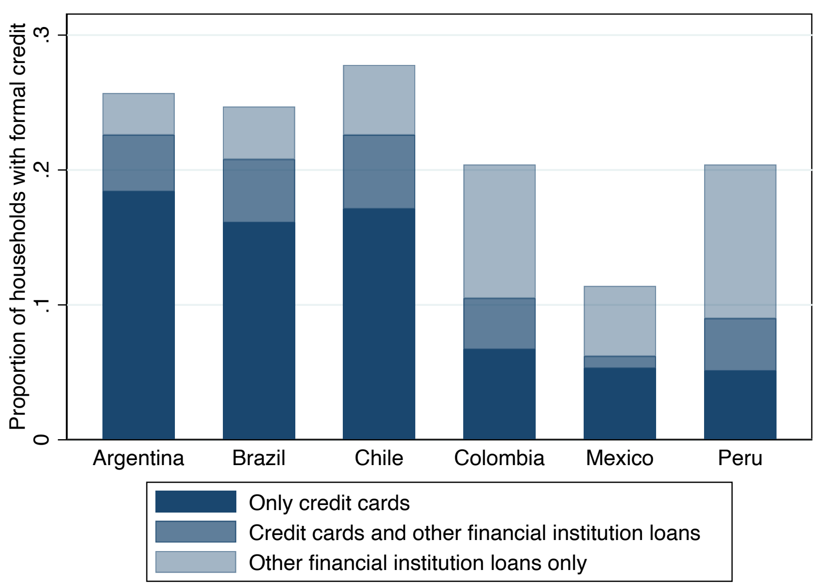
A.2 Additional Figures

Figure OA-1: Overall Credit Card Growth and Share of Households with Credit Cards



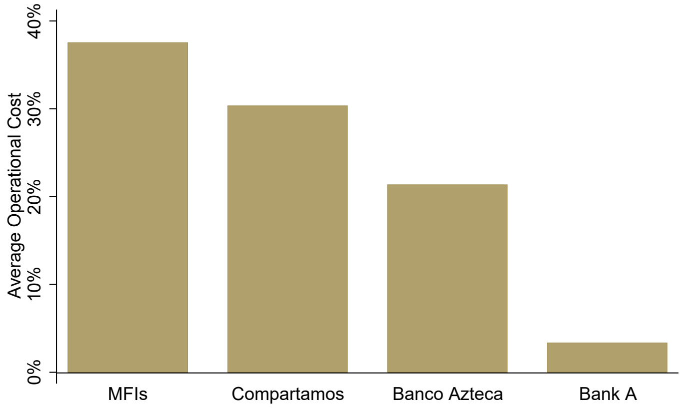
Notes: This figure is constructed using data from the 2004 National Income Expenditure Survey (ENIGH). The x-axis represents (household) income deciles (the 10th decile is the richest decile). The left y-axis—corresponding to the hollow bars—shows the percentage growth in the number of households that 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.

Figure OA-2: Credit card and other borrowing across Latin American countries (2017)



Notes: The source for this figure 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.

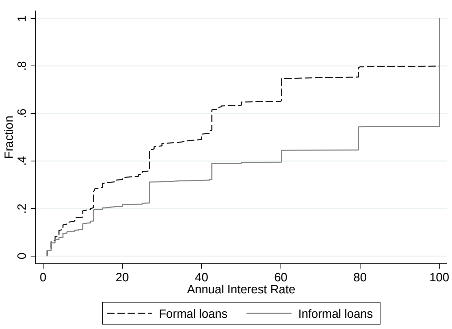
Figure OA-3: Operational Costs (relative to Assets): Compartamos, Azteca, and Bank A



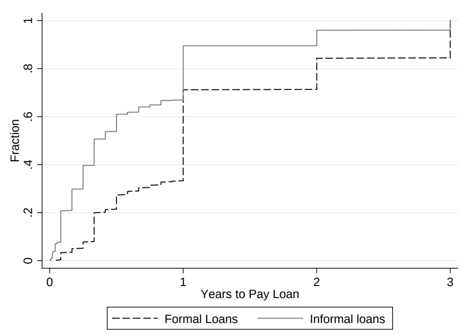
Notes: The cost ratio is defined as the ratio of administrative and promotion spending to total assets. Data is taken from the Mexican Banking Comission (CNBV) at `https://portafoliodeinformacion.cnbv.gob.mx/bm1/Paginas/infosituacion.aspx` (under 040-5Z-R6, indicadores financieros). We average annual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

Figure OA-4: Comparison formal and informal loan market in Mexico

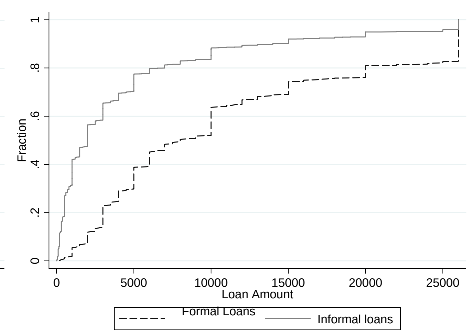
(a) Interest rate



(b) Loan duration



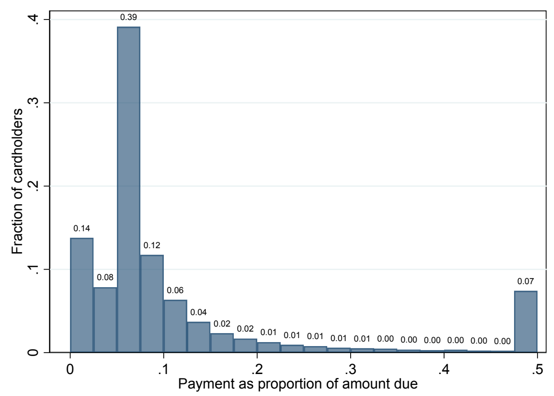
(c) Loan amount



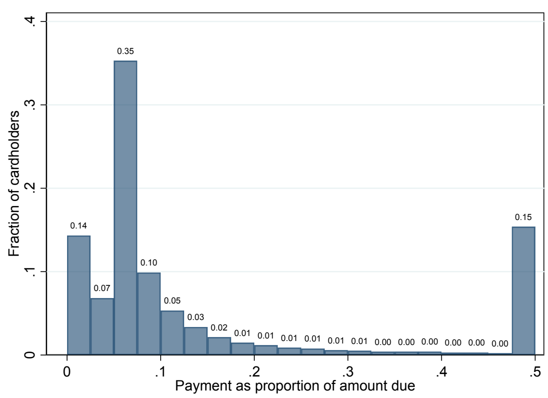
Notes: The above figures compare the formal and informal credit market in Mexico using the annual interest rate (a), the loan tenure in years (b) and the loan amount in pesos (c). This data comes from ENNVIH survey reported by the INEGI on years 2002, 2005, and 2009. The lines represent the cumulative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

Figure OA-5: Payment as a Fraction of Debt Before and During the Experiment

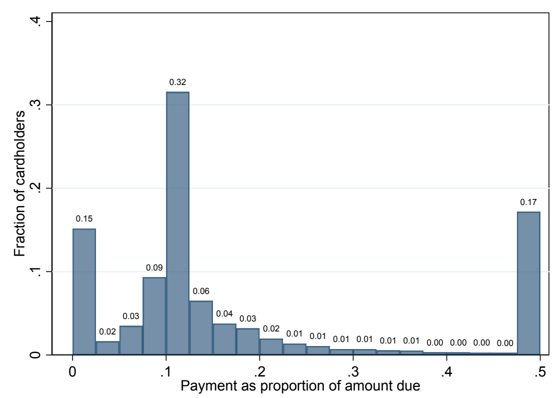
(a) March 2007
(start of experiment)
all treatment arms



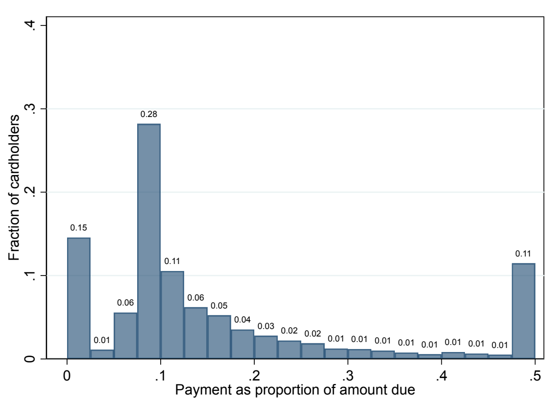
(b) October 2007
(6 months into the experiment)
treatment arms with mp = 5%



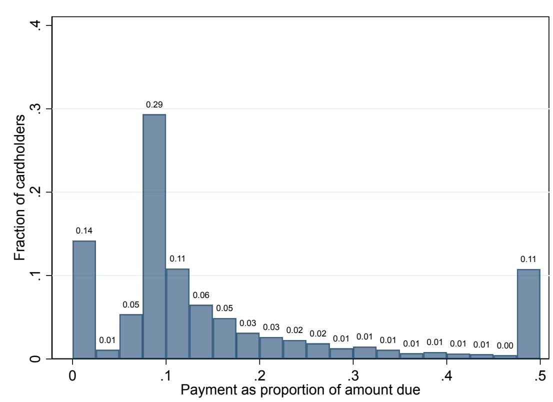
(c) October 2007
(6 months into the experiment)
treatment arms with mp = 10%



(d) June 2012
(3 years after experiment)
treatment arms with mp = 5%



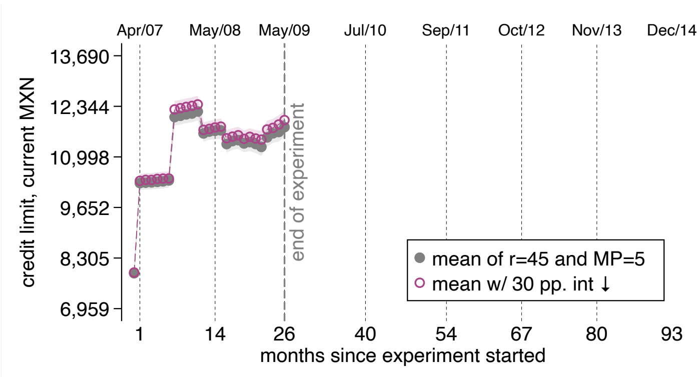
(e) June 2012
(3 years after experiment)
treatment arms with mp = 10%



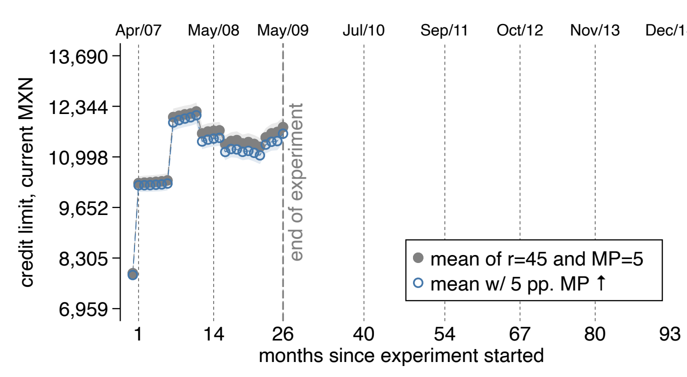
Notes: We plot monthly payment divided by the amount due. In Figure (a) this is the ratio of monthly payments in April 2007 and the amount due in the March 2007 statement. We follow a similar approach (current month payments divided by previous month end balance) in the other panels. We right-censor 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-6: Credit Limits: Levels and "ATEs"

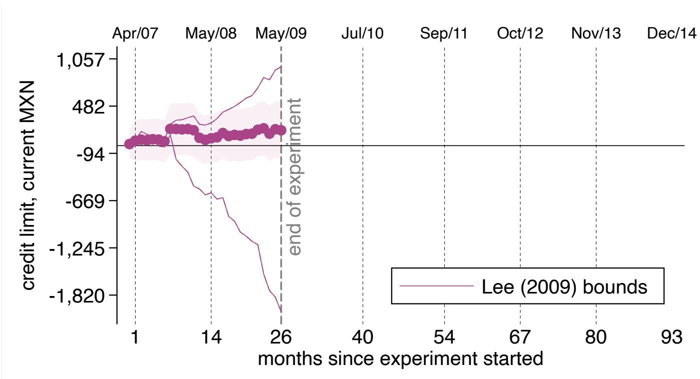
(a) Comparison of Means
(Different Interest Rates)



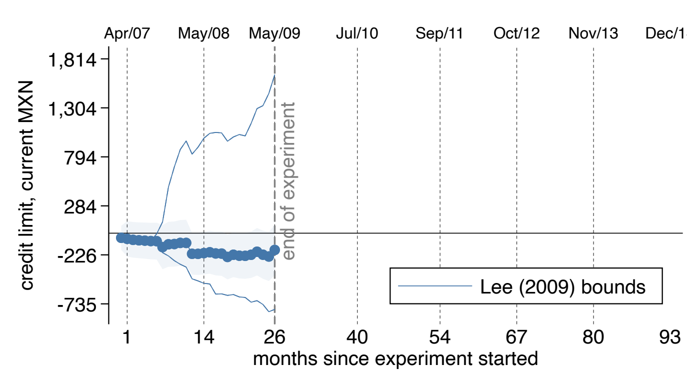
(b) Comparison of Means
(Different Minimum Payments)



(c) "ATE" of 30 pp. Interest Rate Decrease



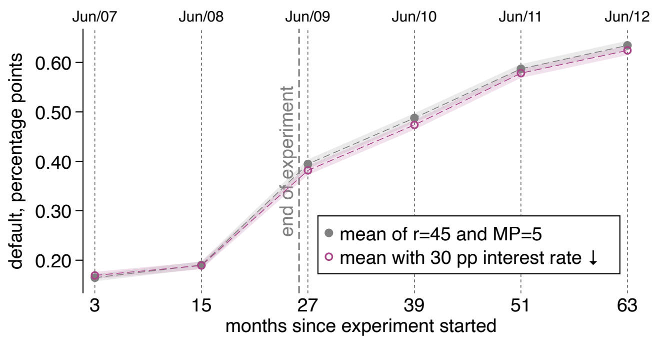
(d) "ATE" of 5 pp. Minimum Payment Increase



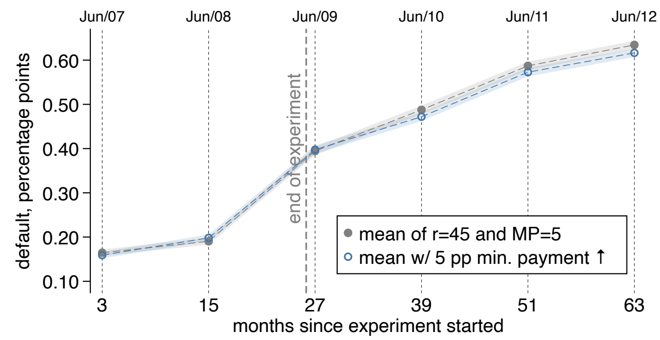
Notes: These figures plot the relationship between interest rates, minimum payments, and credit limits. The figure shows that credit limits were orthogonal to randomization—Table OA-5 formally tests this hypothesis. The dependent variable is the credit limit (conditional on the card being active). Figures on the left examine interest rate changes, and 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 the 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 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 are tightened by strata and treatment arms whenever possible. We refer to these as "average treatment effects" for consistency with the rest of the paper, despite credit limits not reflecting borrower behavior.

Figure OA-7: Spillovers: Default on Other Loans by Bank A and Other Banks

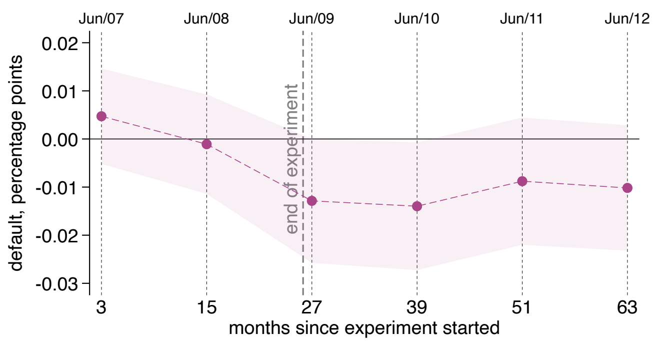
(a) Default Levels by Interest Rates
(Loans with Bank A)



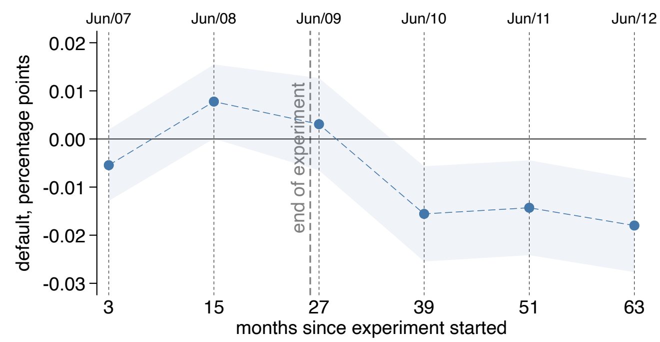
(b) Default Levels by Min. Payments
(Loans with Bank A)



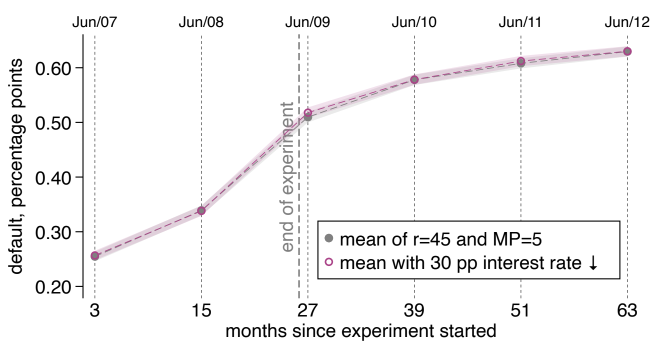
(c) Default in Loans with Bank A
ATE 30 pp. Interest Rate Decrease



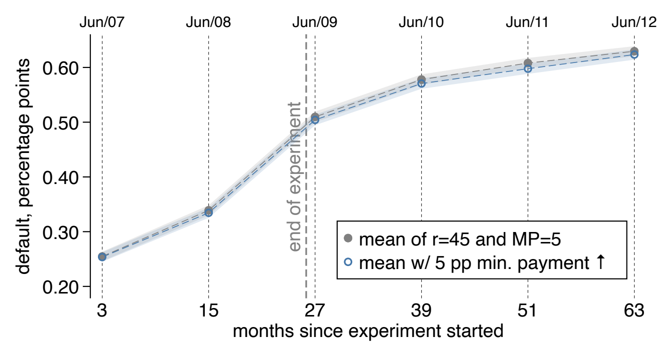
(d) Default in Loans with Bank A
ATE of 5 pp. Minimum Payment Increase



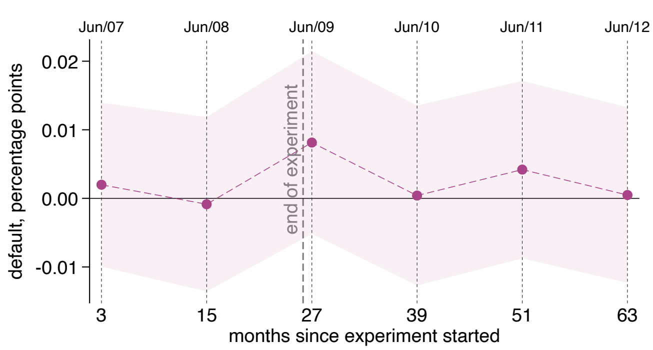
(e) Default Levels by Interest Rates
(Loans with Other Banks)



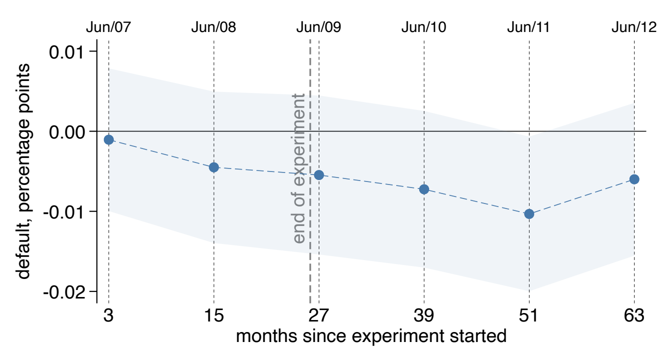
(f) Default Levels by Min. Payments
(Loans with Other Banks)



(g) Default in Loans with Other Banks
ATE 30 pp. Interest Rate Decrease



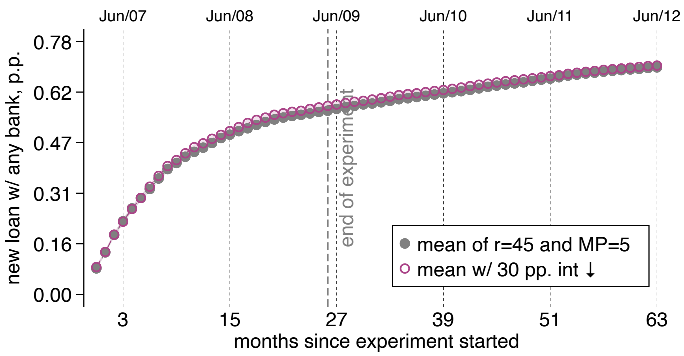
(h) Default in Loans with Other Banks
ATE of 5 pp. Minimum Payment Increase



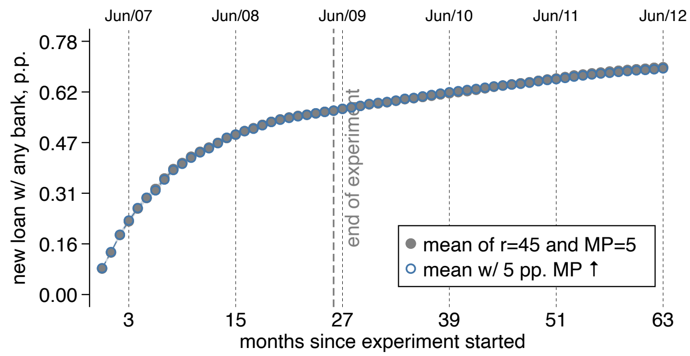
Notes: These figures plot the causal effect of interest rates and minimum payment changes on default in other loans for both Bank A (Panels a to d) and other banks (Panels e to h). The first two panels (a, b, e, f) show comparisons of means, while the second two panels (c, d, g, h) display the average treatment effects (ATE) of interest rate decreases and minimum payment increases.

Figure OA-8: Spillovers: New Loans Issuance

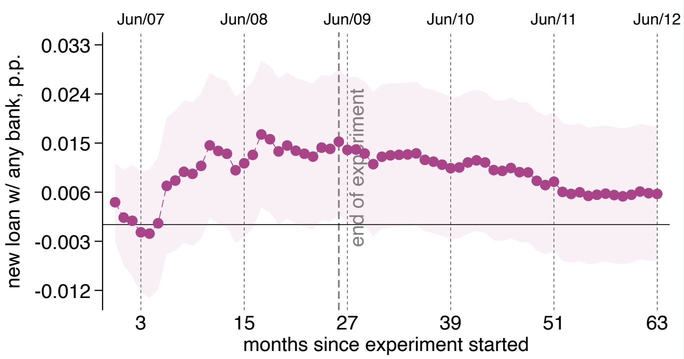
(a) New Bank Loans vs Interest Rates (levels)



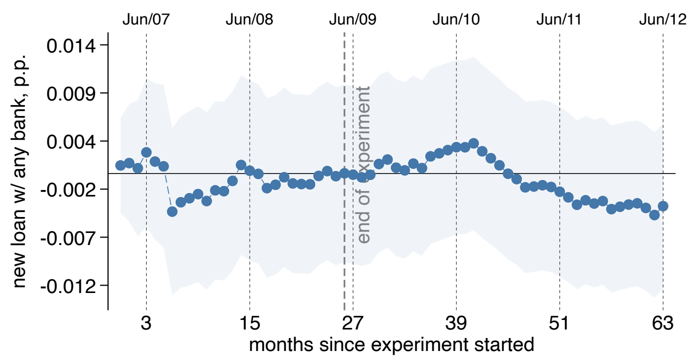
(b) New Bank Loans vs Min Payment (levels)



(c) New bank loans ATE (30 pp Int. Decrease)



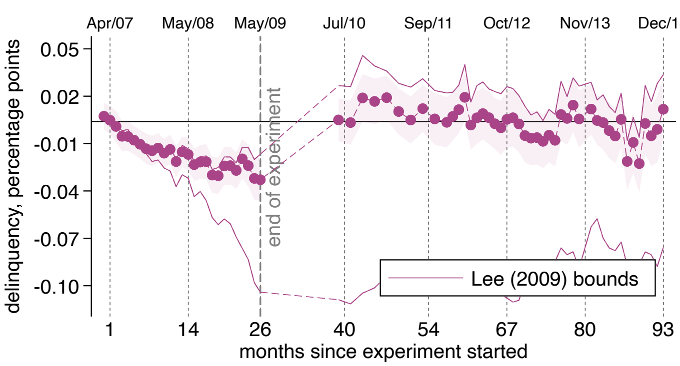
(d) New bank loans ATE (5 pp Min. Pay. Increase)



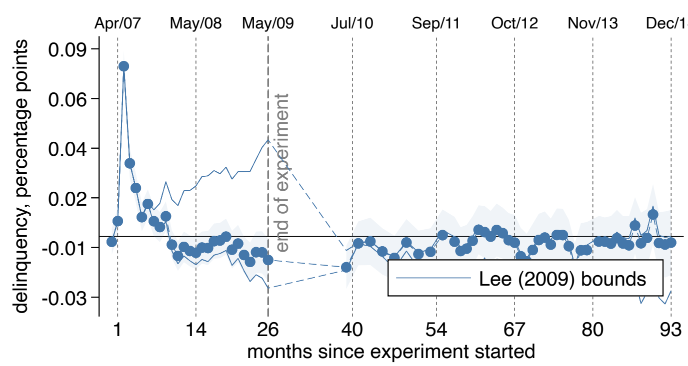
Notes: These figures plot the causal effect of interest rates and minimum payment changes on new bank loan issuance. The dependent variable is a cumulative categorical variable on new loans from March 2007 to the given date. The data source for the dependent variables is the credit bureau. The figures on the left examine interest rate changes. The figures on the right examine minimum payment changes. The panels on top explore levels while the panels on the bottom focus on treatment effects.

Figure OA-9: Non-Cumulative Delinquency ATEs
(Delinquency = Payment Below Required Minimum)

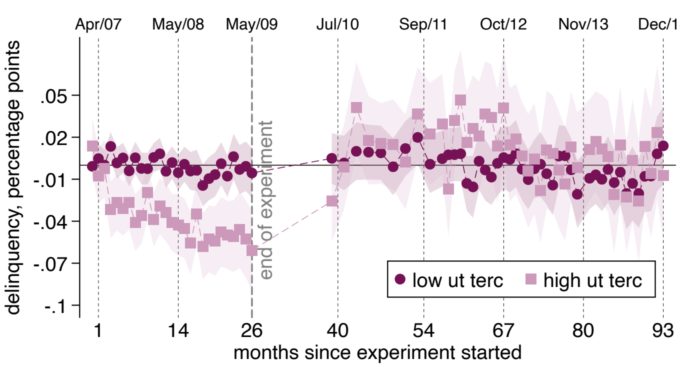
(a) Delinquency ATE of 30 pp. Interest Rate Decrease



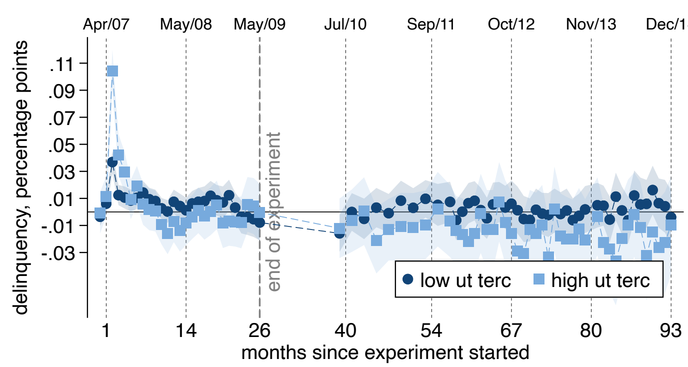
(b) Delinquency ATE of 5 pp. Minimum Payment Increase



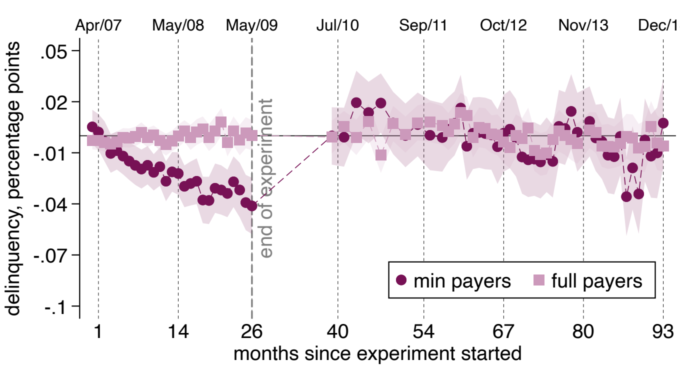
(c) Delinquency ATE of 30 pp. Interest Rate Decrease
(By Baseline Utilization)



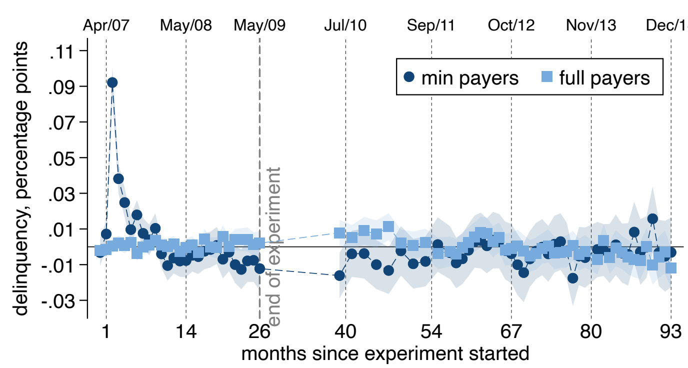
(d) Delinquency ATE of 5 pp. Minimum Payment Increase
(By Baseline Utilization)



(e) Delinquency ATE of 30 pp. Interest Rate Decrease
(By Payment Strata)



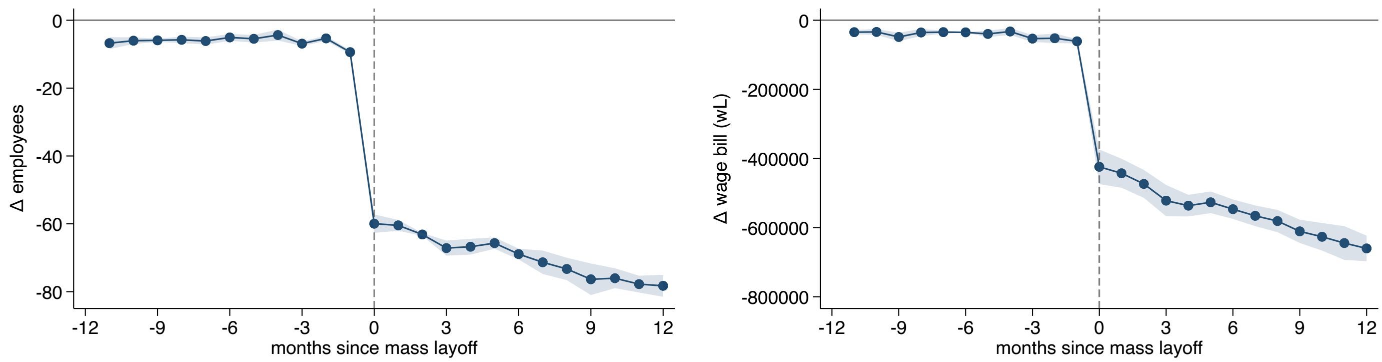
(f) Delinquency ATE of 5 pp. Minimum Payment Increase
(By Payment Strata)



Notes: These figures plot the causal effect of interest rates and minimum payment changes on the non-cumulative delinquency in the experiment credit card for selected subpopulations. Delinquency is defined as a categorical variable indicating if the actual payment is smaller than the required minimum payment. Delinquency is not defined for closed cards (e.g., because of voluntary cancellations or bank-initiated revocations). Figures on the left examine interest rate changes. Figures on the right examine minimum payment changes. These figures plot average treatment effects of interest rates and minimum payments using Equation (1). Panel (a) and (b) show the main treatment effects. Panel (c) and (d) separate borrowers by baseline credit utilization. Credit utilization is defined as "low" or "high" if the ratio of the month-end balance to credit limit at baseline is in the lowest and highest tercile, respectively. Panel (e) and (f) use the baseline payment behavior strata and plot the results for the "minimum payer" and the "full payer" strata while pooling across the months with credit card strata.

Figure OA-10: Effect of Mass Layoffs on Number of Employees and Wage Bill
(IMSS Employment Data Matched to the Credit Bureau)

(a) Effect on Employee Headcount (Firm Level) **(b)** Effect on Wage Bill (wL) (Firm Level)



Notes: This figure plots the effect of mass downsizing events on the number of employees in a given firm, and the total wage bill in pesos. The sample is the IMSS employment data matched to the credit bureau representative sample. An observation in Panels (a) and (b) is a firm-month. We use the methodology developed by de Chaisemartin and D'Haultfoeuille (2024) for these event studies.

B Data

B.1 Cards as First Formal Loan: Sources

In Mexico, cards are the first loan type for 74% of all formal sector borrowers. The corresponding figures for Peru, Colombia, and the U.S. are 83%, 51%, and 50%, respectively. The figures for Mexico are from the authors' calculations. The figures for Colombia are from Banca de las Oportunidades (2016). The figure for Peru was obtained through Universidad del Pacifico and kindly provided by Mirko Daga. The figure for the U.S. comes from Haughwout et al. (2020). There does not appear to be an internationally comparable database that can be used to examine this globally. We provide numbers from all the countries for which we obtained data.

B.2 Data Timeline Explanation

The experiment was designed and executed by Bank A without our input. We became aware of the experiment years after it had concluded, and asked Bank A for the data on the experiment. We were told that the bank had only maintained detailed records for the cards for the experimental period (March 2007 through May 2009) to analyze the experiment. This data is measured monthly with no gaps for the duration and contains all of the variables that the bank kept to analyze the experiment.

After working on the paper for a substantial amount of time we requested Bank A for additional data in 2015. We were told that the bank did not store historical data on-site but instead contracted with a service provider for off-site storage. Obtaining this data was costly (the bank had to submit orders to the provider). As a compromise, for this older period we were able to request a limited set of variables and at a lower frequency (every two months). For years closer to 2015 (when we asked for the data) we were less constrained since the bank had some of the data on-site. In the data transfer process, we lost the data from November 2013 since the data slice we received from Bank A was corrupted.

B.3 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 71 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}. This leaves us with a sample of 397,310 borrowers from the original 415,793. Then, for each $i = 1, \ldots, 50$ we define $w_i = 1/N_i$ where N_i is the number of individuals in the credit bureau sample from Column 3 whose loan tenure in June 2010 falls in $[r_{i-1}, r_i)$. We then use w_i as analytic weights to construct the estimates that we present in Column 4.

B.4 Debt Decomposition

In this section we document that the following identity holds in our data:

$$\text{amount due}_{i,t} = \text{amount due}_{i,t-1} + \text{purchases}_{i,t} - \text{payments}_{i,t} + \text{fees}_{i,t} + \text{debt}_{i,t} \times \text{interest rate}_i.^{41} \quad (4)$$

To test such an equation in our data we use observations with positive debt (as the coefficient on the interaction between debt and interest rate is not identified in the case when debt is zero). The following Table OA-10 summarizes our results. We find that inferred interest rates match closely with experimental interest rates. This suggests that the debt transition equation (4) above is a good approximation to reality and that the data on purchases, debt, payments, and fees is consistent. The R^2=1 means that the formula is virtually an identity in the data.

Table OA-10: Data check: Estimating Debt Dynamic Equation

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

Notes: This table estimates Equation (4) by OLS on months with positive debt. Specifically, 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 on payments equal to -1, for instance, is a result and not an imposed constraint. The same applies to interest rates: the coefficient on $I(r = 25\%)$, i.e., $\gamma_{25} = 0.27$, being close to 0.25 is also a result. One, two, and three stars denote statistical significance at the 5, 1, and 0.1 percent levels, respectively.

[41] Note that if the amount owed was equal to the debt, the identity above could be written more compactly as $C_t = RC_{t-1} + NP_t$+Fees where C_t is debt in period t and NP_t is net purchases in period t. However, the two differ because if payments do not cover purchases in period t then the difference is treated as a loan for the appropriate fraction of the month and interest is accrued on that fraction.

C Default Reduces Access to Formal Credit

First, we estimate the effect of default on subsequent formal sector credit in an instrumental variable setting using experimental changes in contract terms as instruments for default. Second, we estimate the effect of default on subsequent formal sector credit using a selection on observables assumption. Both sets of results suggest default has a strong negative effect on subsequent credit. The dependent variable in both settings is an indicator for whether the borrower obtained any formal credit over a relevant time-horizon.

We focus on the newest borrowers stratum and define our main explanatory variable as an indicator for whether the borrower defaulted within the first 10 months of the experiment. We examine the effect of default during this period on whether the borrower obtained any formal credit during the subsequent k months for $k \in \{3, 9, 12, 18, 24, 48, 60\}$. We instrument default with the experimentally assigned minimum payment and interest rates. We use the first 10 months as this is the date in which our cumulative default measure has the largest difference between the low and high minimum payment groups. The exclusion restriction is that treatment assignment affects subsequent formal credit only through its effect on default on the study card. The results in Table OA-11 show that the probability of having obtained new formal credit up to one year after experimentally induced study card default is 65 pp. lower relative to the non-default counterfactual ($p = .03$).[42] The point estimates of the difference in new credit takeup between defaulters and non-defaulters stay somewhat constant around -50 pp., but the standard errors get wider.

Although the IV regressions have a credible research design, the limited contract term effects on default suggest a somewhat limited instrument strength (the Cragg-Donald first-stage statistic is 31). We therefore explore more descriptive results (implicitly relying on a selection on observables assumption).

To this end, we rely on ordinary least squares estimates. Just like before, the primary explanatory variable is an indicator equal to one if a borrower defaulted on the study card in the 10 months after the experiment started (Mar/07 to Jan/08). The dependent variable is an indicator for whether the borrower obtained a new formal loan of any kind six, twelve, or forty-eight months after February 2008. Default (see Panel A of Table OA-12) on the study card is associated with a 20 pp. decrease in the likelihood of obtaining any new formal sector loans in the next 6 months. This is a large magnitude, given that the mean for non-defaulters is 26 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.[43] These results suggest that, if borrowers want subsequent credit, they are unlikely to be able to get credit cards and, for the small percentage who do get credit, do so using collateralized credit options.

[42] This interpretation assumes constant treatment effects. An accurate characterization of the Local Average Treatment Effect with multiple instruments would require additional assumptions (see e.g., Mogstad et al., 2019) and we do not pursue that here.

[43] 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-13 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.

Table OA-11: Obtaining loans after experimentally-induced default

	New loan from Feb/08 until k months after						
	$k=3$	$k=9$	$k=12$	$k=18$	$k=24$	$k=48$	$k=60$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
default between Mar/07 and Jan/08	-0.448	-0.668*	-0.651*	-0.512	-0.425	-0.520	-0.466
	(0.249)	(0.301)	(0.306)	(0.308)	(0.310)	(0.320)	(0.320)
	[0.072]	[0.027]	[0.033]	[0.096]	[0.171]	[0.105]	[0.146]
Cragg-Donald Wald F-statistic	31.61	31.61	31.61	31.61	31.61	31.61	31.61
Observations	47,954	47,954	47,954	47,954	47,954	47,954	47,954

Notes: This table provides evidence that default decreases in access to subsequent credit. The sample is composed of all borrowers in the newest card strata (6-11M) in the experiment arms (47,594 borrowers). The independent variable is our cumulative default measure from the paper, equal to one if a borrower defaults from Mar/07 (the beginning of the experiment) until Jan/08. We instrument default using the experimentally induced variation in interest rates and minimum payments. The first stage is Equation (1) so the instruments are a $MP = 10\%$ categorical variable and the linear specification for interest rates, $(45/\% - r_i)/30\%$. The independent variable is a categorical variable equal to one if a borrower gets a bank loan (with any provider) from Feb/08 until k months after, with different values of k in each column. We use probability weights to make population statements. Robust standard errors are shown in round parentheses. P-values are shown in squared parentheses. One, two and three stars denote statistical significance at the 5, 1 and 0.1 percent level respectively.

Table OA-12: Probability of getting a new loan or card against default

	New loan from Feb/08 until k months after								
	$k=6$	$k=12$	$k=48$	$k=6$	$k=12$	$k=48$	$k=6$	$k=12$	$k=48$
	Any bank			Any bank except Bank A			Bank A		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. Any loan									
default between Mar/07 and Jan/08	-0.196***	-0.252***	-0.346***	-0.158***	-0.205***	-0.290***	-0.067***	-0.094***	-0.160***
	(0.006)	(0.007)	(0.009)	(0.006)	(0.007)	(0.009)	(0.002)	(0.003)	(0.004)
constant	0.258***	0.333***	0.499***	0.214***	0.281***	0.435***	0.072***	0.101***	0.174***
(non-defaulters dep. var mean)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)
Panel B. Credit cards only									
default between Mar/07 and Jan/08	-0.164***	-0.212***	-0.330***	-0.131***	-0.171***	-0.266***	-0.054***	-0.074***	-0.135***
	(0.004)	(0.005)	(0.006)	(0.004)	(0.004)	(0.005)	(0.002)	(0.003)	(0.003)
constant	0.184***	0.238***	0.372***	0.147***	0.191***	0.303***	0.058***	0.080***	0.146***
(non-defaulters dep. var mean)	(0.003)	(0.003)	(0.003)	(0.002)	(0.003)	(0.003)	(0.002)	(0.002)	(0.002)
Observations	47,954	47,954	47,954	47,954	47,954	47,954	47,954	47,954	47,954

Notes: This table regresses measures of subsequent new card ownership against the previous default on the study card. The sample is composed of all borrowers in the newest card strata (6-11M) in the experiment arms (47,594 borrowers). The observations are at the cardholder 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 10 months after the experiment started (March 2007 to January 2008). 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 February 2008 and August 2008, February 2009, and February 2012 (6, 12, and 48 months). Columns (4), (5) and (6) repeat the exercise but restrict 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-13: Obtaining loans after the first delinquency

	any new loan with any bank (1)	any new loan with other banks (2)	any new loan with bank A (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.

D Are New Borrowers Liquidity Constrained?

The ratio of debt outstanding to a borrower's credit limit is a commonly used measure of liquidity constraints in contexts where such limits or lines of credits are available. In our context, normalizing debt by the credit limit is also helpful as it provides a crude method to account for income variation across borrowers (since the credit limit, at least implicitly, is a function of the bank's perception of borrower income and credit-worthiness and we do not observe income for our entire sample, only for formal workers). Finally we can view this ratio as a proxy for liquidity using the lens of the model as well. In the model $y_1 - C_0$ can be interpreted as a measure of initial liquidity and so consequently higher values of C_0/y_1 indicate tighter liquidity constraints.

Recent and limited participation in the formal credit sector raises the possibility that new clients continue to be liquidity constrained. Evidence of continuing constraints will provide some context for understanding the experimental treatment effects and their heterogeneity. We test for the existence of liquidity 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.[44] Conversely, one can view debt (or more generally consumption) responses to changes in credit limits as evidence of credit constraints.[45] 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 (although as we note below, informal terms are likely much worse than formal terms).

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 lags of one month changes in credit limits.[46] Let Debt_{it} be the amount of debt held by card i at the end of month t, let Limit_{it} denote the credit limit for account i at the beginning of month t and X_{it} denotes a set of controls. Following the main specification in Gross and Souleles (2002) we estimate

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

where Δ is the first-difference operator and β_j represents the incremental increase in debt between month $t-1$ and t associated with a one peso change in credit limit in period $t-j$. The scalar parameter $\theta \equiv \sum_{j=0}^{T} \beta_j$ then provides us with a summary measure of the long-run (T month) total effect of credit limit on debt; we report $\hat{\theta} \equiv \sum_{j=0}^{T} \hat{\beta}_j$ for each regression.[47] Because the bank evaluates a card for credit-limit changes using pre-determined durations, cards that had received a credit limit change further 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.[48]

The results are presented in Table OA-14. In all tables, we adopt the convention of three asterisks denoting

[44] Assuming no wealth effects of the increased limits.
[45] See e.g., Deaton (1991), Carroll (1992), Gross and Souleles (2002).
[46] 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.
[47] Standard errors were computed using the delta method.
[48] See Gross and Souleles (2002) for the same approach.

significance at the .1% level, two asterisks at the 1% significance level and one asterisk at the 5% significance level. Panel A uses debt 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.[49] For Panel B, since we only have annual data, we modify equation (5) and regress one year changes in debt on one year changes in credit limits (i.e., $T = 2$). Column (1) presents results for the entire experimental sample while the subsequent columns estimate the model on the 9 different strata.

First, focusing on the entire sample, we find that after 12 months a credit limit increase of 100 pesos for the study card translates into 32 pesos of additional debt (Row 1). This number remains essentially unchanged when we add controls (not reported) while the IV estimate is substantially larger (73 pesos). This propensity to consume out of increases in the credit limit is about thrice as large as the figure for the US and suggests that these Mexican borrowers are liquidity constrained and significantly more so than their US counterparts.[50]

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 at least three times as (usually much more) 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 liquidity constraints.

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

Variation Across Strata A direct test of whether the strata vary systematically in terms of credit constraints is to estimate Equation (5) separately for each stratum and compare the magnitudes of the estimates of θ across strata. The results are presented in Table OA-14 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).[51] 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

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

[50]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 (2022) who induces experimental variation in credit card limits (in an unnamed European country) and estimates a response of 0.20 (with $T = 9$).

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

for baseline repayment history, increased tenure with the bank is correlated with lower debt responses to credit limit changes.

Table OA-14: Propensity to Use Credit Line Increases (Credit Constraints Proxy)

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

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

E Model

As noted in the text, the model derives comparative statics for three endogenous variables: (a) the binary default decision in the first period (b) Credit card debt at the end of the first period (denoted by C_1) and (c) a binary default decision in the second period. The optimal values of these endogenous variables are examined as a function of the following exogenous variables: (a) the initial debt with which agents start period 1, denoted by C_0, (b) the one period gross interest rate $R = 1 + r$, (c) the required minimum payments in each period $(m_1, m_2) \in (0, 1)^2$[52]; (d) the one period discount factor ($\delta \in (0, 1)$) (e) the continuation value of card ownership ($v > 0$) (f) first period income (y_1) and (g) the distribution for period two income $y_2 \in \{y_L, y_H\}$ with $q \equiv P(y_2 = y_L)$. To ease notation, we define θ as the entire vector of exogenous variables $\theta \equiv (C_0, R, m_1, m_2, \delta, v, y_1, y_L, y_H, q)$.

The agent begins period 1 with accumulated debt C_0 on which she must make a minimum payment of $m_1 RC_0$ in period 1. She observes her income y_1 and must choose whether to default or to continue to hold on to the credit card by making (atleast) the minimum payment $m_1 RC_0$ on her current debt. If she does not default, she makes net purchases P on the card (subject to an upper limit) and has a debt equal to

$$C_1 = P + (1 - m_1)RC_0 \tag{6}$$

at the end of period 1. In principle, P can be negative so we allow agents to pay more than the minimum payment to the bank in period 1. We will, however, require that $C_1 > 0$ so that agents always take on positive debt.[53] Appendix B.4 verifies this decomposition for our experimental data. Finally, $(\epsilon_{10}, \epsilon_{11})$ are independent GEV-1 random variables capturing borrower level heterogeneity related to each choice (i.e., default or no default).

In period 2, the agent realizes income y_2 and must decide whether to make the minimum payment on their card or to default. With probability q the agent draws a very low income ($y_2 = y_L$) and then defaults on the card and earns utility $u(y_L)$. With probability $(1 - q)$ the agent draws income y_H. The agent then chooses whether to default (and consume income y_H) or to make the minimum payment $m_2 RC_1$, consume $y_H - m_2 RC_1$ (which is assumed strictly positive for all values of (m_2, C_1, R)) and obtain continuation value v from holding on to the credit card for future periods (we do not model any additional debt choice in period 2 since we do not require it for our comparative statics). Each agent is also characterized by a vector of independent GEV-I random variables $(\epsilon_{20}, \epsilon_{21})$ capturing heterogeneity in preferences.

We can analyze the model using backward induction, starting with period 2.

E.1 Period 2 Problem

In period 2 the agent draws income y_L with probability q and y_H with probability $(1 - q)$. If the agent has defaulted in period 1 they make no further decisions and consume their income. If the agent has not defaulted in period 1 and draws income y_L they default and consume y_L. This is intended to capture the notion that low enough draws of income precipitate default. If, on the other hand, the agent has not defaulted in period 1 and draws income y_H then they can either choose (a) to make the minimum payment on the card $m_2 RC_1$ and derive utility $u(y_H - m_2 RC_1) + v + \epsilon_{20}$ or (b) choose to default and derive utility $u(y_H) + \epsilon_{21}$. The latter default captures the notion that agent heterogeneity may drive default independent of income.

[52] We find it useful to distinguish between minimum payments in the two periods but not so for the gross interest rate.

[53] We could instead assumed that $P > 0$ (and hence $C_1 > 0$) but our results do not require this stronger assumption.

Let D_2 be a binary variable equal to 1 if the agent defaults in period 2 and 0 otherwise. We next express the probability of default in period 2 as a function of the exogenous variables and the predetermined choice of debt C_1. In what follows, we suppress dependence on the full vector of exogenous variables unless it is relevant. First

$$\{D_2 = 1\} \iff \{y_2 = y_L\} \cup \{y_2 = y_H, v_{20} + \epsilon_{20} < v_{21} + \epsilon_{21}\}$$

where $v_{20} = u(y_H - m_2RC_1) + v$ and $v_{21} = u(y_H)$. Next, assuming that $(\epsilon_{20}, \epsilon_{21}) \perp\!\!\!\perp y_2$

$$\begin{aligned} \mathbb{P}(D_2 = 1|C_1) &= q + (1-q)\mathbb{P}(v_{20} + \epsilon_{20} < v_{21} + \epsilon_{21}) \\ &= q + (1-q)L(v_{21} - v_{20}) \end{aligned} \tag{7}$$

where

$$L(x) = \frac{\exp(x)}{1 + \exp(x)}$$

and

$$v_{21} - v_{20} = u(y_H) - u(y_H - m_2RC_1) - v$$

and we have integrated over the distribution of $(\epsilon_{21}, \epsilon_{20})$. Since the default probability in period 2 is a key object of interest, we define the more convenient function $P_2(C_1, \theta) \equiv \mathbb{P}(D_2 = 1|C_1; \theta)$ and we will often suppress θ in the notation below.

First, we observe that this probability is increasing in q,

$$\frac{\partial P_2}{\partial q} = 1 - L(v_{21} - v_{20}) > 0$$

for given (v_{21}, v_{20}). Thus, unsurprisingly, an increase in the likelihood of lower income draws increases the likelihood of default.

Next, we consider the effect of changes in (m_2, R) on period 2 default keeping debt C_1 fixed—this follows the logic of the model since the debt decision (i.e., the choice of C_1) is made in period 1 and we assume it cannot be revisited in period 2. Unsurprisingly, increases in interest rates and minimum payments unambiguously increase the second period default in this scenario since agents lack margins for adjustment

E.1.1 Increases in Interest Rates Increase Period 2 Default

We first consider how period 2 default changes when interest rates change. The thought experiment is that interest rates change *after* debt C_1 has been chosen. Some algebra yields

$$\frac{\partial P_2}{\partial R} = (1-q)L(v_{21} - v_{20})\,(1 - L(v_{21} - v_{20}))\,u'(y_H - m_2RC_1)m_2C_1 > 0 \tag{8}$$

where the strict inequality follows since we assume $C_1 > 0$ and all the other objects on the right-hand side are strictly positive. This result is also expected—holding debt fixed and increasing interest rates will increase default.

E.1.2 Increases in Minimum Payments Increase Period 2 Default

We next examine default when the period 2 minimum payment m_2 changes. As above, the thought experiment is that m_2 changes after C_1 has been chosen so the channel of default is only through the increase in m_2 reducing consumption. We will use this analysis below when we examine the post-intervention effect of minimum payment changes. Taking derivatives of the choice probability yields

$$\frac{\partial P_2}{\partial m_2} = (1-q)L(v_{21}-v_{20})\left(1-L(v_{21}-v_{20})\right)u'(y_H - m_2 RC_1)RC_1 > 0 \tag{9}$$

As above, increases in period 2 minimum payments lead to an increase in the likelihood of default.

E.1.3 Choosing Optimal Debt C_1

As a prelude to examining period 1 default we characterize the optimal debt decision. Debt responses require agents to have beliefs over period 2 contract terms. In what follows, we will sometimes assume that agents believe that a single minimum payment (and interest rate) will be applicable in both periods. This attempts to capture the study where-in agents were informed of the new contract terms that would apply going forward—i.e., it seems reasonable to assume, based on this, that agents expected the terms to last. A richer analysis might incorporate uncertainty in future contract terms but we do not pursue that here.

We now derive the optimal debt choice in period 1 for the agent. This choice is only relevant when the agent does not default in period 1. In this case, the agents expected payoff from choosing debt level C_1 will be given by

$$Q(C_1,\theta) \equiv u\left(y_1 + P - m_1 RC_0\right) + \delta\mathbb{E}\left(u(y_L)q + (1-q)\max\left\{v_{21}+\epsilon_{21}, v_{20}(C_1)+\epsilon_{20}\right\}\right)$$

where we emphasize the dependence of the period 2 value functions on C_1 and we have already integrated over period 2 income so that the expectation operator is now only with respect to ϵ_2. The GEV assumption enables an analytic form of the expected maximum above so that

$$Q(C_1,\theta) = u\left(y_1 + C_1 - RC_0\right) + \delta\left[u(y_L)q + (1-q)\ln\left(\exp(v_{20}) + \exp(v_{21})\right)\right] \tag{10}$$

where we have also used the fact that $C_1 = P + (1-m_1)RC_0$.

The optimal debt choice C_1^* (assuming an interior solution) is defined implicitly as the solution to the first-order condition:

$$\nabla_{C_1} Q(C_1^*) = u'(y_1 + C_1^* - RC_0) + \delta(1-q)\frac{\exp(v_{20}(C_1^*))}{(\exp(v_{20}(C_1^*)) + \exp(v_{21}))}\frac{\partial v_{20}(C_1^*)}{\partial C_1} = 0$$

where we use the ∇_x notation to denote the derivative of Q with respect to its argument x. The first order conditions simplify to

$$u'(y_1 + C_1^* - RC_0) = m_2 R\delta(1-q)L\left(v_{20}(C_1^*) - v_{21}\right)u'(y_H - m_2 RC_1^*) \tag{11}$$

In order to characterize the optimal debt C_1^* explicitly as a function of the exogenous variables we assume $u(\cdot) \equiv \ln(\cdot)$. This yields

$$\frac{1}{(y_1 + C_1 - RC_0)} = \frac{\delta(1-q)m_2 R\exp(v)}{(y_H - m_2 RC_1)\exp(v) + y_H} \tag{12}$$

and we can solve explicitly for first period debt

$$C_1^* \equiv \frac{(1+\exp(v))\,y_H}{\exp(v)m_2R\,(1+\delta(1-q))} - \frac{\delta(1-q)}{(1+\delta(1-q))}\,(y_1 - RC_0) \tag{13}$$

$$= (1-\alpha)\,\frac{y_H}{L(v)m_2R} + \alpha\,(RC_0 - y_1) \tag{14}$$

where $\alpha \equiv \frac{\delta(1-q)}{1+\delta(1-q)} \in (0,1)$ and $L(\cdot)$ is the logit function as before.

Optimal Debt Can be Increasing in the Interest Rate: The equilibrium relationship between period 1 debt and R reflects the fact that interest is also charged on the accumulated debt C_0. In particular,

$$\frac{\partial C_1^*}{\partial R} = \alpha C_0 + (1-\alpha)\left(\frac{-y_H}{L(v)m_2R^2}\right) \tag{15}$$

which is a convex combination of the two effects—e.g., a infinitesimal increase in R increases debt mechanically by αC_0 and reduces purchases (and thereby debt) by the last term in the expression above. Thus, the partial derivative in eq. (15) can be positive if initial debt C_0 is sufficiently high. This rationalizes the observed positive relationship between debt and interest rates that we document empirically in Appendix I.1.

Optimal Debt Response to Minimum Payment Changes: On the other hand, the effect of changes in the second period minimum payment are unambiguously negative:

$$\frac{\partial C_1^*}{\partial m_2} = -(1-\alpha)\frac{y_H}{L(v)m_2^2R} < 0 \tag{16}$$

The expression for C_1^* in eq. (13) also makes clear that changes in the first period minimum payment m_1 do not affect the optimal debt (this exercise keeps m_2 fixed). To see why note that $C_1^* = P^* + (1-m_1)RC_0$ so that any change in m_1 will be offset by corresponding change in P^*, leaving C_1^* unchanged.

Optimal Purchases Response to Minimum Payment Changes: Examining optimal purchases,

$$P^* = C_1^* - (1-m_1)RC_0 \tag{17}$$

which are increasing in the first period minimum payment m_1 (keeping m_2 fixed). If instead we assume that agents choose C_1^* under the belief that $m_1 = m_2(= m)$, then the purchase response is ambiguous since agents need to account for higher minimum payment in period 2:

$$\frac{\partial P^*}{\partial m} = -(1-\alpha)\frac{y_H}{L(v)m^2R} + RC_0. \tag{18}$$

So that increases in the required minimum payment can increases purchases (when initial debt is above a threshold level). In Figure OA-20(d) below we show that purchases do indeed respond positively to increases in minimum payments even after making adjustments for sample selection—the Lee Bounds for the elasticity are $[+0.18, +0.85]$ which are consistent with eq. (18) being strictly positive suggesting intuitively that the decline in purchases arising from the forward looking part of the optimization is smaller than the increase in purchases required to ensure the Euler equation continues to hold.

E.1.4 Experimental Minimum Payment Increases Can Decrease Post-Experiment Default

We can use the model to think through post-experiment default in the following sense: Suppose agents choose debt C_1 assuming $m_1 = m_2(\equiv m^e)$ where the super-script e captures the notion that these are agents expectations about what the minimum payment will be (and is the same for both periods). In particular, we assume that agents set m^e equal to the experimentally assigned minimum payment in period 1—that is agents expect the minimum payment in period 2 to be the same as i period 1 and make debt choices accordingly.

We then view period 2 as the post experiment period where-in minimum payments are changed ex-post to m_2. We then compute $\frac{\partial P_2(C_1^*(m^e);m_2)}{\partial m^e}$ where $C_1^*(m^e)$ captures the notion that agents expect the minimum payment in period 2 to be m^e and the argument m_2 captures the fact that minimum payment in period 2 is set exogenously to m_2 after $C_1^*(m^e)$ has been decided. While this exercise is inconsistent with perfect foresight (since period 1 beliefs about period 2 minimum payments are not required to be consistent) or rational expectations we think this is a reasonable approximation to the experimental set-up where all contract term changes were unannounced and it seems reasonable that borrowers expected the experimental terms to last. We start with

$$P_2(C_1^*(m^e); m_2) = q + (1-q)L\left[v_{21} - v_{20}\right]$$

where $v_{21} - v_{20} = u(y_H) - u\left(y_H - m_2 R C_1^*(m^e)\right) - v$. Then,

$$\frac{\partial P_2(C_1^*(m^e); m_2)}{\partial m^e} = (1-q)L(1-L)u'\left(y_H - m_2 R C_1^*\right) m_2 R \frac{\partial C_1^*}{\partial m^e}$$

where we have suppressed the argument $v_{21} - v_{20}$ in the $L(\cdot)$ function. With the assumption of log utility, the right hand side simplifies to (see eq. (16))

$$\frac{\partial P_2(C_1^*(m^e); m_2)}{\partial m^e} = -\frac{(1-q)L(1-L)(1-\alpha)y_H m_2}{\left(y_H - m_2 R C_1^*\right) L(v)\left(m^e\right)^2} < 0$$

Thus, agents with higher minimum payments during the experiment (i.e., higher values of m^e) have lower probabilities of default after the experiment (i.e., in period 2 when minimum payments are changed to m_2 but agents debt is already pre-determined).

E.1.5 Experimental Interest Rate Decreases Can Decrease Post-Experiment Default

As above, we can use the model to think through post-experiment default in response to experimental interest rate assignments. Assume agents choose C_1 expecting that the first-period interest rate will hold in the second period as well . We view period 2 as the post-experiment period and with interest rate R_2. We then consider the following counterfactual: holding R_2 fixed, how do changes in borrower beliefs about the common interest rate (denoted by R^e) affect period 2 default. That is to say we compute $\frac{\partial P_2(C_1^*(R^e);R_2)}{\partial R^e}$ where $C_1^*(R^e)$ is the debt choice assuming that R^e will be the common interest rate in both periods (suppressing dependence on the other exogenous variables). As with the argument above, this thought experiment is inconsistent with perfect foresight (or rational expectations) but is a reasonable approximation to the experiment given the bank's surprise announcements of changes to contract terms.

$$P_2(C_1^*(R^e); R_2) = q + (1-q)L\left[v_{21} - v_{20}\right]$$

where $v_{21} - v_{20} = u(y_H) - u\left(y_H - m_2 R_2 C_1^*(R^e)\right) - v$. Then,

$$\frac{\partial P_2(C_1^*(R^e); R_2)}{\partial R^e} = (1-q)L(1-L)u'\left(y_H - m_2 R_2 C_1^*\right) m_2 R_2 \frac{\partial C_1^*}{\partial R^e}.$$

which will be positive iff the last term is positive. Therefore, as long as $\frac{\partial C_1^*}{\partial R^e} > 0$ default will be lower in period 2 (i.e., post-experiment) for borrowers who were in the lower-interest rate arm during the experiment (i.e., those who had a lower R in period 1 and expected the same to hold in period 2) relative to those in the higher interest rate arm. Appendix E.1.3 outlines conditions under which this derivative is positive and Appendix I.1 demonstrates that empirically debt is increasing in the interest-rate (during the experiment).

E.2 Period 1 Problem

We next turn to the default probability in period 1—as mentioned previously, the key difference between this and the second period analysis is that we allow debt to respond to changes in contract terms while computing default probabilities.

In period 1, the agent will default (i.e., $D_1 = 1$) if $v_{11} + \epsilon_{11} > v_{10}\left(C_1^*\right) + \epsilon_{10}$ where

$$\begin{aligned} v_{11} &= u(y_1) + \delta\left(qu(y_L) + (1-q)u(y_H)\right) \\ v_{10} &= u\left(y_1 + C_1^* - RC_0\right) + \delta\left(u(y_L)q + (1-q)\ln\left(\exp(v_{20}(C_1^*) + \exp(v_{21})\right)\right) = Q(C_1^*(\theta); \theta) \end{aligned}$$

where $Q_1(C^*; \theta)$ is defined in eq. (10) and we emphasize the dependence on the vector of exogenous variables. As before, using the GEV distributional assumptions for $(\epsilon_{11}, \epsilon_{10})$ we obtain

$$P_{D_1}(\theta) \equiv \mathbb{P}(D_1 = 1|\theta) = L\left(v_{11} - Q(C_1^*)\right) \tag{19}$$

and we will occasionally refer to $L\left(v_{11} - Q(C_1^*)\right)$ as L_1 for brevity.

E.2.1 Increases in Interest Rates Increase Default iff $C_1^* + RC_0 > 0$

Consider first the effect of a change in the one period interest rate R. Taking derivatives with respect to R and applying the envelope theorem yields

$$\frac{\partial P_{D_1}}{\partial R} = -L_1(1-L_1)\left(\nabla_R Q(C_1^*, \theta) + \nabla_{C_1} Q(C_1^*)\frac{\partial C_1^*}{\partial R}\right) = -L_1(1-L_1)\nabla_R Q(C_1^*, \theta) \tag{20}$$

Next,

$$\begin{aligned} \nabla_R Q(C_1; \theta) &= -C_0 u'(y_1 + C_1 - RC_0) + \delta(1-q)\frac{\exp(v_{20})}{(\exp(v_{20}) + \exp(v_{21}))}\frac{\partial v_{20}(R)}{\partial R} \\ &= -C_0 u'(y_1 + C_1 - RC_0) + \delta(1-q)L\left(v_{20}(C_1) - v_{21}\right)\left(-m_2 C_1 u'(y_H - m_2 RC_1)\right) \end{aligned}$$

Next, using the first-order conditions for the optimal debt choice eq. (11) and substituting into the above expression,

$$\nabla_R Q(C_1^*;\theta) = -\left(m_2\delta(1-q)L\left(v_{20}(C_1^*) - v_{21}\right)u'(y_H - m_2RC_1^*)\right)(C_1^* + RC_0) \tag{21}$$

so that

$$\frac{\partial P_{D_1}}{\partial R} = L_1(1-L_1)\left(\left(m_2\delta(1-q)L\left(v_{20}(C_1^*) - v_{21}\right)u'(y_H - m_2RC_1^*)\right)(C_1^* + RC_0)\right) > 0 \iff C_1^* + RC_0 > 0.$$

E.2.2 Increases in Minimum Payments Increase Default iff $C_1^* > 0$

In this section, we assume that agents make default and debt choices assuming a single common minimum payment ($m_1 = m_2 = m$) applies to both periods and we evaluate the effects of changing this common minimum payment on default in period 1.

As before, taking derivatives with respect to m and applying the envelope theorem yields

$$\frac{\partial P_{D_1}}{\partial m} = -L_1(1-L_1)\left(\nabla_m Q(C_1^*,\theta) + \nabla_{C_1} Q(C_1^*)\frac{\partial C_1^*}{\partial m}\right) = -L_1(1-L_1)\nabla_m Q(C_1^*,\theta) \tag{22}$$

Next,

$$\nabla_m Q(C_1^*;\theta) = -\delta(1-q)L\left(v_{20} - v_{21}\right)u'(y_H - mRC_1^*)RC_1^* \tag{23}$$

so that

$$\frac{\partial P_{D_1}}{\partial m} > 0 \iff C_1^* > 0$$

i.e., agents have positive borrowing on the card at the end of period 1. Empirically, this is the case for the vast majority of borrowers.

If we assume logarithmic utility $C_1^* > 0$ if and only appropriately discounted second period income in the high state is sufficiently high:

$$(1-\alpha)y_H > L(v)m\alpha\left(y_1 - RC_0\right)$$

where the discount rate applied to second period income incorporates income uncertainty, i.e., $(1-\alpha) = 1/\left(1+\delta(1-q)\right)$. Thus, under this assumption, increases in minimum payments (announced in period 1 and allowing agents to adjust their debt levels accordingly) unambiguously increase default.

In the case where agents assume $m_1 \neq m_2$, reproducing the argument above yields

$$\frac{\partial P_{D_1}}{\partial m_2} > 0 \iff C_1^* > 0.$$

E.2.3 Increases in m_1 do not affect Default.

Small changes in minimum payments in m_1 (holding m_2 fixed) do not affect default. This is because agents can adjust purchases correspondingly, leaving overall debt and default unchanged.[54]

[54] With log utility, we can show that $\frac{\partial P^*}{\partial m_1} = RC_0$ so that as long as agents have positive initial debt, purchases will increases with increases in m_1 - see the discussion around Equation (18). Empirically, we observe increases in purchases in response to increases in minimum payments; see Figure OA-20(d).

There are at least two ways to conclude that changes in m_1 do not affect default in either period. First, for period 1, examining Equation (19) we see that m_1 does not enter the function $Q(C_1;\theta)$. A more brute-force approach (that yields the same conclusions) replaces C_1 with $P + (1 - m_1)RC_0$ in $Q(\cdot)$ so that $P_1 = L\left(v_{11} - Q(P^*, \theta)\right)$ where

$$Q(P, \theta) = u\left(y_1 + P - m_1 RC_0\right) + \delta\left(qu(y_L) + (1-q)\ln\left\{\exp(v_{20}(P) + v_{21}\right\}\right)$$

and $P^* = \operatorname{argmax}_P Q(P, \theta)$. Then, as in the cases above, taking derivatives and applying the envelope theorem yields

$$\frac{\partial P_{D_1}}{\partial m_1} = -L_1(1 - L_1)\nabla_{m_1} Q(P^*, m_1)$$

where we have emphasized the direct dependence of $Q(\cdot)$ on m_1.

$$\nabla_{m_1} Q(P^*, m_1) = u'(y_1 + P^* - mRC_0)(-RC_0) + \delta(1-q)L(v_{20} - v_{21})u'\left(y_H - m_2 R\left(P^* + (1-m_1)RC_0\right)\right)\left(m_2 R^2 C_0\right) \tag{24}$$

Next, using the first-order conditions for maximizing $Q(\cdot)$ with respect to P

$$\frac{\partial Q}{\partial P} = 0 \implies u'(y_1 + P - mRC_0) = \delta(1-q)L(v_{20} - v_{21})u'\left(y_H - m_2 R\left(P + (1-m_1)RC_0\right)\right)(m_2 R) \tag{25}$$

and substituting eq. (25) into eq. (24) we conclude that $\frac{\partial P_1}{\partial m_1} = 0$. To see that default in period 2 is unaffected by m_1 we observe that changes in m_1 leave the state variable debt C_1^* unchanged and hence second period default will remain unchanged.

E.2.4 Worse Income Distributions Increases Default

We operationalize this notion by increases in q (i.e., by replacing a given second period income distribution by one that it first-order stochastically dominates). As before, taking derivatives and applying the envelope theorem yields

$$\frac{\partial P_{D_1}}{\partial q} = -L_1(1 - L_1)\left\{\left(\delta\left(u(y_L) - u(y_H)\right) - \nabla_q Q(C_1^*; \theta)\right\}\right. .$$

Since $u(\cdot)$ is increasing and $y_H > y_L$, the first term in the curly parentheses is negative. Next,

$$\nabla_q Q(C_1, \theta) = \delta\left(u(y_L) - \ln\left(\exp(v_{20}) + \exp(v_{21})\right)\right) < 0$$

where the inequality follows since $y_H > y_L$, $v_{21} = u(y_H)$ and $u(\cdot)$ is strictly increasing. Therefore,

$$\frac{\partial P_{D_1}}{\partial q} = -L_1(1 - L_1)\left\{\left(\delta\left(u(y_L) - u(y_H)\right) - \nabla_q Q(C_1^*; \theta)\right\} > 0\right. \tag{26}$$

E.2.5 Liquidity Constraints and Heterogeneity

As argued on p. OA - 40, we can view the normalization C_0/y_1 as a proxy for liquidity constraints. Holding first-period income fixed, we can then examine the default responses to changes in C_0 as reflecting responses to changing liquidity.

As above, we take derivatives and apply the envelope theorem,

$$\frac{\partial P_{D_1}}{\partial C_0} = -L_1(1 - L_1)\nabla_{C_0} Q(C_1^*; \theta)$$

and

$$\nabla_{C_0} Q(C_1, \theta) = -Ru'(y_1 + C_1 - RC_0)$$

so that

$$\frac{\partial P_{D_1}}{\partial C_0} = L_1(1 - L_1)Ru'(y_1 + C_1^* - RC_0) > 0 \tag{27}$$

which is consistent with the means presented in Figure OA-13(b).

The model also clarifies the conditions under which the negative effects of contract term changes will be exacerbated by baseline debt. In particular, taking derivatives of the right-hand side of eq. (27) with respect to the common minimum payment m

$$\frac{\partial^2 P_{D_1}}{\partial C_0 \partial m} > 0 \iff u''\left(y_1 + C_1^* - RC_0\right) \frac{\partial C_1^*}{\partial m} > u'\left(y_1 + C_1^* - RC_0\right)\left(1 - 2L_1\right) \frac{\partial Q(C_1^*, C_0)}{\partial m}$$

Assuming log utility we can see that the left-hand side is strictly positive since $u''(\cdot) < 0$ and $\partial C_1^* / \partial m < 0$ by eq. (16). Further, $\partial Q(C_1^*, m)/\partial m < 0$. [55] Therefore, a sufficient condition for the cross-partial to be positive is that $L_1 < 1/2$—i.e., the probability of default in period 1 is sufficiently low. This in turn is equivalent to requiring $Q(C_1^*) > v_{11}$—i.e., that the deterministic part of the payoff from not defaulting in period 1 is greater than the payoff from defaulting. Assuming this is reasonable, the model then predicts that increases in minimum payments will increase default more for agents with higher values of C_0—the and we view the latter as a proxy for liquidity constraints.

E.3 Newer Borrowers

In the data newer borrowers are characterized by lower and more volatile incomes and fewer alternative credit sources (as measured by the number of formal sector loans in the credit bureau). We map these two features into (a) higher probabilities of a lower income realization (i.e., higher values of q) or lower values of y_H and (b) a higher valuation for holding on to the credit card v.

With respect to income Appendix E.2.4 implies that newer borrowers (i.e., those with higher q) have higher default rates and similar arguments show that this is true when we view newer borrowers as having lower values of y_H.

We next focus on the continuation value v and demonstrate that treatment responses are muted for agents with higher continuation values—that is, newer borrowers are less responsive to contract term changes. In particular, we will outline conditions under which the default response to interest rate changes is muted for newer borrowers.

We begin by defining a monotone function of the default elasticity with respect to the interest rate and then deriving conditions under which this function is decreasing in the continuation value v.[56] First, assuming

[55] Note, that the envelope theorem implies that $\partial Q(C_1^*(m), m)/\partial m = \partial Q(C_1^*, m)/\partial m$.

[56] The monotone transformation and the examination of elasticity are carried out to simplify the analysis.

log-utility and using the results from Appendix E.1.5 and substituting in Equation (12) yields

$$\epsilon_{DR} \equiv \frac{R\partial P_1}{P_1 \partial R} = \frac{(1 - L_1)(C_1^* + RC_0)}{(y_1 + C_1^* - RC_0)} > 0.$$

Next, define $g(v) \equiv \log(\epsilon_{DR})$. We will show that $\frac{\partial g}{\partial v} < 0$ which implies that $\frac{\partial \epsilon_{DR}}{\partial v} < 0$ so that newer borrowers (i.e., those with higher values of v) will be less responsive to interest rate changes than older borrowers (those with lower continuation values).

Some algebra yields

$$\frac{\partial g}{\partial v} = -L_1 \frac{\partial Q}{\partial v} + \frac{\partial C_1^*}{\partial v} \frac{y_1 - 2RC_0}{(y_1 + C_1^* - RC_0)(C_1^* + RC_0)} \tag{28}$$

where

$$\frac{\partial Q}{\partial v} = \delta(1 - q) \frac{(y_2 - m_2 R C_1^*) \exp(v)}{(y_H - m_2 R C_1^*) \exp(v) + y_H}$$

which is strictly positive under our assumptions above. Therefore, the first term in Equation (28) is strictly negative. Next, some calculations yield

$$\frac{\partial C_1^*}{\partial v} = -\frac{(1 - \alpha)\, y_H}{m_2 R \exp(v)} < 0$$

which is negative and decreasing in v. Thus, a sufficient condition for the last term in Equation (28) to be strictly negative, and hence for $\frac{\partial \epsilon_{DR}}{\partial v} < 0$ is either (a) $y_1 > 2RC_0$ (i.e., initial card debt is sufficiently small relative to income) or (b) if not (i.e., if $y_1 < 2RC_0$) then the continuation value v be high enough such that the last term in eq. (28) is smaller than the first term in absolute value. Under these conditions, it follows then that newer borrowers—with higher values of v—will be less responsive to changes in interest rates relative to older borrowers—who have lower continuation values.

F Estimating Default Treatment Effects with Duration Models

To assess the sensitivity of our results to our specific choice of outcome (binary cumulative default) and to unobserved heterogeneity, we estimate a set of duration models in this section. We show that our OLS estimates based on Equation (1) closely resemble those obtained from the duration models (both with and without unobserved heterogeneity), and we therefore continue to focus on the OLS estimates in the main body of the paper.

F.1 Basic Duration Models

We begin by estimating a standard model of the following form with parametric hazard:

$$\begin{aligned}\lambda(MP_i, r_i, t) &= \exp\left[\delta_0 + \delta_1 \mathbb{1}\left\{MP_i = 10\%\right\} + \delta_2(45\% - r_i)/30\%\right] \alpha t^{(\alpha-1)} \\ &\equiv \exp\left(x_i'\delta\right) \alpha t^{(\alpha-1)}. \end{aligned} \tag{29}$$

The baseline hazard is thus a Weibull (with unknown parameter α), and the proportional hazard has the usual exponential form. The Weibull parameterization implies a strictly monotone hazard function, which is a strong assumption (we assess its appropriateness and evaluate alternatives below).

Given the shape of the hazard rate, the proportion of borrowers who default by month t—the analogue to our cumulative default measure—is given by:

$$\begin{aligned} F(t; MP_i, r_i; \theta) &= 1 - \exp\left[-\int_0^t \lambda(MP_i, r_i, s)\,\mathrm{d}s\right] \\ &= 1 - \exp\left[-t^\alpha \exp\left(x_i'\delta\right)\right], \end{aligned} \tag{30}$$

where $F(\cdot)$ is the cumulative distribution function for the time to default variable T and $\theta' \equiv (\alpha, \delta')$. We use this formula to estimate the analogous treatment effects to those in Equation (1) for a given θ. For instance, the analogue to α_t from Equation (1), the proportion of borrowers who default by month t, is given by

$$\tilde{\alpha}_t \equiv F(t; MP_i = 5\%, r_i = 45\%; \theta) \tag{31}$$

$$= 1 - \exp(t^\alpha \exp(x_{i,B}'\delta)), \tag{32}$$

with $x_{i,B} = (1, 0, 45)$. Similarly, the analogue to β_t (the effect of a 5 pp increase in the minimum payment) from Equation (1) can be calculated as

$$\tilde{\beta}_t \equiv F(t; MP_i = 10\%, r_i = 45\%; \theta) - F(t; MP_i = 5\%, r_i = 45\%; \theta) \tag{33}$$

$$= \exp\left(t^\alpha \exp\left(x_{i,B}'\delta\right)\right) - \exp\left(t^\alpha \exp\left(x_{i,MP}'\delta\right)\right), \tag{34}$$

where $x_{i,MP} = (1, 1, 45)$. Similarly, the analogue to γ_t (the effect of a 30 pp decrease in interest rates) from Equation (1) is

$$\tilde{\gamma}_t \equiv F(t; MP_i = 5\%, r_i = 15\%; \theta) - F(t; MP_i = 5\%, r_i = 45\%; \theta) \tag{35}$$

$$= \exp\left(t^\alpha \exp\left(x_{i,B}'\delta\right)\right) - \exp\left(t^\alpha \exp\left(x_{i,R}'\delta\right)\right), \tag{36}$$

where $x_{i,R} = (1, 0, 15)$.

We estimate the model using maximum likelihood (the likelihood function is well-behaved and implemented in Stata) with probability weights throughout to make population statements. It is worth noting that the hazard model is considerably more parsimonious than the month-by-month estimation of Equation (1)—the hazard model estimates treatment effects over the entire 26-month study using only four parameters (α and the δ vector in Equation (29)) while the OLS estimates 3×26 parameters. This parsimony comes at the expense of making extremely strong assumptions on the hazard function—that it is monotone and that the interventions proportionally affect the hazard rate. As we show below, these assumptions are likely too strong for the minimum payment intervention.

F.2 Duration Models with Unobserved Heterogeneity

We model unobserved heterogeneity using frailty, parameterizing it as a Gamma distribution, to better understand the relative roles of duration dependence, unobserved heterogeneity and treatment effects. We parameterize the hazard as

$$\lambda_i(x,t) = \nu_i \exp(x_i'\delta)\alpha t^{(\alpha-1)} \tag{37}$$

where $\nu_i \sim \text{Gamma}(\rho,\rho)$ and $\theta' = (\alpha, \delta', \rho)$. The new hazard slightly modifies the formula for the proportion of borrowers who default by month t. Given the unobserved frailty distribution we choose, the proportion of borrowers who default is given by:

$$\begin{aligned} F(t; MP_i, r_i; \theta) &= 1 - \int_0^\infty \exp\left[-\int_0^t \lambda_i(MP_i, r_i, s)\mathrm{d}s\right] f_\nu(\nu_i;\rho)d\nu_i \\ &= 1 - \int_0^\infty \exp\left[-\nu t^\alpha \exp\left(x_i'\delta\right)\right] f_\nu(\nu;\rho)d\nu \end{aligned} \tag{38}$$

and we use Equation (38) to recompute $\tilde{\alpha}_t$, $\tilde{\beta}_t$, and $\tilde{\gamma}_t$ using the $F(\cdot)$ above.

F.3 Duration Model Results

Table OA-15 shows our coefficient estimates for δ in the first panel and for the logarithm of α and ρ in the second panel. As expected, higher interest rates and higher minimum payments are associated with higher hazard estimates. We generally find positive duration dependence ($\alpha > 1$) consistent with the default patterns in e.g., the control group. The coefficient estimates as well as the standard errors with and without frailty are similar, suggesting that (the gamma modeling of) unobserved heterogeneity is not a primary concern for the estimated treatment effects.[57]

Figure OA-11 facilitates the comparison of the estimates in Table OA-15 to those estimated using Equation (1) by computing cumulative default at the end of the experiment using Equations (31), (33) and (35). It plots the OLS treatment effect estimates at endline and the various duration models' estimated proportion of defaulters at endline. In general, both the duration model estimates are quite similar to the OLS estimates. This suggests that our conclusions are robust to alternative estimation strategies and that accounting for unobserved heterogeneity (at least in the form specified above) and/or duration dependence does not appear to change the estimated treatment effects.

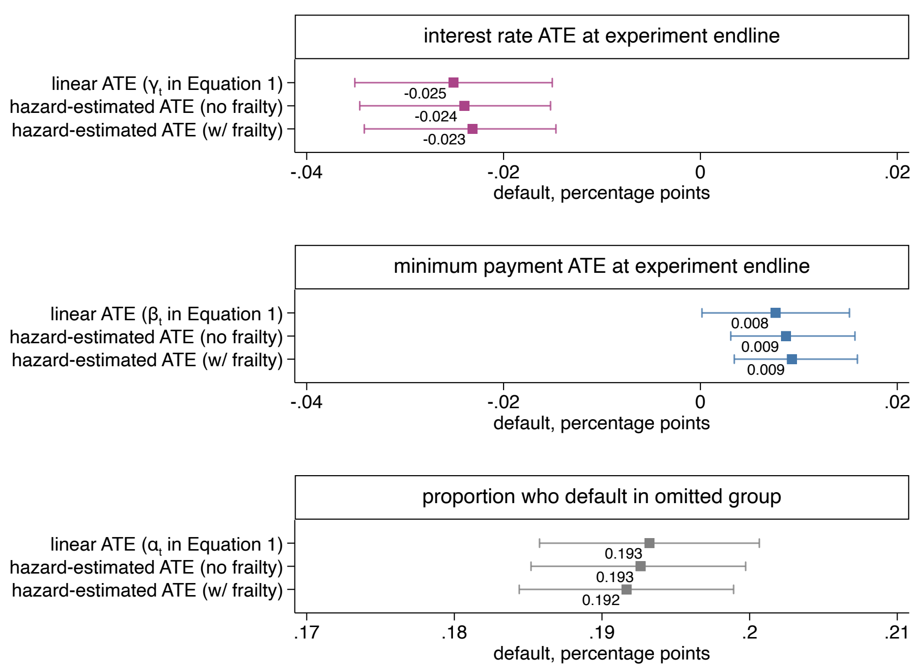
[57] As is often the case (see e.g., Wooldridge, 2010), the estimate of duration dependence α is higher when we incorporate unobserved heterogeneity although the difference is not substantively consequential here since the duration model without frailty already exhibited positive duration dependence (i.e., $\hat{\alpha} > 1$ in the first-column).

Table OA-15: Duration Model Coefficient Estimates

	(1)	(2)
$(45\% - r_i)/30\%$	-0.147***	-0.160***
	(0.031)	(0.034)
$\mathbb{1}\{MP_i = 10\%\}$	0.049*	0.060*
	(0.023)	(0.027)
Constant	-7.560***	-7.782***
	(0.062)	(0.101)
log of α	0.614***	0.669***
	(0.010)	(0.023)
log of ρ		0.162
		(0.480)
unobserved frailty	no	yes

Notes: This table plots the coefficient estimates for the hazard models. The first panel shows the values of the proportional hazard part of the model, δ in Equations (29) and (37). The second panel shows the coefficient estimates for the shape parameter α for the baseline hazard, (in particular, $\ln(\alpha)$), and the parameter for the variance of the unobserved frailty, ρ. Column (1) shows the coefficient estimates assuming no frailty, and Column (2) shows the estimates when frailty is Gamma (ρ, ρ) distributed. Robust standard errors are shown in parentheses.

Figure OA-11: Comparison of Equation (1) and Hazard Models at Experiment Endline



Notes: This figure plots the comparison of the average treatment effect estimates obtained using month-by-month OLS estimation of Equation (1) with cumulative default as the dependent variable to those estimated using hazard models. Appendices F.1 and F.2 provide the estimation details for the hazard model-based average treatment effects. We plot results for May 2009, the experiment endline. The top panel plots the effect of a 30 pp decrease in interest rates, the middle panel plots the effect of a 5 pp increase in minimum payments, and the bottom panel plots the proportion of borrowers who default in the excluded group. Standard errors are estimated using 200 bootstrap samples at the individual borrower level.

In addition to the results above, we also explored whether the simple hazard model above can match the entire trajectory of treatment effects plotted in Figures 4(c) and 4(d). The simple version of the model captures the default dynamics for the interest rate changes quite well but not those for the minimum payment intervention.

This is in large part because the minimum payment treatment effects are not monotone over time. Enriching the simple model by allowing for time-varying coefficients in the proportional hazard allows us to better capture the minimum payment treatment effect dynamics. Indeed, we can recover the OLS minimum payment dynamics with three time-varying indicators: one that covers the first six months, one covering the next eleven, and one for the remaining duration.[58] Figure OA-12 displays both the treatment effects obtained by estimating Equation (1) month-by-month and the treatment effects implied by the hazard model (with time-varying covariates) and we observe that they are quite close.

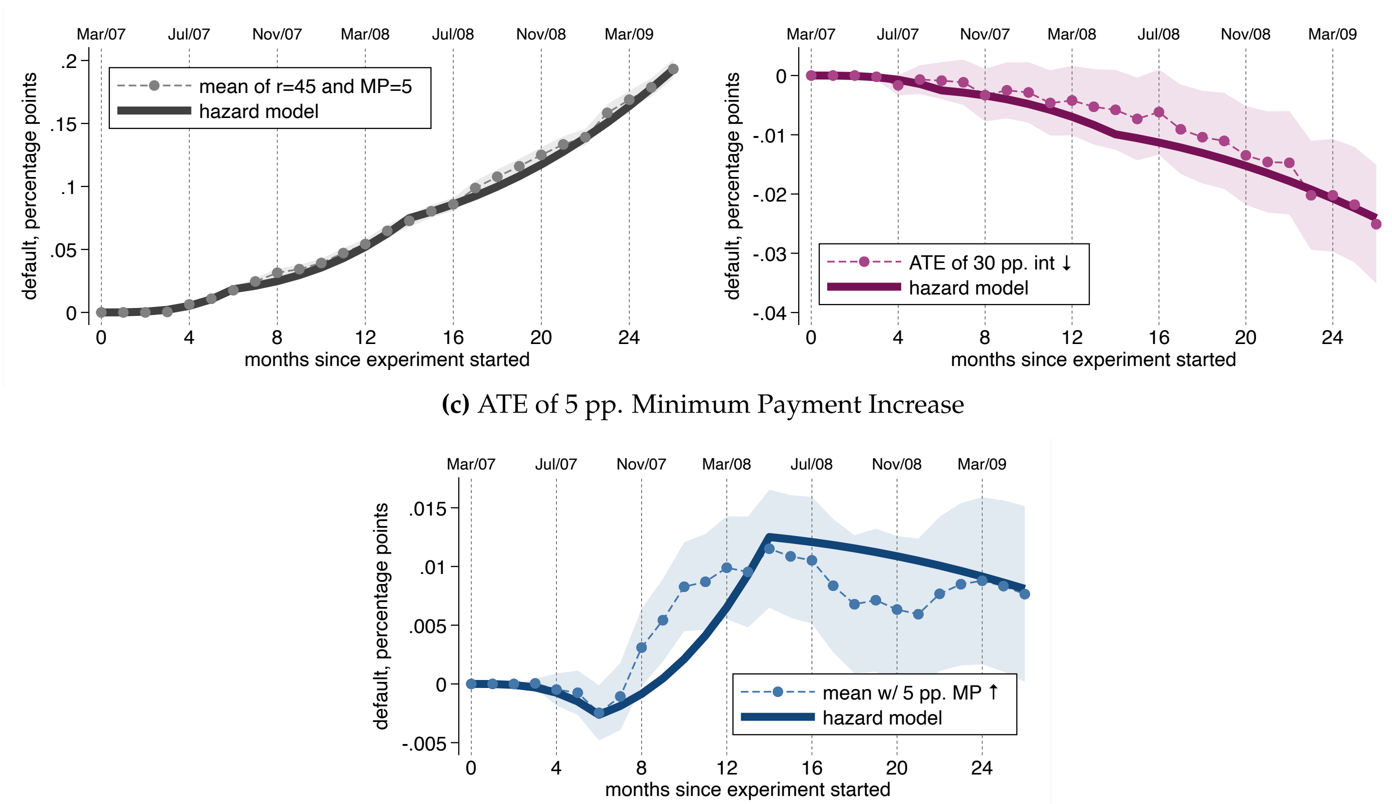
In summary, the results from this section provide evidence that our experimental results are robust to alternative estimation approaches (and in particular to accounting for duration dependence and unobserved heterogeneity).

Figure OA-12: Default estimates Comparison: Equation (1) vs. Hazard (Duration) models

(a) Proportion Who Default in Excluded Group

(b) ATE of 30 pp. Interest Rate Decrease

(c) ATE of 5 pp. Minimum Payment Increase



Notes: This figure plots the comparison of the average treatment effect estimates using the nonparametric estimation of Equation (1) using cumulative default as the dependent variable to the ones estimated using hazard models. We use a hazard model without frailty and modify the proportional hazard part of Equation (29) by interacting the minimum payment treatment dummy with three categorical variables: one for Mar/07 to Aug/07, one for Sep/07 to Jul/07, and one for the remaining period until May/09. We also add the three time groups (and exclude the constant) into the proportional hazard. The dots, dashed lines, and confidence intervals correspond to our estimates of the main specification in the paper (Figure 4). The thick lines are our hazard point estimates. We plot results for the experimental period. Panel (a) plots the proportion of borrowers who default in the excluded group; panel (b) plots the effect of a 30 pp decrease in interest rates; and panel (c) plots the effect of a 5 pp increase in minimum payments.

[58] We also investigated whether modeling duration using competing risks (i.e., distinguishing between cancellations and non-default) changes our conclusions. We find that the the competing risks model replicates the duration model results for the interest rate intervention and provides qualitatively similar results for the minimum payment intervention

G Prediction Exercises

We carried out an incentivized prediction exercise using the Social Science Prediction Platform (SSPP) which asked respondents to predict the sign and magnitude of the effect of the interest rate and minimum payment interventions on credit card default over different time horizons in a setting similar to ours.

The sample consisted of 72 respondents, of whom 64% reported themselves as being academics, and 76% reported being economists. 82% of the sample had a post-graduate degree (with about 47% of the entire sample reporting a PhD). 69% of the sample reported having carried out five or more predictions on the platform, and 51% reported North America as their location.

With respect to the interest rate intervention, 70% of the sample predicted default would be lower in the 15% arm relative to the 45% arm at the end of an 18-month experiment. Of this sub-sample, the median predicted decline in default was 4 pp (mean 5 pp), which is substantially larger than our experimental estimate over the same horizon (1.03 pp). Over the next five years after the end of the experiment, 42% of respondents believed that default would be lower in the 15% arm, and the median post-experiment difference in default between the two arms was also predicted to be 4.5 pp (mean 5 pp). 27% of respondents predicted no difference between the two arms.

With respect to the minimum payment intervention, 73% of respondents predicted that default would be higher at the end of the 18-month experiment in the 10% minimum payment arm (relative to the 5% minimum payment arm, both with an APR of 45%). Among this sub-sample, the median predicted increase in default was 5 pp (mean 6.4 pp) for the higher minimum payment arm (compared to the actual estimated ATE of 0.8 pp). 36% of respondents predicted no difference in default between the lower and higher minimum payments five years after the end of the intervention (with 33% predicting higher default). Of the 31% of the sample that predicted declines in post-experimental default in the previously higher minimum payment arm, the median predicted decline was 3 pp (mean 3.9 pp).

We conducted similar prediction exercises with five senior Mexican officials who all had experience working in the Mexican Central Bank. Respondents filled out the same survey as respondents on the SSPP and, in addition, answered questions about the likelihood of formal unemployment (using the Section 7 definitions).

With respect to the interest rate intervention, all five respondents predicted default to be lower in the 15% arm relative to the 45% arm at the end of the 18-month experiment. The average predicted decline in default was 8.6 pp (more than eight times the estimated ATE of 1.03 pp). Over the five years after the experiment ended, 3 respondents believed default would be lower in the 15% arm (the mean predicted decrease was 4.7 pp), while one respondent each believed that default would be the same in each arm or that default would be higher in the lower interest rate arm.

With respect to the minimum payment intervention, there was considerable disagreement among respondents. Two respondents predicted that default would be higher at the end of the 18-month experiment in the 10% minimum payment arm, while two predicted default would be lower. Among those who predicted an increase in default, the average predicted increase was 5.5 pp (and among those who predicted a decrease, the predicted decrease was -6.5 pp). Overall, the average prediction was a decrease in default of 0.4 pp (compared to an *increase* in default of 0.8 pp in the experiment). Five years after the end of the experiment, two respondents predicted no difference in default between the previously higher and previously lower minimum payment arms, while two respondents predicted higher default. Overall, the average prediction was an increase in default of 2.4 pp in the higher minimum payment arm five years after the end of the experiment.

Finally, the respondents were much more sanguine about the likelihood of formal unemployment (defined as at least one month out of formal unemployment): predicting that on average 19% of a sample of formally employed new borrowers would experience a spell of unemployment over a three-year period (we estimate the number to be 43%).

H Treatment Effect Heterogeneity

While the default elasticities are modest on average, they could mask considerable heterogeneity. The explicit stratified design and the large sample size imply that we are well-positioned to examine heterogeneity in treatment effects. In this section, we link such heterogeneity to our organizing framework to better understand the mechanisms underlying borrower behavior.

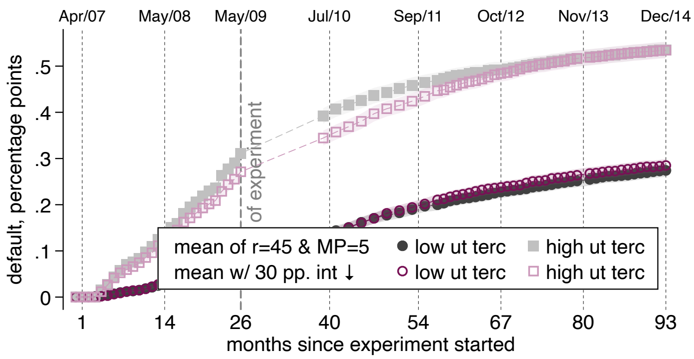
Heterogeneity by Liquidity Constraints: We examine the variation in treatment effects by liquidity using the baseline ratio of debt outstanding to the credit limit on the study card (see the discussion in Section 3, p. 13 and Appendix D on the use of this ratio as a measure of liquidity). We define a borrower as being more liquidity constrained if the ratio of their baseline balance to their credit limit was in the highest tercile (and less constrained if their ratio was in the lowest tercile). This measure is also consonant with our model, where we show that tightening liquidity constraints (interpreted here as increases in C_0 keeping y_1 fixed) exacerbate the effect of minimum payments on default (see Appendix E.2.5).

Figure OA-13(b) shows that, consistent with the model's predictions, during the first year, the minimum payment intervention disproportionately increased default among those with a high level of baseline credit utilization (i.e., those who are more liquidity constrained)—by the end of the first year the ATE is almost 2 pp. for those with high levels of baseline credit utilization while it is close to zero for those with low utilization levels. After the first year, the ATE for this group begins to decline. By the end of the experiment, the ATEs were virtually identical for both groups. This longer-run pattern is consistent with increased minimum payments, reducing the repayment burden over the longer term (by reducing debt) and decreasing default. This intuition finds further support in the post-experimental ATEs, which continue to fall for the high utilization arm while hovering near zero for the low utilization arm.

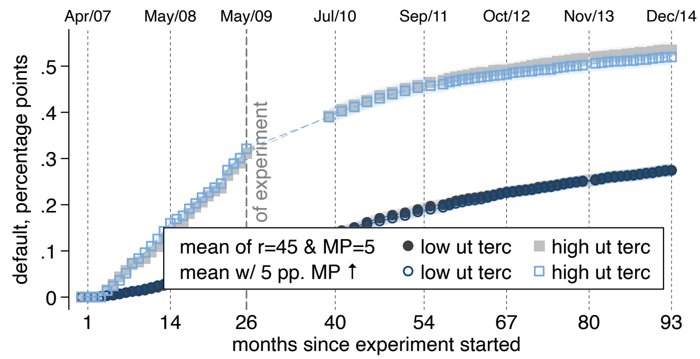
Figure OA-13(c) also shows that the declines in default due to interest rate decreases are almost entirely concentrated among borrowers with high levels of baseline credit utilization on the study card. This is consistent with the argument that the declines in interest rates affected debt (and therefore default) primarily by reducing the interest accrued on previously accumulated debt (see Appendix I.1 for details). It is also consistent with Figure OA-14, which shows that the reductions in debt from lower interest rates were much larger for borrowers with high levels of baseline credit utilization.

Figure OA-13: Default Levels and ATEs by Baseline Credit Utilization (= Amount Due / Limit)

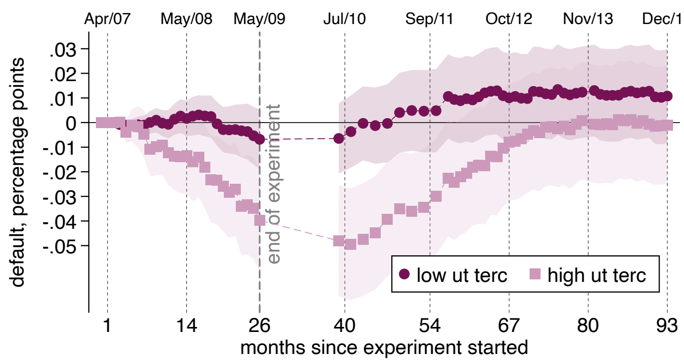
(a) Default Levels by Interest Rates



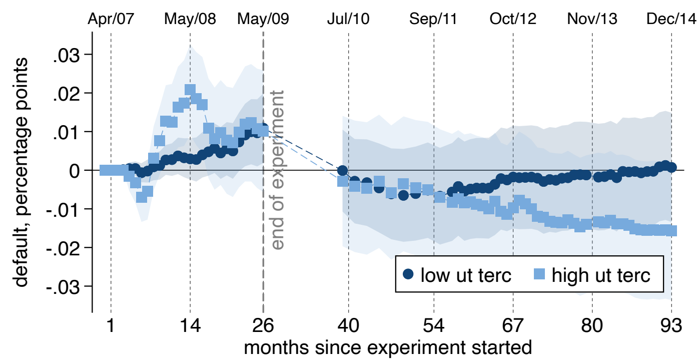
(b) Default Levels by Minimum Payments



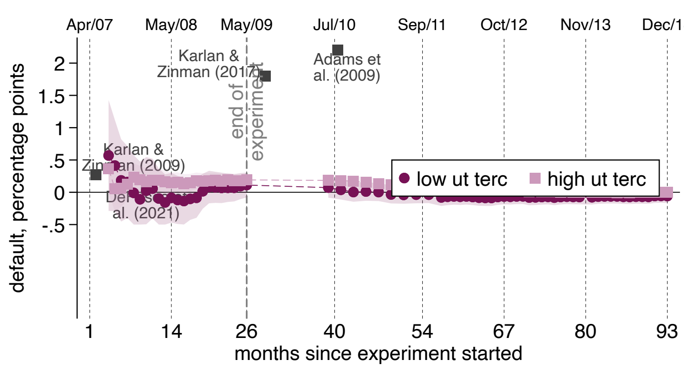
(c) Default ATE of 30 pp. Interest Rate Decrease



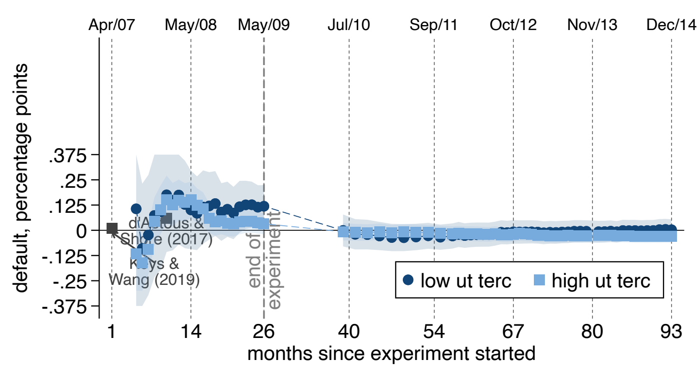
(d) Default ATE of 5 pp. Minimum Payment Increase



(e) Comparison to the Literature
Interest Rate Default ATE elasticities

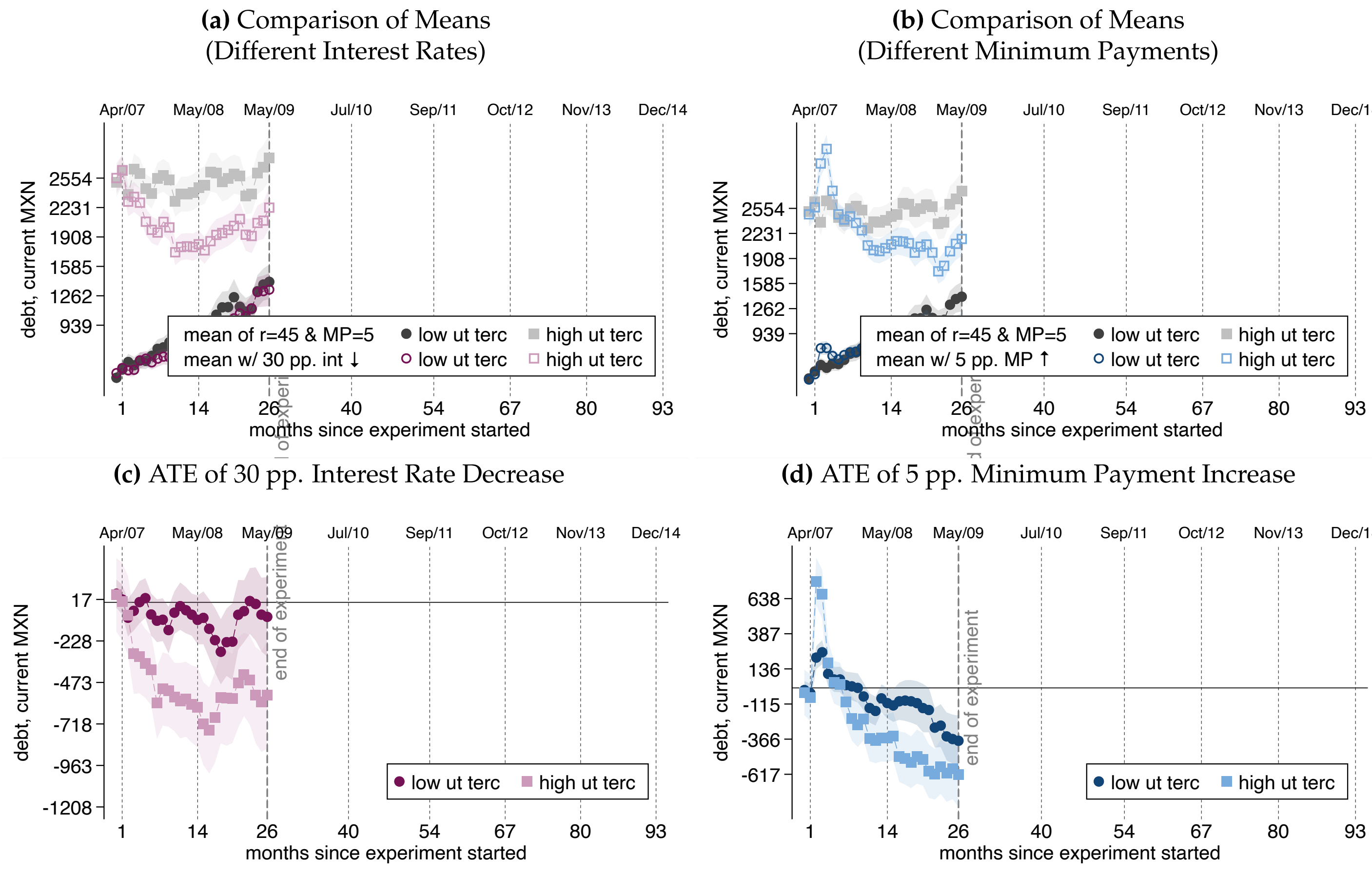


(f) Comparison to the Literature
Minimum Payment Default ATE elasticities



Notes: These figures plot the causal effect of interest rate and minimum payment changes on default in the study card. We separate borrowers using the ratio between amount due and credit limit, defining these two groups as "low" if in the lowest tercile or "high" if in the highest. Figures on the left examine interest rate changes, while 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 the 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 (d) computes the average treatment effect of a 5 pp minimum payment increase. Panel (e) computes the elasticity of default by calculating the average treatment effect in percent terms (i.e., γ_t/α_t in Equation 1) and dividing it by $(45 - 15)/45$. Similarly, 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%.

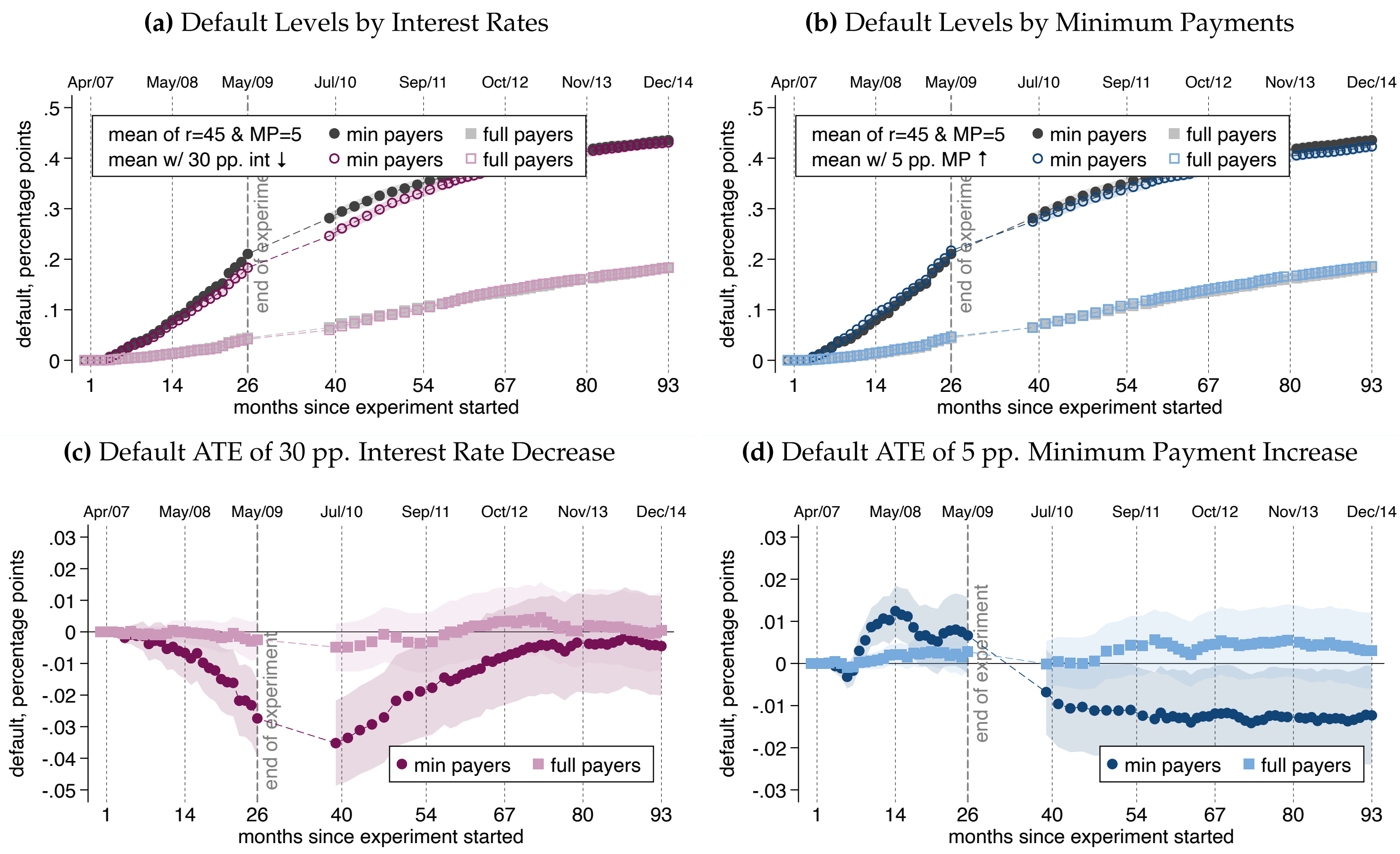
Figure OA-14: Debt ATEs
(By Initial Credit Utilization = Amount Due/Limit)



Notes: These figures plot the causal effect of interest rate and minimum payment changes on default in the study card. We separate borrowers using the ratio between amount due and credit limit, defining these two groups as "low" if in the lowest tercile or "high" if in the highest tercile. Figures on the left examine interest rate changes, while 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 pink dotted line in Panel (a) plots the share of cardholders that default over time when the 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 (d) computes the average treatment effect of a 5 pp minimum payment increase. Panel (e) computes the elasticity of default by calculating the average treatment effect in percent terms (i.e., γ_t/α_t in Equation 1) and dividing it by $(45 - 15)/45$. Similarly, 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%.

Heterogeneity by Repayment Behavior: Figure OA-15 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 finding is consistent with the previous rationale since full-payers have substantially lower baseline debt utilization rates (39% versus 89% for minimum payers, and baseline debt is 101 vs 1300 in peso terms).

Figure OA-15: Default Levels and ATEs by Baseline Payment Strata



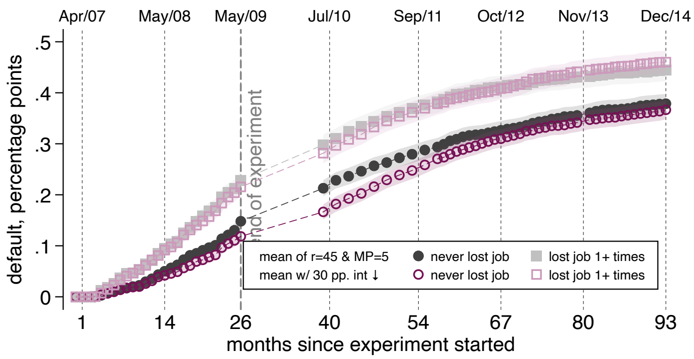
Notes: These figures plot the causal effect of interest rate and minimum payment changes on default in the study card. We separate borrowers using the payment behavior strata and restrict the analysis to borrowers who pay close to the minimum payment and those classified as full payers. Figures on the left examine interest rate changes, while 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 the 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 calculating the average treatment effect in percent terms (i.e., γ_t/α_t in Equation 1) and dividing it by $(45 - 15)/45$. Similarly, Panel (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%.

Heterogeneity by Labor Force Attachment: Finally, we calculate treatment effects separately for borrowers with strong versus weak formal baseline labor market attachment for completeness. We restrict our sample to borrowers 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 before the experiment from January 2004 to February 2007, and those that have lost employment at least once as having weak labor market attachment.[59] Figure OA-16(a) shows that while default rates for borrowers with weaker labor force attachment are much higher than those for borrowers with stronger attachment, the ATEs do not statistically differ between the two groups.

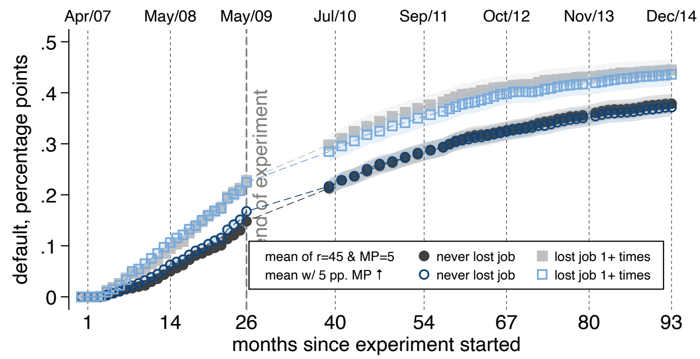
[59] Of those employed for at least one month between January 2004 and February 2007 (50% of our individuals with CURPs), 42% have low labor market attachment in this definition.

Figure OA-16: Default Levels and ATEs by Pre-Experiment Formal Labor Attachment

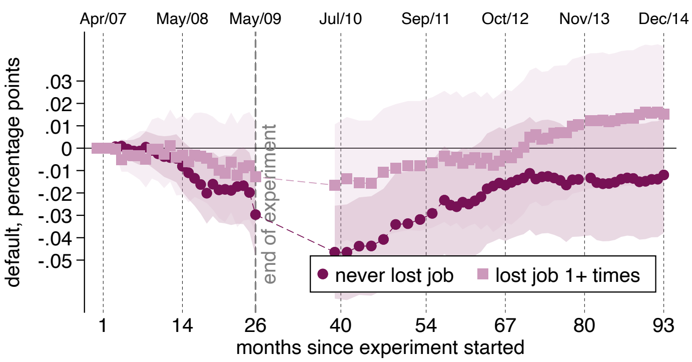
(a) Default Levels by Interest Rates



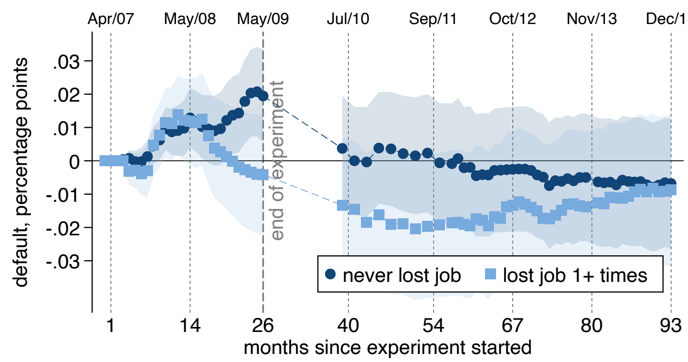
(b) Default levels by Minimum Payments)



(c) Default ATE of 30 pp. Interest Rate Decrease

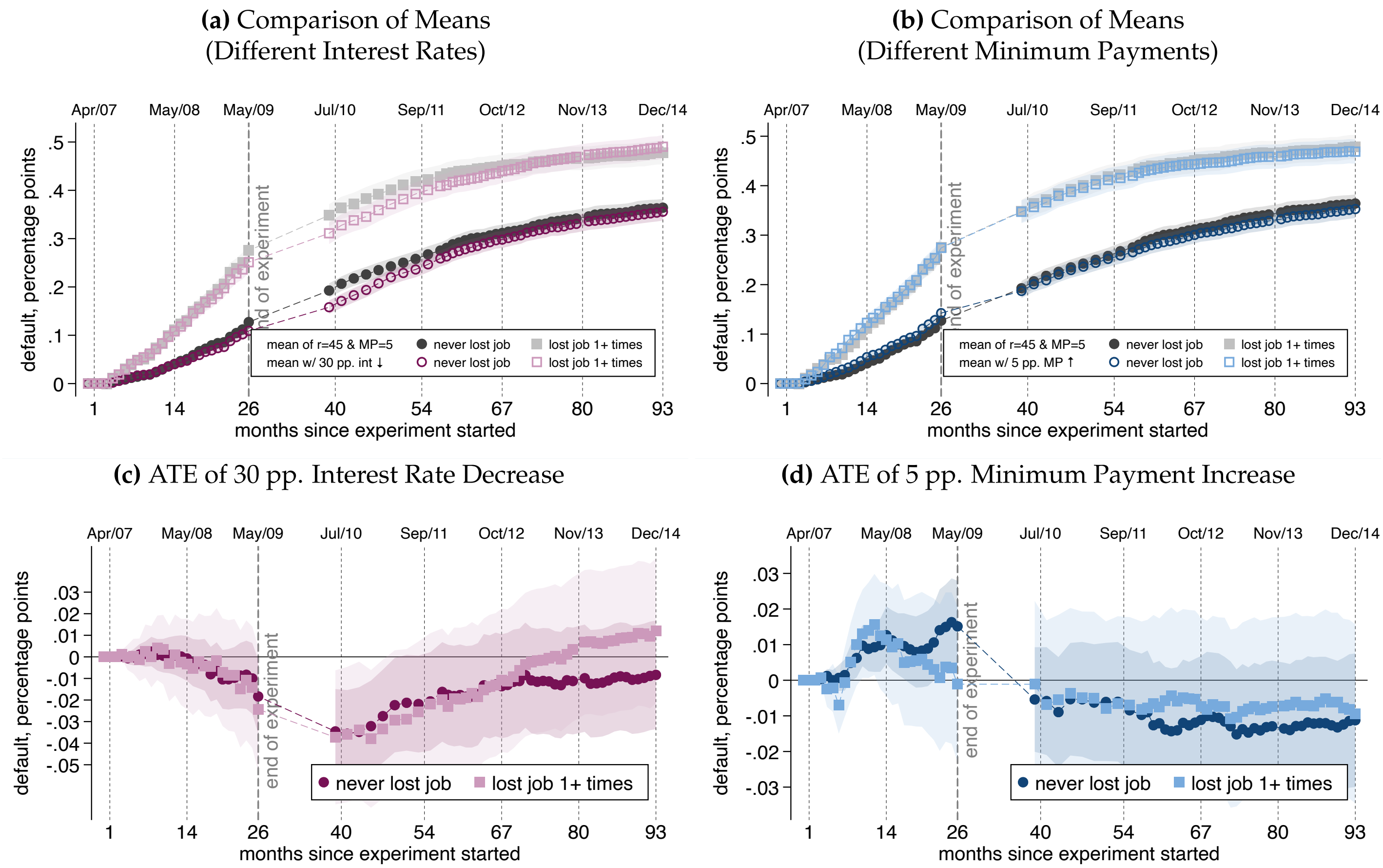


(d) Default ATE of 5 pp. Minimum Payment Increase



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 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-17: ATEs of Contract Terms on Default by Formal Sector Labor Attachment During Experiment (Share of Cardholders that Default)



Notes: These figures plot the causal effect of interest rate and minimum payment changes on default in the study card. The analysis is restricted to borrowers who were formally employed for at least one month during the experiment (March 2007 to May 2009) and separates borrowers based on employment continuity (i.e., the "never lost job" group) or those who experienced job loss one or more times. Figures on the left examine interest rate changes, while 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 the 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 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.

I 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-18. 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-16.[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 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.

I.1 Effect of Interest Rate Reductions on Debt

Figure OA-18 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 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 positive debt response to interest rate increases.[61]

The positive effect of interest rate increases on debt may seem counter-intuitive since borrowers appear to respond to price (interest rate) increases by increasing quantities (debt). However, this apparent contradiction can be resolved once we recognize that borrowers begin the experiment with previously accumulated debt

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

[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 much larger at -2.9; Dehejia et al. (2012) provide estimates in the range of $[-0.73. -1.04]$.

which also accrues interest at the experimentally assigned rates. To begin with, we can see this most clearly in the model where optimal debt is given by eq. (13) and its derivative with respect to R is given by eq. (15) which we reproduce here for ease of reference:

$$\frac{\partial C_1^*}{\partial R} = \alpha C_0 + (1-\alpha)\left(\frac{-y_H}{L(v)m_2R^2}\right)$$

As the discussion on p.OA - 27 in that section notes, increases in interest rates affect debt in two ways: (a) they first increases debt "mechanically" since previously accumulated debt C_0 now accrues interest at a higher rate (this component is captured by the first term in the derivative above); (b) increases in R decrease debt since they decrease purchases—this is the last term above and is the expected price effect. If the first term dominates the last term then debt will increase with interest rate increase. This logic is common to all credit card borrowing and so in that sense is not peculiar to our setting.

Next, we examine the empirical analogues of these quantities in the experimental data. First, Figures OA-20(c) and OA-20(e) show that, consistent with the model as well as economic intuition, purchases increase in response to lowered interest rates. The point estimates are consistently positive throughout the study though the Lee Bounds become quite wide after the first year.[62] Figure OA-21(e) shows a similar pattern for net purchases (i.e., purchases minus payments) as well.[63] Despite the increases in net purchases, overall debt declines in response to the interest rate declines as noted above. This suggests that the first term in Equation (15) dominates the last term—i.e., the decline in the interest accrued on previously accumulated debt outweighs the increased debt due to increased purchases.

We can also demonstrate this using a complementary approach. Let C_t denote debt in period t and P_t denote net purchases in period t. Next, using the relationship $C_t = P_t + RC_{t-1}$ (which approximately holds in our data, see Appendix B.4) we can derive the relationship between debt in any period t and baseline debt C_0 as $C_t = \sum_{s=1}^{t} R^s P_{t-s} + R^t C_0$. Then, it follows that

$$\frac{\partial C_t}{\partial R} > 0 \iff tR^{t-1}C_0 > \sum_{s=1}^{t} sR^{s-1}\frac{\partial P_{t-s}}{\partial R}. \tag{39}$$

We view this as the formalization of the notion that debt is increasing in the interest rate if and only if the "compounding" effect of baseline debt (the left-hand side of the inequality above) exceeds the behavioral response to changed interest rate changes (the right hand side of the inequality). Figure OA-18(c) shows that debt is increasing in the interest rate during the experiment, i.e., that the left-hand side is true; the above then implies the inequality on the right-hand side must hold as well (that is the compounding effect dominates the behavioral effect).

As our final piece of evidence we examine the heterogeneity in the ATE of interest rate on debt by baseline debt utilization (measured as the ratio of debt outstanding to credit limit at baseline)—Figure OA-13(c) shows that the interest rate induced declines in debt are much larger among borrowers with high levels of baseline indebtedness.

Jointly, these facts suggest that the relatively large negative debt response to interest rate declines arises

[62] By the end of the experiment our preferred estimates of the purchase elasticity are $[-0.60, +0.02]$ so are consistent with a range of (mostly positive) purchase responses to interest rate declines (see Table OA-18).

[63] Figure OA-19(e) shows that payments decline in response to the reduced interest rates (again consistent with the importance of compounding).

from the fact that lower interest rates result in outstanding debt (in particular already accumulated debt at baseline) being compounded at a correspondingly lower rate. This decline more than offsets the increase in net purchases.

We believe these findings on debt are more generally applicable to credit-card borrowing among populations with substantial pre-existing debt. Both policy and popular attention focuses on the effect of increased interest rates in increasing debt (as observed in the experiment). For instance, a recent piece by researchers at the New York Federal Reserve (Haughwout et al., 2023) notes that increased interest rates lead to increased monthly payments via effects on card balances. The impact of increased interest rates being applied to the stock of previously existing debt is also commonly noted in the popular press (see e.g., Consumer Reports) as a pitfall for credit card borrowers to be aware of. Thus in this sense, we believe our debt responses to interest rate changes should be of wider interest.

I.2 Effect of Minimum Payment Increases on Debt

Debt response to the minimum payment increase follows an interesting pattern. Figures OA-18(b) and OA-18(d) show that debt increases markedly in the third and fourth month of the experiment in response to the increase in minimum payments. 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 primarily from late payment fees due to delinquencies.[64] We provide two pieces of evidence to support this argument. First, we observe a sharp rise in delinquencies in the initial months of the experiment (particularly in months 3, 4 and 5) which coincides with subsequent increased delinquency fees and is also followed by increased debt (recall that the vast majority of borrowers did not make their payments in full). Second, we observe an *increase* in net payments (payments minus purchases) during this period (particularly in months 3–5) so that the increase in debt cannot be a consequence of increased purchases (since net purchases *decline*).

The debt ATE turns negative by month 9 and declines gradually for the rest of the experiment though the Lee bounds become increasingly wide so that by the end of the experiment we cannot rule out declines (of 687 pesos or an elasticity of -0.31) or increases (461 pesos or an elasticity of +0.21), see column (2) in Table OA-16. Thus, our results are consistent with the conclusion that doubling the minimum payment had a moderate effect on reducing debt (when using the left hand side Lee bound).[65]

Both purchases and payments increase in response to the increased minimum payments (see Figures OA-19(d) and OA-20(d) and Tables OA-17 and OA-18). The increase in purchases is consistent with the theoretical framework and the logic of inter-temporal optimization, in particular see Equation (18) and surrounding discussion. The increase in payments for the 10% arm is the expected treatment response. Overall, the net effect of the two ATEs is time-varying. Figure OA-21(f) shows net purchases (or equivalently the negative net pay-

[64] 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.

[65] 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 (see column (4) in Table OA-16).

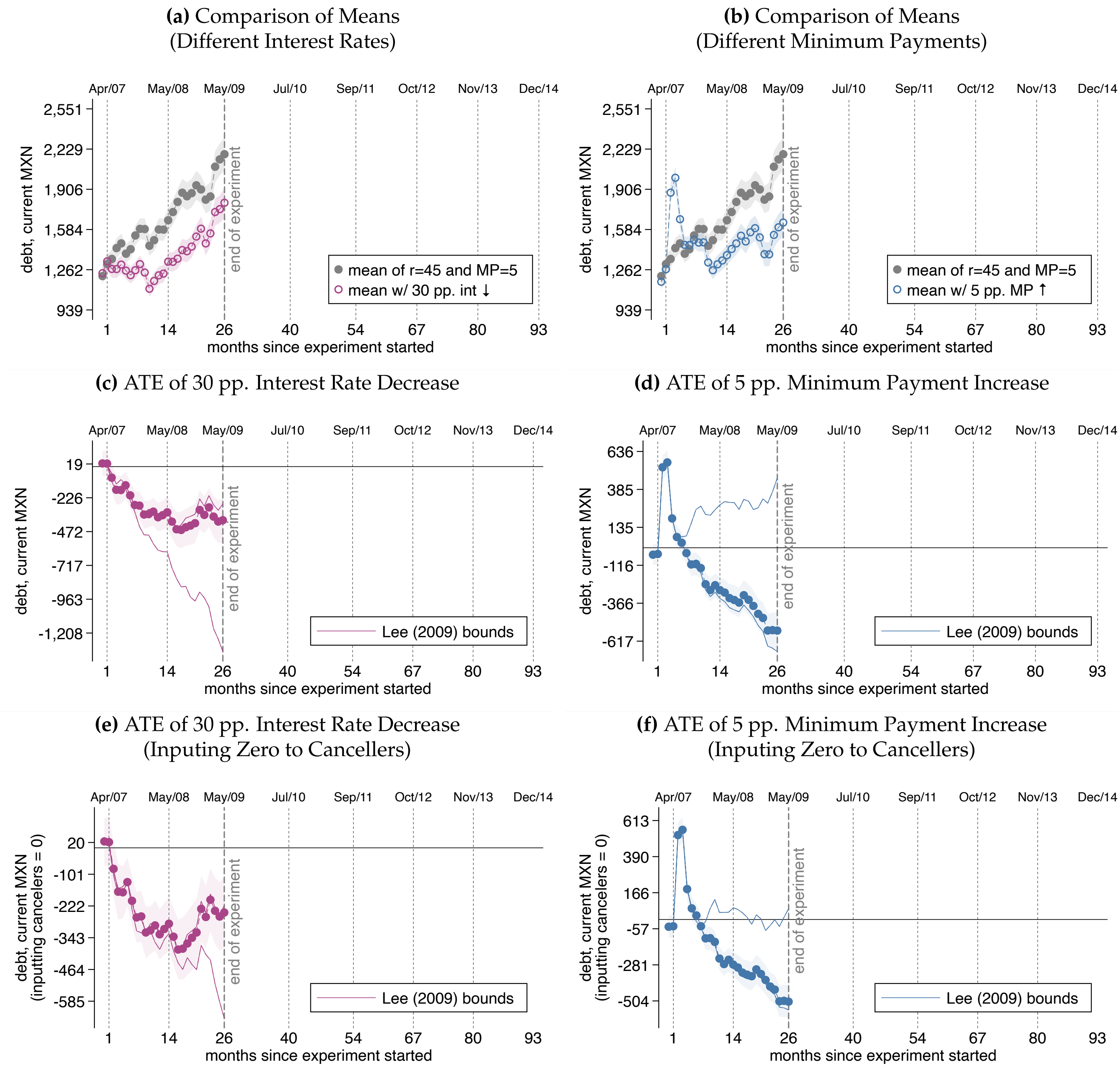
ments) falling steeply in months three and four before increasing and settling near about 30 pesos by about month 12 and hovering there for the remainder of the experiment. However, sample selection due to default is a serious concern after about month 9 and the Lee bounds are consistent with both substantive increases as well as decreases in net purchases by the end of the experiment.

Table OA-16: Debt ATEs

	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)
$(45\% - r_i)/30\%$	-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-18: Debt ATEs
(MXN Among Active Cards)



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.

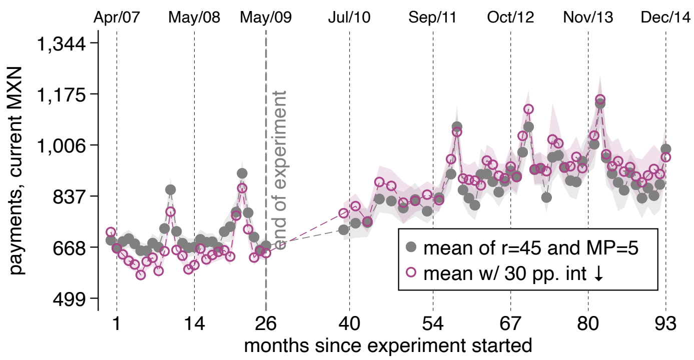
Table OA-17: Treatment Effects on Monthly Payments

	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]

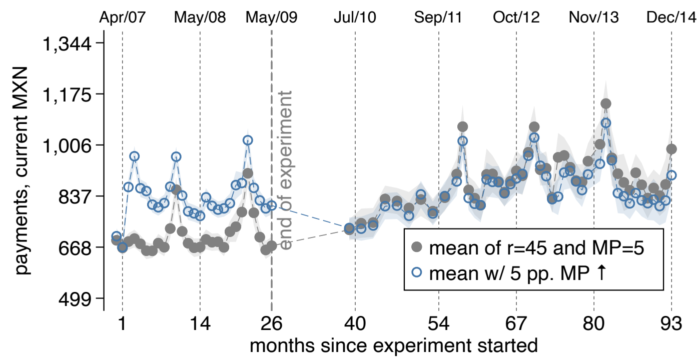
Notes: All regressions use sample weights. Each column is a different regression. The dependent variable is monthly payments. Columns (1), (3), (5), and (7) are estimated using outcomes 6 months after the start of the intervention and the remainder are for outcomes at the end of the experiment. Columns (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-19: Payments ATEs
(MXN Among Active Cards)

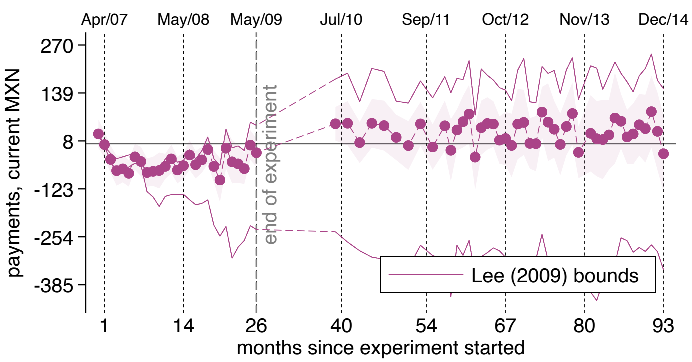
(a) Comparison of Means
(Different Interest Rates)



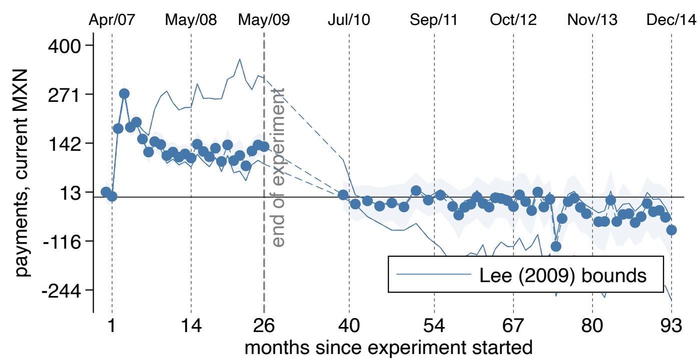
(b) Comparison of Means
(Different Minimum Payments)



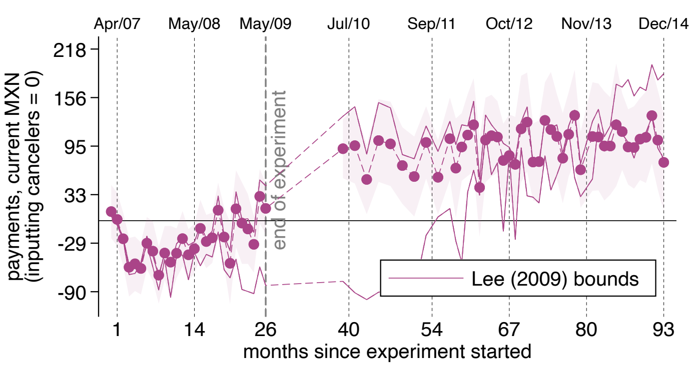
(c) ATE of 30 pp. Interest Rate Decrease



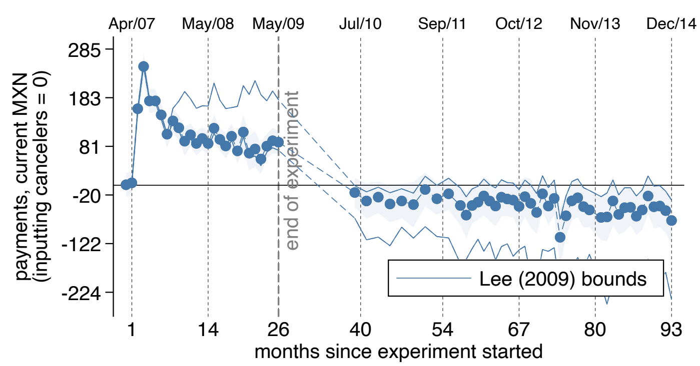
(d) ATE of 5 pp. Minimum Payment Increase



(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.

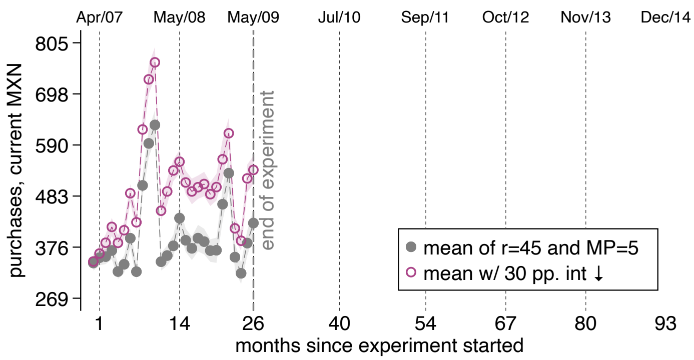
Table OA-18: Treatment Effects on Monthly Purchases

	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]

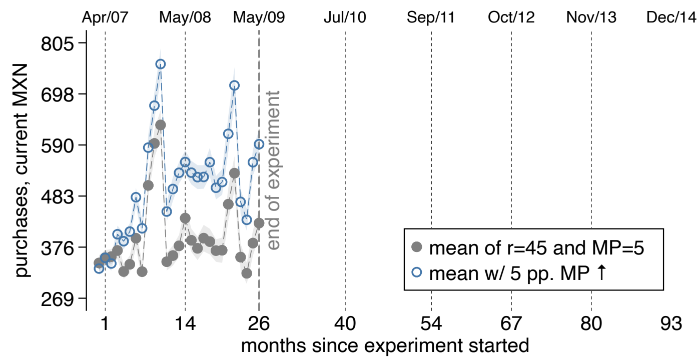
Notes: All regressions use sample weights. Each column is a different regression. The dependent variable is monthly purchases. Columns (1), (3), (5), and (7) are estimated using outcomes 6 months after the start of the intervention and the remainder are for outcomes at the end of the experiment. Columns (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-20: Purchases ATEs
(MXN Among Active Cards)

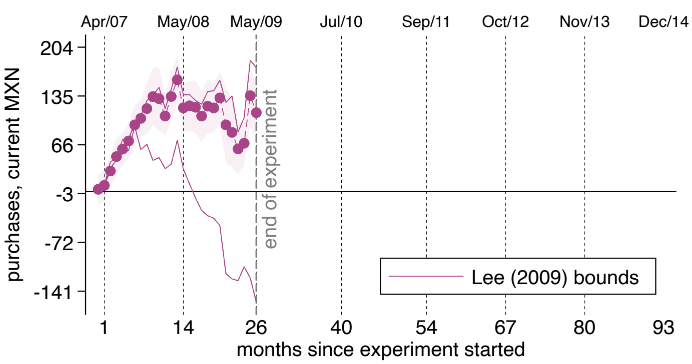
(a) Comparison of Means
(Different Interest Rates)



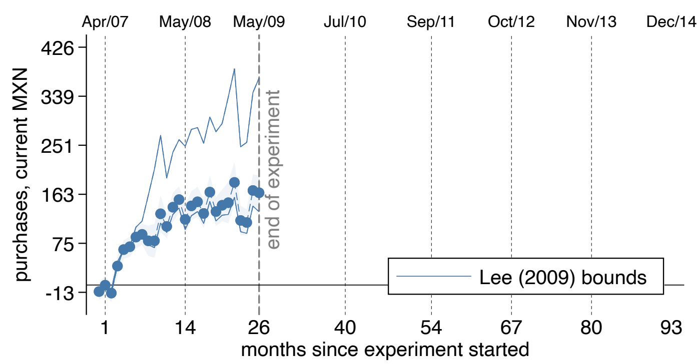
(b) Comparison of Means
(Different Minimum Payments)



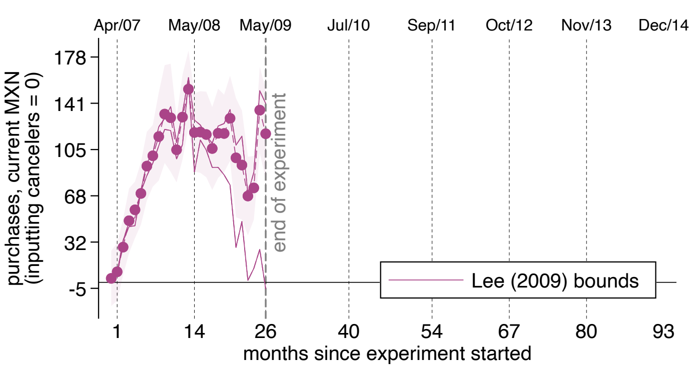
(c) ATE of 30 pp. Interest Rate Decrease



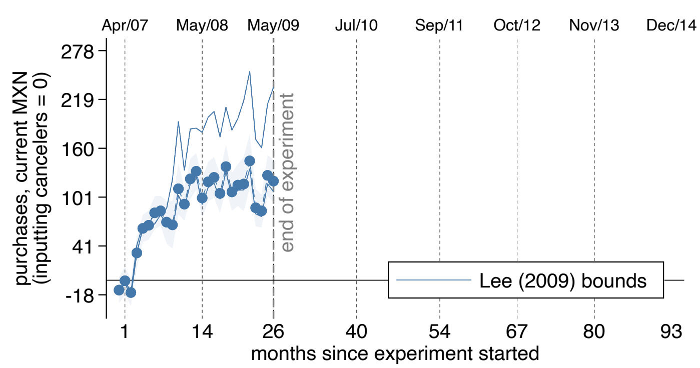
(d) ATE of 5 pp. Minimum Payment Increase



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



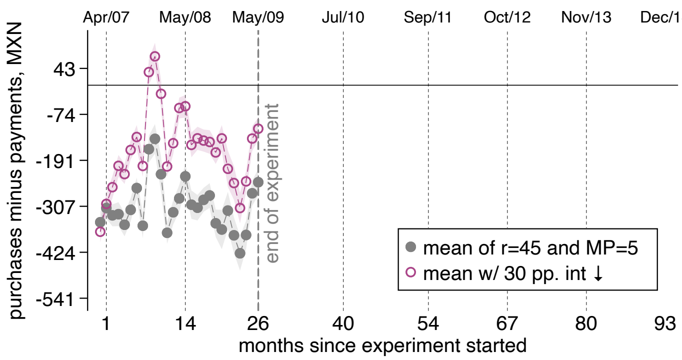
(f) ATE of 5 pp. Minimum Payment Increase
(Inputing Zero to Cancellers)



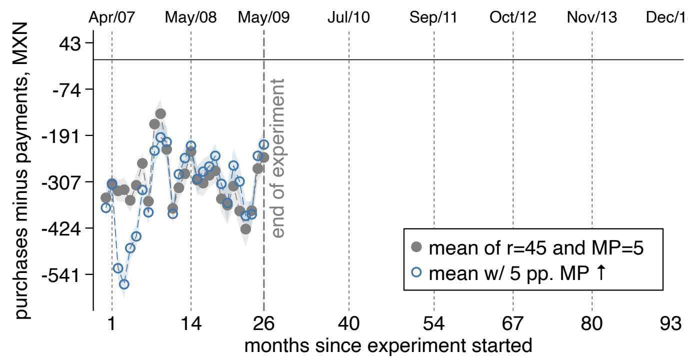
Notes: These figures plot the causal effect of interest rates and minimum payment changes on purchases in the experiment credit card. We only observe purchases in the experimental period. Figures on the left examine interest rate changes. Figures on the right examine minimum payment changes. The grey 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 post-experiment period.

Figure OA-21: Net Purchases (Purchases Minus Payments) ATEs
(MXN Among Active Cards)

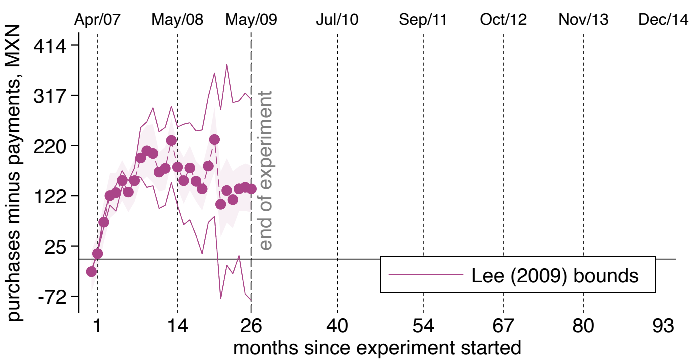
(a) Comparison of Means
(Different Interest Rates)



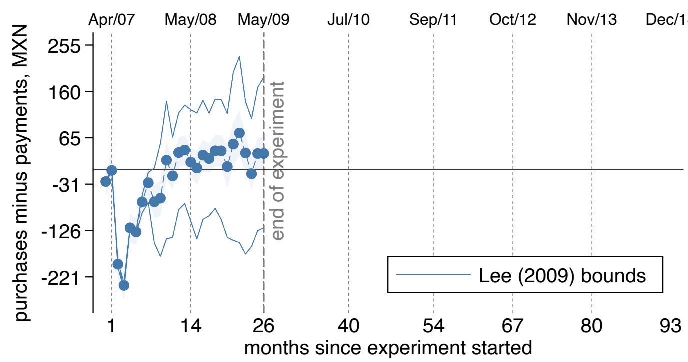
(b) Comparison of Means
(Different Minimum Payments)



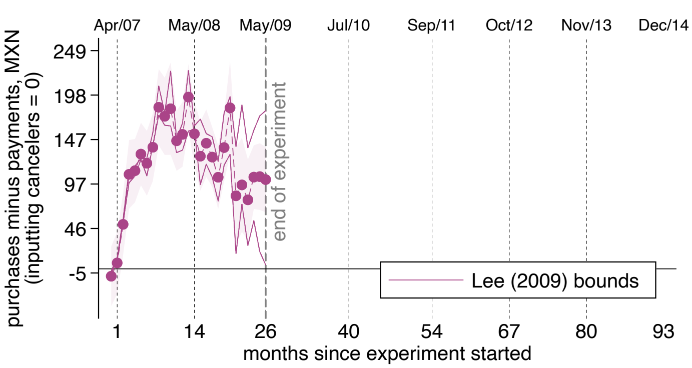
(c) ATE of 30 pp. Interest Rate Decrease



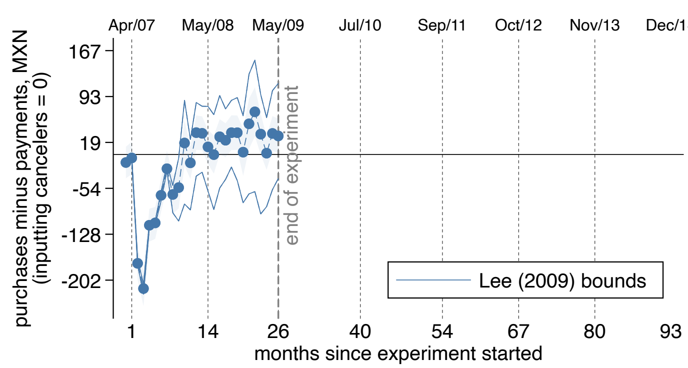
(d) ATE of 5 pp. Minimum Payment Increase



(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 net purchases in the experiment credit card. Net purchases are defined as the monthly purchases minus the monthly payments at the card level. 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 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, Panel (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 purchases for the periods post-experiment.

J Comparing Default From Contract Terms and Unemployment

In the main text we document three primary results. First, default rates are higher for new borrowers (compared to older ones). Second, default rates are only modestly affected by even substantial changes in contract terms.[66] Third, job displacement results in substantial default. We document a 7.6 pp. increase in default from job displacement which is substantially more than the 1.03 pp. increase in default (from a 30 pp. interest rate increase) over the same 18-month horizon.[67]

In this section we carry out a set of simple back-of-the-envelope calculations to compare the magnitudes of the three "shocks"—minimum payment increases, interest rate increases and unemployment—on a *normalized* basis by estimating their effects on cash flow. We do so in order to adjust for the fact that job displacement is likely a stronger "shock" than the "shock" from a thirty-point increase in interest rates or a doubling of the minimum payment. Our unique setting—evaluating the three shocks (two of them experimental) on the same large sample over a common time-period—enables a particularly compelling comparison.

We carry out the calculation in two steps. First, we estimate the effect of each "shock" (unemployment, interest rate increases, minimum payment increases) on debt servicing costs (or the corresponding reduction in free cash flow), as we explain in detail in Appendix J.1. Second, we divide the default ATE for each shock by the corresponding reduction in cash flow to arrive at a measure of the effect of each shock on default on a per-peso basis—that is the change in default per-peso change in cash flow (or debt servicing costs) arising from each shock—in Appendix J.2. We describe this in greater detail below.

J.1 Effect of Contract Terms and Unemployment on "Free Cash Flow"

J.1.1 Measuring the Effect of Contract Term Changes on "Free Cash Flow"

We use the minimum required payment to stay current (mpd_{it}), in pesos, as a measure of the burden on cash flow (or equivalently a measure of debt servicing costs). There are two important reasons for this: (a) minimum payment due includes interest but also fees and is a realistic measure of the money requirement to stay current, and (b) this measure is equally defined for all treatment arms.

The minimum payment intervention (i.e., the comparison between a 5% and 10% minimum payment) mechanically increases the required minimum payment in the short run (when debt is unchanged) by raising the fraction of the debt that needs to be repaid. In the longer run, as debt adjusts (either mechanically or due to borrower responses) then the required minimum payment could decrease. Increases in interest rates, to the extent that they increase debt (which they do as we document), will increase the required minimum payment.

Figure OA-22 shows the coefficient estimates for the treatment effects using our main regression specification Equation (1) with mpd_{it} as the dependent variable. The panels on the left show that the required minimum payment is increased for borrowers in the 45% interest rate group. As we argued above, the increase in debt (and hence the increase in debt servicing costs) is largely driven by the impact of the increased interest rate on previously accumulated debt. Thus, in contexts where accumulated debt is higher one can expect correspondingly larger increases in debt servicing costs. The panels on the right show that the debt servicing costs are

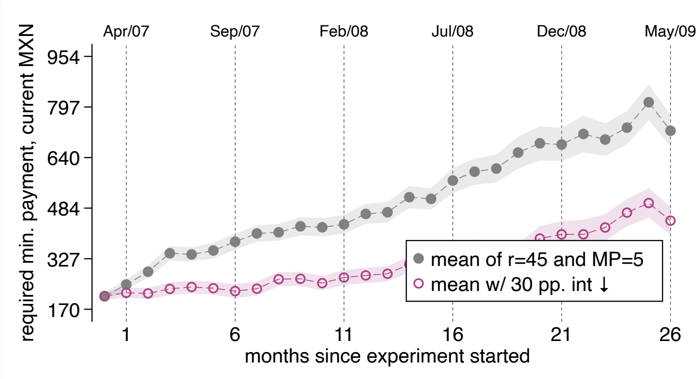
[66] The contract term changes appear to be on the upper-end of what is feasible in a policy sense.

[67] Coincident with our research, other researchers have examined related issues such as whether mortgagee default can be viewed as strategic or driven by negative life events. For instance, Ganong and Noel (2022) find that mortgage default (in the U.S.) is more likely to be driven by negative life events, which are inferred from bank account data.

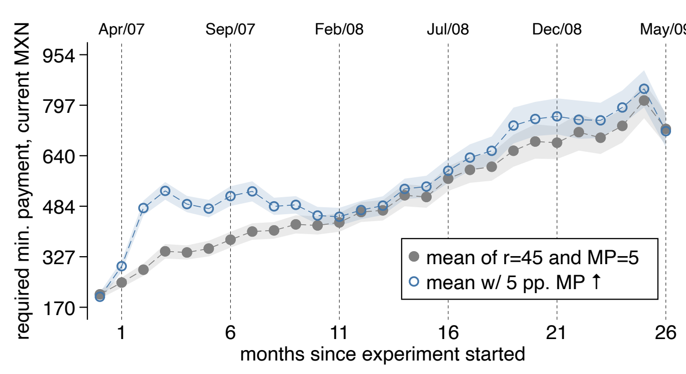
higher in the 10% minimum payment arm for approximately the first year following which they decline and are close to zero by the end of the experiment. This pattern is consistent with the hypothesis that servicing costs increase in the short run as debt is fixed and then decline in the longer run as debt declines (this is also consistent with the debt treatment effects documented in Figure OA-18 modulo the fee increases in the very short term).

Figure OA-22: Treatment Effect of Contract Terms on Required Minimum Payments (mpd)

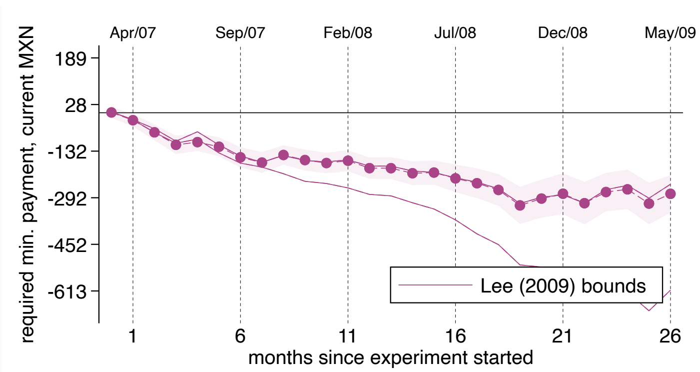
(a) Required Minimum Payments (mpd), by Interest Rates



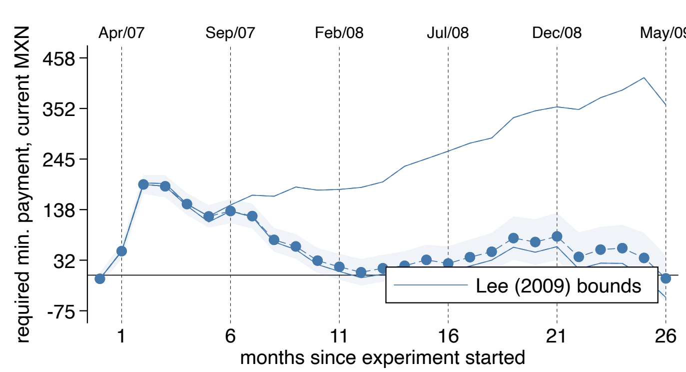
(b) Required Minimum Payments (mpd), by Minimum Payments



(c) mpd ATE of 30 pp. Interest Rate Decrease



(d) mpd ATE of 5 pp. Minimum Payment Increase



Notes: These figures plot the causal effect of interest rates and minimum payment changes on the required minimum 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 required minimum 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 required minimum 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 construct a measure of the 18-month change in mpd to facilitate comparisons with the unemployment shocks, which are measured at 18 months. To do so, we sum all of the estimated treatment effects for the mpd_{it}—i.e., estimates of the parameters ($\sum_{t=1}^{18} \beta_t, \sum_{t=1}^{18} \gamma_t$) from Equation (2) in the main paper. These are intended to capture the 18 month change in debt servicing costs as a result of each intervention (i.e., a 5 pp. increase in minimum payments or a 30 pp. increase in interest rates). The estimates are shown in Table OA-19. For the interest rate intervention, we find that over 18 months the required minimum payments were (in total) $2,917 MXN lower for the 15% arm relative to the 45% arm. For the minimum payments, the required minimum payment was $1,325 MXN higher in the 10% arm relative to the 5% arm. These figures are relatively small relative to borrower income over this period (average monthly income at baseline for those formally employed was $ 13,855MXN pesos)—it is reasonable to assume that default effects may be higher in contexts where the cash flow effects are larger. We also present the implied Lee (2009) lower- and upper-bound estimates (by summing the period-by-period coefficients) to account for selection and investigate in Appendix J.5 how our per-peso default estimates change under these cases.

Table OA-19: Estimates of the Effect of Contract Term Changes on Cash Flow
March 2007 to September 2008

	Summed coefficients (1)	Estimation results (2)	Lee (2009) bounds (3)
$(45\% - r_i)/30\%$	$\sum_{t\leq 18} \gamma_t$	-2917	[-4173, -2805]
		(82)	
$\mathbb{1}\{MP_i = 10\%\}$	$\sum_{t\leq 18} \beta_t$	1325	[1167, 3421]
		(61)	
Constant	$\sum_{t\leq 18} \alpha_t$	7996	-
		(64)	

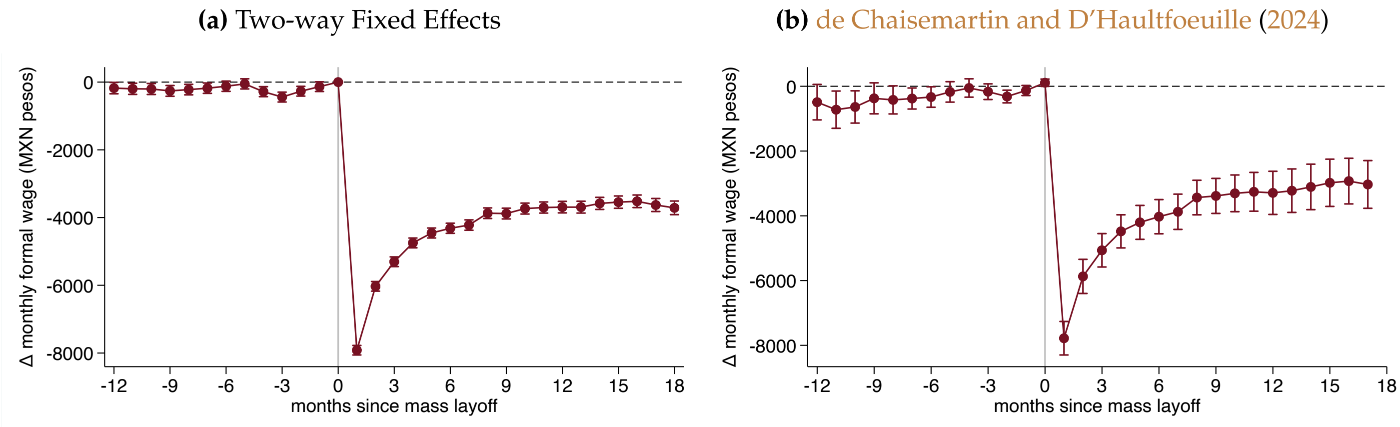
Notes: These figures present our estimates for the effect of the contract terms on the free cash flow 18 months after the start of the experiment. We use the main specification in the paper, Equation (1), regressing the required minimum payment in pesos in the (month-by-month). We then sum the month-by-month coefficients and calculate standard errors via the delta method. Similarly, we compute the Lee (2009) month-by-month and compute the sum of the bounds.

J.1.2 Measuring the Effect of Unemployment on "Free Cash Flow"

We carry out this exercise in two steps. First, we compute the loss in *formal sector earnings* arising from a displacement shock using the same econometric design as in eq. (3). We then adjust this figure by an estimate of the share of formal earnings replaced in the *informal sector* to arrive at the change in *total* earnings due to formal job loss.

We begin by computing the change in formal sector earnings over the 18 months following unemployment. This is given by the sum of the month-by-month coefficient estimates (from month 1 to month 18) in Panel (a) of Figure OA-23 and is equal to $77,555 MXN pesos. For comparison, the numbers are similar if we were to use the de Chaisemartin and D'Haultfoeuille (2024) point estimates ($70,288 MXN pesos) shown in the figure.

Figure OA-23: Effect of Job Displacement on Formal Sector Income



Notes: This figure plots the two-way fixed effects estimates of the effect of being displaced by a mass layoff on formal sector income. The figures are estimated using eq. (3) and the methodology by de Chaisemartin and D'Haultfoeuille (2024) but using monthly formal sector earnings (among formal sector workers) as a dependent variable.

Next, we compute the change in *total earnings (including informal earnings)* due to formal sector unemployment. This is particularly important in our setting with a large informal sector (approximately half of the Mexican labor force is informally employed) and frequent movement between the informal and formal sectors (see e.g., Donovan et al., 2023, for a recent discussion). Since the IMSS does not contain information on informal earnings we turn to Mexico's official labor force survey, the ENOE (for the period 2005-2015). The ENOE follows workers over five quarters (as a rotating panel) and tracks both informal as well as formal employment and earnings. Using this data, we estimate that 82% of workers who lose formal employment in a given quarter are informally employed in the next quarter. Hence, it is important to account for informal earnings in any analysis of the effect of formal sector unemployment on total earnings.

Based on the survey rounds close to our sample period (2005-2015), we construct a sample of workers we can follow before and after losing a formal job. We implement the selection steps described in Table OA-20. As explained, we defined "treated" individuals as those who were formally employed (i.e. had a IMSS registered job) in the first two waves of the ENOE and lost their formal (IMSS) job in the third wave. To the extent that the involuntarily unemployed are less likely to find informal employment, our correction will over-estimate the cushioning effect of informal employment.[68] We define as "control" the individuals who were formally employed throughout the five waves where they are interviewed. Given this sample, event 0 corresponds to wave 3 for each respondent who lost their job. The sampling restrictions leave us with 208,185 workers, each followed in five waves of the ENOE.

[68] We cannot implement the mass layoff approach with the ENOE data for three reasons. First, the ENOE lacks firm identifiers needed to construct layoff events. Second, while it collects self-reported reasons for job separation, non-response is high (43%) and correlated with observables. Finally, social desirability concerns (Krumpal, 2013) likely affect reporting accuracy, and we cannot independently verify these self-reports or definitively link them to specific job loss events.

Table OA-20: ENOE Data Processing Steps

	Description	Worker-Quarters
0	Original sample	17,965,475
1	Drop respondents of age < 18	6,004,204
2	Drop agriculture workers	693,910
3	Keep people observed in all 5 quarters	7,747,241
4	Define treated individual = 1 if IMSS = 1 in waves 1 and 2, not in wave 3	-
5	Define treated individual = 0 if IMSS = 1 in all 5 waves	-
6	Keep those individuals for whom treated is defined	2,479,195
	Final sample (worker-quarters)	1,040,925
	Workers in final sample	208,185

In order to estimate the effect of formal sector unemployment on overall earnings (i.e., formal and informal earnings) we estimate the following regression equation via OLS:

$$Y_{it} = \alpha_i + \theta_{mq} + \beta T_{it} + \varepsilon_{it} \tag{40}$$

where α_i are respondent fixed effects, θ_{mq} are municipality $\times$ quarter fixed effects, T_{it} is an indicator equal to 1 when the formal sector worker lost her formal job (which, given our sample construction only happens on wave 3) and remains equal to one for all subsequent periods (i.e., for quarters 4 and 5). So, β measures the change in outcome Y_{it} comparing workers who lost their job in the 3rd quarter vs those who did not, after partialling out the specified regressors. We examine three outcomes: (1) an indicator equal to one if the respondent reports having a job (formal or informal) in period t; (2) average monthly earnings from formal (IMSS) employment per month for that quarter; and (3) average earnings per month from all sources (formal and informal) for that quarter. Respondents report earnings stating the frequency of payments which Mexico's statistical agency converts to a monthly figure.

Table OA-21 presents the results. Panel (a), Column (1) shows that losing a formal job has an effect of only 24 pp on the likelihood of being employed in any sector (formal or informal) of the three subsequent quarters. A large majority of workers find informal employment almost immediately after. Figure OA-24 shows the associated event-studies disaggregating employment into formal and informal work. Panel (a) is zero before losing the formal job and equals minus one right after losing it by construction. About 60% of workers go back to the formal sector the quarter after. Panel (b) shows that more than 80% of workers who lost a formal job are already working on an informal job one quarter after losing the formal job.

Given the transitions of formal to informal employment, we estimate how much of the formal sector income is replaced in the informal sector. Column (2) of Table OA-21 shows that monthly income from formal employment decreases by $5,651 MXN in the subsequent three quarters after losing formal employment (about 72% of their previous formal monthly earnings). It does not fall by 100% because some workers obtain formal employment within the next 3 quarters. Column (3) shows that total earnings (the quantity we most care about) fall by $1,555 MXN, substantially less than formal earnings. That is, the fall in total earnings is just 27.5% of the fall in formal earnings (1555/5651). Using this 27.5% figure as a scaling factor we infer that the fall in total earnings from job displacement is $21,328MXN (i.e., 0.275×77555). We also verify that our results are robust to using the methods proposed in Borusyak et al. (2024); de Chaisemartin and D'Haultfoeuille (2024) to estimate displacement effects.

We calculate the above numbers using the standard population weights from the ENOE. These numbers are designed to make the sample representative of all workers in the country. To assess whether the scaling factor would be different for a sample resembling the experimental sample, we re-weight observations to match key demographics of the experimental sample. Panel B of Table OA-21 shows that the scaling factor would remain very similar at 27.3%.

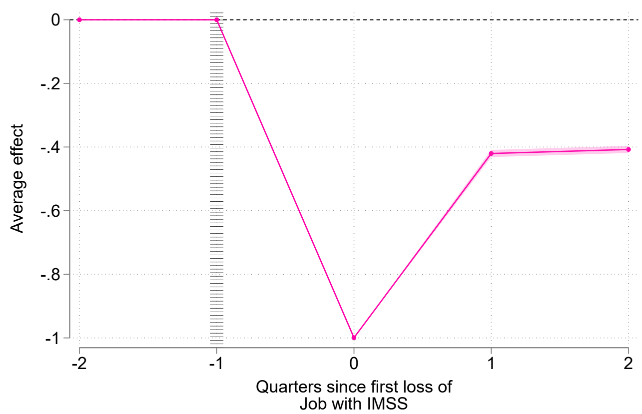
Table OA-21: Effects of Losing IMSS Job on Formal and Informal Sector Earnings

	(1) Employed	(2) Income from IMSS	(3) Any Income
Panel A. Results using population weights			
IMSS job loss	-0.241***	-5651***	-1555***
	(0.006)	(80)	(107)
Panel B. Results reweighting observations to match experimental sample			
IMSS job loss	-0.216***	-6094***	-1664***
	(0.004)	(63)	(79)
Worker FEs	✓	✓	✓
Municipality × Quarter FEs	✓	✓	✓
Wave 2 dep. var. (e.g., pre-treatment) mean for treated workers	1	7785	7785

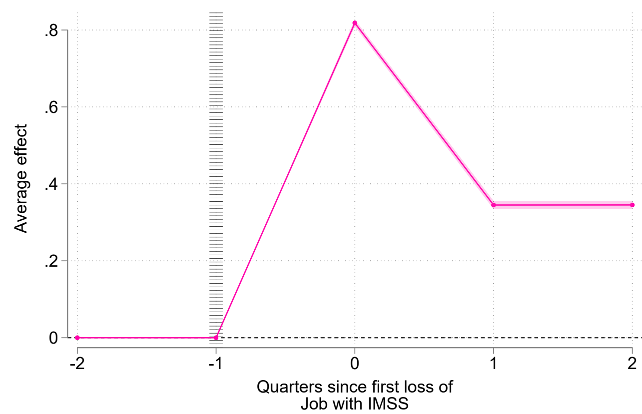
Notes: This table uses Mexico's official employment survey (ENOE) to calculate the income loss among formal sector workers. We create a panel of adult non-agricultural sector workers that were consecutively surveyed for 5 months and define "treated" and "control" workers to capture the effect of losing a formal IMSS job as explained in Table OA-20. The main independent variable is an indicator equal to one in all subsequent periods after a worker loses their IMSS job. The dependent variable in Column (1) is an indicator equal to one if the respondent reports having a job in period t. In Column (2) the dependent variable measures income as formal (IMSS) employee per month for that quarter. In column (3), we measure all work income as an employee from formal and informal jobs. Income is measured as monthly income. Panel A uses the standard population weights provided by the survey. In Panel B, we reweight observations within age, marriage status and gender deciles to match the experimental sample proportions.

Figure OA-24: Event Study - Effects on Labor conditions

(a) Has formal job



(b) Has informal job



Notes: This table uses Mexico's official employment survey (ENOE) to calculate the likelihood of being employed (formally and informally) after losing their (formal) job. We create a panel of adult non-agricultural sector workers that were consecutively surveyed for 5 months and define "treated" and "control" workers to capture the effect of losing a formal IMSS job as explained in Table OA-20. The main independent variable is an indicator equal to one in all subsequent periods after a worker loses their IMSS job, interacted with quarters since first job loss dummies. We omit the coefficient one period before the first loss. The dependent variable in Panel (a) is an indicator equal to one if the respondent reports having a job in period t in the formal sector. In Panel (b) is an indicator equal to one if the respondent reports having a job in period t in the informal sector.

J.2 Effect of Contract Terms and Unemployment on Default per-Peso Change in "Free Cash Flow"

J.2.1 Effect of Contract Term Changes

Roughly speaking, we arrive at the per-peso effect of each intervention by dividing the effect on default (of each intervention) by the effect on debt servicing costs (of the same intervention). Concretely, and following Equation (1), we define:

$$\begin{aligned}\text{default}_{it} &= \alpha_t + \beta_t \cdot \mathbb{1}\{MP_i = 10\%\} + \gamma_t \cdot (45\% - r_i)/30\% + \varepsilon_{it} \\ mpd_{it} &= \rho_t + \mu_t \cdot \mathbb{1}\{MP_i = 10\%\} + \kappa_t \cdot (45\% - r_i)/30\% + \nu_{it}\end{aligned}$$

and

$$\lambda_t^{IR} \equiv \frac{\gamma_t}{\sum_{\tau \le t} \kappa_\tau} \qquad \text{(normalized effect of interest rate changes)}$$

$$\lambda_t^{MP} \equiv \frac{\beta_t}{\sum_{\tau \le t} \mu_\tau} \qquad \text{(normalized effect of minimum payment changes)}$$

where (γ_t, β_t) are defined as in the main specification of the paper using cumulative default as the dependent variable. Both the denominator and the numerator in each of the $\lambda_t^{\cdot}$ terms are defined cumulatively (since we measure default cumulatively) and we impose no exclusion restrictions. We scale both the numerator and the denominator so that the ratio can be interpreted as percentage points per thousand pesos of additional required minimum payments. We do not interpret our estimates of $\lambda_t^{\cdot}$ as instrumental variable estimates of the effect of debt-servicing costs on default. This is because we do not need such an interpretation in order to compare the per-peso effects of each intervention.

Table OA-22 presents estimates for the $\lambda_{18}^{\cdot}$ parameters (i.e., measured 18 months after the treatment started), revealing that a 1,000 peso increase in minimum payments due from a 30 pp. increase in interest rates is associated with a 0.36 pp. increase in default. Analogously, a 1,000 peso increase in minimum payments due from a 5 pp. increase in the minimum payment required is associated with a 0.51 pp. increase in default. Statistically, the two estimates are indistinguishable so we conclude that both contract-term interventions had similar per-peso effects on default.

J.2.2 Effect of Formal Job Loss

We estimate the per-peso effect of formal job loss on default by dividing of the effect of formal job loss on default (7.6 pp. as shown in Figure 9(a) and explained in the main body of the paper) by the effect of formal job loss on total earnings. As explained in Appendix J.1.2, we estimate the loss of total earnings to be $21,343 MXN.[69] We then estimate $\hat{\lambda}_{18}^{U} = 7.66/(21343) \times 1000 \approx 0.36$. Thus, a 1000 peso decline in "free cash flow" due to displacement is associated with a 0.36 pp. increase in default.

To compare the per-peso unemployment effect estimate with those of contract terms, we estimate joint coavariance matrices for the three coefficients $(\lambda_{18}^{U}, \lambda_{18}^{MP}, \lambda_{18}^{IR})$ via bootstrap with 1,000 repetitions using a stratified sampling at the strata $\times$ treatment level. We then use these to compute the Wald estimator of the three effects being equal to each other . We cannot reject the null that the three coefficients have the same size (the p-value is 0.78), so statistically all interventions had the same per-peso effects on default.

Table OA-22: Comparison of Normalized Treatment Effects
(September 2008 - 18 months)

	Per-Peso Effect on Default
Interest rate effect (λ_{18}^{IR})	0.356
	(0.132)
Minimum payment effect (λ_{18}^{MP})	0.502
	(0.208)
Unemployment effect (λ_{18}^{U})	0.390
	(0.098)
H_0 : contract terms have the same size (p-value)	0.559
H_0 : all treatment effects have the same size (p-value)	0.786

Notes: The table presents estimates of $\lambda_{18}^{\cdot}$ which is defined in Appendix J.2.1 for the contract term changes and Appendix J.2.2 for unemployment. Estimates are standardized to be read as pp. per thousand peso increments (i.e., multiplied by 1000). Standard errors are computed by bootstrap. We compute p-values for $\lambda_{18}^{MP} = \lambda_{18}^{IR}$ (i.e., contract terms have the same size) and $\lambda_{18}^{U} = \lambda_{18}^{MP} = \lambda_{18}^{IR}$ (i.e., all treatment effects have the same size).

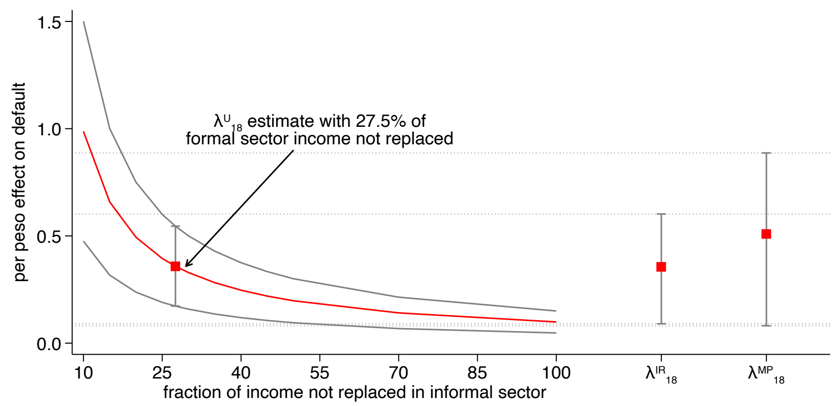
J.3 Robustness to Alternative Estimates of Informal Sector Earnings

Our results on the similarity of the per-peso effects of unemployment and the contract term changes are robust to a range of alternative assumptions about the fraction of income that is not replaced in the informal sector. The red line in Figure OA-25 plots the possible values for the per peso displacement effect on default for alternative values of the fraction of income that is not replaced in the informal sector, along with confidence intervals. Here, the red line is contained in our confidence interval for the per-peso effect of the 30 pp. interest rate decreases

[69] As noted above, we compute this by scaling formal sector earnings using an estimated adjustment factor from the ENOE.

from 100% replacement to about 25% replacement. The red line is also contained for the per-peso effect of the 5 pp. increase in minimum payments from 100% to about 17% of replacement.

Figure OA-25: Comparison of Effect Sizes After Normalizing by Change in Cash Flow



Notes: This figure plots the comparison of the effect of the different shocks on default. Standard errors and confidence intervals are obtained via bootstrap. The per peso unemployment effect is explained above and here is computed under alternative scenarios of the fraction of income that is not replaced in the informal sector.

Finally, as a sanity check we compared our new estimates of earning losses due to job loss once we account for the informal sector to previously published estimates (focusing on the earning losses the first twelve months in both). Table OA-23 shows that for Denmark, job displacement entails a 16% earning loss (at the 12 month mark) while the comparable figure for the US is typically in the mid-30% range. If we do not take into account all earnings—formal and informal—we would conclude for our sample that earnings decline by 61% in a 12-month period, a number that is much larger than previous estimates.

Table OA-23: Comparison of Income Loss with Other Estimates in Literature

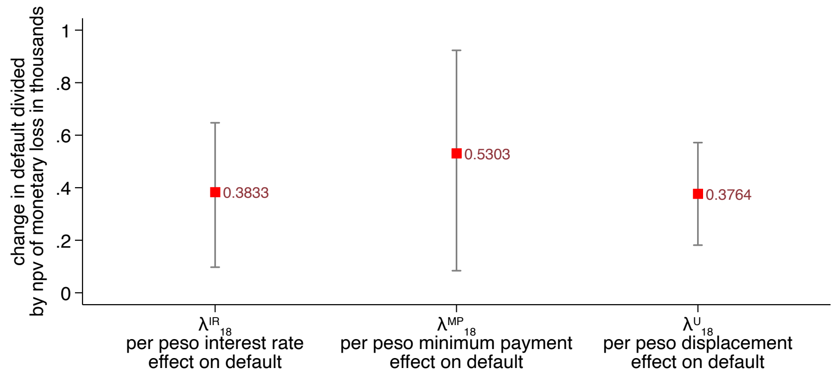
	Country and Time	12-Month Income Loss (%)
Our estimate with 27.5% of income not replaced	Mexico, 2000s	17
Couch and Placzek (2010b)	US, CT, 90s	32-33
Davis and Von Wachter (2011)	National US, 90s	35
Humlum et al. (2023)	Denmark, 2010s	16
Jacobson et al. (1993)	US, PA, 90s	26-40

Notes: This table summarizes many of the estimates available in the literature of the income loss in the first year after a mass layoff. The number from Couch and Placzek (2010b) is taken from the third paragraph of page 572. The number from Davis and Von Wachter (2011) is taken from Table 1 of page 22. The number from Humlum et al. (2023) is taken from page 81 of the working paper version. The number from Jacobson et al. (1993) is taken from Table 1 of Couch and Placzek (2010b).

J.4 Robustnesss to Discounting Monetary Variables

We repeat our estimation exercise but discounting monetary variables (i.e., mpd in the case of the contract term changes and the total earnings lost because of job displacement) using the 28-day interbank lending rate (TIIE for its acronym in Spanish) from the Mexican Central Bank website. During this period, annual rates ranged between 7.5% and 8%. The cash flow shocks (i.e., the denominators of all three λ_{18} estimates) would be slightly smaller, and the overall per-peso effects would be marginally larger. However, the comparison between the three shocks would remain the same. For example, the λ_{18}^{IR} we report in the paper is 0.36, while the one we would obtain with discounting is 0.38.

Figure OA-26: Effect Sizes After Normalizing Default / NPV of Cash Flow Shock

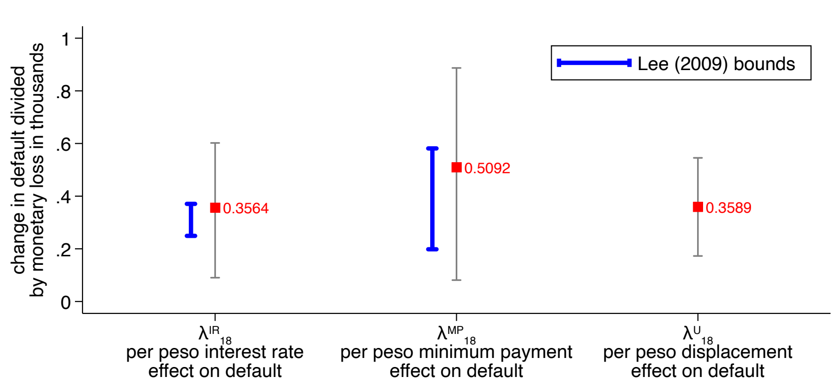


This figure compares the per-peso effect on default from three shocks. Standard errors were obtained via bootstrap. To compare the per-peso unemployment effect estimate with those of contract terms we estimate the joint covariance matrix for $(\lambda_{18}^{U}, \lambda_{18}^{MP}, \lambda_{18}^{IR})$ using the bootstrap with 1,000 repetitions using stratified sampling at the strata $\times$ treatment level.

J.5 Robustness to Selection

Figure OA-27 below plots how the point estimates for λ_{18}^{MP} and λ_{18}^{IR} change when accounting for attrition using the Lee (2009) bound estimates from Table OA-19. To compute the thick blue lines, we divide the point estimate of default for each average treatment effect by the end points of the bounds. We find that $\lambda_{18}^{IR} \in [0.249, 0.371]$ and $\lambda_{18}^{MP} \in [0.198, 0.581]$.

Figure OA-27: Effect Sizes After Normalizing Default / Cash Flow Shock



This figure compares the per-peso effect on default from three shocks. Standard errors in thin grey lines were obtained via bootstrap. To compare the per-peso unemployment effect estimate with those of contract terms we estimate the joint covariance matrix for $(\lambda_{18}^{U}, \lambda_{18}^{MP}, \lambda_{18}^{IR})$ using the bootstrap with 1,000 repetitions using stratified sampling at the strata $\times$ treatment level.