

# Contract Terms, Employment Shocks, and Default in Credit Cards\*

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## Abstract

The tension between limiting default rates and expanding financial access in developing countries is particularly acute for credit card borrowing, which is increasingly how borrowers access formal credit. Concerns over high default rates have led to contract-term restrictions such as higher minimum payments and interest rate ceilings, despite limited evidence on their effectiveness. We use a nationwide RCT to study new borrower responses to large experimental contract-term changes for a card that accounted for 15% of all first-time formal loans in Mexico. We find default is high and unresponsive to even large interest rate declines for the newest borrowers, and that a doubling of the required minimum payment does not reduce default. Matching the experimental subjects to their administrative employment histories, we find that unemployment shocks are common for newer borrowers and that plausibly exogenous job separation shocks have large effects on default.

**Keywords:** unemployment; credit default; minimum payment; interest rates; financial inclusion; moral hazard; credit cards.

**JEL:** O16, G21, D14, D82.

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# 1 Introduction

Policymakers in developing countries face two imperatives that are potentially in tension with each other. On the one hand, limiting default in credit markets is viewed as key to financial stability. On the other hand, expanding formal credit to underserved populations is increasingly seen as critical for growth and welfare.<sup>1</sup> The tension arises from the observation that default rates are typically higher for such populations.

This tension is particularly evident with credit-card borrowing, increasingly the most common way for new borrowers to access formal credit in many countries. For instance, in Mexico, it was 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.<sup>2</sup> This growth in card borrowing among financially inexperienced populations has been accompanied by increasing concerns among policymakers about card default. Such concerns have led to legal mandates restricting credit card contract terms (e.g., a floor on minimum payments or interest rate caps) in many countries, including Canada, Chile, Mexico, Taiwan, Turkey, and Indonesia.<sup>3</sup>

Despite its growing role in expanding credit access, greater policy scrutiny, and regulation, credit card borrowing in developing countries remains relatively understudied—particularly relative to other recent approaches to expanding credit access (e.g., micro-finance).<sup>4</sup> Perhaps as a result, policy discussions lack a coherent theoretical underpinning and credible empirical evidence on both the determinants of high default rates for new borrowers and the effectiveness of policy alternatives. For instance, interest rate regulations to limit credit card default typically presume strategic considerations. In contrast, default explanations based on adverse life events suggest a stronger role for broader social protection programs. The extant literature on credit cards in developing countries is silent on these issues.

In this paper, we provide a simple clarifying framework that makes explicit the assumptions needed for commonly proposed policies to reduce default risk as intended by policymakers. Second, we provide experimental evidence on the effectiveness of contract terms in limiting default among new borrowers.

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<sup>1</sup>A growing literature documents the causal link between financial development and improved economic outcomes (see, e.g., [Bruhn and Love, 2014](#); [Burgess and Pande, 2005](#), for India and Mexico, respectively). At the same time, a substantial fraction of the world’s population lacks access to formal financial services, including formal credit. [Banerjee and Duflo \(2010\)](#) report that only 6% of the funds borrowed by the poor (in a survey across 13 countries) come from formal sources. The World Bank estimates that 60% of adults in developing countries do not use *any* formal financial services (see, e.g., [Demirgüç-Kunt and Klapper, 2012](#); [World Bank, 2017](#)).

<sup>2</sup>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.

<sup>3</sup>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](#)). Turkey and Indonesia have both mandated interest rate ceilings for credit cards. See, e.g., [Web Archive Link](#) for Turkey and [Web Archive Link](#) for Indonesia. See also [Cuesta and Sepulveda \(2023\)](#); [Nelson \(2020\)](#).

<sup>4</sup>As context, there were approximately 2.3 million micro-finance clients in Mexico in 2009, while the single credit card we study, targeted at borrowers with non-existent or limited credit histories, had 1.3 million customers at the time ([Pedroza, 2010](#), and authors’ calculations).

Finally, we show that a plausibly exogenous adverse life event—job separation during a mass layoff—substantially affects credit card default.

We study credit card use and default among borrowers with limited or no formal credit histories in Mexico. Default rates for this population are high—in our sample, the newest borrowers (those who had been customers of the bank for only 6–11 months) defaulted at almost twice the rate (36% versus 18%) as those with the longest tenure (those who had been with the bank for at least two years). These rates have been a persistent concern for regulators (as noted in [Banco de Mexico, 2008, 2009, 2010](#)) concerned about lender risk and borrowers’ future access to formal credit.<sup>5</sup> In fact, the randomized experiment studied in this paper arose directly from central bank concerns about default among new borrowers, whose perceived economic vulnerability provided additional reasons for policy and academic interest.

We begin by providing a simple optimizing framework to interpret the subsequent estimated effects of contract terms and unemployment. We then estimate the causal effect of contract terms on card default using a randomized experiment by our partner bank (henceforth Bank A) on a credit card targeted at new borrowers (henceforth the study card or Card A) that accounted for approximately 15% of all first-time formal sector loan products in the country. The sizeable nationwide experiment allocated a large stratified random sample of 144,000 *pre-existing* study card borrowers to 8 treatment arms that varied annual interest rates between 15%, 25%, 35% and 45%, and monthly minimum payments between 5% and 10%, for 26 months, from March 2007 to May 2009. The substantial experimental variation and the large sample size allow for precise estimation of treatment effect heterogeneity over a range of contract terms and population strata (in our empirical results, we use three asterisks to denote significance at the 0.001 level). In addition, the sampling scheme ensures that the experimental results are representative of the bank’s national population of study card customers.

Turning to the results, reducing the annual interest rate from 45% to 15% has *no* effect on default for the newest borrowers over the 26-month experiment. This result is somewhat surprising, and we provide a rationale based on our theoretical framework. Evaluating the effect of policy-relevant rate changes (e.g., 10 pp.) also yields precisely estimated null effects. For the sample as a whole and the largest experimental rate increase (of 30 pp.), default decreased by 2.6 pp. on a base 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](#)).

Quantity restrictions, as implied by raising minimum payments, are another potential policy tool. As noted above, policymakers in many countries, worried that low minimum payments for new and inexperienced borrowers could increase default, have advocated raising minimum payments.<sup>6</sup> 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 a higher payment today, which may increase current default. Liquidity constraints may be particularly relevant

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<sup>5</sup>Similar concerns have been raised elsewhere. See e.g., [Black and Morgan \(1999\)](#); [Livshits \(2022\)](#) for the U.S.

<sup>6</sup>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/MkYbV0>). 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 Köszegi, 2016](#); [Heidhues and Köszegi, 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](#)).

as, at the start of the experiment, 73% of cardholders' monthly payments were less than 10% (of the amount due). On the other hand, higher minimum payments, *ceteris paribus*, tend to reduce future debt and decrease debt-burden-driven default in the longer run. We formalize these ideas by viewing them through the lens of the stylized model we develop in [Section 5](#). Strikingly, we find that doubling minimum payment does not reduce default during the experiment—the point estimate is a 0.8 pp. *increase* (the corresponding elasticity is +0.04). Consistent with the importance of liquidity constraints, the increase in default is entirely concentrated among borrowers with high levels of baseline debt utilization.

The considerable variation in contract terms could have affected borrower behavior with other lenders. For instance, higher minimum payments could have led to borrowers substituting away to other lenders. We match our study sample to credit bureau data and find that the interventions did not affect default across other formal lenders. In addition, we find no evidence of crowd-out (or crowd-in) of borrowing from other lenders. These results are true both during the experiment and three years after it ended, consistent with [Angelucci et al. \(2015\)](#) and [Karlan and Zinman \(2019\)](#).

The estimated default responses to contract term changes are at variance with predictions we elicited from five senior Mexican officials and on the Social Science Prediction Platform (SSPP,  $n=72$ ). For instance, officials predicted an 8.6 pp. decrease in default from a 30 pp. interest rate decrease over an 18-month horizon (compared to the estimated ATE of 1.03 pp.). Likewise, SSPP respondents predicted default ATEs that were substantially larger than the estimated experimental ATEs. The difference between expert predictions and the experimental results leaves open the question of what other actions might reduce default, and perhaps just as importantly, suggests that an improved quantitative understanding of the economic forces that shape default is needed.

In the paper's final section, we turn to this issue and find that, in contrast to the contract-term changes, default is responsive to unemployment shocks. We match the experimental sample to its monthly employment histories in the Mexican Social Security database (the Instituto Mexicano del Seguro Social or IMSS) and use mass layoffs as a measure of involuntary separation.<sup>7</sup> Job loss is common: of those employed at least one month in the formal sector between January 2004 and March 2007 (i.e., prior to the experiment, 45% of our sample), 43% experienced at least one month out of formal employment. Newer borrowers are more likely to experience unemployment: those who had the study card for less than a year before the experiment are 1.34 times more likely to be unemployed than those who had the card for more than two years. Using an event-study design, we estimate that displacement (i.e., job loss that occurs as part of a mass layoff) leads to a 6.1 pp. increase in the probability of default on the study card in the next eighteen months—a third of the mean default rate of 19%. These magnitudes are substantial and consistent with the hypothesis that new borrowers are vulnerable to large shocks that precipitate default. We repeat the exercise using a much larger nationally representative sample of one million borrowers and find similar results. To our knowledge, these are the first estimates of the effects of formal sector job loss on default in a developing country.

We draw three lessons from these results. First, despite the regulatory emphasis on increasing minimum payments to protect inexperienced borrowers, they are ineffective at reducing contemporaneous

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<sup>7</sup>We observe identifiers (CURPS) for 89% of our experimental sample and can locate (59%) of these in the IMSS data. See [Section 2](#) for more details.

default in our setting, even over 26 months. Second, ex-post (i.e., conditional on selection) decreases in interest rates do little to mitigate default among our sample of pre-existing new borrowers (in contrast to models of interest-rate-driven moral hazard that predict lower default). This is unfortunate since ex-ante screening through credit scoring methods is difficult for new borrowers, given their limited credit histories (e.g., see [Lieberman et al., 2018](#)). In fact, since default elasticities are increasing in bank tenure, interest rate changes are least effective in mitigating default for the newest borrowers, precisely those for whom the asymmetric information problem is likely the most acute. Third, the weaker labor force attachment of newer borrowers in our experimental sample and the substantial effects of job separation on study card default suggest that adverse life events may play an important role in determining continued access to formal credit for populations such as those under study.

In addition to policymakers, our work should also be of direct interest to economists more broadly. We connect with several strands in the literature on credit markets. A recent literature identifies lack of access to formal financial services as a general problem in developing countries ([Dabla-Norris et al., 2015](#); [Demirgüç-Kunt and Klapper, 2012](#); [Dupas et al., 2018](#)). It advocates supply-side interventions aimed at increasing financial inclusion. We provide a detailed empirical analysis using a widely used, popular product specifically targeted at those with limited credit histories. Our work also adds to an earlier literature that critiques institutional, typically state-led and agricultural, lending to the poor (see, e.g., [Adams et al., 1984](#)). Compared to this literature, instead of taking the limited formal private sector engagement with poor borrowers as *prima facie* evidence for inviability, we provide detailed evidence on a private sector bank's attempts to use ex-post contract terms to limit default among its lower-income borrowers.

Research on credit cards among inexperienced populations in development economics is scarce despite their increasingly important role as the source of entry into the formal credit sector. [Ponce, Seira, and Zamarripa \(2017\)](#) and [De Giorgi, Drenik, and Seira \(2021\)](#) examine credit cards in Mexico but do not focus on new borrower populations or products targeted specifically at them. In the United States, [Keys and Wang \(2019\)](#) study anchoring on minimum payments using an event-study design, while [d'Astous and Shore \(2017\)](#) use a difference-in-differences type approach on a non-experimental change in minimum payments.<sup>8</sup>

While a substantial literature has focused on the importance of contract terms and interest-rate driven moral hazard as drivers of default (e.g., [Banerjee and Duflo, 2010](#); [Karlan and Zinman, 2009](#)), our matched employee-borrower data allows us to evaluate the role of employment shocks relative to those of contract terms within a common sample of new borrowers in a developing country context. [Keys \(2018\)](#) is a closely related paper analyzing the effect of 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 (PSID). 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

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<sup>8</sup>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.

job separation (see, e.g., [Couch and Placzek, 2010](#); [Flaaten et al., 2019](#); [Jacobson et al., 1993](#)). Our paper complements research studying the connections between labor and credit markets and social insurance (primarily in the United States). For instance, [Herkenhoff \(2019\)](#) studies the effect of 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 credit default in a country with limited social protection and benchmark the credit market effects against 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 some context for the rapid increase in credit cards in Mexico and characterizes new card holders. [Section 4](#) describes the experiment. [Section 5](#) provides a simple model to frame the interpretation of the experimental results. [Section 6](#) reports the experimental results, and [Section 7](#) estimates the effect of job displacement on default. [Section 8](#) concludes. Due to space constraints, some robustness analyses and secondary figures and tables are reported in the Online Appendices (OA).

## 2 Data and Summary Statistics

The paper’s primary focus is on borrowers with the study card for which Bank A implemented experimental changes in contract terms. We obtained data on study card borrowers who were part of the experiment and matched them to two relevant data sources. The first is credit bureau data, where we observe every (formal) loan held by the study card sample, which we use to examine spillovers. The second source is employer-employee data from the IMSS, which we use to study the effects of job loss.

In addition, we obtained several cross-sectional random samples (of one million borrowers each) from the credit bureau. These snapshots enable us to compare our study card borrowers to borrowers in Mexico in general. We also match these snapshots to the employer-employee 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 more information in [Appendix B.1](#). We now describe the data sets in more detail.

**Study Card and Bank Data (Experimental Sample):** We use detailed data from one of Mexico’s largest commercial banks (Bank A) and a product (the study card or Card A) that accounted for 15% of first-time loans nation-wide 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 (e.g., see [Figure OA-9](#)). In 2011, these stores accounted for 43% of all household expenditures at all supermarkets and 16% of all household expenditures in Mexico.<sup>9</sup>

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<sup>9</sup>We thank Marco Gonzalez-Navarro for kindly carrying out the calculations using data from [Atkin et al. \(2018\)](#).

**Figure 1: Timeline for the Datasets**

**1. Bank data:**

Monthly card-level data of the study card from Mar/07 to May/09, bimonthly from Jun/10 to Dec/11 and monthly from Jan/12 to Dec/14.

**2. Credit Bureau data:**

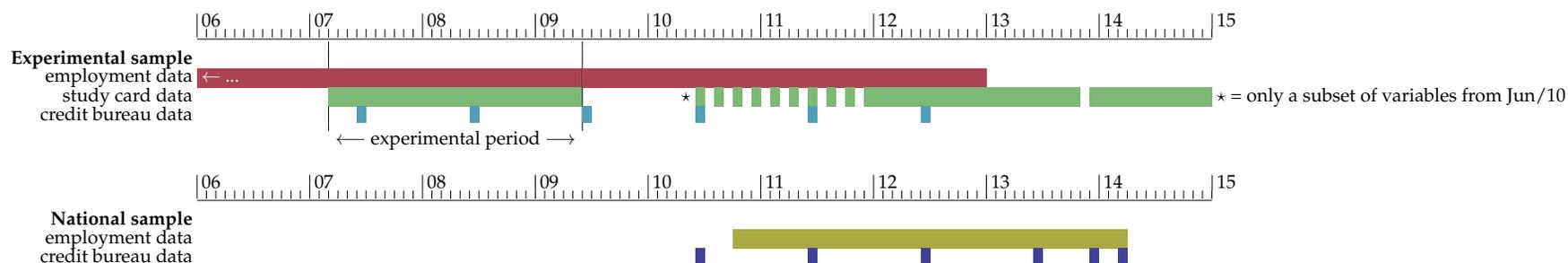
Loan-level data matched to the experimental sample for Jun/07 to Jun/12, annually.

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

**3. Social security employment data:**

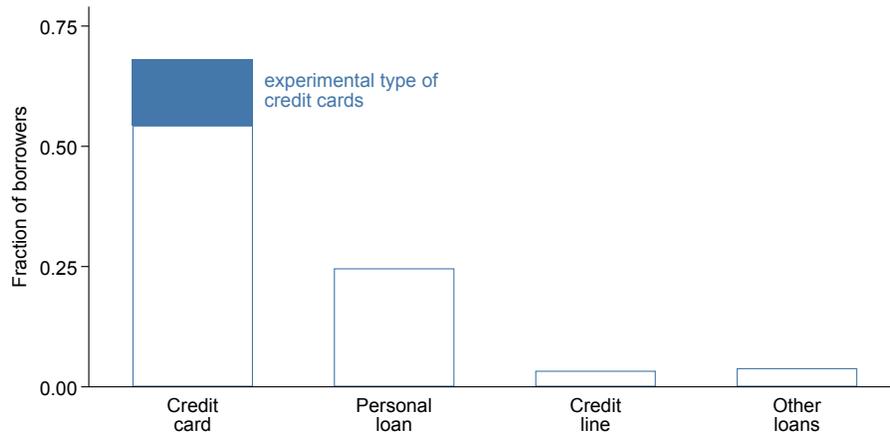
Individual-level data matched to the experimental sample, monthly information from Jan/04 to Dec/12.

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



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

**Figure 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 type of card (described in Section 2) we study.

The card was specifically targeted at low-income borrowers with no or limited credit history (internally, the bank referred to them as the C, C- and D customer segments). Consistent with this, the study card was the first formal loan product for 47% of our study sample, and for 57% it was their first credit card. Customers for the study card approached bank kiosks in supermarkets (located all over Mexico) and completed a brief paper application. The card had an initial credit limit of approximately 7,000 pesos, an annual interest rate of 55 basis points over the base rate, and a monthly minimum payment of 4% of the total amount outstanding. The card was initially offered in 2003, and by 2009, Bank A had approximately 1.3 million clients, a substantial financial inclusion effort in a country with approximately 11 million cards at the time.

*Sample:* The sampling frame consisted of all study card holders who had paid at least the minimum amount due in each of the last six months through January 2007 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 Card A 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 [Table OA-2](#)) in all of our analysis to ensure our results are representative of the sampling frame. We examine the external validity of the sample for the national population of new borrowers in [Table 1](#).

*Variables:* We have monthly data on purchases, payments, debt, credit limits, and cancelations from

March 2007 to May 2009. We observe default from March 2007 to December 2014 but at different frequencies 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. 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.<sup>10</sup>

Since default is a key outcome for the analysis, we describe it in some detail here. In keeping with the legal definition, default is defined as three consecutive monthly payments that are each less than the minimum payment due. In such instances, it is Bank A’s policy to revoke the study card automatically (there is no appeal procedure). The default measure at time  $t$  is a cumulative measure: i.e.,  $Y_{it} = 1$  if  $i$  has defaulted in any month  $s \leq t$  and 0 otherwise. This allows us to carry out all default estimation 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 being driven by the treatment, making the estimand difficult to interpret. We assess the sensitivity of our results to this choice of default measure by estimating duration models in [Appendix F](#) and find that they yield nearly identical treatment effects. Finally, we also observe some basic demographic variables—age, gender, marital status and residential zip code.

**Credit Bureau Data (Matched to Experimental Sample):** A borrower appears in the credit bureau if they have had a loan with a formal financial intermediary.<sup>11</sup> 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 experimental 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 will refer to this data as the *matched* CB data.

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

**IMSS Employment Data (Matched to Experimental 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 definition, employment in the formal sector.<sup>12</sup> Absence from the IMSS data can thus be interpreted

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<sup>10</sup>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.

<sup>11</sup>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](#)).

<sup>12</sup>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

as absence from the formal sector. We observe monthly data from January 2004 to December 2012. For each worker and each month they are 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 CURPS 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. The IMSS data does not record informal employment—official Mexican statistics suggest around one half of Mexican workers are informal, which is roughly similar to the fraction of study card holders who were not matched to an IMSS entry (see below).

**IMSS Employment Data (Matched to the population representative CB):** We also obtained additional monthly Mexican social security data from October 2011 to March 2014, which we matched to the population representative CB data.<sup>13</sup> 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 job displacement and default.

**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 finds a set of borrowers in the CB data that matches the tenure of the experimental sample in the formal credit market (measured by the year of their first loan of the experimental sample; see Online Appendix B.3 for details). Finally, for comparison, Column 5 considers a sub-sample of experienced borrowers—those with a credit history of 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 13,855 pesos compared to 14,759 for recent and

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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).

<sup>13</sup>The only difference between the matched IMSS data and this one (besides the period) is that instead of the CURP, we only observe tax identifiers (known as RFC), which are 13-digit strings identifying tax-payers.

**Table 1: Summary Statistics and Baseline Characteristics**

	Experimental sample (1)	Experimental sample (2)	Credit bureau sample		
			≥ 1 Card Holders (3)	New borrowers (matched) (4)	Experienced borrowers (5)
<i>Panel A. Information from the experimental sample dataset</i>					
Month of measurement	March 2007	May 2009			
Payments	711 (1,473)	908 (1,811)	-	-	-
Purchases	338 (1,023)	786 (2,064)	-	-	-
Debt	1,198 (3,521)	5,940 (6,160)	-	-	-
Credit limit	7,879 (6,117)	12,376 (9,934)	-	-	-
Credit score	645 (52)	-	-	-	-
(%) Consumers for whom experiment is their first card	57	-	-	-	-
(%) Consumers who default between Mar/07 - May/09	17	-	-	-	-
<i>Panel B. Information from the credit bureau dataset</i>					
Month of measurement	June 2007	June 2010	June 2010	June 2010	June 2010
Mean card limit (all cards)	15,776 (15,776)	18,475 (17,557)	49,604 (32,596)	22,082 (28,710)	56,187 (43,032)
Total credit line (all loans)	53,652 (70,292)	64,804 (79,994)	53,718 (103,503)	49,348 (87,855)	139,804 (162,568)
Tenure in months of oldest credit	68 (54)	100 (51)	79 (87)	68 (57)	206 (85)
<i>Panel C. Demographic information</i>					
Month of measurement	June 2007	June 2010	June 2010	June 2010	June 2010
(%) Male	52	-	47	47	53
(%) Married	62	-	50	48	47
Age (in years)	39 (6)	42 (6)	45 (19)	44 (18)	58 (22)
Monthly Income	7,521 (8,662)	8,364 (10,475)	-	-	-
<i>Panel D. Comparable income estimates</i>					
Month of measurement	October 2011	-	October 2011	October 2011	October 2011
Monthly Income <sup>‡</sup>	13,855 (11,244)	-	14,391 (12,949)	14,759 (12,885)	22,641 (15,928)
Observations	162,000	97,248 (Panel A) 150,672 (Panel B & C)	221,151	57,450	55,120

*Notes:* This table presents means and standard deviations for selected variables from the experimental sample and three different credit bureau sub-samples. 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 (RFCs).

22,641 for experienced borrowers.<sup>14</sup> 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%. 41% of the study card borrowers were employed in the formal sector in March 2007, when the experiment started.

### 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 cards in Mexico grew from 10 million in the first quarter of 2004 to 24.6 million in the last quarter of 2011, with a substantial part of the growth being concentrated among lower-income individuals (see [Figure OA-10\(a\)](#) and [Banco de México, 2016](#)). The study card played a vital role in this expansion, accounting for 15% of all first-time formal sector loans in 2010 in Mexico. This pattern is typical throughout Latin America, as many borrowers use only credit cards in their formal loan portfolio (see [Figure OA-11](#)).

This desire to pursue low-income clients appears to have been in part inspired by the success of Banco Compartamos and Banco Azteca.<sup>15</sup> However, Compartamos and Azteca pursue markedly different strategies than those pursued by Bank A. Compartamos primarily uses joint liability via group lending, while Azteca requires collateral, typically household durables. Both lenders expend considerable resources on face-to-face interactions and home visits for loan collection.<sup>16</sup> In contrast, Bank A relies on traditional bank credit card approval and monitoring methods based on individual uncollateralized lending, distance monitoring, credit scoring methods for screening, and standard bank debt collection mechanisms. These traditional methods are cheaper than those employed by Compartamos and Azteca, with operating expenses relative to assets being an order of magnitude smaller (see [Figure OA-12](#)). Whether these lower-cost traditional methods work for new-to-banking borrowers is an open question. The concern is that default may be substantially higher, although at least in theory, contract terms could be used to mitigate it.

**New Borrowers Have Low Credit Scores:** The subjects in our experiment, borrowers with limited or no credit histories, unsurprisingly, tend to have low credit ratings. The strata-weighted mean credit score for our primary sample (645) is low in absolute terms—borrowers with scores below 670 are typically ineligible for standard credit card products ([Drenik et al., 2018](#)). They also have low credit limits. In our study sample, the (weighted) mean credit limit for the study card was relatively low at 12,376 pesos in May 2009. For comparison, in 2010, the mean card limit was 49,604 pesos for those with at least one active card in the credit bureau.

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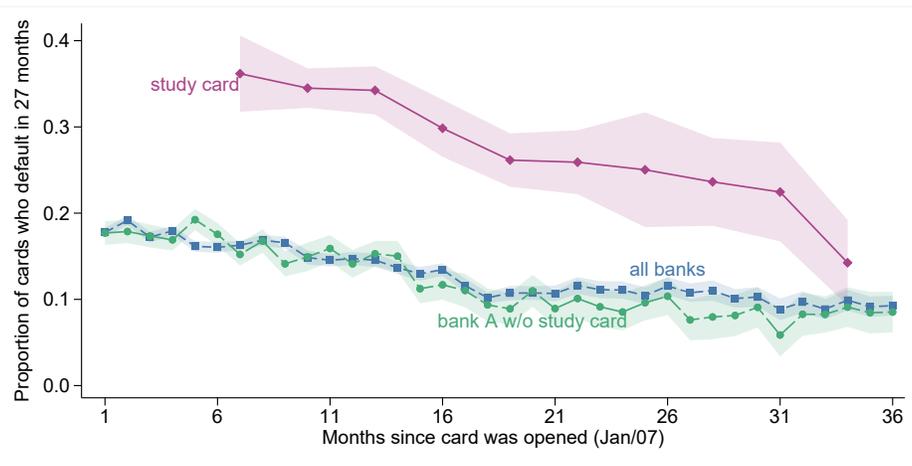
<sup>14</sup>For comparison, the average monthly per capita income in Mexico in 2007 was 4,984 pesos. Our experimental sample's 25th and 75th percentiles of income are 2,860 and 19,535 pesos, respectively. In comparison, they are 2,580 and 6,000 pesos for the country as a whole.

<sup>15</sup>See e.g., <https://goo.gl/7HufqG>; <https://goo.gl/vi2EYK>; <https://goo.gl/sjgoAn>.

<sup>16</sup>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.”

**Default is High for New Borrowers and Declines with Tenure:** During our 26-month study, approximately 19 percent of the control group defaulted on their card (computed using stratum weights), compared to an average cumulative 26-month default rate of 12 percent for a random sample of cards in the credit bureau during the same period. As a further point of comparison, default rates for the micro-lender Compartamos are less than 1% (Karlan and Zinman, 2019). Figure 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).

**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 on or before May 2009 (y-axis) against card tenure as of January 2007 (x-axis). The red diamonds show, for the control group of our study card, the proportion of cardholders that default by the months since the card was opened (binned into quarters). The control group averages are constructed using stratum weights. The blue squares and green circles repeat the sampling exercise in the credit bureau data. The blue squares use all cards, whereas the green circles restrict attention to Bank A cards that are not the same type as the card we study.

Figure 3 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).<sup>17</sup> Default on the study card is twice as high as that on Bank A’s other cards—consistent with the study card being a “financial inclusion” product targeted at those with lower incomes and limited credit histories. Default rates for Bank A’s other cards are similar to those at other banks.

In principle, high default rates could reflect a low default cost or limited benefits from the card. We provide evidence against this view below by (a) documenting the costs of formal sector credit default for and (b) providing evidence of the benefits of formal credit in terms of its lower cost and the large borrower debt response to credit limit increases.

**Default Reduces Access to Formal Credit:** Perhaps unsurprisingly, default reduces subsequent formal sector borrowing. We document the magnitude of the effect using two complementary approaches, sum-

<sup>17</sup>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.

marizing the results here with the details relegated to [Appendix D](#). 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 ( $p = .03$ ). Second, using a selection on observables assumption, we show that default on the study card is associated with the absence of any subsequent credit card up to four years later.

**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 [Gross and Souleles](#), 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 E](#) where we begin by showing that debt is responsive to changes in credit limits for both the study card debt and total card debt. A 100 peso increase in the study card's credit limit translates into 32 pesos of additional debt (the I.V. estimates are more than twice as large). These event-study 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 22 pesos for borrowers in the lowest tercile of the debt-to-limit ratio at baseline relative to 59 pesos for borrowers in the highest tercile (i.e., those most constrained at baseline by the measure). Similarly, borrowers paying close to the minimum had debt responses about three to ten times as large as 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 duration for formal and informal loans.<sup>18</sup> We find that informal loan terms are significantly worse than formal ones. [Table OA-3](#) shows the results from regressing contract terms on an indicator for a formal loan and controls. First, the average annual informal loan interest rate is 291%. In contrast, the corresponding rate for formal loans is 94 points lower (col. 1). The average loan size is 3,658 pesos for informal loans and 9,842 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-13](#) shows that the distribution of interest rates for informal loans first-order stochastically dominates the distribution for formal loan rates, while the opposite is true for loan terms and loan amounts. These results are robust to controlling for income and wealth proxies (columns 2, 4, and 7). The loan terms and duration results also survive the addition of household fixed effects.<sup>19</sup> While not dispositive, these results suggest that informal loan terms are onerous (compared to formal loan terms), incentivizing borrowers to maintain access to formal credit.

The last three observations suggest that formal credit is attractive to borrowers, and the default is

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<sup>18</sup>We define a loan as formal if the lender is a bank and informal otherwise. Informal loan sources comprise co-operatives (13%), money-lenders (8%), relatives (38%), acquaintances (20%), work(11%), pawn-shops (5%), and others (5%). Consistent with the evidence from a range of developing countries (see e.g., [Banerjee and Duflo, 2010](#)), only 6% of borrowers have any formal loans, and 91% of borrowers have only informal loans. We do not observe informal sector loans in our bank data.

<sup>19</sup>Only about 4.3% of households hold formal and informal sector loans, so the identifying variation in the fixed-effects model arises from a small (and likely selected) sample.

consequential. This context will help interpret both the default levels and experimental responses.

## 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 12 months (both measured in January 2007).<sup>20</sup> 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. 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 outstanding (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-14](#)). 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 [Karlan and Zinman, 2009](#), who vary both components independently).

These are substantial changes in contract terms. For instance, the interest rate 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 [Web Archive Link](#) for Indonesia. Within Mexico, using cross-sectional data on interest rate variation across lenders, the experimental variation in interest rates is equivalent to moving from the 20<sup>th</sup> to the 80<sup>th</sup> percentile in the interest-rate distribution (see, e.g., the [Banco De Mexico Report](#)). We do not have finer-grained data (e.g., within bank and/or borrower category), but we conjecture that the range of such variation would be much lower than the across-lender variation as lenders typically specialize in different segments. Similarly, the mandated increases in minimum payments (e.g., in Mexico and Quebec) are well below the 10% enforced in the experiment

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<sup>20</sup>For borrowers with less than 12 months, the entire available history was used for stratification.

(at about 1.5% and 5%, respectively). Thus, the experimental contract terms changes lie on the upper end of the policy feasible changes regulators contemplate.

The two different minimum payments and four different interest rates yield eight unique contract terms (see [Table OA-4](#)). 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.<sup>21</sup> Consequently, we do not use the hold-out group as a contrast. We use the 5% minimum payment and the 45% interest rate group (abbreviated to (45, 5)) as the comparison group and refer to it as the base arm or base group. Panel A of [Table OA-5](#) in the Online Appendix tests the randomization procedure and shows that treatment assignment is uncorrelated with baseline observables for the initial sample, as well as for the sample that did not attrit for the entire duration of the experiment.

[Figure 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-6](#) and [Figure OA-15](#)).<sup>22</sup> The experiment ended in May 2009, when all participants received a letter stating their new contract terms. The new contract terms were the standard conditions with an interest rate of approximately 55% and a minimum payment of 4%.

## 5 A Framework for Default, Contract Terms, and Income Shocks

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 C.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

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<sup>21</sup>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.

<sup>22</sup>Although not the focus of this paper, in an interesting and complementary paper [Aydin \(2022\)](#) analyses the effects of randomized changes in credit limits.

given by  $C_1 = P + (1 - m_1)RC_0$ . [Appendix B.2](#) 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 (outlined in [Appendix G](#)).

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:

$$\begin{aligned} & u(y_1) + \epsilon_{11} && \text{if the borrower defaults} \\ & u(y_1 + P - m_1RC_0) + \epsilon_{10} && \text{if the borrower does not default,} \end{aligned}$$

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 exogenous variables.<sup>23</sup> 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  $m_2RC_1$  and continue using the card in the future. Thus, conditional on the high-income realization, the second period utility is given by:

$$\begin{aligned} & u(y_H) + \epsilon_{21} && \text{if the borrower defaults} \\ & v + u(y_H - m_2RC_1) + \epsilon_{20} && \text{if the borrower does not default,} \end{aligned}$$

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.<sup>24</sup> 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 ( $y_H > m_2RC_1$ ), and that 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. This reduces the agent's period 2 decision to either default or make the minimum payment and remain in good standing.

In [Appendix C](#), 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:

<sup>23</sup>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.

<sup>24</sup>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.

(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  $\theta$  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 C](#). Our first prediction examines default responses in period 1. We begin by considering changes in minimum payments when borrowers assume the same minimum payments 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 C.2.2](#) for the argument).

**Prediction 1.** Assume 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 (admittedly ad-hoc) 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 C.1.4](#) for a proof).

**Prediction 2.** Assume agents make debt choices ( $C_1^*$ ) 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 through which to view the policy prescriptions outlined in [Section 1](#). In particular, policies advocating for higher minimum payments as a means to limit default are difficult to rationalize in the model with perfect foresight as the model implies that increased minimum payments will increase default for any positive debt level.

Next, interest rates affect the choice problem in two ways. First, interest rate changes apply to previously accumulated debt  $C_0$  (consistent with the experiment), so that e.g., increases in interest rates will mechanically increase this component of overall debt. Second, changes in interest rates apply to new debt (i.e., to purchases made on the card in period 1). The overall effect of interest rate changes on default then depends on both these effects.

**Prediction 3.** Period 1 default (when agents can adjust debt responses) is increasing in the interest rate  $R$  (as long as  $C_1^* + RC_0 > 0$ ).

The latter 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). 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 unemployment shocks in the context of our empirical application.

**Prediction 4.** 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 ( $q$ ).

## 6 Does Changing Contract Terms Reduce Default for New Borrowers?

**Main specification:** For ease of exposition, our primary specification is

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

estimated on the sample of 144,000 individuals in the eight treatment arms using stratum weights (as defined in [Table OA-2](#)).  $Y_{it}$  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.

We interpret  $\alpha_t$  as the mean value of  $Y_{it}$  in month  $t$  for the excluded group (i.e., the  $r = 45\%$  and  $MP = 5\%$  treatment arm),  $\beta_t$  as the average treatment effect of increasing the minimum payment to 10%, and  $\gamma_t$  as the effect of decreasing interest rates to 15%. We estimate [Equation \(1\)](#) with and without stratum-by-month fixed effects and find almost identical results for  $\beta_t$  and  $\gamma_t$ . 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).

[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-7](#), which yield similar estimates. We also test both assumptions and cannot statistically reject them.<sup>25</sup> We only discuss estimates from [Equation \(1\)](#) for ease of interpretability.

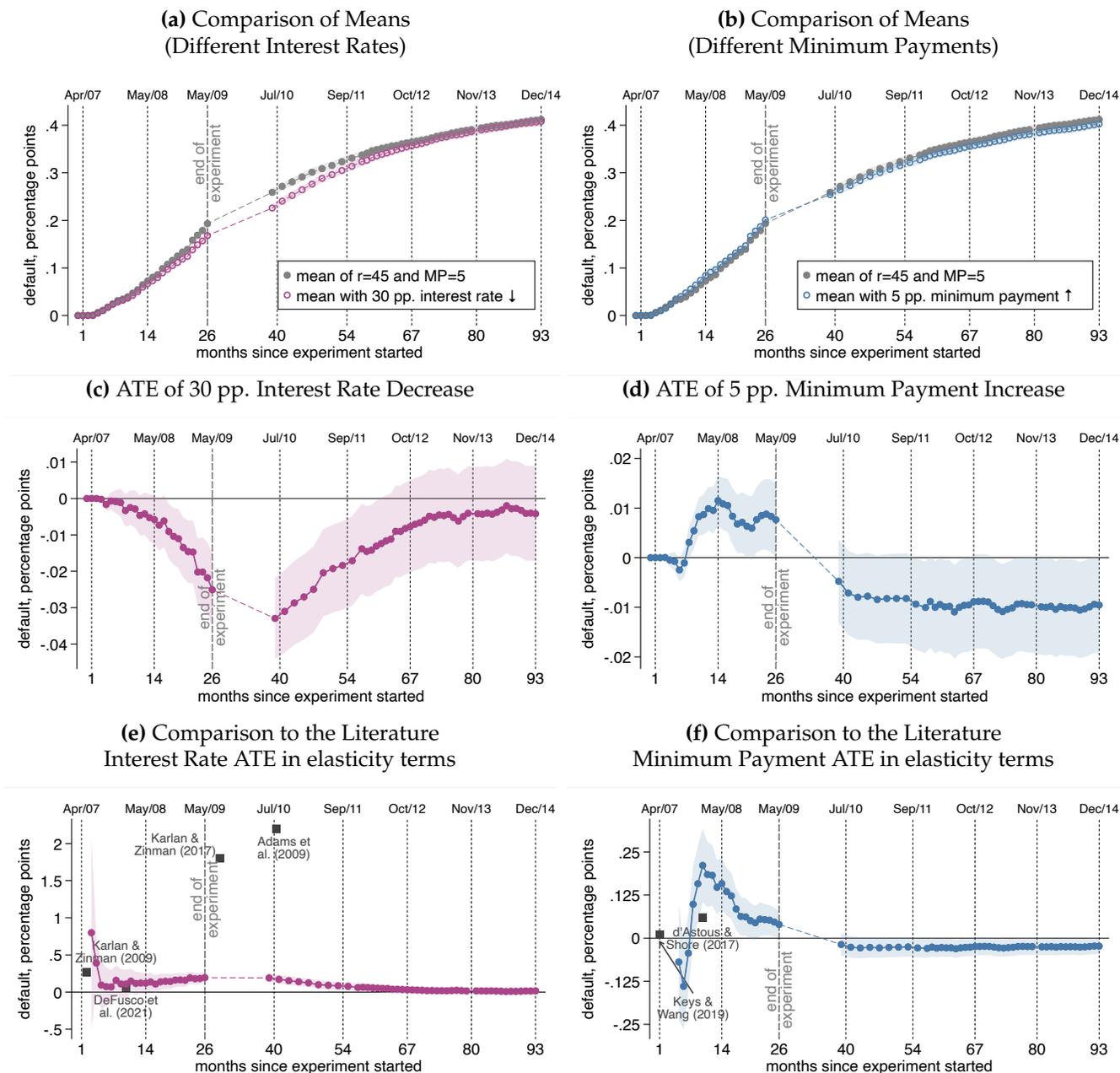
Given the large number of estimated monthly treatment effects  $\{\beta_t, \gamma_t\}_t$  over seven years, we present the results succinctly in two ways. First, we present the estimates graphically in [Figure 4](#), plotting monthly means and treatment effects from March 2007 through December 2014,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

### 6.1 Default on the Study Card

**Increasing Minimum Payments Does Not Reduce Default during the Experiment:** The experiment doubled the minimum payment from 5% to 10% from April 2007 through May 2009. [Figure 4\(b\)](#) plots

<sup>25</sup>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. The full details are in [Table OA-7](#).

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



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

the evolution of default in response to this intervention for the next several years. We see from [Figure 4\(d\)](#) that the ATE 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. The default ATE rose sharply starting at nine months and peaked at approximately 1 pp. about 14 months into the intervention. The effect then subsequently hovered around that point for the remainder of the experiment, and by the end of the experiment, the minimum payment increase had increased default by 0.8 pp. (see col (3) in [Table OA-7](#)). This finding is at variance with arguments (by regulators and policymakers, referenced in [Section 1](#)) advocating for (and legislating) minimum payment increases as a means for *decreasing* default. The implied 26-month elasticity is +0.04, and the confidence intervals rule out negative values. Thus, we find no evidence that even large increases in minimum payments decrease contemporaneous default, even over relatively long horizons.

**Mechanisms:** Several pieces of evidence suggest that the increase in default in the first year is consistent with tightened liquidity constraints. First, the increase in the default ATE is entirely concentrated amongst borrowers with the highest debt utilization rates (at baseline) who are arguably more liquidity-constrained than borrowers with lower utilization rates (see the discussion on [page 13](#) of [Section 3](#) on the use of debt utilization rates as a measure of liquidity constraints). We show this in detail below on [page 27](#) and see also [Figure OA-20\(b\)](#). Second, the increase in default is preceded by a sharp increase in delinquencies (i.e., the failure to make the minimum payment), particularly in months 3, 4, and 5, as shown in [Figure OA-21\(b\)](#) and this increase in the delinquency ATE only occurs among borrowers with the highest debt utilization rates (see [Figure OA-21\(d\)](#)).

Each such delinquency incurs a fee of 350 pesos, further exacerbating repayment concerns. Indeed, we find a sharp rise in debt mirroring the rise in delinquencies, so delinquency fees likely cause an increase in the repayment burden during this period, and so can also be viewed as arising from liquidity constraints.<sup>26</sup> Finally, [Figures OA-19\(d\)](#) and [OA-21\(f\)](#) show that default and delinquency increases are likewise almost entirely concentrated among borrowers in the minimum-payer stratum, which is the most liquidity constrained relative to the other (experimentally specified) payment strata (the debt utilization rate for minimum payers at baseline is about 85%, which is more than twice the rate for full-payers).

After the first year, the ATE remains roughly 1 pp. through the end of the experiment. Given the cumulative nature of the outcome variable, this rough constancy of the ATE implies there was very little contemporaneous differential default by the treatment arm after the first year or so. Therefore, the increase in default due to higher minimum payments during the 26-month experiment predominantly arises from default in the first year, which, as argued above, is driven at least partly by liquidity constraints. This increase is consistent with our theoretical framework in [Section 5](#). In particular, [Prediction 1](#) states that under the model's assumptions, increases in minimum payments will increase default in period 1.<sup>27</sup> Comparing our findings to other results in the literature in [Figure 4\(f\)](#) and [Table OA-8](#), we

<sup>26</sup>See [Appendix G.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.

<sup>27</sup>In particular, in the model the debt elasticity with respect to the minimum payment is insufficient to offset the increases

find that our elasticities for minimum payments are of the same order of magnitude as documented in earlier, albeit non-experimental, work.

**Long-Run Effects of Increasing Minimum Payments:** In May 2009, all study cards were returned to their pre-experiment minimum payment level (4%), and interest rates were returned to their pre-experiment levels. There is evidence, however, that the previous two-year experimental increase in minimum payments had persistent long-term effects. [Figure 4\(b\)](#) plots treatment effects for five and a half years after the intervention ended. In contrast to the findings above (i.e., during the intervention), the post-intervention point estimates are consistently negative, with around a 1 pp. decline in default for the higher minimum payment arm.

**Mechanisms:** According to the model detailed in [Appendix C](#), borrowers who anticipate continuing with the 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 2](#), formally shown in [Appendix C.1.4](#).

Bank A did not inform borrowers of the change in contract terms in advance of the experiment and likewise did not inform them of the duration of the changed terms either. Under these circumstances, it seems reasonable to assume that borrowers in the higher minimum payment arm expected minimum payments to remain so. In contrast, those in the lower minimum payment arms might reasonably expect the lower minimum payments to persist. The lower debt in the higher minimum payment arm at the end of the experiment (see [Figure 4\(f\)](#)) provides some support for this mechanism. Unfortunately, since we do not observe debt after the experiment, we cannot trace longer term debt-responses.<sup>28</sup>

**The Effect of Interest Rate Changes on Default:** [Figure 4\(a\)](#) plots the evolution of default for the (45, 5) and (15, 5) arms using estimated coefficients from [Equation \(1\)](#), and [Figure 4\(c\)](#) plots the corresponding treatment effects. Default declined gradually during the experiment in response to the lower interest rates. By the end of the intervention, default from a 30 pp. reduction in interest rates was approximately 2.5 pp. lower relative to a default rate of 19% for the (45, 5) arm, and the estimates are statistically significant at the 0.001 level (see also [Table OA-7](#)). The implied 26-month elasticity of default is +0.20, which is considerably lower than those in, e.g., [Adams et al. \(2009\)](#); [Karlan and Zinman \(2019\)](#), though in the same range as, e.g., [DeFusco et al. \(2021\)](#); [Karlan and Zinman \(2009\)](#).<sup>29</sup> Finally, the effects of policy-relevant changes in interest rates (e.g., 10 pp. as discussed above) are substantially smaller (.84 pp.,  $p < .001$ ) over the same horizon.

**Mechanisms:** The framework in [Section 5](#) predicts that lowering interest rates will decrease default ([Prediction 3](#)) and [Appendices C.1.3](#) and [C.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 \(13\)](#) shows that

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risk of default from a higher required minimum payment. Empirically, this debt elasticity is relatively small—our preferred estimates are  $\epsilon_{Cm} \in [-0.31, +0.04]$  (see [Figure OA-28](#)).

<sup>28</sup>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 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.

<sup>29</sup>See [Figure 4\(e\)](#) and [Table OA-8](#) for a more detailed comparison. Note that although default decreased by 2.5 pp., this was from a 30 pp. reduction in interest rates (typical changes in interest rates are substantially smaller).

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.<sup>30</sup>

These patterns are indeed what we observe in the experiment. First, we document that purchases (as well as purchases net of payments) increase in response to interest rate declines (see [Figures OA-30\(c\)](#), [OA-30\(e\)](#) and [OA-31\(c\)](#)) consistent with downward sloping demand. Despite this, overall debt *declines* (see [Figures OA-28\(c\)](#) and [OA-28\(e\)](#)). [Appendix G.1](#) examines the effect of interest rate declines on debt in detail, where we conclude that the debt elasticity to the interest rate is indeed positive (our preferred estimates are  $\epsilon_{Cr} \in [+0.18, +0.54]$ ).<sup>31</sup> Finally, in [Appendix B.2](#), we empirically verify that debt can be decomposed into past debt and current (net) purchases (thereby lending support to our modeling of debt). We then show that this decomposition implies that debt will increase in the interest rate if and only if the "compounding" effect (formalized in that appendix) exceeds the new purchase response.

**Long-Run Effects of Interest Rate Changes:** As noted above, all study borrowers were returned to the same contract terms after the end of the experiment. [Figure 4\(c\)](#) displays the effects on default until December 2014. Default continues to be lower in the lower interest rate arm for about three years after the experiment ends—the estimates gradually decline to about an ATE of -1 pp. by May 2011, after which they become statistically indistinguishable from zero. Thus, the 26-month reduction in interest rates decreased subsequent default for nearly three years after the intervention ended, with the elasticities ranging between .1–.2 during this time.

**Mechanisms:** This decline is consistent with the model, which predicts that agents with lower interest rates during the experiment default at lower rates after the experiment since they have lower debt by the end. [Appendix C.1.5](#) describes the theoretical argument in greater detail (and [Appendix C.1.3](#) provides a rationale for why lower interest rates lead to lower debt). As noted earlier, we do not observe debt after the experiment ends, so we cannot examine long-term debt responses, but [Appendix G.1](#) confirms that by the end of the experiment, the lower interest rate arms had lower debt than the higher interest rate arms.

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

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<sup>30</sup> The literature distinguishes between at least three channels in understanding the effect of varying interest rates on default: (a) the "debt burden" channel describes the idea that higher interest rates increase debt mechanically, and this makes repayment harder; (b) the "pure current incentive effect" or "concurrent" moral hazard, viz. the incentive effect of higher current interest rates on default (holding debt constant); (c) the "pure future incentive effect," or dynamic moral hazard, arises if *future* interest rates from the lender are higher (while holding current debt and interest rates constant). In our case, interest rate changes apply to all current and future debt for the foreseeable future. Therefore, a muted default response as we find implies that the contributions from all three channels are correspondingly small. Of course, there are types of moral hazard unrelated to interest rates.

<sup>31</sup> As noted in the relevant appendices, the results for debt and purchases are only suggestive since sample selection renders the Lee bounds, in some cases, too wide to be informative.

and  $p = .22$  in 2012).

**Summary and discussion:** Taken together, these results imply that two of the standard tools routinely used by large financial institutions to control default have smaller contemporaneous effects on new borrower behavior than those previously documented in the literature and typically presumed in policy discussions. In particular, comparing the experimental results to elicited predictions from five Mexican officials (in [Appendix H](#)) reveals that the latter hold considerably optimistic beliefs on the efficacy of interest rates and minimum payments to limit default. For instance, the officials predicted a decline in default of 8.6 pp. from a 30 pp. reduction in interest rates and predicted that default would decline by .4 pp. in response to a 5 pp. increase in minimum payments (the estimated ATEs at the relevant horizons were 1.03 pp. and an *increase* of .8 pp., respectively). Results from an incentivized prediction exercise on the Social Science Prediction Platform with 72 respondents (of whom 82% had a post-graduate degree) revealed similarly optimistic beliefs (see [Appendix H](#) for details).

Moreover, as noted above, when we use the experiment to evaluate the effect of policy-relevant changes in contract terms, the implied treatment effects are substantially smaller. Finally, as we demonstrate below, there is significant heterogeneity in the impact of the interventions, which will provide us both with a better sense of the underlying mechanisms and reinforce the main points here.

From this perspective, it is perhaps unsurprising that Bank A subsequently reduced its interactions with new borrowers. [Figure OA-10\(b\)](#) shows the trend in the current stock and new issues of the study card. After vigorously issuing the study card for several years, the bank ceased issuing new cards for this population in 2009. In personal conversations, bank officials claimed that the card had not achieved its profitability objectives and that high default played a role in their final decision. The closing of the card appears to have had large effects on overall borrowing by new borrowers: [Figure OA-16](#) shows that the closing of the study card coincided with a decrease of close to 25 pp. in the fraction of new loans going to new borrowers in Mexico as a whole.

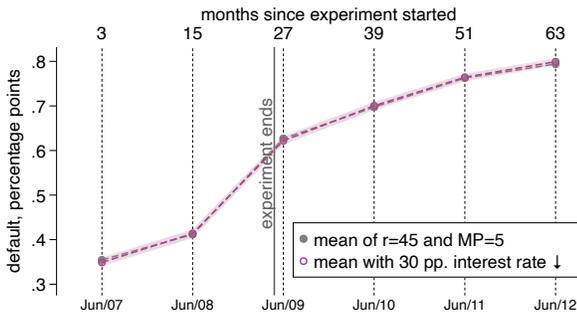
## 6.2 Spillover Effects

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

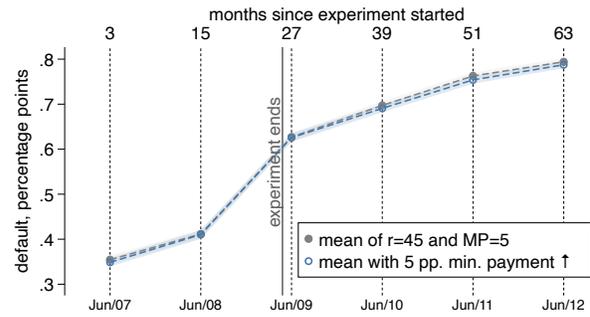
We first examine default on other loan products in [Figures 5](#), [OA-17](#) and [OA-18](#). The dependent variable in Panels (a) to (d) of [Figure 5](#) is equal to one if a cardholder has defaulted on at least one loan with *any* lender in the credit bureau at the given date. The dependent variable in Panels (e) to (h) is a cumulative measure of new loans equal to one if a cardholder has opened a new loan with *any* lender from the beginning of the experiment to the given month. Similarly, [Figures OA-17](#) and [OA-18](#) decompose spillovers by examining default on other loans from Bank A and loans from any other bank, respectively. We find that default on other loan products is largely unresponsive to interest rate and

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

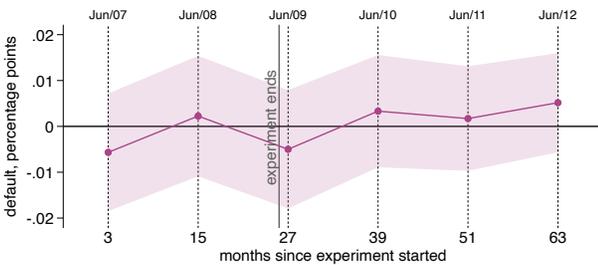
**(a) Comparison of Means w/ Different Interest Rates**



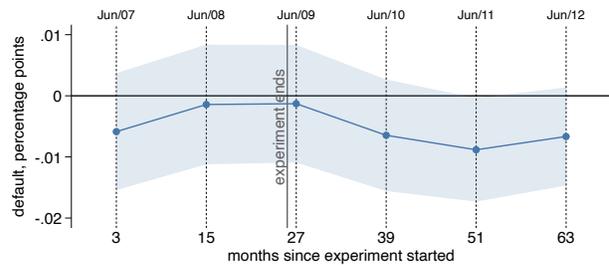
**(b) Comparison of Means w/ Different Min. Payments**



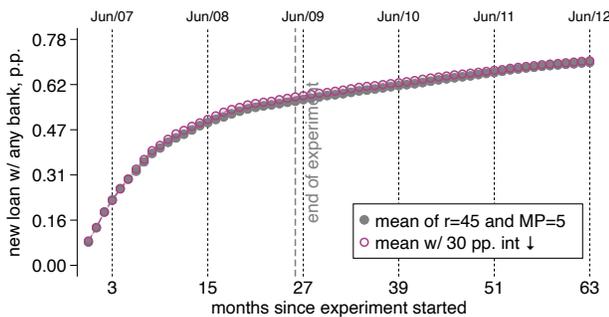
**(c) ATE of 30 pp. Interest Rate Decrease**



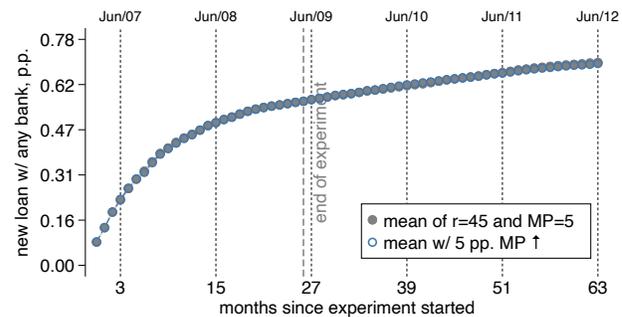
**(d) ATE of 5 pp. Minimum Payment Increase**



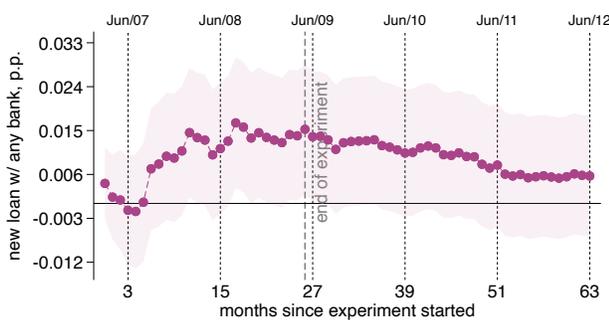
**(e) Comparison of Means w/ Different Interest Rates**



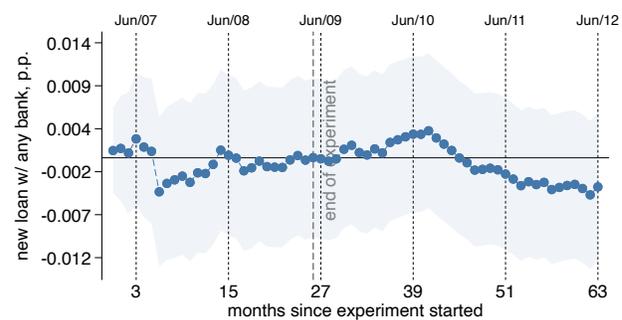
**(f) Comparison of Means w/ Different Min. Payments**



**(g) ATE of 30 pp. Interest Rate Decrease**



**(h) ATE of 5 pp. Minimum Payment Increase**



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

minimum payment changes, both during the experiment and a half-decade after it ended.<sup>32</sup>

In addition, we do not find any changes in cancelations with other lenders in response to contract-term changes in the study card. We also do not find evidence of crowd-out or crowd-in from other lenders along the extensive margin (see Panels (f) to (i) of [Figure OA-18](#)). These results hold both during the experiment and five years after it ended. [Angelucci et al. \(2015\)](#); [Karlan and Zinman \(2019\)](#) similarly find no spillovers in the number of loans or lenders in a micro-finance context.

### 6.3 Mechanisms via 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.

We consider the two stratifying variables—tenure with Bank A (i.e., newer vs. older borrowers) and repayment behavior prior to the experiment (minimum payers vs. full payers), as well as two variables that were not used for pre-experiment stratification—the baseline debt to credit limit ratio (the debt utilization rate) and baseline labor force attachment.

**Heterogeneity by Borrower Tenure:** Treatment effects for newer borrowers are of direct policy interest given the regulator’s concerns over default risk for inexperienced clients cited above. Further, since newer borrowers have the highest default rates and lenders have the least information about them, their responsiveness to contract-term changes is of particular interest to lenders as a potential mechanism for limiting default. Finally, examining and documenting heterogeneity by credit market experience is of interest to researchers as well since such differences may motivate the development of further models.

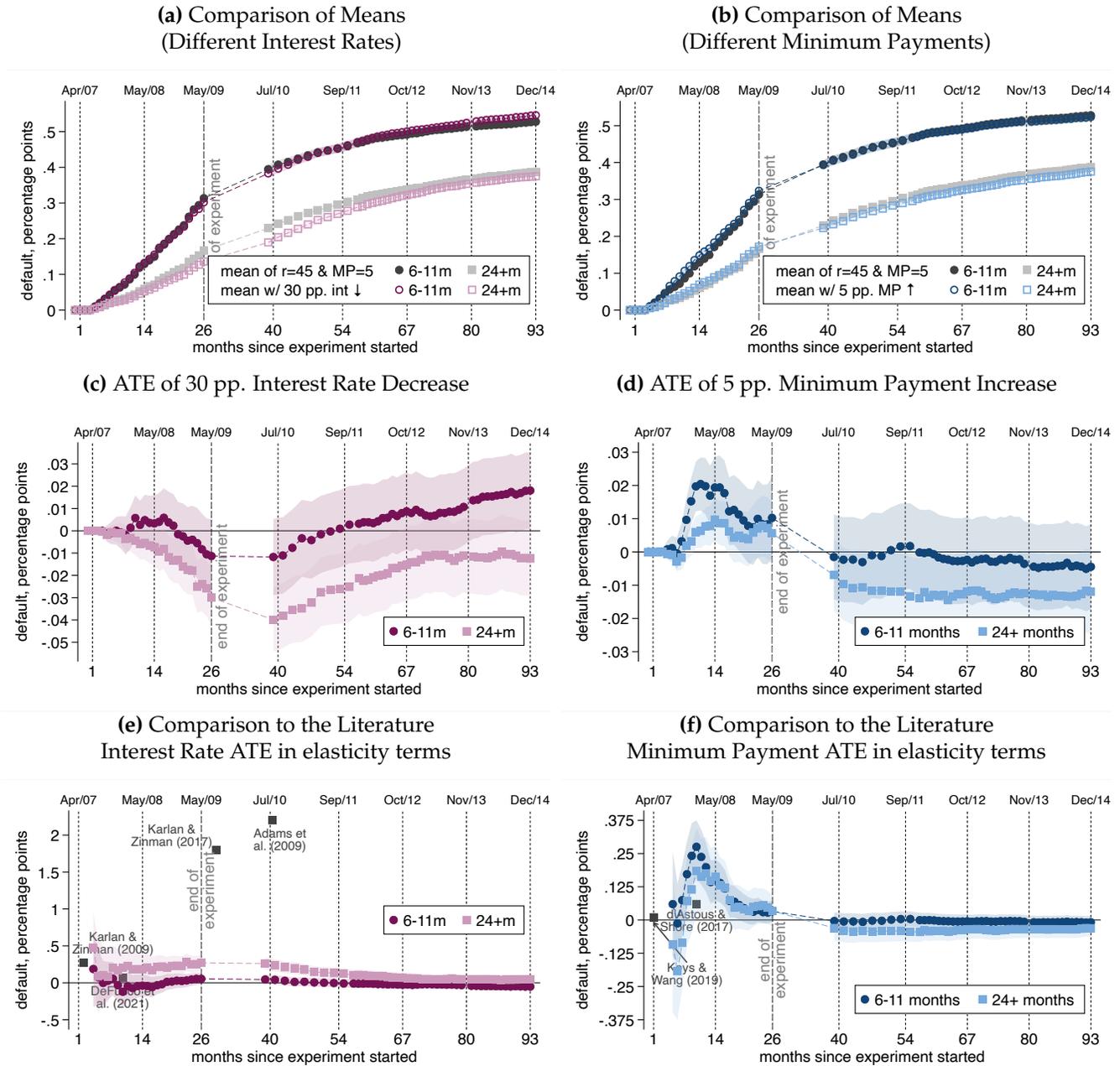
We find that for newer borrowers, i.e., those who had been with Bank A for 6–11 months as of January 2007, the interest rate elasticity of default is not statistically different from zero (the point estimate is +0.05) while the corresponding elasticity for borrowers who had been with the bank for more than two years is five times larger at +0.25 (and significant at conventional levels). [Figure 6](#) graphs both treatment effects over time and shows that the treatment effects for new borrowers are consistently smaller in absolute terms than those for the oldest. The new borrower elasticity is substantially smaller than others documented in the literature. It is an order of magnitude smaller than those documented in [Adams et al. \(2009\)](#); [Karlan and Zinman \(2019\)](#) and about a fifth of the elasticities documented in [DeFusco et al. \(2021\)](#); [Karlan and Zinman \(2009\)](#). The lower elasticity is striking because it suggests that interest rate declines are much less effective at reducing default for newer borrowers for whom asymmetric information and default problems are likely the most severe (indeed, default in the base group was 18% for the oldest group of borrowers and 36% for the newest borrowers).

However, newer borrowers may also vary in other important dimensions from older borrowers. We therefore, re-estimate treatment effects after including a range of baseline covariates (as well as interact-

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<sup>32</sup>The only exception is a small decrease in default (3%, or 2 pp. out of a 61 pp. basis) among other Bank A loans in the high minimum payment arm.

**Figure 6: Treatment Effect of Contract Terms on Default by Months with Credit Card**  
(Share of Cardholders that Default)



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

ing the covariates with treatment indicators) and find that the differential treatment effect between older and newer borrowers remains (as seen in [Table OA-10](#)).<sup>33</sup> While not dispositive, these results suggest that the observed treatment effects for newer borrowers are not driven by age, labor force attachment, or earnings (or more broadly, the set of observables controlled for).

Within the framework of [Section 5](#), the difference between these elasticities can be rationalized by differences in the continuation value  $v$  of having the card for newer versus older borrowers. Newer borrowers may value the card more than older borrowers because they have fewer outside credit options in the formal sector (due to their limited credit histories). Consistent with this, we find that newer borrowers, on average, are less likely to have another card with another bank at baseline. Using credit bureau data, 64% of the 6–11 month strata cardholders have a card with another bank. In contrast, the corresponding figure for those in the 24+M strata is 78%. In the model, this can be formalized as newer borrowers having a higher continuation value,  $v$ , and in [Appendix C.3](#), we show that higher values of  $v$  imply muted responses to interest rate changes.<sup>34</sup>

**Heterogeneity by Liquidity Constraints:** Next, 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 E](#) 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 C.2.5](#)).

[Figure OA-20\(b\)](#) 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-20\(c\)](#) 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 G.1](#) for

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<sup>33</sup>The covariates (interactions between covariates and treatment indicators are also included) included are: strata indicators, age, earnings, labor force attachment, study card utilization, gender, age, and other card ownership.

<sup>34</sup>*Ceteris paribus*, a higher continuation value for newer borrowers implies lower default in general (and not just in response to interest rate changes). However, this is counteracted by the extent to which newer borrowers have lower and/or more volatile incomes. For instance, holding  $v$  and debt fixed, if  $q$  for newer borrowers is higher (or  $y_H$  is lower) than the  $q$  (or  $y_H$ ) for older borrowers, then overall default will be higher for newer borrowers. In support of this, we find that the average monthly income for newer borrowers is lower than that for older borrowers (measured in 2007)—the numbers are 8,315 pesos as against 10,459 pesos. Further, [Section 7](#) shows that newer borrowers are more likely to experience unemployment spells (which we can interpret as having higher values of  $q$ ). Under these configurations, the model can qualitatively reconcile higher default among newer borrowers (relative to older borrowers) and a lower response to changes in interest rates.

details). It is also consistent with [Figure OA-22](#), which shows that the reductions in debt from lower interest rates were much larger for borrowers with high levels of baseline credit utilization.

**Heterogeneity by Repayment Behavior and Labor Force Attachment:** [Figure OA-19](#) 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).

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.<sup>35</sup> [Figure OA-23\(a\)](#) 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.

## 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 remains high. [Section 6](#) showed that even significant contract-term changes do comparatively little to mitigate default even over the relatively sizeable experimental range of variation and do not provide much evidence for default being driven by interest-rate-driven moral hazard. In this section, we argue that new borrowers are vulnerable to frequent, large shocks that precipitate default. This is a simple but relatively unexplored hypothesis in the financial development literature, which has typically focused on asymmetric information and high fixed lending costs (see, e.g., the survey in [Banerjee and Duflo, 2010](#)). We focus on one particular shock—job separation in the formal sector—which we observe using our matched borrower-employee data.

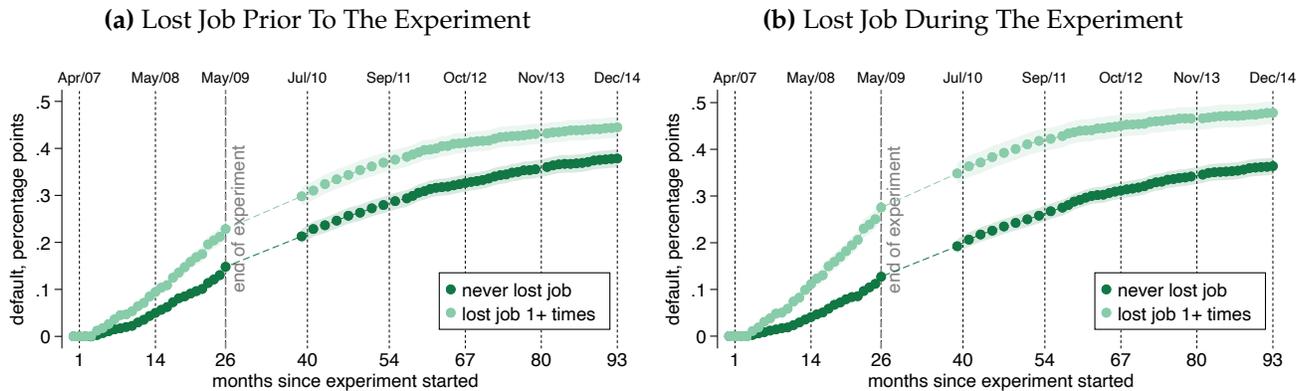
Job loss is an appealing candidate shock for several reasons. First, job loss is common in our experimental sample: of those employed at least one month in the formal sector between January 2004 and March 2007 (45% of the experimental sample), 43% experienced at least one month out of formal sector employment. 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—for developed countries with near-universal formal sector employment—has shown that job loss results in both short- and long-term earnings losses ([Couch and Placzek, 2010](#); [Flaen 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 our sample, we find that default for borrowers with a stronger pre-experimental attachment to the labor

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<sup>35</sup>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.

force (26% of our sample) is substantially lower than for borrowers with weaker attachment (by 8 pp.) at the end of the experiment (see Figure 7(a)). We can incorporate unemployment in the framework of Section 5 by viewing it as a first-order stochastically dominated period two-income distribution, which will increase default (Prediction 4).

**Figure 7: Default in Experiment Credit Card by Job Status**  
(Comparison of Means in the  $r = 45, MP = 5$  Group)



*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).

Thus, while it is reasonable to conjecture that job loss is a significant negative shock, its precise effect on loan default is less clear. On the one hand, limited unemployment insurance suggests that unemployment shocks affect default more directly. On the other hand, informal insurance and informal employment are common in developing countries like Mexico, and they could potentially mitigate the effect of job loss on default. That is, while the sign of the effect of job loss on loan default is not controversial, the magnitude remains largely an open empirical question bedeviled by endogeneity concerns.<sup>36</sup>

Given the difficulty of explicit randomization, work on the effects of job loss has focused on quasi-experimental methods. Jacobson et al. (1993) pioneered the use of 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 Sullivan and von Wachter (2009) and Couch and Placzek (2010); Flaaen et al. (2019). The key idea is that job loss during such mass layoff events (referred to as displacement) is more likely to be an involuntary separation and, thus, potentially orthogonal to displaced worker characteristics.

<sup>36</sup>There is some work (e.g., Maloney, 1999; Meza et al., 2022) suggesting limited labor market segmentation (between the formal and informal sectors) in Mexico, making it plausible that displaced workers may seek and find informal employment after formal job loss. However, the evidence on the extent and direction of (output or employment) cyclicity between the Mexican formal and informal sectors is too limited (see Ohnsorge and Yu, 2022, for a general discussion on the theory and evidence on this front) for us to draw any conclusions about the precise role of the informal sector in attenuating or exacerbating displacement effects.

The literature then compares the outcomes of displaced 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’ default potential 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 such an assumption plausible in our context for several 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.

As in this literature, we focus on firms with more than 50 employees and use the universe of formal employment data from the IMSS to define a mass layoff month as the first month in which the year-on-year employment decrease at a firm exceeds 30 percent of average employment in the 12 months prior to the experiment. The size and layoff definitions are standard in the literature (see, e.g., [Davis and Von Wachter, 2011](#); [Flaen et al., 2019](#)) and yield 872 mass layoff events for our experimental sample throughout the experiment (Mar/07-May/09). At the firm level, mass layoffs decrease employment by 60 employees on average (about 27% of the average number of employees in a firm) and the wage bill by \$424,000 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 month). [Figure OA-25](#) shows event study graphs for total employees and wage bill using the estimation approach in [de Chaisemartin and D’Haultfoeuille \(2022\)](#), which confirm the extensive effects of mass layoffs on these outcomes.

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

$$\text{default}_{it} = \alpha_i + \gamma_t + \sum_{k \neq 0} \beta_k \times \mathbb{1}\{t - \tau_i = k\} + \varepsilon_{it}, \quad (2)$$

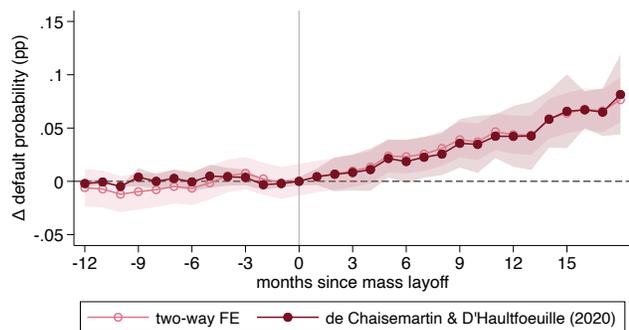
where  $\alpha_i$  and  $\gamma_t$  are individual and month fixed-effects. With this specification, we can compare borrower behavior before and after a displacement event (i.e., job separation as part of a mass layoff). We also include dummies for leads and lags and to provide suggestive evidence for parallel trends. In addition to the standard two-way fixed effects model, we use the staggered difference-in-difference methodology developed by [de Chaisemartin and D’Haultfoeuille \(2022\)](#), which remains valid even with heterogeneous and dynamic displacement effects on default. We estimate a “fully-dynamic” model (in the language of [Borusyak et al., 2023](#)), 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 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. \(2023\)](#).<sup>37</sup>

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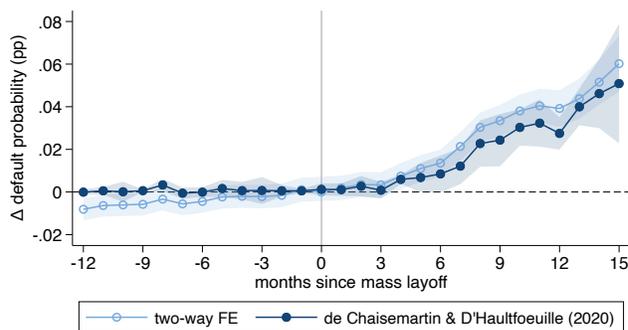
<sup>37</sup>[Keys \(2018\)](#) uses U.S. household survey data to examine the effects of the receipt of unemployment insurance on

**Figure 8: Job Displacement and Default**

**(a) Experimental Sample & Default in Experiment Card**



**(b) Full Credit Bureau & Default in Any Loan**



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

Figure 8(a) shows the effect of job separation as part of a mass layoff on default for our experimental sample during the experiment. The dependent variable is cumulative default in the experimental card—the same outcome as in the previous sections. We estimate no differential pre-trends in default between displaced and non-displaced workers before separation, suggesting that their behavior in the credit market was similar prior to separation. We find that one year after separation, borrowers are 4.8 pp. more likely to default on the study card, and this figure increases to 6.1 pp. after eighteen months.

Figure 8(b) repeats our estimation exercise using the intersection of the representative one million 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 extends our sample considerably and appears to be the first time such an estimation has been carried out. The larger sample yields substantially more mass-layoff events (8,723) and finds quantitatively similar results to those above, thereby providing a measure of external validity to our study estimates.

The estimated 18-month effect on default of 6.1 pp. is almost six times the effect of a 30 pp. increase in interest rates (which is 1.03 pp. at the 18-month horizon) and seven times larger than the effect of doubling minimum payments over the same horizon (.8 pp.). Documenting these substantially larger effects on a common sample of newer borrowers and over a common time frame is a valuable exercise in and of itself, as it can help order policy priorities (i.e., comparative magnitudes matter for policy). However, it

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bankruptcy filing in a standard TWFE framework. Our approach uses administrative data to define both default and unemployment and mass layoffs; 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) is also related, 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.

is also instructive to attempt to normalize the two kinds of interventions in a way that allows us to quantify the relative “strength” of an unemployment shock relative to an interest rate increase of (say) 30 pp. We do so by carrying out a simple back-of-the-envelope calculation that quantifies the difference in the magnitudes of the two shocks using a common metric (both evaluated at the 18-month horizon) and thus attempts to make them more directly comparable. The calculation details are described in [Appendix I](#) and we summarize the results here.

We attempt the comparison in two different ways. First, we perform a simple accounting exercise to understand the relative magnitudes of the two shocks by calculating the 18-month changes in *income* generated by the two shocks (under admittedly restrictive assumptions). We find that a displacement event generates a 3.2 times larger income loss than a 30 pp. increase in interest rates and generates 5.9 times more default than a 30 pp. change in interest rates. Under this calculation, the effect of job displacement on default is approximately twice ( $5.9/3.2 = 1.84$ ) as large as the effect of the 30 pp. interest rate increase once we normalize by the size of the income shock.

In the second approach, under a different set of assumptions, we examine the effects of each shock via a common intermediate outcome—debt—linked to default.<sup>38</sup> According to our estimates, interest rates would have to rise by a factor of 5 (from a baseline of 45%) to generate a debt profile that would result in the same default rates as those generated by the displacement-induced debt profile. Our conclusion from these two exercises is that the magnitude of a displacement shock is approximately 2–6 times larger than a 30 pp. increase in interest rates even after normalization.<sup>39</sup>

**Discussion.** We offer four conclusions from the event-study regressions and the previous experimental analysis. First, job displacement has substantial and persistent negative effects on default, even in a context with widespread informal employment and insurance (see, e.g., [Morduch, 2004](#); [Ohnsorge and Yu, 2022](#), on the role of informal insurance and informal employment as buffers from economic disruptions). Second, even after the normalizations above, displacement-induced default is far higher than default arising from large increases in contract terms, potentially because job loss has negative effects in many domains beyond income (as documented elsewhere). Third, that both effects were estimated from the same study sample is particularly reassuring since it eliminates some of the obvious problems in such comparisons. Finally, the starkness of the findings combined with the high frequency of unemployment in our context 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 narrative consistent with the attractiveness of formal credit terms, large baseline default levels, relatively modest treatment effects, and large unemployment effects is that new borrowers value the study card and that consequently even very large increases in interest rates do not substantially increase default. However, the frequency and consequences of job loss are more complicated to mitigate, and, as in the model, borrowers have no recourse but to default.

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<sup>38</sup>See e.g., [Adams et al. \(2008\)](#); [Aydin \(2023\)](#); [Bizer and DiMarzo \(1992\)](#); [Dobbie and Song \(2020\)](#); [Ganong and Noel \(2020\)](#); [Parlour and Rajan \(2001\)](#) for literature linking debt to default.

<sup>39</sup>Our calculations suggest that interest rates would need to rise from 45% to 228%, so  $(228 - 45) = 183$  and  $183/30 \approx 6$ .

## 8 Conclusion

Credit card borrowing is an increasingly common way for borrowers to first access formal sector credit in many developing countries and has received increased attention from policymakers and regulators. In this paper, we examine a large-scale effort by a commercial Mexican bank to expand credit by issuing credit cards to poor and financially inexperienced new borrowers. We combine detailed card-level data for a bank product that accounted for 15% of all first-time formal loans in Mexico in 2010 with individual employment histories and a nationwide randomized experiment with 144,000 borrowers.

We find that default rates are high overall and higher for newer borrowers. Next, we use an RCT to assess default sensitivity to key contract terms. We find that doubling the minimum payment increased default during the experiment, contrary to the beliefs underpinning recent regulations but consistent with our modeling framework. We also find that large increases in interest rates have modest effects on default and no effect on new borrowers.

In stark contrast, default in our experimental sample is very responsive to plausibly exogenous formal job separation. The frequency of separations and the magnitude of the default effects emphasize the fragility of economic conditions for new borrowers. While beyond the scope of this paper, negative shocks more generally could help explain the puzzle of why lower-income borrowers recurrently fall into debt (see, e.g., the discussion in [Karlan et al., 2019](#)).

Bank A stopped issuing the study card in 2009. We speculate that the combination of the ineffectiveness of contract terms in limiting default documented here, combined with the limited information available for screening borrowers, contributed to the bank's decision. Given the difficulty of modifying default behavior, improving the screening of loan applicants would appear to be critical. This screening is clearly challenging for borrowers with limited histories. However, there has been progress with mobile (see e.g., [Björkegren and Grissen, 2019](#)) and other kinds of data. A broader question is how much the distance-lending model, such as the one adopted by Bank A and other commercial banks (individual lending, credit-score-based screening, remote monitoring, and collection) can expand credit to underserved populations with limited credit histories. Finally, while we focus on job loss, illness and other negative shocks could also be important. Given the prevalence of such shocks it seems important to examine 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](#)).

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# Contract Terms, Employment Shocks, and Default in Credit Cards

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Appendix – For Online Publication

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## A Additional Tables and Figures

### A.1 Additional Tables

**Table OA-2: Sampling weights**

	Cardholder's payment behavior			Total (4)
	Minimum payer (1)	Part-balance payer (2)	Full-balance payer (3)	
	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-3: 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*** (31)	-108** (48)	-7.08 (38)	6,184.3*** (288)	4,926*** (484.3)	3,934*** (659.3)	0.554*** (0.034)	0.544*** (0.058)	0.491*** (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-4: Experimental Design**

<i>Panel A: Stratification</i>				
	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

<i>Panel B: Sample Sizes for Arms Within Strata</i>		
Interest Rate	Minimum payment	
	10%	5%
15%	2000	2000
25%	2000	2000
35%	2000	2000
45%	2000	2000
Hold out group	2,000	

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

**Table OA-5: Randomization Check - Baseline Statistics for March 2007**

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

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

**Table OA-6: Credit Limits and Treatment Arms**

Months since experiment started:	0	4	7	11	15	19	22	26
	Mar/07	Jul/07	Oct/07	Feb/08	Jun/08	Oct/08	Jan/09	May/09
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$(45\% - r_i)/30\%$	20 (87)	78 (129)	204 (157)	188 (165)	102 (154)	123 (161)	194 (166)	187 (180)
$\mathbb{1}\{MP_i = 10\%\}$	-48 (64)	-77 (96)	-144 (117)	-101 (123)	-198 (114)	-224 (119)	-225 (123)	-174 (134)
Constant	7,901*** (65)	10,333*** (96)	12,058*** (116)	12,207*** (122)	11,703*** (115)	11,356*** (120)	11,264*** (122)	11,790*** (134)
$H_0 = \text{no ATEs}$	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-7: Experimental Effects of Contract Terms on Default**

Months since experiment started:	Experimental period			Post-experimental period					
	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)
<i>Panel A. Main specification</i>									
$(45\% - r_i)/30\%$	-0.001 (0.001)	-0.006 (0.004)	-0.025*** (0.005)	-0.033*** (0.006)	-0.020*** (0.006)	-0.013* (0.006)	-0.005 (0.007)	-0.004 (0.007)	-0.004 (0.007)
$\mathbb{1}\{MP_i = 10\%\}$	-0.001 (0.001)	0.011*** (0.003)	0.008* (0.004)	-0.005 (0.004)	-0.008 (0.005)	-0.009* (0.005)	-0.010* (0.005)	-0.010* (0.005)	-0.010 (0.005)
Constant	0.011*** (0.001)	0.086*** (0.003)	0.193*** (0.004)	0.259*** (0.004)	0.309*** (0.005)	0.349*** (0.005)	0.373*** (0.005)	0.396*** (0.005)	0.412*** (0.005)
<i>Panel B. Fully saturated model</i>									
$\mathbb{1}\{r = 15, MP = 5\}$	0.001 (0.002)	-0.002 (0.005)	-0.026*** (0.008)	-0.038*** (0.009)	-0.021* (0.009)	-0.014 (0.010)	-0.004 (0.010)	-0.004 (0.010)	0.001 (0.010)
$\mathbb{1}\{r = 15, MP = 10\}$	-0.001 (0.002)	0.007 (0.005)	-0.014 (0.008)	-0.039*** (0.009)	-0.028** (0.009)	-0.024** (0.010)	-0.016 (0.010)	-0.015 (0.010)	-0.014 (0.010)
$\mathbb{1}\{r = 25, MP = 5\}$	0.001 (0.002)	-0.003 (0.005)	-0.022** (0.008)	-0.036*** (0.009)	-0.022* (0.009)	-0.017 (0.010)	-0.013 (0.010)	-0.017 (0.010)	-0.014 (0.010)
$\mathbb{1}\{r = 25, MP = 10\}$	-0.000 (0.002)	0.007 (0.005)	-0.008 (0.008)	-0.028** (0.009)	-0.020* (0.009)	-0.012 (0.010)	-0.012 (0.010)	-0.015 (0.010)	-0.014 (0.010)
$\mathbb{1}\{r = 35, MP = 5\}$	0.003 (0.002)	0.006 (0.006)	0.000 (0.008)	-0.007 (0.009)	0.003 (0.009)	0.005 (0.010)	0.005 (0.010)	0.005 (0.010)	0.006 (0.010)
$\mathbb{1}\{r = 35, MP = 10\}$	0.002 (0.002)	0.016** (0.006)	-0.001 (0.008)	-0.018* (0.009)	-0.010 (0.009)	-0.009 (0.010)	-0.004 (0.010)	-0.007 (0.010)	-0.001 (0.010)
$\mathbb{1}\{r = 45, MP = 10\}$	0.001 (0.002)	0.013* (0.006)	0.005 (0.008)	-0.016 (0.009)	-0.015 (0.009)	-0.018 (0.010)	-0.019 (0.010)	-0.019* (0.010)	-0.015 (0.010)
Constant ( $r = 45, MP = 5$ )	0.009*** (0.001)	0.083*** (0.004)	0.193*** (0.006)	0.263*** (0.006)	0.309*** (0.007)	0.348*** (0.007)	0.374*** (0.007)	0.398*** (0.007)	0.412*** (0.007)
<i>Panel C. Hypothesis testing with fully saturated model (p-values)</i>									
$r$ 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 categorical 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 minimum 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-8: Comparisons with the Literature**

Paper	Outcome	Table (page)	Point Estimate (S.E.)	Elasticity (S.E.)
Karlan and Zinman (2009)	Account in Collection	3 (p.37)	-1.60 (1.58)	+0.27 (0.14)
Adams et al. (2009)	Default Hazard	4 (p.28)	1.022 (.002)	+2.2 (0.2)
d’Astous and Shore (2017)	Default	p.3	.04	+0.06
Keys and Wang (2019)	Delinquency	2 (p.542)	.4 (.3)	+0.01 (.12)
Karlan and Zinman (2019)	Delinquency	5 (p.42)	-1.96 (1.45)	+1.80
DeFusco et al. (2021)	Charge-Off	(p.2)	.1	.01

Notes: We use the working paper version of Karlan and Zinman (2009), Table 3 cols (4) and (5) for the “repayment burden effect.” The table reports a decline from 13.9 to 12.3 in the percentage of accounts in collection status over a four month period. The difference between the high and the low interest rate was on average 350 basis points. We use the high risk category upper bound for the interest rate of 11.75 percent as the base rate and convert the monthly interest rates to APR to facilitate comparisons (the calculation is  $(-1.6/13.9)(279/-120) = .27$ ). For Karlan and Zinman (2019) we use the results from Table 5 (col (4), Panel B) that show delinquencies decline by 1.96 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 .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-9: Habit Formation on 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*** (150)		616*** (150)		295** (110)	
MP = 10	5.1 (138)	7.3 (28)	4.7 (138)	7.5 (28)	44 (86)	3.8 (28)
Min. payer	1383*** (158)	-475*** (59)	1383*** (157)	-478*** (59)	224* (108)	-433*** (34)
MP = 10 × Min. payer	-159 (233)	32 (40)	-160 (233)	32 (40)	-26 (157)	28 (39)
Amount due		0.097** (0.035)		0.097** (0.036)		0.14 (0.075)
Strata FE	no	no	yes	yes	yes	yes
Current card terms	no	no	no	no	yes	yes
Dependent variable mean	6680	748	6680	748	6680	748
Observations	33,206	33,206	33,206	33,206	33,206	33,206
R-squared	0.0084	0.1683	0.0118	0.1689	0.5109	0.1780

Notes: Robust standard errors are shown in parentheses. The sample is those cards that (i) participated in the experiment (ii) remained opened by 2010, and (iii) were assigned to either the highest or lowest interest rate groups (eg. [r = 15, MP = 5], [r = 15, MP = 10], [r = 45, MP = 5], and [r = 15, MP = 10]). Each column represents a different regression. Columns (2), (4) and (6) have as a dependent variable the amount paid (“Payments”) on June 2010, as a function of the minimum payment that was assigned during the experiment and debt (“Amount due”). We are most interested in the coefficient of MP = 10 in the even columns which measures the effect of having been subjected to higher MP in the past on payment amount in the future when the MP is no longer high, conditional on current debt. Since debt can be endogenous, we instrument for debt using the interest rate group cardholders were assigned to. Not instrumenting for debt leads to similar conclusions regarding the effect of MP10. We also allow for a differential treatment effect for those in the “minimum-payment” strata. The dependent variable of Columns (1), (3) and (5) is the amount due on June 2010. Columns (1) and (2) show the regression equations without additional controls. Columns (3) and (4) add the months with credit cards strata dummies. Columns (5) and (6) add both the months with credit cards strata dummies as well as current contract terms, namely the interest rate and the required minimum payment in pesos in June 2010. One, two and three stars denote statistical significance at the 5, 1 and 0.1 percent level respectively.

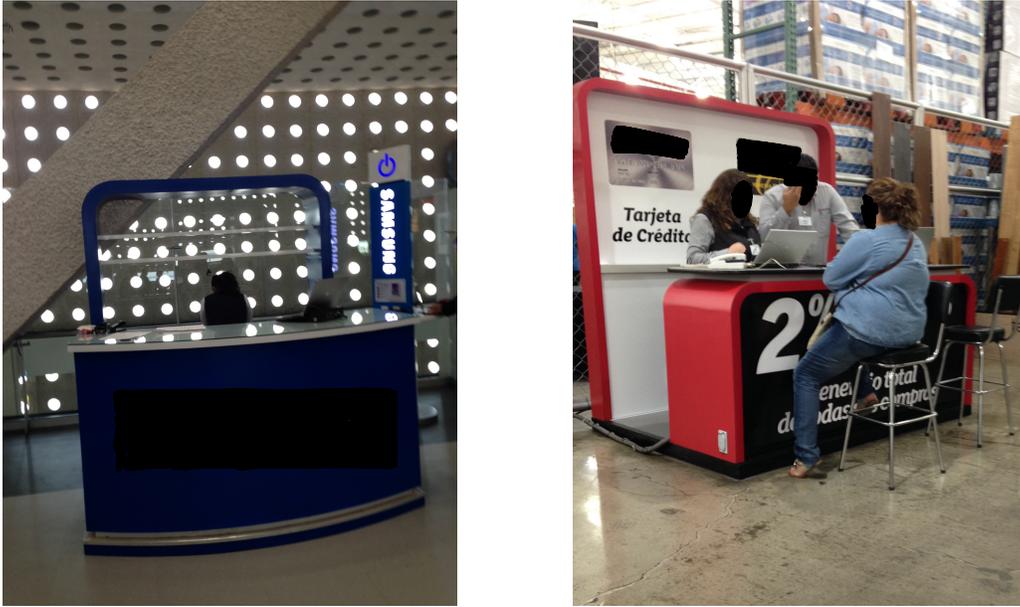
**Table OA-10: Difference in Average Treatment Effect Across Months With Card Strata**  
(Cumulative Default by May 2009 - Experiment Endline)

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

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

## A.2 Additional Figures

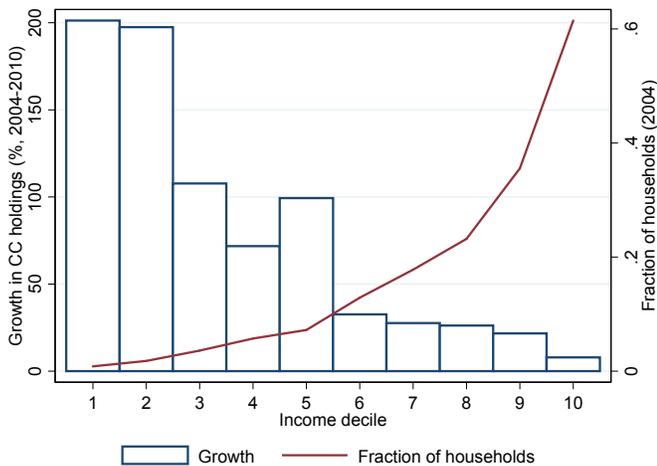
Figure OA-9: Example of Promotional Kiosks



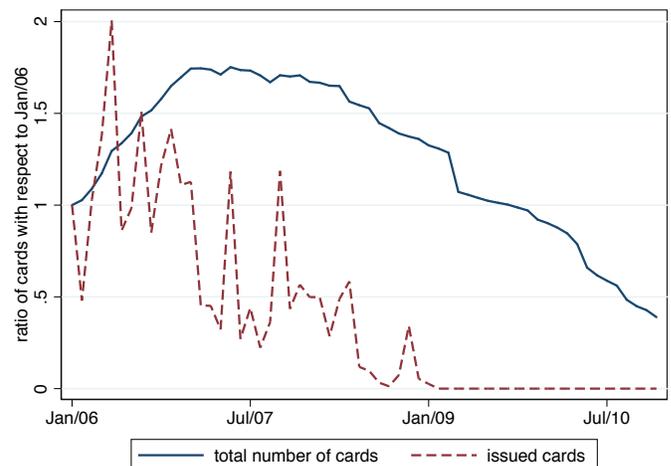
Notes: These kiosks do not necessarily correspond to those for our study card (for confidentiality reasons). They are similar to the ones Bank A used to sign up individuals for the study card.

Figure OA-10: Overall credit card growth, and study card's share and evolution.

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

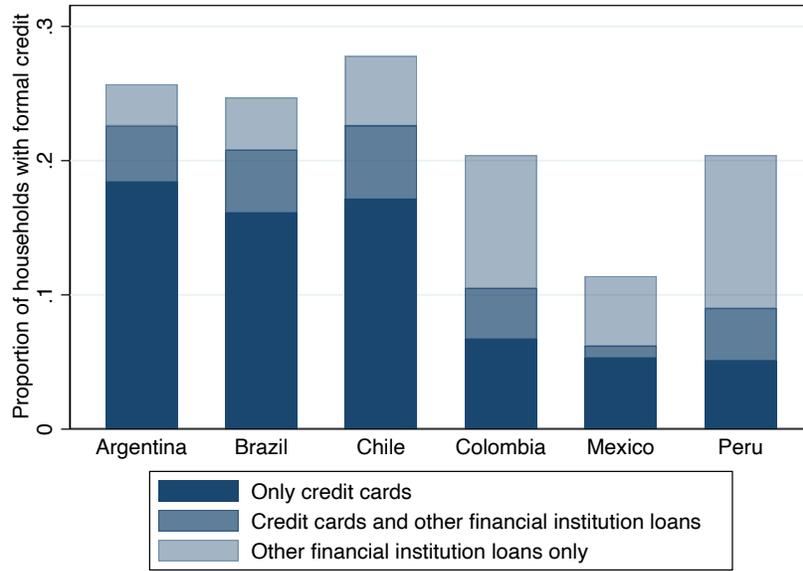


(b) Study Card Stocks and Flows



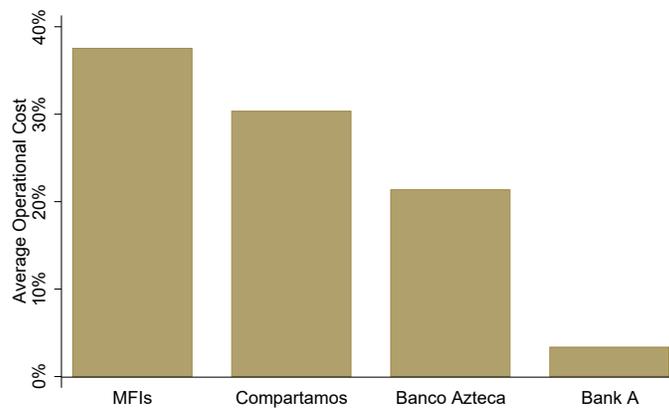
Notes: Panel (a) is constructed using data from the 2004 National Income Expenditure Survey (ENIGH). The X-axis represents (household) income deciles (the 10th decile is the richest decile). The left Y-axis—corresponding to the hollow bars—shows the percentage growth in the number of households that have at least one credit card from 2004 to 2010. The right Y-axis—associated with the red line—plots the fraction of households in each income decile that have at least one card in 2004. Panel (b) is constructed using credit bureau data from 2012 on Card A. For confidentiality purposes we normalize the January 2006 values for both the total number of study cards and the number of issued study cards to 1. The solid blue line represents the total number of study cards in a given month (stock). The red dashed line represents the flow of study cards: the total number of new study cards issued in a given month.

**Figure OA-11: Credit card and other borrowing across Latin American countries (2017)**



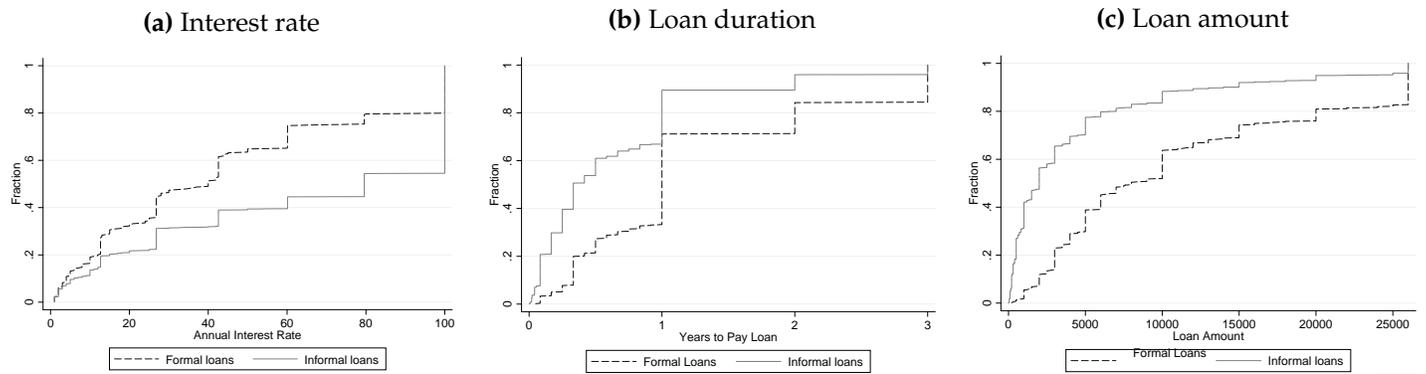
*Notes:* Source is the 2017 World Bank Global Findex Database. The figure shows the proportion of adults who have had credit in the past 12 months for selected countries in Latin America. Formal credit is defined as credit issued by a bank or another type of financial institution. Credit holders are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

**Figure OA-12: Operational Costs (relative to Assets): Compartamos, Azteca, and Bank A**



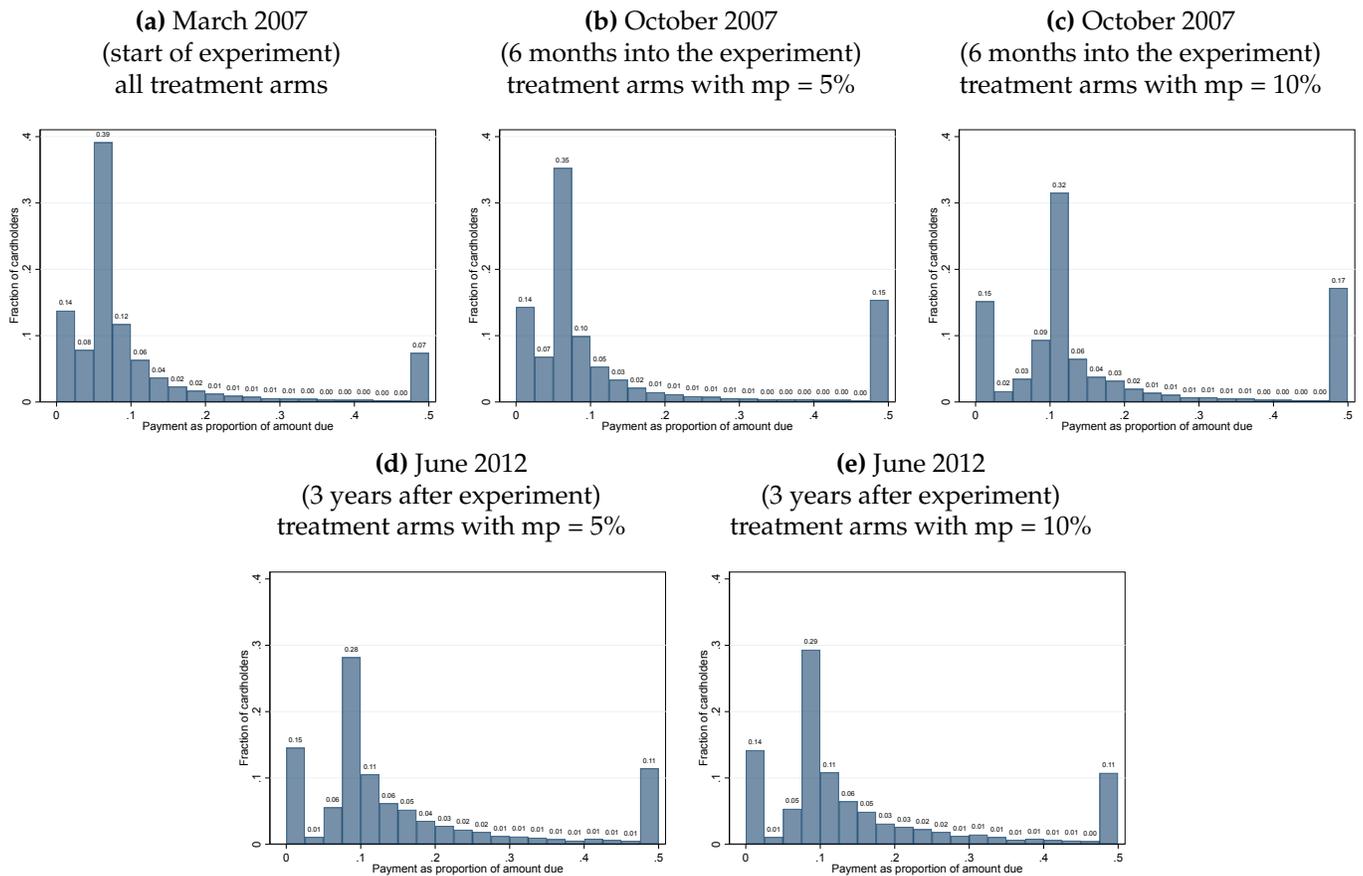
*Notes:* The cost ratio is defined as the ratio of administrative and promotion spending to total assets. Data is taken from the Mexican Banking Commission (CNBV) at <https://portafoliodeinformacion.cnbv.gob.mx/bm1/Paginas/infosituacion.aspx> (under 040-5Z-R6, indicadores financieros). We average annual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

**Figure OA-13: Comparison formal and informal loan market in Mexico**



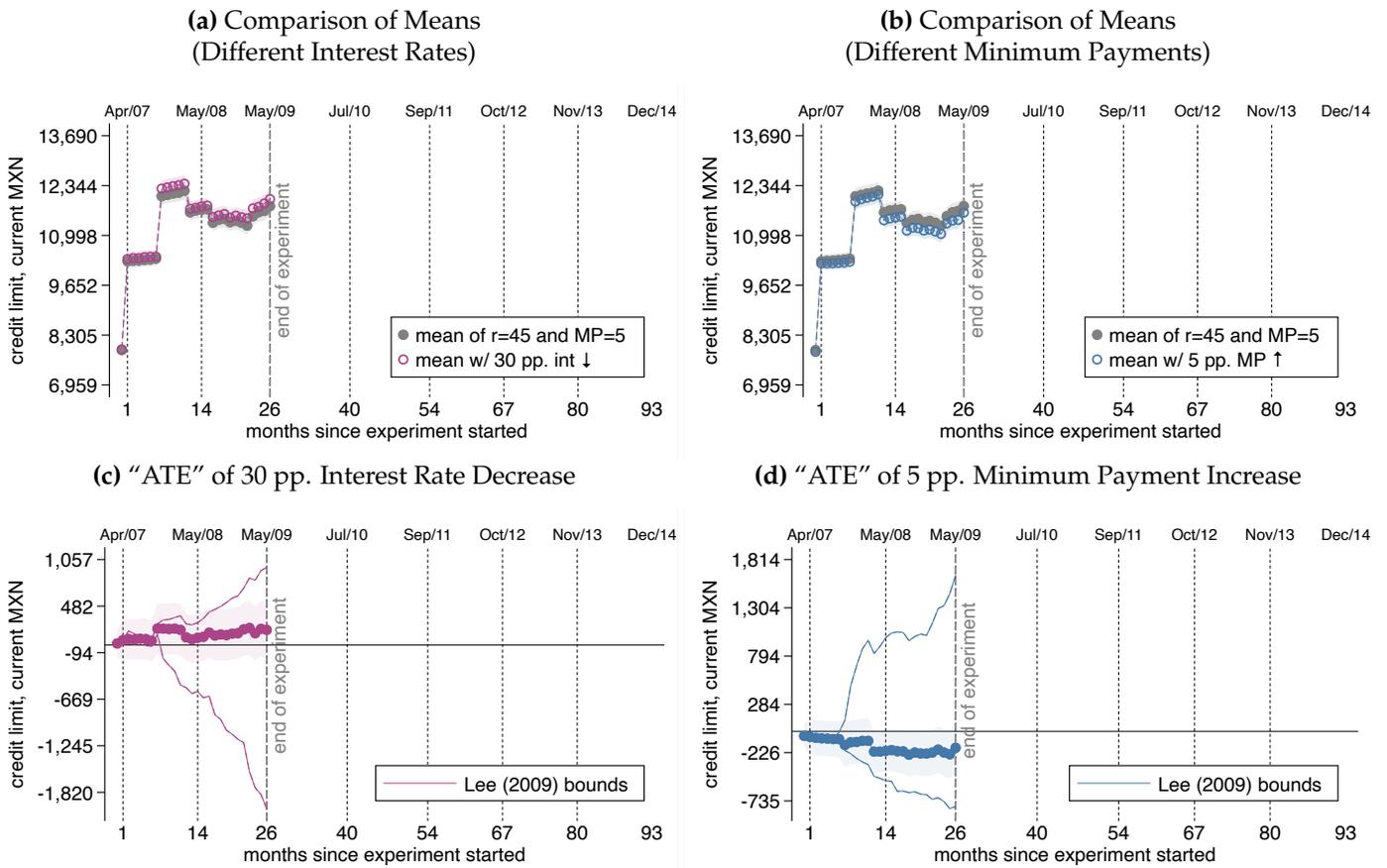
Notes: The above figures compare the formal and informal credit market in Mexico using the annual interest rate (a), the loan tenure in years (b) and the loan amount in pesos (c). This data comes from ENNVIH survey reported by the INEGI on years 2002, 2005, and 2009. The lines represent the cumulative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

**Figure OA-14: Payment as a Fraction of Debt Before and During the Experiment**



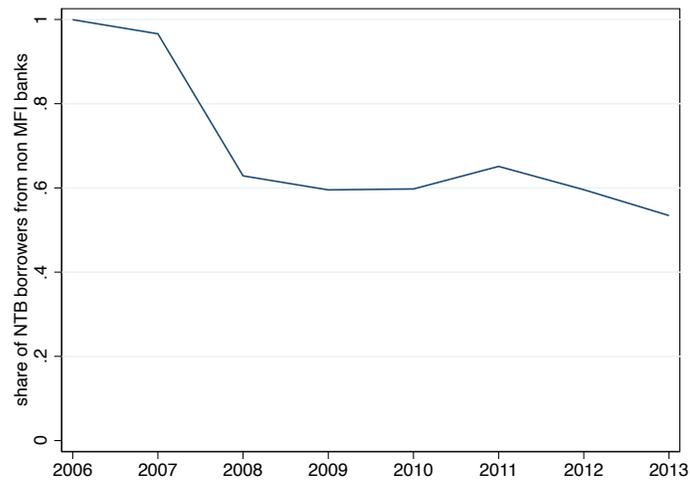
Notes: We plot monthly payment divided by the amount due. In Figure (a) this is the ratio of monthly payments in April 2007 and the amount due in the March 2007 statement. We follow a similar approach (current month payments divided by previous month end balance) in the other panels. We right-censor 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-15: Credit Limits by Month by Treatment Arms**



*Notes:* These figures plot the relationship between interest rates, minimum payment and credit limits. The figure shows credit limits were orthogonal to randomization—Table OA-6 formally tests this hypothesis. The dependent variable is credit limit (conditional on the card being active). 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 call them “average treatment effects” despite credit limits not being about borrower behavior for consistency with the rest of the paper.

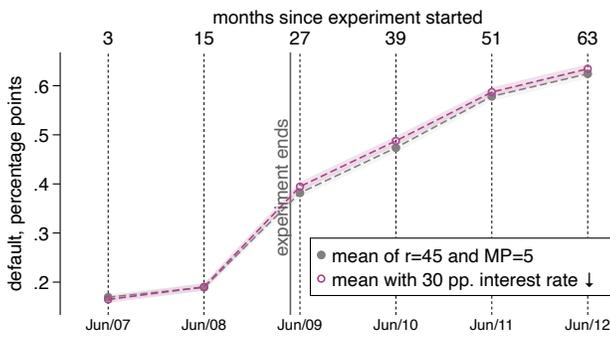
**Figure OA-16: Study Card Demise Coincides With Smaller Share Of Loans Going To New Borrowers**  
(Share of New Loans Going to New Borrowers in Mexico, All Lenders)



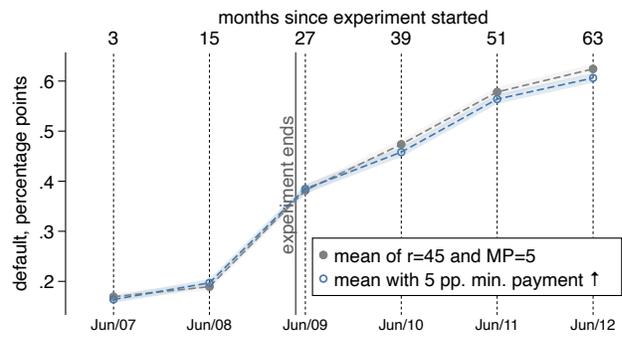
*Notes:* This figure plots the fraction of total newly originated loans (for the whole of Mexico) going to borrowers with no previous formal credit history each year, from 2006 when our study card was in its peak throughout the period in which Bank A reduced its rate of issuance (2007-2009), and when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

**Figure OA-17: Spillovers to Bank A: Default on Other Bank A Loans (top) & New Bank A Loans (bottom)**

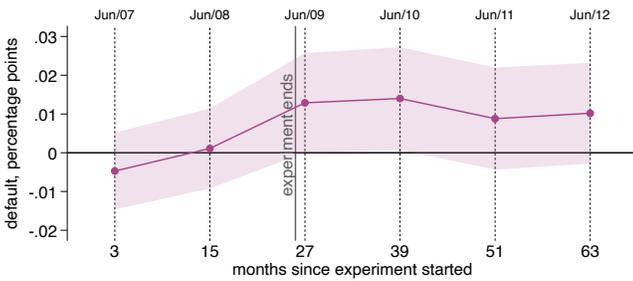
**(a) Comparison of Means w/ Different Interest Rates**



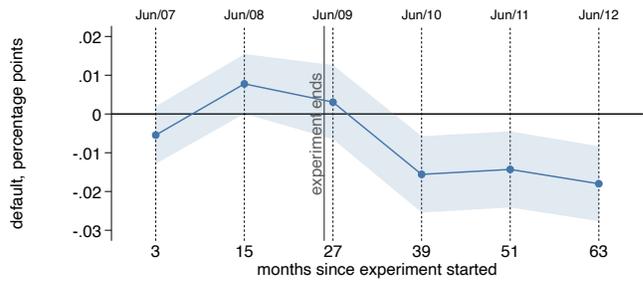
**(b) Comparison of Means w/ Different Min. Payments**



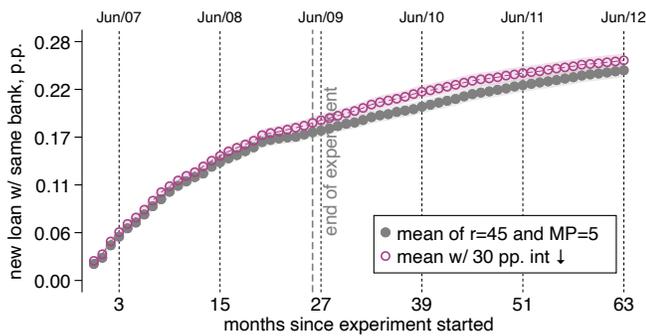
**(c) ATE of 30 pp. Interest Rate Decrease**



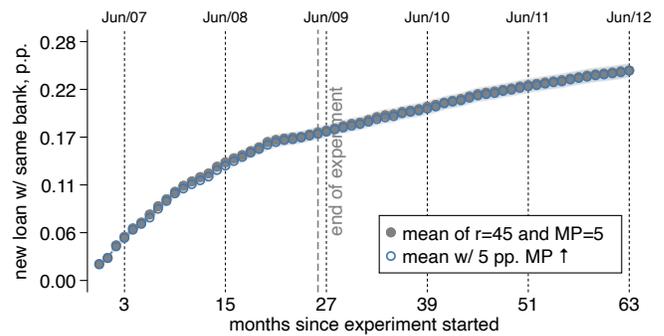
**(d) ATE of 5 pp. Minimum Payment Increase**



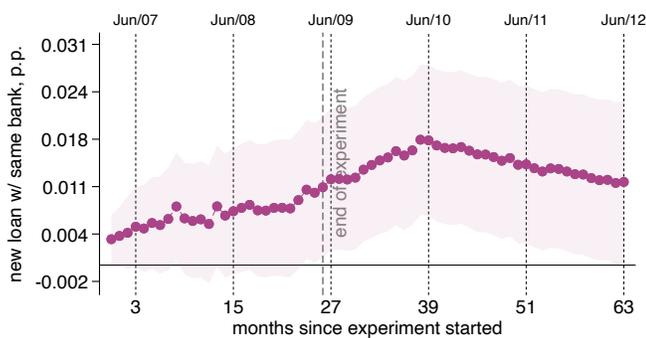
**(e) Comparison of Means w/ Different Interest Rates**



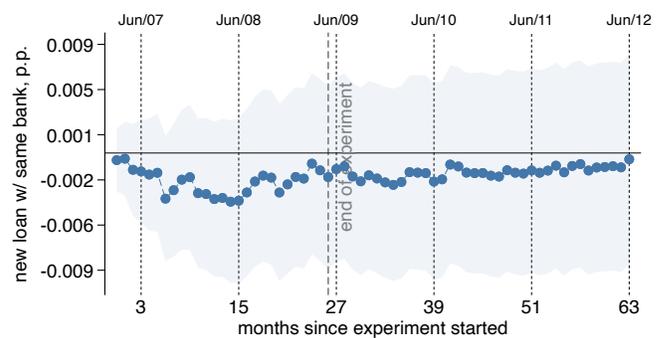
**(f) Comparison of Means w/ Different Min. Payments**



**(g) ATE of 30 pp. Interest Rate Decrease**



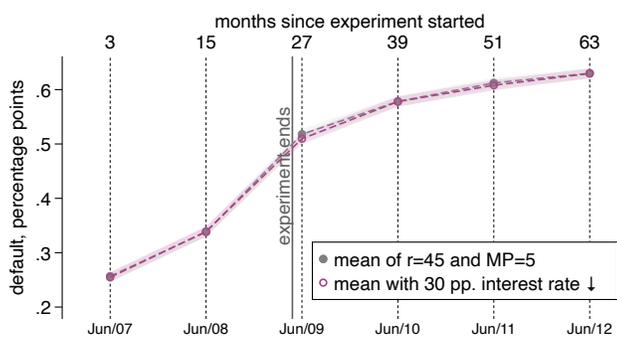
**(h) ATE of 5 pp. Minimum Payment Increase**



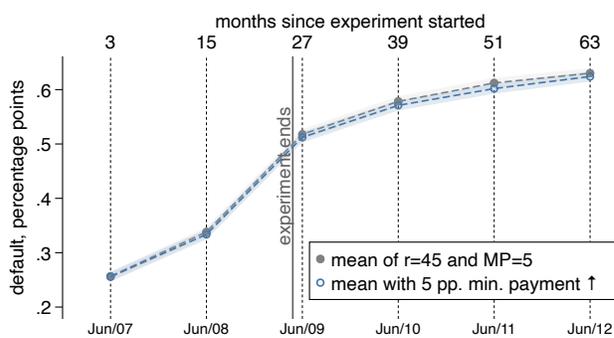
*Notes:* These figures plot the causal effect of interest rates and minimum payment changes on default in other loans in the same bank and on new Bank A loan issuances. The dependent variable is default in any loan issued by Bank A except for the experiment credit card (a to d) and a cumulative categorical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

**Figure OA-18: Spillover to Other Banks: Default on Other Bank Loans (top) & New Bank Loans (bottom)**

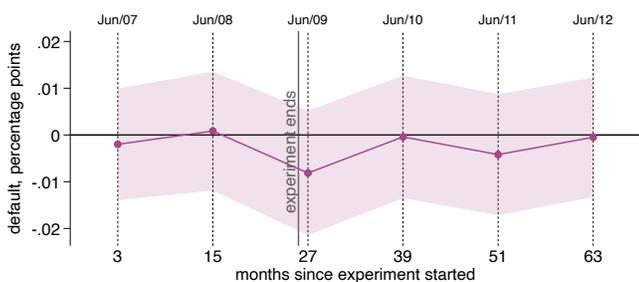
**(a) Comparison of Means w/ Different Interest Rates**



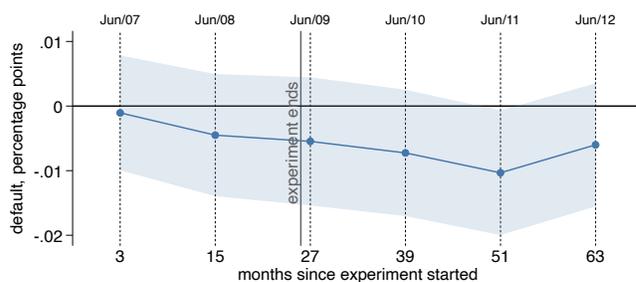
**(b) Comparison of Means w/ Different Min. Payments**



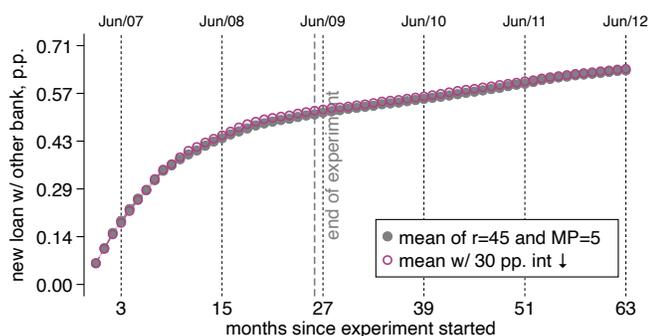
**(c) ATE of 30 pp. Interest Rate Decrease**



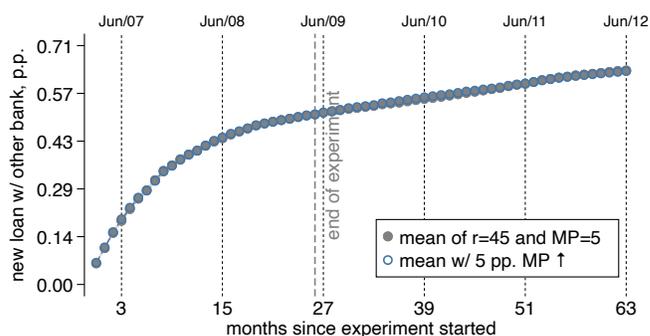
**(d) ATE of 5 pp. Minimum Payment Increase**



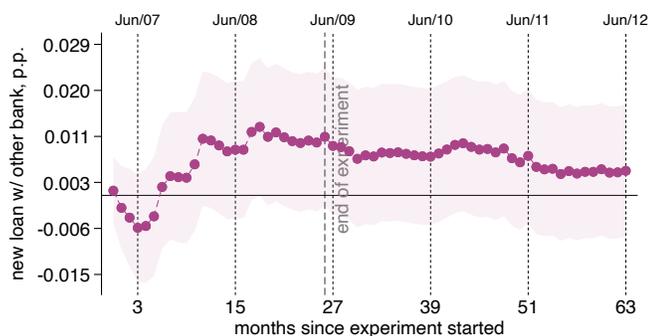
**(e) Comparison of Means w/ Different Interest Rates**



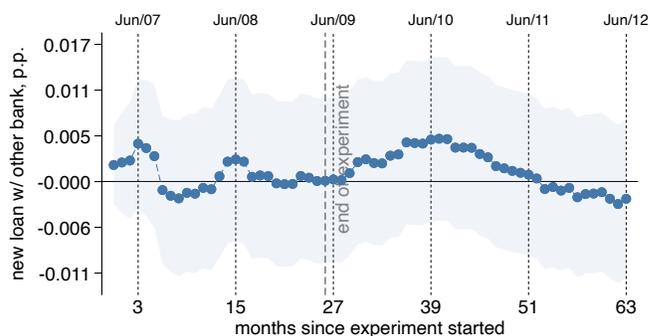
**(f) Comparison of Means w/ Different Min. Payments**



**(g) ATE of 30 pp. Interest Rate Decrease**

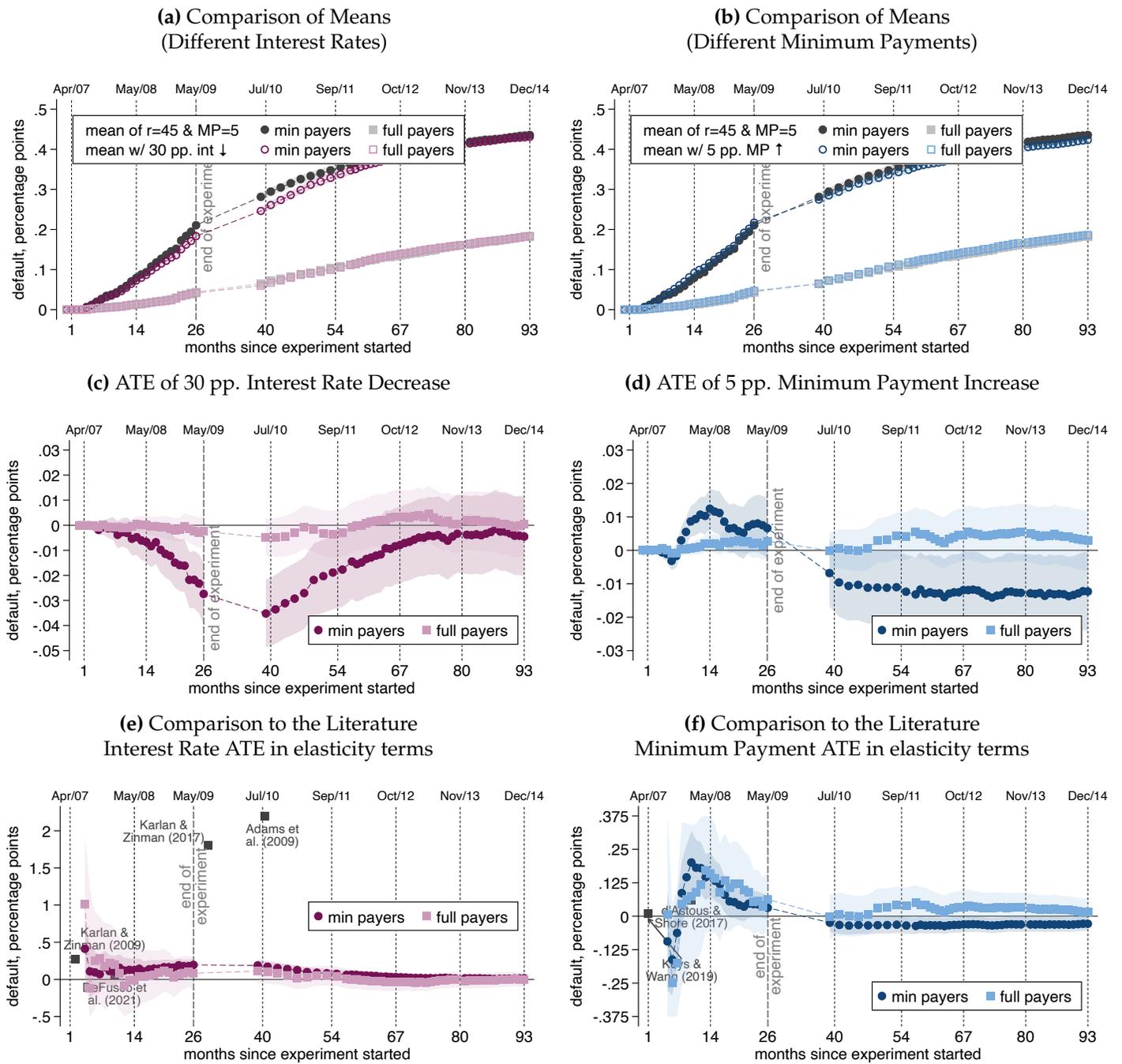


**(h) ATE of 5 pp. Minimum Payment Increase**



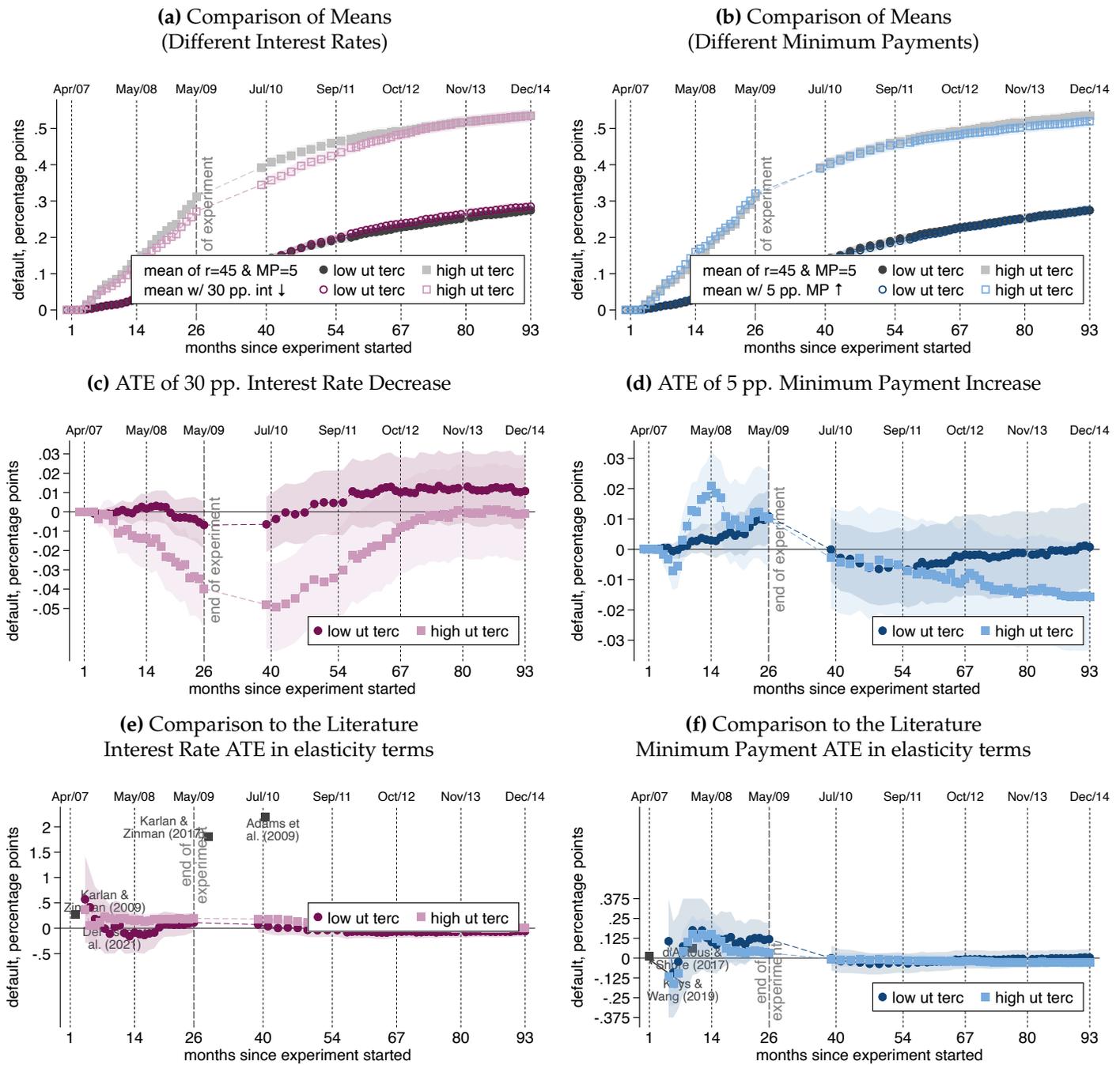
*Notes:* These figures plot the causal effect of interest rates and minimum payment changes on default in other loans in any bank (except for Bank A) and on new loan issuances (in any loan except for Bank A). The dependent variable is default in any loan issued by any bank (except for Bank A) (a to d)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

**Figure OA-19: Treatment Effect of Contract Terms on Default by Payment Behavior**  
(Share of Cardholders that Default)



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

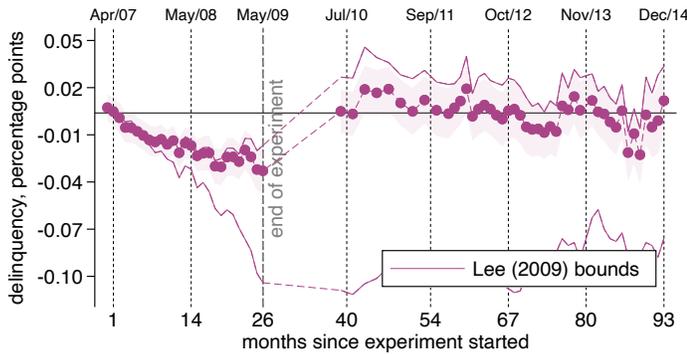
**Figure OA-20: Treatment Effect of Contract Terms on Default**  
(By Baseline Credit Utilization = Amount Due / Limit)



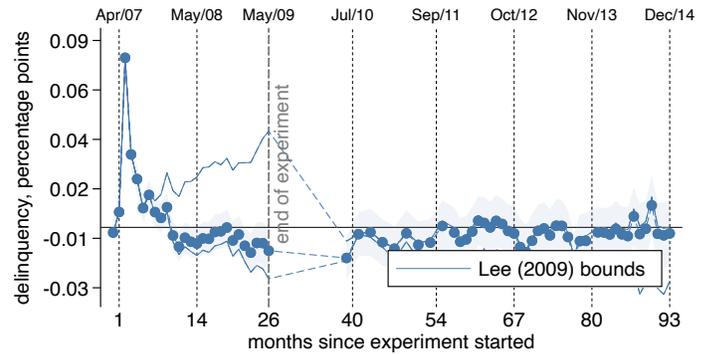
*Notes:* These figures plot the causal effect of interest rates and minimum payment changes on default in the experiment credit card. We separate borrowers using the ratio between amount due and credit limit, defining these two groups as “low” if in the lowest tercile or “high” if in the highest. Figures on the left examine interest rate changes. 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 computing the average treatment effect in percent terms (i.e.,  $\gamma_t/\alpha_t$  in Equation 1) and dividing it by  $(45 - 15)/45$ . Similarly, Panel (f) computes the elasticity of default (i.e.,  $\beta_t/\alpha_t$  in Equation 1 divided by  $(10 - 5)/5$ ) with respect to a minimum payment increase from 5 to 10%.

**Figure OA-21: Treatment Effect of Contract Terms on Non-Cumulative Delinquency**  
(Payment Below Required Minimum, percentage points)

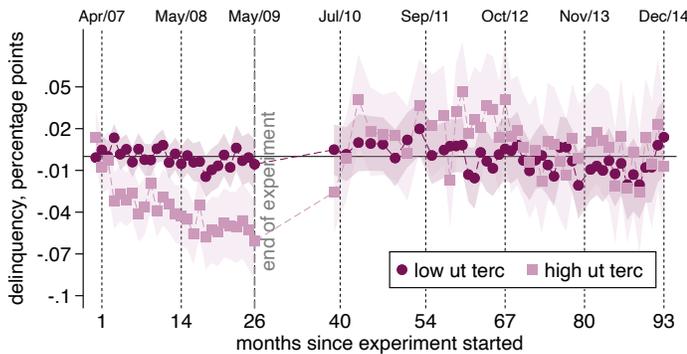
**(a) ATE of 30 pp. Interest Rate Decrease**



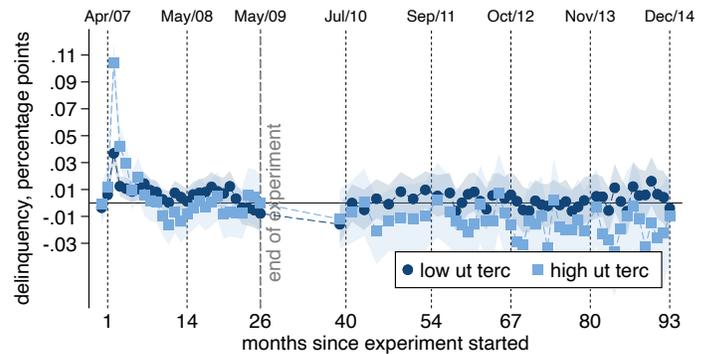
**(b) ATE of 5 pp. Minimum Payment Increase**



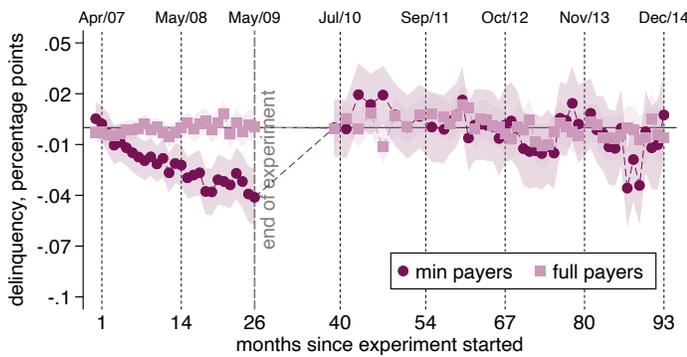
**(c) ATE of 30 pp. Interest Rate Decrease  
(By Baseline Utilization)**



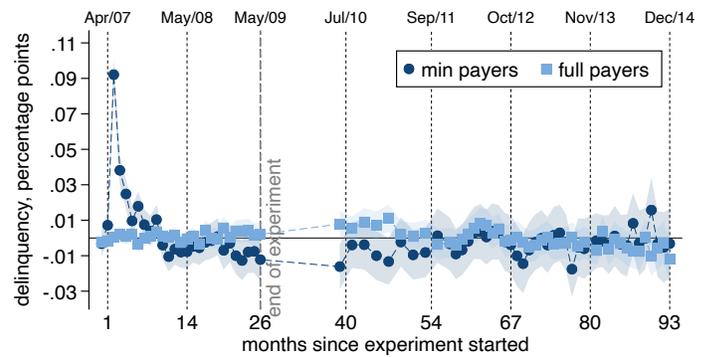
**(d) ATE of 5 pp. Minimum Payment Increase  
(By Baseline Utilization)**



**(e) ATE of 30 pp. Interest Rate Decrease  
(By Payment Behavior)**

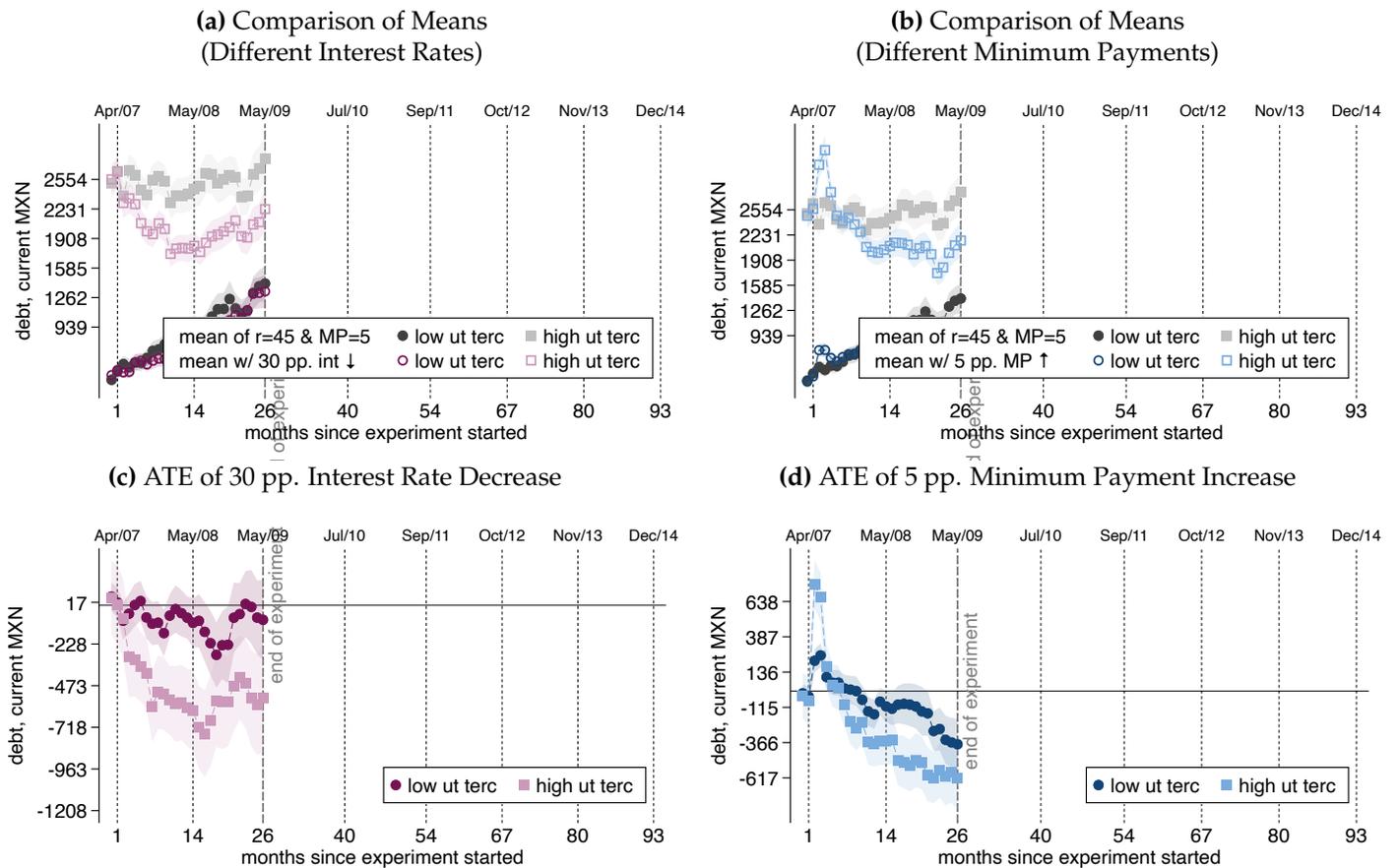


**(f) ATE of 5 pp. Minimum Payment Increase  
(By Payment Behavior)**



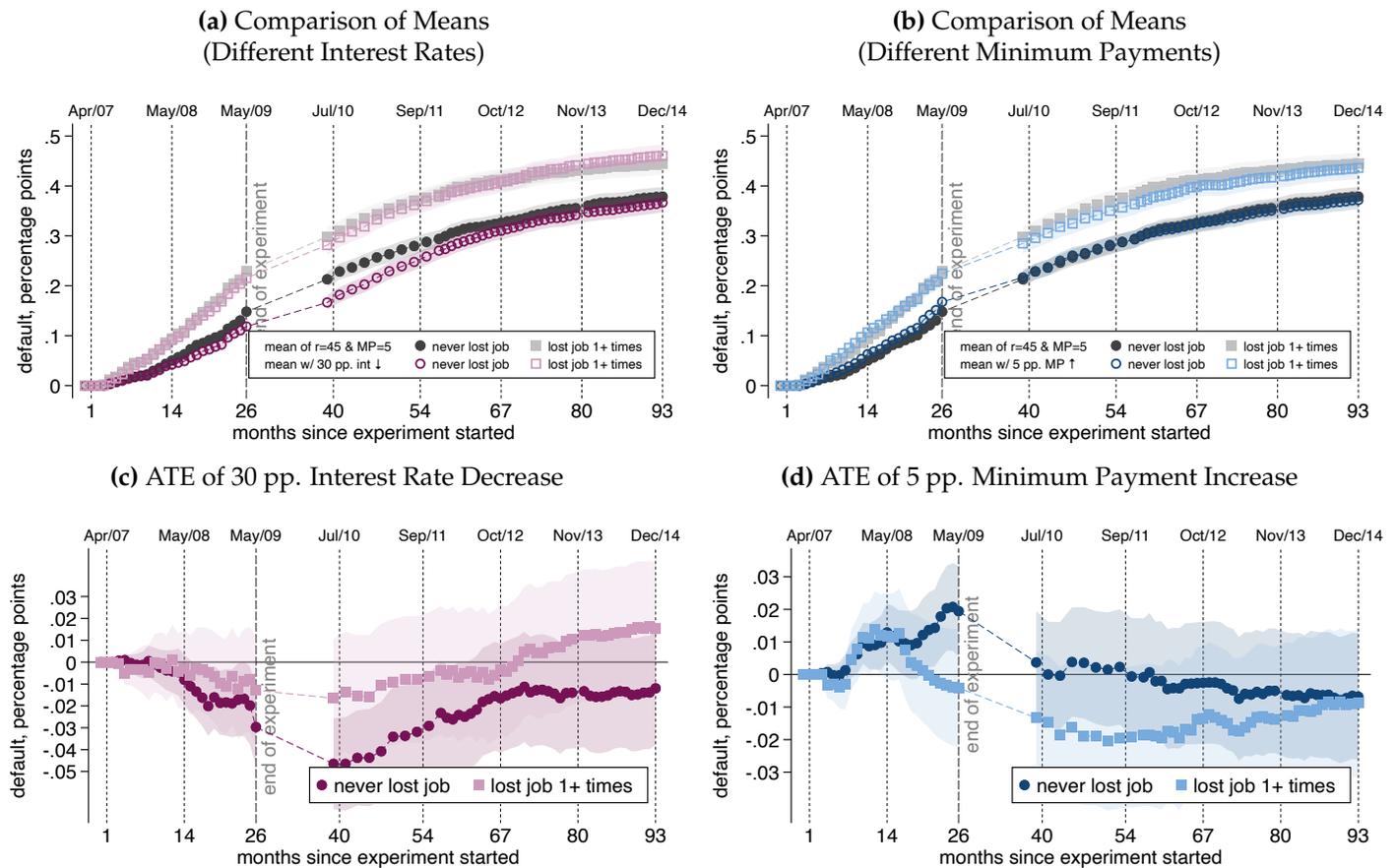
*Notes:* These figures plot the causal effect of interest rates and minimum payment changes on the non-cumulative delinquency in the experiment credit card for selected subpopulations. Delinquency is defined as a categorical variable indicating if the actual payment is smaller than the required minimum 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 "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-22: Treatment Effects of Contract Terms on Debt**  
(By Initial Credit Utilization = Amount Due/Limit)



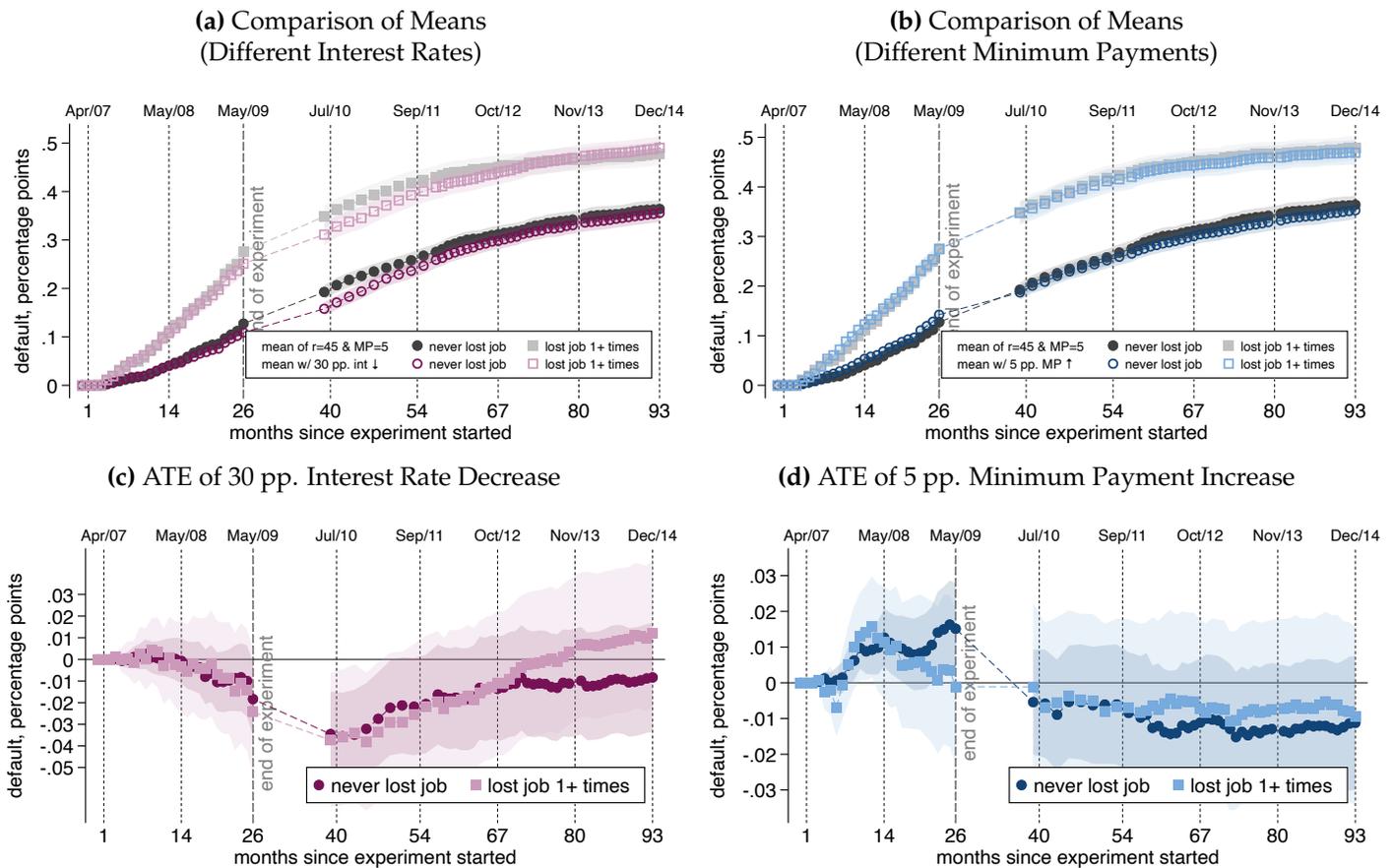
*Notes:* These figures plot the causal effect of interest rates and minimum payment changes on default in the experiment credit card. We separate borrowers using the ratio between amount due and credit limit, defining these two groups as “low” if in the lowest tercile or “high” if in the highest tercile. Figures on the left examine interest rate changes. 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 computing the average treatment effect in percent terms (i.e.,  $\gamma_t/\alpha_t$  in Equation 1) and dividing it by  $(45 - 15)/45$ . Similarly, Panel (f) computes the elasticity of default (i.e.,  $\beta_t/\alpha_t$  in Equation 1) divided by  $(10 - 5)/5$  with respect to a minimum payment increase from 5 to 10%.

**Figure OA-23: Treatment Effects of Contract Terms on Default by Pre-Experiment Formal Labor Attachment (Share of Cardholders that Default)**



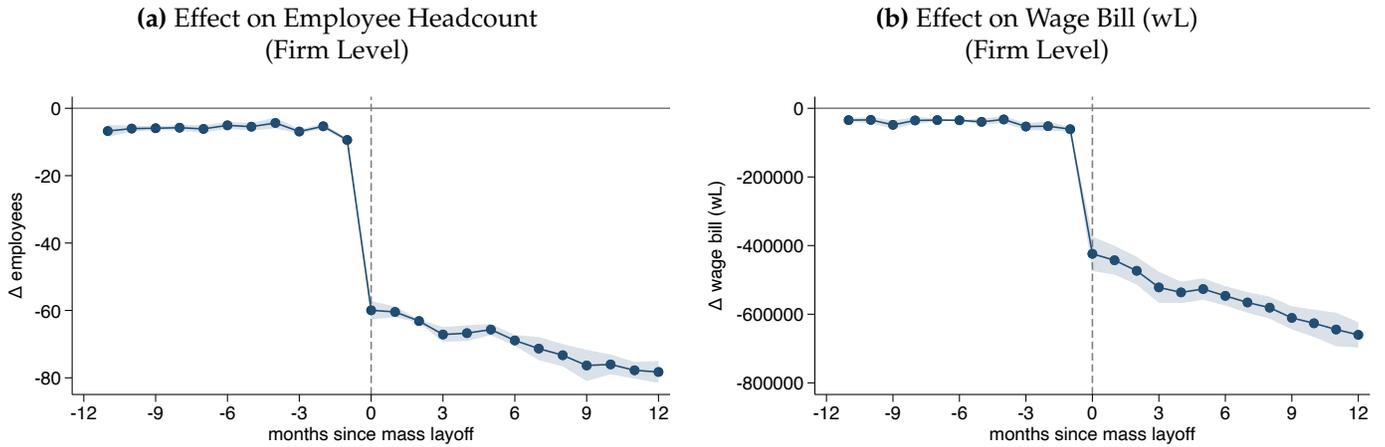
*Notes:* These figures plot the causal effect of interest rates and minimum payment changes on default in the experiment credit card. The figures restrict to borrowers that were formally employed for at least one month before the experiment (Jan/04-Feb/07), and separates borrowers based on whether they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

**Figure OA-24: 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 rates and minimum payment changes on default in the experiment credit card. The figures restrict to borrowers that were formally employed for at least one month during the experiment (March/07-May/09), and separates borrowers based on whether they were continuously employed (i.e., the 'never lost job' group) or not (lost job 1+ times). Figures on the left examine interest rate changes. Figures on the right examine minimum payment changes. The grey dots in Panels (a) and (b) plot the share of cardholders that default over time in the ( $r = 45\%$ ,  $MP = 5\%$ ) group. The red dotted line in Panel (a) plots the share of cardholders that default over time when interest rate is decreased by 30 pp. from 45% to 15%. The difference between the two lines in Panel (a) is plotted in Panel (c) and corresponds to the average treatment effect of a 30 pp. interest rate decrease from 45% to 15%. Similarly, Panels (b) plots the comparison of the share of cardholders that default when the minimum payment increases by 5 pp. relative to the ( $r = 45\%$ ,  $MP = 5\%$ ) group; and Panel (d) computes the average treatment effect of a 5 pp. minimum payment increase.

**Figure OA-25: Effect of Mass Layoffs on Employees and Wage Bill**  
 (IMSS Employment Data Matched to the Credit Bureau)



*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 \(2022\)](#) for these event studies.

## B Data

### B.1 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.2 Debt Decomposition

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

$$\text{amount due}_{i,t} = \text{amount due}_{i,t-1} + \text{purchases}_{i,t} - \text{payments}_{i,t} + \text{fees}_{i,t} + \text{debt}_{i,t} \times \text{interest rate}_i.^{40} \quad (3)$$

To test such an equation in our data we use observations with positive debt (as the coefficient on the interaction between debt and interest rate is not identified in the case when debt is zero). The following [Table OA-11](#) summarizes our results. We find that inferred interest rates match closely with experimental interest rates. This suggests that the debt transition equation (3) above is a good approximation to reality and that the data on purchases, debt, payments, and fees is consistent. The  $R^2=1$  means that the formula is virtually an identity in the data.

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<sup>40</sup>Note that if the amount owed was equal to the debt, the identity above could be written more compactly as  $C_t = RC_{t-1} + NP_t + \text{Fees}$  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.

**Table OA-11: Data check**

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

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

### B.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 68 months with a standard deviation of 54 months. Using the individuals from the experimental sample in (described in Section 2) and focusing in March 2007, we construct 50-quintiles for the tenure in months of the oldest credit. Doing so gives us values  $r_1, \dots, r_{49}$  where those cardholders whose loan tenure falls between  $[r_i, r_{i+1})$  are in the  $(i+1)$ -th quintile. We can define  $r_0$  and  $r_{50}$  as the min and max values for the tenure to have the first and last 50-quintile groups defined. By construction, we have the same amount of cardholders in each  $[r_i, r_{i+1})$  region.

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

## C 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  $(m_1, m_2) \in (0, 1)^{241}$ ; (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  $\theta$  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  $m_1RC_0$  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  $m_1RC_0$  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

$$C_1 = P + (1 - m_1)RC_0 \quad (4)$$

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.<sup>42</sup> Appendix B.2 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  $m_2RC_1$ , consume  $y_H - m_2RC_1$  (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-1 random variables  $(\epsilon_{20}, \epsilon_{21})$  capturing heterogeneity in preferences.

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

### C.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  $m_2RC_1$  and derive utility  $u(y_H - m_2RC_1) + v + \epsilon_{20}$  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.

<sup>41</sup>We find it useful to distinguish between minimum payments in the two periods but not so for the gross interest rate.

<sup>42</sup>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  $v_{20} = u(y_H - m_2 R C_1) + v$  and  $v_{21} = u(y_H)$ . Next, assuming that  $(\epsilon_{20}, \epsilon_{21}) \perp\!\!\!\perp y_2$

$$\begin{aligned} \mathbb{P}(D_2 = 1|C_1) &= q + (1 - q)\mathbb{P}(v_{20} + \epsilon_{20} < v_{21} + \epsilon_{21}) \\ &= q + (1 - q)L(v_{21} - v_{20}) \end{aligned} \tag{5}$$

where

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

and

$$v_{21} - v_{20} = u(y_H) - u(y_H - m_2 R C_1) - v$$

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  $\theta$  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

### C.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

$$\frac{\partial P_2}{\partial R} = (1 - q)L(v_{21} - v_{20})(1 - L(v_{21} - v_{20}))u'(y_H - m_2 R C_1)m_2 C_1 > 0 \tag{6}$$

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.

### C.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

$$\frac{\partial P_2}{\partial m_2} = (1 - q)L(v_{21} - v_{20}) (1 - L(v_{21} - v_{20})) u'(y_H - m_2 RC_1) RC_1 > 0 \quad (7)$$

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

### C.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

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

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  $\epsilon_2$ . The GEV assumption enables an analytic form of the expected maximum above so that

$$Q(C_1, \theta) = u(y_1 + C_1 - RC_0) + \delta [u(y_L)q + (1 - q) \ln(\exp(v_{20}) + \exp(v_{21}))] \quad (8)$$

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  $\nabla_x$  notation to denote the derivative of  $Q$  with respect to its argument  $x$ . The first order conditions simplify to

$$u'(y_1 + C_1^* - RC_0) = m_2 R \delta (1 - q) L(v_{20}(C_1^*) - v_{21}) u'(y_H - m_2 RC_1^*) \quad (9)$$

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

$$\frac{1}{(y_1 + C_1 - RC_0)} = \frac{\delta(1 - q)m_2 R \exp(v)}{(y_H - m_2 RC_1) \exp(v) + y_H} \quad (10)$$

and we can solve explicitly for first period debt

$$C_1^* \equiv \frac{(1 + \exp(v)) y_H}{\exp(v) m_2 R (1 + \delta(1 - q))} - \frac{\delta(1 - q)}{(1 + \delta(1 - q))} (y_1 - RC_0) \quad (11)$$

$$= (1 - \alpha) \frac{y_H}{L(v) m_2 R} + \alpha (RC_0 - y_1) \quad (12)$$

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,

$$\frac{\partial C_1^*}{\partial R} = \alpha C_0 + (1 - \alpha) \left( \frac{-y_H}{L(v) m_2 R^2} \right) \quad (13)$$

which is a convex combination of the two effects—e.g., a infinitesimal increase in  $R$  increases debt mechanically by  $\alpha C_0$  and reduces purchases (and thereby debt) by the last term in the expression above. Thus, the partial derivative in eq. (13) 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 G.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:

$$\frac{\partial C_1^*}{\partial m_2} = -(1 - \alpha) \frac{y_H}{L(v) m_2^2 R} < 0 \quad (14)$$

The expression for  $C_1^*$  in eq. (11) 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,

$$P^* = C_1^* - (1 - m_1)RC_0 \quad (15)$$

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:

$$\frac{\partial P^*}{\partial m} = -(1 - \alpha) \frac{y_H}{L(v) m^2 R} + RC_0. \quad (16)$$

So that increases in the required minimum payment can increase purchases (when initial debt is above a threshold level). In [Figure OA-30\(d\)](#) 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. (16) 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.

### C.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: Assume 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 in 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 in 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 notion 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

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

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

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

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. (14))

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

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).

### C.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.

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

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

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

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 C.1.3](#) outlines conditions under which this derivative is positive and [Appendix G.1](#) demonstrates that empirically debt is increasing in the interest-rate (during the experiment).

## C.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  $v_{11} + \epsilon_{11} > v_{10}(C_1^*) + \epsilon_{10}$  where

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

where  $Q_1(C^*; \theta)$  is defined in [eq. \(8\)](#) 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

$$P_{D_1}(\theta) \equiv \mathbb{P}(D_1 = 1|\theta) = L(v_{11} - Q(C_1^*)) \quad (17)$$

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

### C.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

$$\frac{\partial P_{D_1}}{\partial R} = -L_1(1 - L_1) \left( \nabla_R Q(C_1^*, \theta) + \nabla_{C_1} Q(C_1^*) \frac{\partial C_1^*}{\partial R} \right) = -L_1(1 - L_1) \nabla_R Q(C_1^*, \theta) \quad (18)$$

Next,

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

Next, using the first-order conditions for the optimal debt choice eq. (9) and substituting into the above expression,

$$\nabla_R Q(C_1^*; \theta) = - (m_2 \delta (1 - q) L (v_{20}(C_1^*) - v_{21}) u'(y_H - m_2 R C_1^*)) (C_1^* + R C_0) \quad (19)$$

so that

$$\frac{\partial P_{D_1}}{\partial R} = L_1 (1 - L_1) ((m_2 \delta (1 - q) L (v_{20}(C_1^*) - v_{21}) u'(y_H - m_2 R C_1^*)) (C_1^* + R C_0)) > 0 \iff C_1^* + R C_0 > 0.$$

### C.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

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

Next,

$$\nabla_m Q(C_1^*; \theta) = -\delta (1 - q) L (v_{20} - v_{21}) u'(y_H - m R C_1^*) R C_1^* \quad (21)$$

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:

$$(1 - \alpha) y_H > L(v) m \alpha (y_1 - R C_0)$$

where the discount rate applied to second period income incorporates income uncertainty, i.e.,  $(1 - \alpha) = 1 / (1 + \delta(1 - q))$ . 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

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

### C.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.<sup>43</sup>

<sup>43</sup>With log utility, we can show that  $\frac{\partial P^*}{\partial m_1} = R C_0$  so that as long as agents have positive initial debt, purchases will increase with increases in  $m_1$  - see the discussion around Equation (16). Empirically, we observe increases in purchases in response to increases in minimum payments; see Figure OA-30(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 (17) 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  $P_1 = L(v_{11} - Q(P^*, \theta))$  where

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

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

$$\frac{\partial P_{D1}}{\partial m_1} = -L_1(1 - L_1) \nabla_{m_1} Q(P^*, m_1)$$

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

$$\nabla_{m_1} Q(P^*, m_1) = u'(y_1 + P^* - m_1 RC_0)(-RC_0) + \delta(1 - q)L(v_{20} - v_{21})u'(y_H - m_2 R(P^* + (1 - m_1)RC_0))(m_2 R^2 C_0) \quad (22)$$

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

$$\frac{\partial Q}{\partial P} = 0 \implies u'(y_1 + P - m_1 RC_0) = \delta(1 - q)L(v_{20} - v_{21})u'(y_H - m_2 R(P + (1 - m_1)RC_0))(m_2 R) \quad (23)$$

and substituting eq. (23) into eq. (22) 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.

#### C.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

$$\frac{\partial P_{D1}}{\partial q} = -L_1(1 - L_1) \{(\delta(u(y_L) - u(y_H)) - \nabla_q Q(C_1^*; \theta))\}.$$

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

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

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

$$\frac{\partial P_{D1}}{\partial q} = -L_1(1 - L_1) \{(\delta(u(y_L) - u(y_H)) - \nabla_q Q(C_1^*; \theta))\} > 0 \quad (24)$$

#### C.2.5 Liquidity Constraints and Heterogeneity

As argued on p. 27, 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

$$\frac{\partial P_{D_1}}{\partial C_0} = L_1(1 - L_1)Ru'(y_1 + C_1^* - RC_0) > 0 \quad (25)$$

which is consistent with the means presented in [Figure OA-20\(b\)](#).

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. \(25\)](#) with respect to the common minimum payment  $m$

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

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. \(14\)](#). Further,  $\partial Q(C_1^*, m)/\partial m < 0$ .<sup>44</sup> 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.

### C.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 C.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  $v$ .<sup>45</sup> First, assuming

<sup>44</sup>Note, that the envelope theorem implies that  $\partial Q(C_1^*(m), m)/\partial m = \partial Q(C_1^*, m)/\partial m$ .

<sup>45</sup>The monotone transformation and the examination of elasticity are carried out to simplify the analysis.

log-utility and using the results from [Appendix C.1.5](#) and substituting in [Equation \(10\)](#) 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

$$\frac{\partial g}{\partial v} = -L_1 \frac{\partial Q}{\partial v} + \frac{\partial C_1^*}{\partial v} \frac{y_1 - 2RC_0}{(y_1 + C_1^* - RC_0)(C_1^* + RC_0)} \quad (26)$$

where

$$\frac{\partial Q}{\partial v} = \delta(1-q) \frac{(y_2 - m_2RC_1^*) \exp(v)}{(y_H - m_2RC_1^*) \exp(v) + y_H}$$

which is strictly positive under our assumptions above. Therefore, the first term in [Equation \(26\)](#) is strictly negative. Next, some calculations yield

$$\frac{\partial C_1^*}{\partial v} = -\frac{(1-\alpha)y_H}{m_2R \exp(v)} < 0$$

which is negative and decreasing in  $v$ . Thus, a sufficient condition for the last term in [Equation \(26\)](#) 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. \(26\)](#) 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.

## D 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-12](#) 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 ( $p = .03$ ).<sup>46</sup> 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-13](#)) 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.<sup>47</sup> 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.

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<sup>46</sup>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.

<sup>47</sup>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-14](#) 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-12: Access to loans after experimentally-induced default**

	New loan from Feb/08 until $k$ months after						
	$k = 3$ (1)	$k = 9$ (2)	$k = 12$ (3)	$k = 18$ (4)	$k = 24$ (5)	$k = 48$ (6)	$k = 60$ (7)
default between Mar/07 and Jan/08	-0.448 (0.249) [0.072]	-0.668* (0.301) [0.027]	-0.651* (0.306) [0.033]	-0.512 (0.308) [0.096]	-0.425 (0.310) [0.171]	-0.520 (0.320) [0.105]	-0.466 (0.320) [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-13: 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)
<i>Panel A. Any loan</i>									
default between Mar/07 and Jan/08	-0.196*** (0.006)	-0.252*** (0.007)	-0.346*** (0.009)	-0.158*** (0.006)	-0.205*** (0.007)	-0.290*** (0.009)	-0.067*** (0.002)	-0.094*** (0.003)	-0.160*** (0.004)
constant (non-defaulters dep. var mean)	0.258*** (0.003)	0.333*** (0.003)	0.499*** (0.003)	0.214*** (0.003)	0.281*** (0.003)	0.435*** (0.003)	0.072*** (0.002)	0.101*** (0.002)	0.174*** (0.003)
<i>Panel B. Credit cards only</i>									
default between Mar/07 and Jan/08	-0.164*** (0.004)	-0.212*** (0.005)	-0.330*** (0.006)	-0.131*** (0.004)	-0.171*** (0.004)	-0.266*** (0.005)	-0.054*** (0.002)	-0.074*** (0.003)	-0.135*** (0.003)
constant (non-defaulters dep. var mean)	0.184*** (0.003)	0.238*** (0.003)	0.372*** (0.003)	0.147*** (0.002)	0.191*** (0.003)	0.303*** (0.003)	0.058*** (0.002)	0.080*** (0.002)	0.146*** (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-14: Access to 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*** (0.00)	-0.02*** (0.00)	-0.01*** (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  $I(\text{After 1st Del for } i)_{it}$ . We estimate by OLS the regression  $y_{it} = \alpha_i + \gamma_t + \beta I(\text{After 1st Del for } i)_{it} + \epsilon_{it}$ , where  $y_{it}$  is an indicator for borrower  $i$  getting a new loan (any kind of loan) in period  $t$  with any bank (column 1), non-Bank A (column 2), or Bank A (column 3). The table reports estimated  $\beta$ 's, as well as the mean of the dependent variable in the periods before default;  $\beta$  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.

## E 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.<sup>48</sup> Conversely, one can view debt (or more generally consumption) responses to changes in credit limits as evidence of credit constraints.<sup>49</sup> 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.<sup>50</sup> 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

$$\Delta Debt_{i,t} = \delta_t + \sum_{j=0}^T \beta_j \Delta Limit_{i,t-j} + \gamma' X_{i,t} + \epsilon_{i,t} \quad (27)$$

where  $\Delta$  is the first-difference operator and  $\beta_j$  represents the incremental increase in debt between month  $t - 1$  and  $t$  associated with a one peso change in credit limit in period  $t - j$ . The scalar parameter  $\theta \equiv \sum_{j=0}^T \beta_j$  then provides us with a summary measure of the long-run (T month) total effect of credit limit on debt; we report  $\hat{\theta} \equiv \sum_{j=0}^T \hat{\beta}_j$  for each regression.<sup>51</sup> 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.<sup>52</sup>

The results are presented in [Table OA-15](#). In all tables, we adopt the convention of three asterisks denoting

<sup>48</sup> Assuming no wealth effects of the increased limits.

<sup>49</sup> See e.g., [Deaton \(1991\)](#), [Carroll \(1992\)](#), [Gross and Souleles \(2002\)](#).

<sup>50</sup> 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.

<sup>51</sup> Standard errors were computed using the delta method.

<sup>52</sup> 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.<sup>53</sup> For Panel B, since we only have annual data, we modify equation (27) 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.<sup>54</sup>

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 (27) 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 (27) separately for each stratum and compare the magnitudes of the estimates of  $\theta$  across strata. The results are presented in Table OA-15 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).<sup>55</sup> This pattern is confirmed across the remaining seven strata: controlling for tenure with the bank, poorer repayment histories are correlated with higher estimates of  $\theta$  and

<sup>53</sup> Adding non-revolving loans would induce a mechanical effect as debt is equal to the limit for these.

<sup>54</sup> 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  $T = 9$ ).

<sup>55</sup> The IV estimates are substantially larger for the most constrained stratum—a 214 peso increase in debt—but unchanged for the least constrained stratum.

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

**Table OA-15: Evidence for Credit Constraints: Cumulative Effect of Credit Limit Changes on Debt**

	All (1)	6-11 months			12-23 months			24+ months		
		Minimum (2)	Two + (3)	Full (4)	Minimum (5)	Two + (6)	Full (7)	Minimum (8)	Two + (9)	Full (10)
<i>Panel A. Bank A's debt (dependent variable) and Card A's credit limit (independent variable)</i>										
Baseline estimate	0.32*** (0.04)	0.69*** (0.06)	0.41*** (0.04)	0.23*** (0.03)	0.56*** (0.05)	0.47*** (0.05)	0.13*** (0.02)	0.33*** (0.06)	0.13*** (0.03)	0.03** (0.01)
IV estimate	0.73*** (0.14)	2.14*** (0.32)	1.24*** (0.28)	0.47 (0.37)	1.60*** (0.28)	1.06** (0.39)	0.09 (0.09)	0.62** (0.19)	0.52 (0.27)	-0.08 (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 (2292)	184 (3631)	102 (2771)	59 (1756)	100 (2639)	55 (2092)	23 (1163)	95 (2863)	43 (2174)	23 (1272)
Mean changes in limit	-104 (1460)	-141 (1532)	-115 (1452)	-105 (1486)	-97 (1149)	-90 (1129)	-77 (1177)	-100 (1446)	-97 (1487)	-120 (1956)
Mean utilization	0.52 (2.96)	0.72 (.34)	0.59 (3.07)	0.39 (.33)	0.68 (3)	0.58 (3.56)	0.4 (4.81)	0.64 (.35)	0.53 (3.6)	0.3 (2.82)
Median utilization	0.5	0.81	0.58	0.33	0.78	0.58	0.3	0.71	0.51	0.2
<i>Panel B. Total debt across all cards (dependent variable) and total credit limit across all cards (independent variable)</i>										
Baseline estimate	0.29*** (0.01)	0.37*** (0.03)	0.40*** (0.02)	0.32*** (0.02)	0.42*** (0.03)	0.35*** (0.02)	0.19*** (0.02)	0.29*** (0.02)	0.24*** (0.02)	0.15*** (0.01)
IV estimate	0.45*** (0.05)	1.17*** (0.12)	0.76*** (0.07)	0.51*** (0.04)	0.84*** (0.09)	0.45*** (0.06)	0.37*** (0.04)	0.38*** (0.07)	0.34*** (0.06)	0.24*** (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 (4402)	1440 (7023)	889 (5220)	549 (3342)	808 (5045)	453 (3886)	258 (2140)	577 (5095)	360 (3769)	198 (2257)
Mean changes in limit	657 (2228)	485 (2058)	558 (2163)	722 (2438)	564 (1726)	584 (1807)	744 (2131)	730 (2246)	711 (2285)	770 (2820)
Mean utilization	0.45 (.38)	0.67 (.42)	0.5 (.38)	0.33 (.31)	0.62 (.39)	0.47 (.37)	0.28 (.28)	0.54 (.37)	0.42 (.35)	0.22 (.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 (27); 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 (27)) 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. (27) 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. (27) 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.

## 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

$$\begin{aligned}\lambda(MP_i, r_i, t) &= \exp[\delta_0 + \delta_1 \mathbb{1}\{MP_i = 10\%\} + \delta_2(45\% - r_i)/30\%] \alpha t^{(\alpha-1)} \\ &\equiv \exp(x'_i \delta) \alpha t^{(\alpha-1)}.\end{aligned}\tag{28}$$

The baseline hazard is thus a Weibull (with unknown parameter  $\alpha$ ) and the proportional hazard has the usual exponential form. The Weibull parameterization implies a strictly monotone hazard function which is a strong assumption (and 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:

$$\begin{aligned}F(t; MP_i, r_i; \theta) &= 1 - \exp\left[-\int_0^t \lambda(MP_i, r_i, s) ds\right] \\ &= 1 - \exp\left[-t^\alpha \exp(x'_i \delta)\right].\end{aligned}\tag{29}$$

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 eq. (1) for a given  $\theta$ . For instance, the analogue to  $\alpha_t$  from Equation (1), the proportion of borrowers who default by month  $t$  is given by

$$\tilde{\alpha}_t \equiv F(t; MP_i = 5\%, r_i = 45\%; \theta)\tag{30}$$

$$= 1 - \exp(t^\alpha \exp(x'_{i,B} \delta)).\tag{31}$$

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

$$\tilde{\beta}_t \equiv F(t; MP_i = 10\%, r_i = 45\%; \theta) - F(t; MP_i = 5\%, r_i = 45\%; \theta)\tag{32}$$

$$= \exp(t^\alpha \exp(x'_{i,B} \delta)) - \exp(t^\alpha \exp(x'_{i,MP} \delta))\tag{33}$$

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

$$\tilde{\gamma}_t \equiv F(t; MP_i = 5\%, r_i = 15\%; \theta) - F(t; MP_i = 5\%, r_i = 45\%; \theta)\tag{34}$$

$$= \exp(t^\alpha \exp(x'_{i,B} \delta)) - \exp(t^\alpha \exp(x'_{i,R} \delta))\tag{35}$$

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

We estimate the model using maximum likelihood (the likelihood function is well-behaved and implemented in Stata) using 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 estimated parameters ( $\alpha$  and the  $\delta$  vector in Equation (28)) while the OLS estimates  $3 \times 26$  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

$$\lambda_i(x, t) = \nu_i \exp(x_i' \delta) \alpha t^{(\alpha-1)} \quad (36)$$

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:

$$\begin{aligned} F(t; MP_i, r_i; \theta) &= 1 - \int_0^\infty \exp \left[ - \int_0^t \lambda_i(MP_i, r_i, s) ds \right] f_\nu(\nu_i; \rho) d\nu_i \\ &= 1 - \int_0^\infty \exp \left[ -\nu t^\alpha \exp(x_i' \delta) \right] f_\nu(\nu; \rho) d\nu \end{aligned} \quad (37)$$

and we use Equation (37) to recompute  $\tilde{\alpha}_t$ ,  $\tilde{\beta}_t$ , and  $\tilde{\gamma}_t$  using the  $F(\cdot)$  above.

## F.3 Duration Model Results

Table OA-16 shows our coefficient estimates for  $\delta$  in the first panel and for the logarithm of  $\alpha$  and  $\rho$  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.<sup>56</sup>

Figure OA-26 facilitates the comparison of the estimates in Table OA-16 to those estimated using Equation (1) by computing cumulative default at the end of the experiment using Equations (30), (32) and (34). 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.

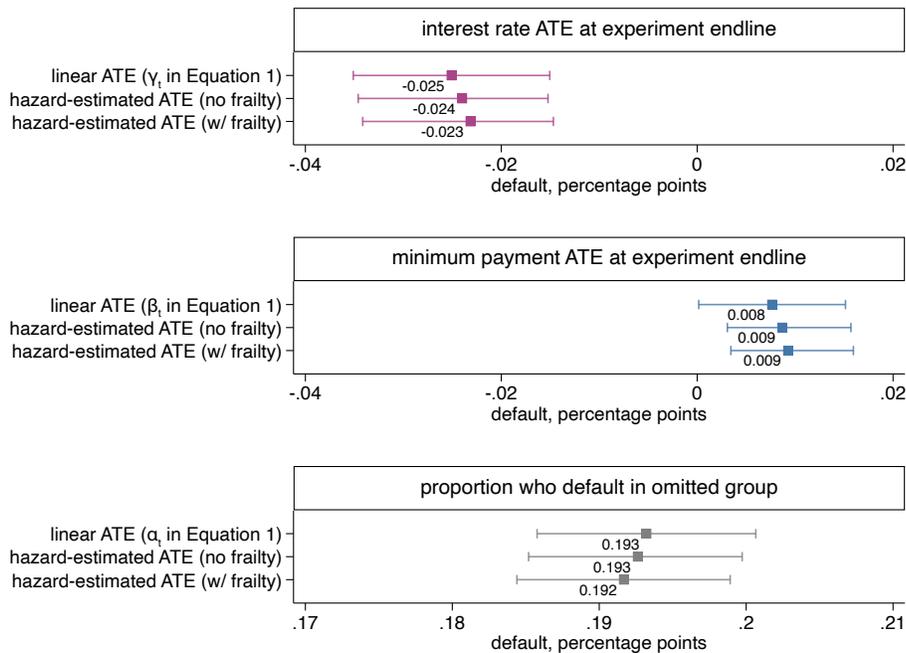
<sup>56</sup>As is often the case (see e.g., Wooldridge, 2010), the estimate of duration dependence  $\alpha$  is higher when we incorporate unobserved heterogeneity although the difference is not substantively consequential here since the duration model without frailty already exhibited positive duration dependence (i.e.,  $\hat{\alpha} > 1$  in the first-column).

**Table OA-16: Duration Model Coefficient Estimates**

	(1)	(2)
$(45\% - r_i)/30\%$	-0.147*** (0.031)	-0.160*** (0.034)
$\mathbb{1}\{MP_i = 10\%\}$	0.049* (0.023)	0.060* (0.027)
Constant	-7.560*** (0.062)	-7.782*** (0.101)
log of $\alpha$	0.614*** (0.010)	0.669*** (0.023)
log of $\rho$		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,  $\delta$  in Equations (28) and (36). The second panel shows the coefficient estimates for the shape parameter  $\alpha$  for the baseline hazard, (in particular,  $\ln(\alpha)$ ), and the parameter for the variance of the unobserved frailty,  $\rho$ . Column (1) shows the coefficient estimates assuming no frailty, and Column (2) shows the estimates when frailty is Gamma ( $\rho, \rho$ ) distributed. Robust standard errors are shown in parentheses.

**Figure OA-26: Comparison of Equation (1) and Hazard Models at Experiment Endline**



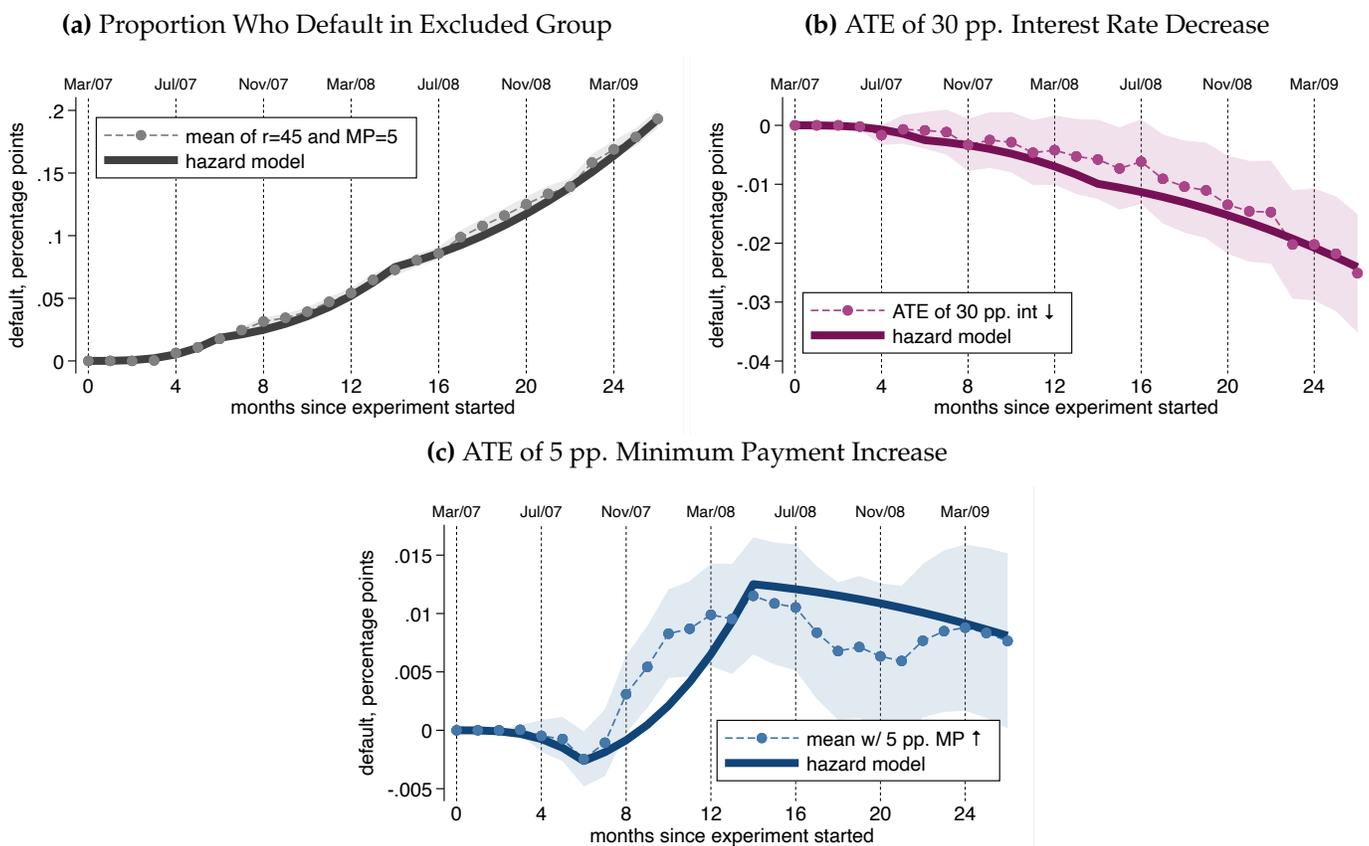
Notes: This figure plots the comparison of the average treatment effect estimates using month-by-month OLS estimation of Equation (1) using cumulative default as the dependent variable to the ones estimated using hazard models. Appendices F.1 and F.2 provide the estimation details for the hazard estimated 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.<sup>57</sup> Figure OA-27 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-27: Comparison of Equation (1) and Hazard with Time-Varying Coefficients**



*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 (28) 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.

<sup>57</sup>We also investigated whether modeling duration using competing risks (i.e., distinguishing between cancellations and non-default) changes our conclusions. We find that 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 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 cancellations) it seems plausible to impute a value of zero to outcomes in the periods after card exit. Such a strategy is useful when we are interested in the effects of the treatment on the outcome without distinguishing between the extensive and intensive margins.

We use [Equation \(1\)](#) as our estimating equation and plot the monthly means and treatment effects results graphically in [Figure OA-28](#). 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-17](#).<sup>58</sup>

For each estimand we present point estimates and account for attrition using bounds. We view attrition in two distinct ways and thus provide two sets of bounds. First, we consider all card exits regardless of reason (i.e., cancellations, revocations and the other category) as attrition. Second, we set all post-exit outcomes for card cancellers to zero and only consider the 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.

### G.1 Effect of Interest Rate Reductions on Debt

[Figure OA-28](#) 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.<sup>59</sup>

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

<sup>58</sup>Since we do not observe debt after the experiment ends, we cannot plot treatment effects on debt after May 2009.

<sup>59</sup>Other papers examining debt responses to interest rate variation are [Karlán 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 [Karlán 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. (11) and its derivative with respect to  $R$  is given by eq. (13) which we reproduce here for ease of reference:

$$\frac{\partial C_1^*}{\partial R} = \alpha C_0 + (1 - \alpha) \left( \frac{-y_H}{L(v)m_2 R^2} \right)$$

As the discussion on p.OA - 28 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-30(c) and OA-30(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.<sup>60</sup> Figure OA-31(c) shows a similar pattern for net purchases (i.e., purchases minus payments) as well.<sup>61</sup> 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 (13) 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.2) 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

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

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-28(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-20(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

<sup>60</sup>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-19).

<sup>61</sup>Figure OA-29(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.

## G.2 Effect of Minimum Payment Increases on Debt

Debt response to the minimum payment increase follows an interesting pattern. [Figures OA-28\(b\)](#) and [OA-28\(d\)](#) 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.<sup>62</sup> 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-17](#). 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).<sup>63</sup>

Both purchases and payments increase in response to the increased minimum payments (see [Figures OA-29\(d\)](#) and [OA-30\(d\)](#) and [Tables OA-18](#) and [OA-19](#)). The increase in purchases is consistent with the theoretical framework and the logic of inter-temporal optimization, in particular see [Equation \(16\)](#) 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-31\(d\)](#) shows net purchases (or equivalently the negative net pay-

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<sup>62</sup>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.

<sup>63</sup>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-17](#)).

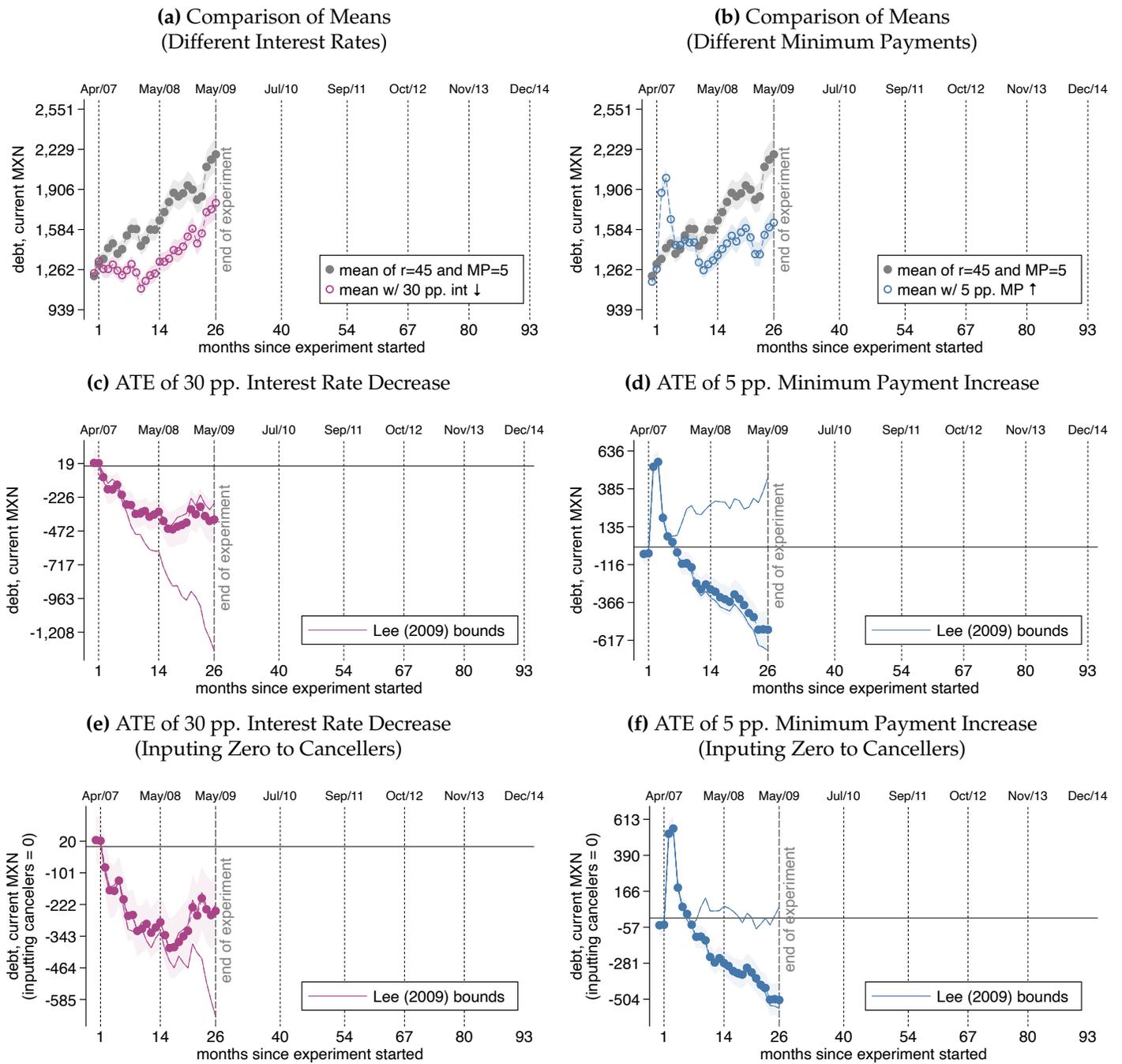
ments) falling steeply in months three and four before increasing and settling near about 30 pesos by about month 12 and hovering there for the remainder of the experiment. However, sample selection due to default is a serious concern after about month 9 and the Lee bounds are consistent with both substantive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

**Table OA-17: Treatment Effects on Monthly Debt**

Months since experiment started	Standard Outcome		Inputting cancelers = 0		6-11M w/ Card Strata		24+M w/ Card Strata	
	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*** (50)	-389*** (83)	-202*** (48)	-246*** (69)	-251* (111)	-548*** (142)	-206*** (62)	-360*** (103)
$\mathbb{1}\{MP_i = 10\%\}$	33 (37)	-547*** (62)	25 (36)	-509*** (52)	52 (82)	-813*** (106)	47 (46)	-508*** (78)
Constant	1,426*** (39)	2,187*** (67)	1,384*** (38)	1,807*** (56)	2,776*** (82)	3,442*** (113)	1,142*** (49)	1,989*** (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 $\varepsilon r$	[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 $\varepsilon MP$	[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-28: Treatment Effect of Contract Terms on Debt**  
(Debt in Current 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.

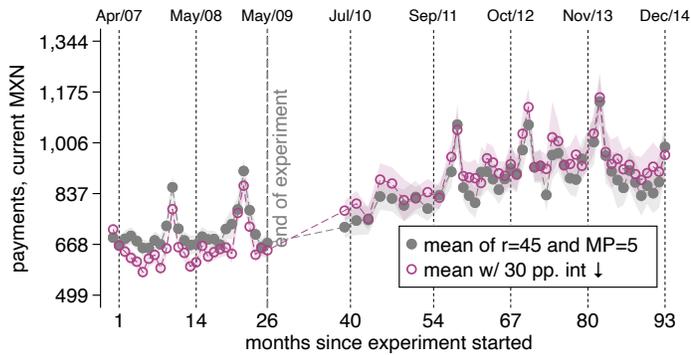
**Table OA-18: Treatment Effects on Monthly Payments**

Months since experiment started	Standard Outcome		Inputting cancelers = 0		6-11M w/ Card Strata		24+M w/ Card Strata	
	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* (18)	-24 (20)	-29 (17)	16 (17)	-44 (27)	-32 (31)	-31 (23)	-16 (25)
$\mathbb{1}\{MP_i = 10\%\}$	153*** (13)	133*** (15)	147*** (12)	90*** (13)	206*** (20)	149*** (22)	145*** (17)	128*** (19)
Constant	656*** (12)	673*** (15)	615*** (11)	545*** (12)	721*** (19)	638*** (21)	657*** (15)	691*** (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 $\varepsilon r$	[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 $\varepsilon MP$	[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]

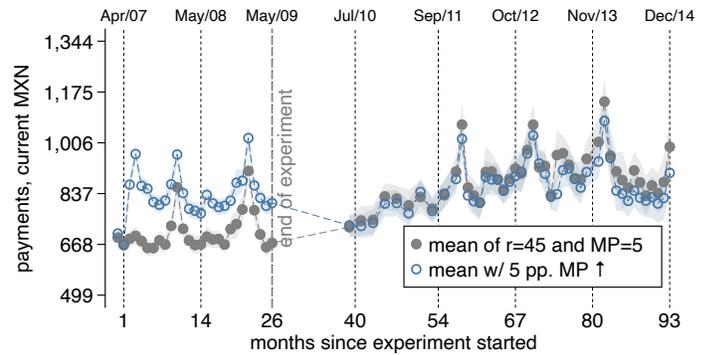
Notes: All regressions use sample weights. Each column is a different regression. The dependent variable is monthly payments. Columns (1), (3), (5), and (7) are estimated using outcomes 6 months after the start of the intervention and the remainder are for outcomes at the end of the experiment. Columns (3) and (4) input 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-29: Treatment Effect of Contract Terms on Payments**  
(Payments in Current MXN Among Active Cards)

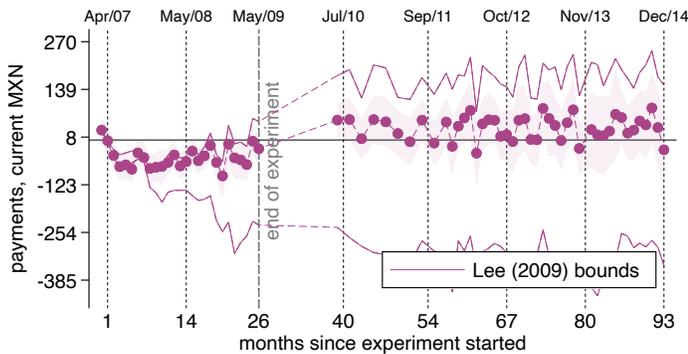
**(a) Comparison of Means**  
(Different Interest Rates)



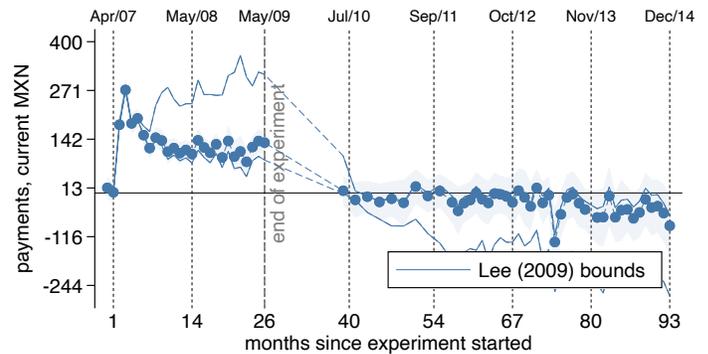
**(b) Comparison of Means**  
(Different Minimum Payments)



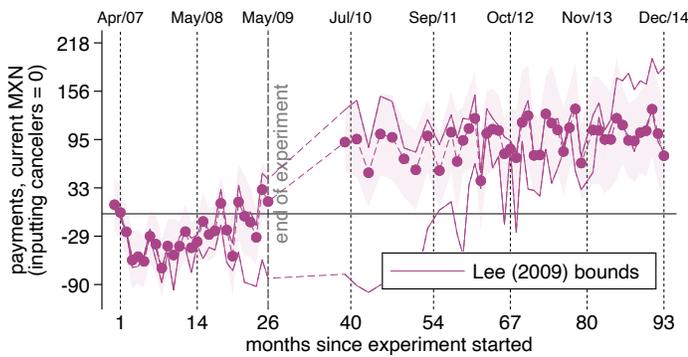
**(c) ATE of 30 pp. Interest Rate Decrease**



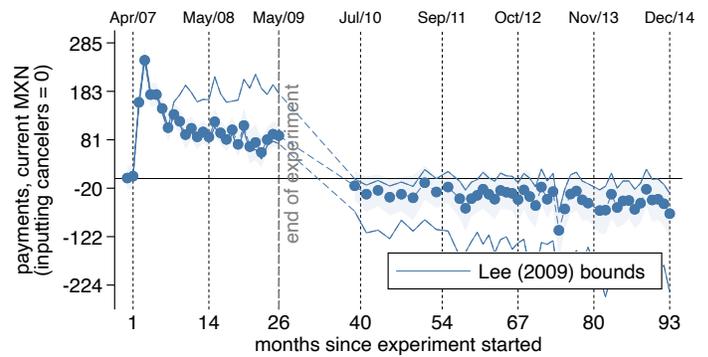
**(d) ATE of 5 pp. Minimum Payment Increase**



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



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



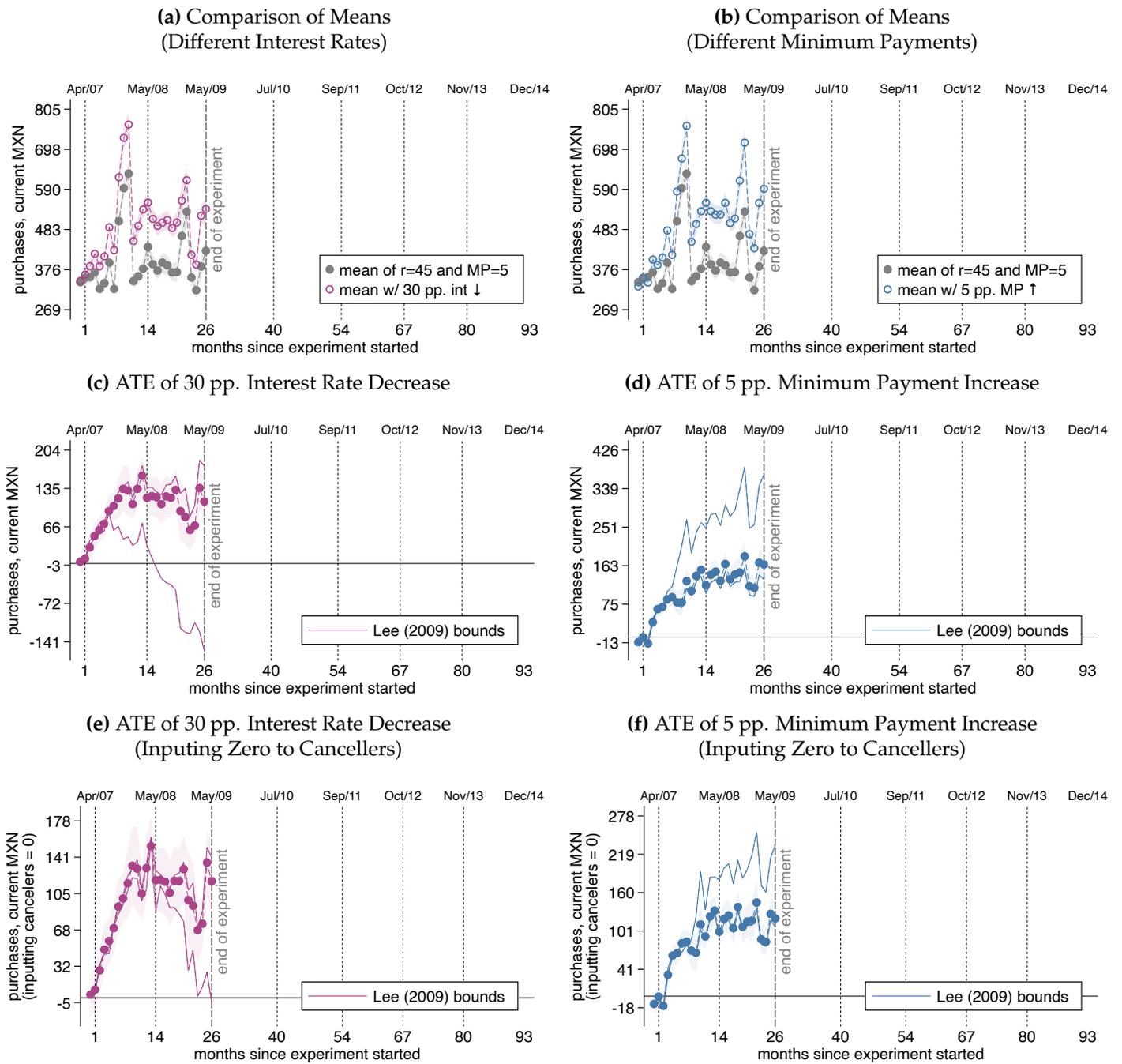
*Notes:* These figures plot the causal effect of interest rates and minimum payment changes on payments in the experiment credit card. Figures on the left examine interest rate changes. Figures on the right examine minimum payment changes. The grey dots in Panels (a) and (b) plot the average amount paid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

**Table OA-19: Treatment Effects on Monthly Purchases**

Months since experiment started	Standard Outcome		Inputting cancelers = 0		6-11M w/ Card Strata		24+M w/ Card Strata	
	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*** (14)	112*** (21)	92*** (14)	117*** (18)	75*** (20)	76* (31)	103*** (18)	119*** (27)
$\mathbb{1}\{MP_i = 10\%\}$	86*** (10)	165*** (16)	82*** (10)	120*** (13)	122*** (14)	163*** (22)	78*** (14)	165*** (20)
Constant	395*** (10)	427*** (14)	383*** (10)	350*** (12)	428*** (14)	414*** (24)	403*** (13)	442*** (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 $\varepsilon r$	[-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 $\varepsilon MP$	[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]

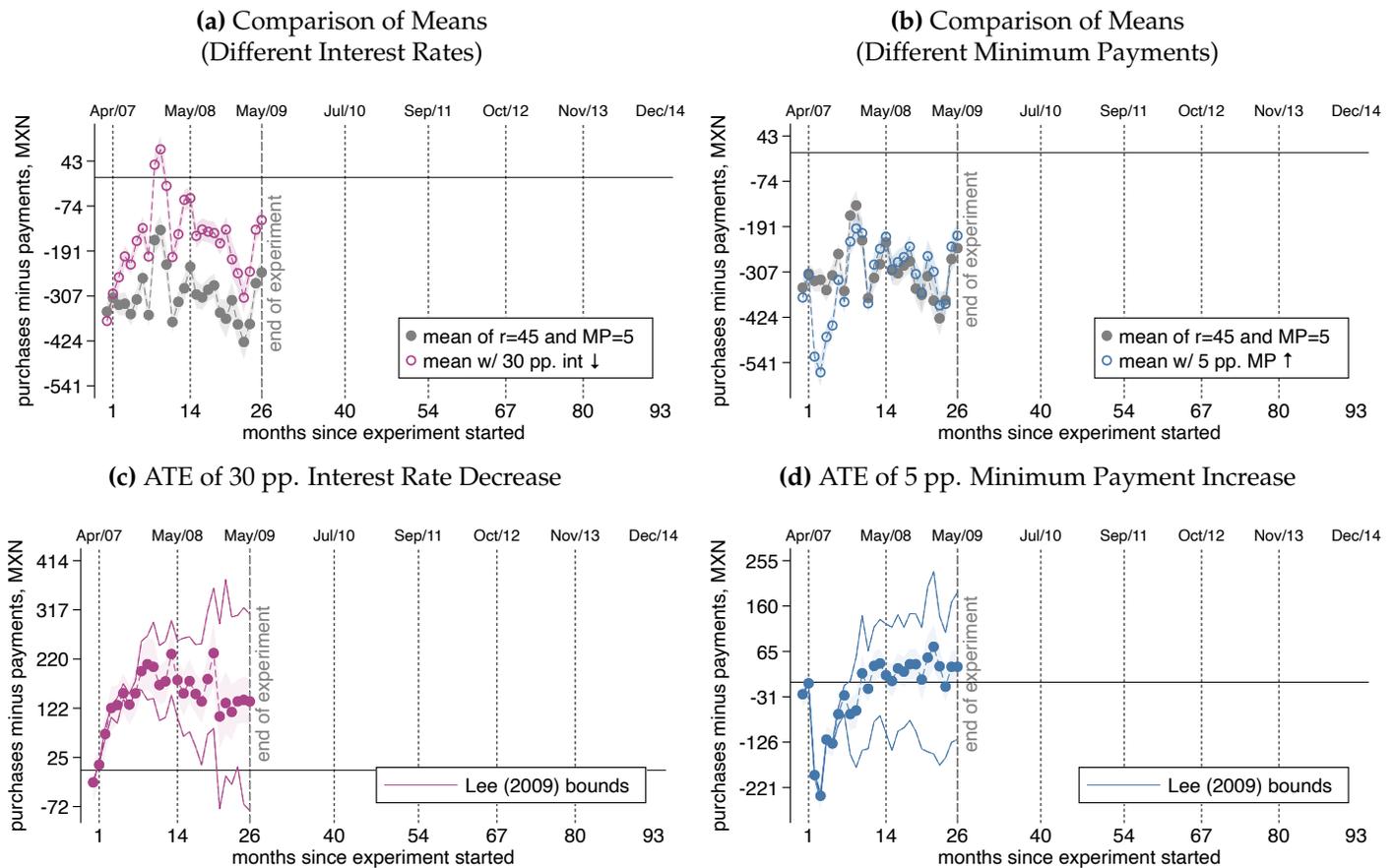
Notes: All regressions use sample weights. Each column is a different regression. The dependent variable is monthly purchases. Columns (1), (3), (5), and (7) are estimated using outcomes 6 months after the start of the intervention and the remainder are for outcomes at the end of the experiment. Columns (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-30: Treatment Effect of Contract Terms on Purchases**  
(Purchases in Current MXN Among Active Cards)



*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-31: Treatment Effect of Contract Terms on Net Purchases**  
(Purchases Minus Payments, constant MXN)



*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.

## H 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 Ph.D). 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 being 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 (which is 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 of the 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 .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 3.9pp).

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 same definitions used in [Section 7](#)).

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 of the 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 and that default would higher in the lower interest rate arm.

With respect to the minimum payment intervention, there was considerable disagreement among respondents. 2 respondents predicted that default would be higher at the end of the 18-month of the experiment in the 10% minimum payment arm while 2 predicted default to 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 .4 pp. (compared to an *increase* in default of .8 pp. in the experiment). Five years after the end of the experiment, 2 respondents predicted no difference in default between the previously higher and previously lower minimum payment arms (2

respondents predicted higher default). Overall, the average prediction was an increase in default of 2.4 pp. in the higher minimum payment arm 5 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%).

# I Comparing Default from Displacement and Interest Rate Changes

In the main text we document three primary results on credit card default. First, default rates are high particularly for new borrowers. Second, these high default rates are only modestly affected by even substantial changes in contract terms.<sup>64</sup> Third, job displacement results in substantial increases in default. Our preferred point estimate for the latter is a 6.1 pp. increase in default from displacement which is approximately thrice the ATE from the very large 30 pp. interest rate intervention.<sup>65</sup>

In this section we attempt a simple back-of-the-envelope calculation that compares the magnitudes of these two quite different shocks. This comparison is not intended to rationalize why the effects of displacement are much larger than those from interest rate changes—rather it is intended as an attempt to place them on the same footing (i.e. normalize them in some fashion). These calculations are only intended to be suggestive and we highlight the strong assumptions required in the discussion below.

We are not aware of other work that compares the effects of experimental changes in interest rates with the effects of job displacement on the same sample of borrowers. This common setting allows us to benchmark the two economic forces against each other (as requested by a reviewer) since they differ substantially in their force (i.e., job displacement is a stronger shock than a thirty-point increase in interest rates). We use two different approaches to benchmark the two shocks—we outline the first method in [Appendix I.1](#) and the second method in [Appendix I.2](#).

## I.1 Accounting Exercise to Compare Effect Sizes

We first attempt to put both shocks on an equal footing by comparing the implied change in **income** arising from each shock. We do so using an ad-hoc calculation of the net present value of a given income stream  $Y = \{Y_t\}_{t=1}^{18}$  at a given interest rate  $r$ :

$$\text{NPV}(Y, r) = \sum_{t=1}^{18} \frac{Y_t}{(1+r)^{t-1}}$$

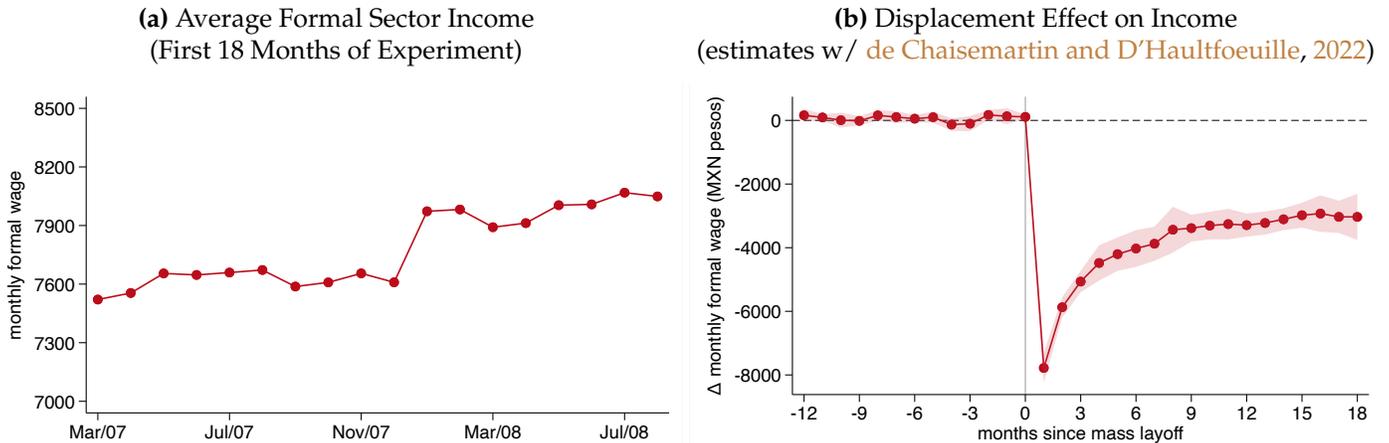
where  $Y$  is a specific income stream and  $r$  is the (monthly) interest rate used to discount income future income. [Figure OA-32\(a\)](#) plots the income stream for the average borrower in our experimental sample during the first 18 months of the experiment. [Figure OA-32\(b\)](#) plots the estimate of the effect of displacement on income (i.e. changes in income) using the methodology from [de Chaisemartin and D’Haultfoeuille \(2022\)](#). We use these estimates to produce the effect of a displacement shock on income as explained below.

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<sup>64</sup>Indeed, the contract term changes appear to be on the upper-end of what is feasible in a policy sense, suggesting a limited role for theories of interest-rate-driven moral hazard.

<sup>65</sup>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.

**Figure OA-32: Income and Income Shocks in the Experimental Sample**



Notes: Panel (a) plots the average income from Mar/07 to Aug/08 (the first 18 months of the experimental period) among borrowers in the experimental sample that work in the formal sector. Panel (b) plots the effect of mass downsizing events on income in the experimental card. An observation is an individual-month. We use the methodology developed by de Chaisemartin and D'Haultfoeuille (2022) for this event study. The displaced borrower definition is identical to the one we described in Section 7. The dependent variable is the formal sector wage for each borrower in the experimental sample.

Let  $Y_a$  denote the income stream plotted in Figure OA-32(a) and let  $Y_d$  denote the income stream obtained by incorporating the incremental income changes (post period 0) in Figure OA-32(b) to  $Y_a$ .<sup>66</sup> We view  $Y_d$  as a crude approximation to the post-displacement 18 month income stream. Using an interest rate of 15% we find  $NPV(Y_d, .15) = .49NPV(Y_a, .15)$  so that by this measure job displacement in month 0 reduces (the NPV of) the subsequent 18 month income stream (compared to the counter-factual income of no displacement being given by  $NPV(Y_a, .15)$ ) by 51%. Next,  $NPV(Y_a, .45) = .84NPV(Y_a, .15)$  which we interpret to mean that an increase in the discounting interest rate of 30 pp. reduces the (NPV) of the 18 month no-displacement income stream by 16%.<sup>67</sup> Taken together, these two results imply that the income loss from displacement generates an income loss that is 3.2 times larger than the income loss from a 30 pp. increase in interest the rate.<sup>68</sup>

To finish our comparison of both sets of shocks, we now look at the effect of both shocks **on default** over the same time horizon. As shown in Figure 4(c), at the 18-month horizon, 1.03 pp. more borrowers in the  $r = 45\%$  group default than those in the  $r = 15\%$  group. By contrast, Figure 8(a) shows that displaced borrowers are 6.1 pp. more likely to be in default over the same horizon. This comparison suggests that displacement shocks generate  $(6.10/1.03)$  5.9 times more default than interest rate changes. Under this calculation, the effect of job displacement on default is approximately twice  $(5.9/3.2 = 1.84)$  as large as the effect of the 30 pp. interest rate increase once we normalize by the size of the income shock.

## I.2 Using Debt as a Common Intermediate Outcome

In this section we attempt to examine the effects of each shock on a common intermediate outcome linked to default. We focus on the role of debt as the intermediate outcome. This clearly requires strong assumptions

<sup>66</sup> $Y_a$  for the first 18 months of the experiment (as shown in Figure OA-32(a)) is \$125,000. To create the displaced income sequence  $Y_d$ , we use the average income and sum the displacement effects shown in Figure OA-32(b) and obtain an estimate of \$61,000. This calculation assumes  $Y_a$  is a good proxy for the income among displaced borrowers in the absence of a displacement event. We also ignore any uncertainty in these calculations.

<sup>67</sup>The net present value of income when compounding at a 45% annual interest rate is \$105,000.

<sup>68</sup> $(NPV(Y_a, .15) - NPV(Y_d, .15)) / (NPV(Y_a, .15) - NPV(Y_a, .45)) = \frac{.51}{.16} \approx 3.2$ .

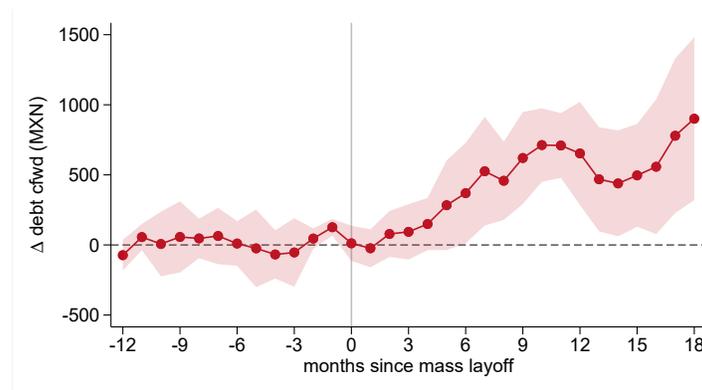
as the shocks are very different (and only one of them is purely experimental) but doing so allows us to make comparisons across the two kinds of shocks. To begin with, both shocks are expected to increase debt and we show this is indeed the case empirically—this is unsurprising for unemployment (given consumption smoothing motives) but is also true in our context for interest rate increases since, as [Appendix G.1](#) demonstrates, the “compounding” effect of interest rate increases dominate the behavioral response (i.e. purchase reductions in response to the interest rate increases). Both shocks also increase default.

We arrive at our comparison by computing the ratio of the effect of each shock on default and its corresponding effect on debt—loosely speaking, this would be comparing two Wald-IV-like estimates of debt on default. In one case we compute this ratio using unemployment as the excluded variable (in a regression of credit card default on debt) while in the second case we estimate the effect using a proportional hazard duration model.<sup>69</sup> We explain both and compare their magnitudes below.

### I.2.1 Unemployment → Debt → Default

In [Section 7](#) we estimate that job displacement increases default on average by 6.1 pp. in the subsequent 18 months. Using the same event-study methodology (see [Figure OA-33](#)) we estimate that job displacement increases debt by 901 pesos in the subsequent 18 months. If we make the strong assumption that debt is the only channel through which displacement affects default (in addition to the assumptions justifying the event-study), we can estimate the effect of job displacement induced debt on default as the ratio of the two reduced-form effects (i.e. as a Wald-IV estimate)—doing so yields that that a 1000 peso increase in debt arising from job displacement leads to a 6.8 pp. increase in default.<sup>70</sup>

**Figure OA-33: Job Displacement and Debt**  
(estimates using [de Chaisemartin and D’Haultfoeuille, 2022](#))



*Notes:* This figure plots the effect of mass downsizing events on debt in the experimental card. An observation is an individual-month. We use the methodology developed by [de Chaisemartin and D’Haultfoeuille \(2022\)](#) for this event study. The displaced borrower definition is identical to the one we carefully described in [Section 7](#). The dependent variable is the experimental card’s debt (in MXN pesos). To keep the panel balanced, we used the debt in the last month that the card was open.

The exclusion restriction justifying the Wald-IV estimate is clearly extremely strong. Unemployment likely affects default through channels other than debt, for example, displacement could decrease permanent income or worsen physical or mental health—all of which could increase default independent of the debt channel.<sup>71</sup>

<sup>69</sup>The duration model allows us to model the relationship between debt and default over time in an intuitively appealing way.

<sup>70</sup> $(1000/901) * 6.1 = 6.77$ .

<sup>71</sup>But this will serve to make our point: we will conclude that the effects of job loss are stronger than those of interest rate changes as

Conversely, unemployment may also increase the value of the card as a technology to smooth consumption. The objective of the back-of-the-envelope calculation above is primarily to provide a benchmark under a set of transparent assumptions.

## I.2.2 Interest rate → Debt → Default

An analogous strategy to the one above would examine the effect of interest rate induced changes in debt on default. However, there are two difficulties with a straight-forward application of the previous approach. First, unlike displacement which is a single event, the interest-rate intervention is in place over a 26 month period and the stock of debt evolves during this time in response to the sustained increase in interest rates.

One natural approach to address the time-varying path of debt would be to use a duration model approach that directly models the likelihood of default in period  $t$  as a function of time-varying covariates (conditional on not defaulting through period  $t - 1$ ) and incorporating unobserved heterogeneity. We can then use the estimates from the duration model to compute the probability of default over the relevant time-period (e.g., 18 months, which is the latest time period in our event study estimates) as a function of different debt profiles. A simple specification is a proportional hazard model of the form  $\lambda(t, x_{it}) = \alpha t^\alpha \exp(x'_{it}\beta)$  where  $x_{it} = (1, \mathbf{debt}_{it})$ ,  $\beta$  is a conformable vector of unknown coefficients and  $\alpha$  is a measure of duration dependence.

One immediate issue is that debt is not strictly exogenous (it is what is referred to as an internal covariate by [Kalbfleisch and Prentice, 2002](#)) and given the relatively limited work on duration models with endogeneity (see e.g., [Abbring and van den Berg, 2005](#), for a discussion) we do not include debt directly as a covariate but instead use a proxy that while closely related to debt is exogenous in the sense of e.g., [Wooldridge \(2010, Ch. 22\)](#). The proxy is “mechanical” debt, defined as  $\mathbf{md}_{it} \equiv (1 + r_i)^t \mathbf{debt}_{i0}$  where  $r_i$  is borrower  $i$ 's experimentally assigned interest rate and  $\mathbf{debt}_{i0}$  is their baseline debt (i.e. debt at the start of the experiment). In fact, the correlation between  $\mathbf{md}_{it}$  and borrower  $i$ 's true debt over the experimental period is 0.82.<sup>72</sup> With this choice of covariate, we can estimate  $\theta \equiv (\alpha, \beta)$  using standard maximum-likelihood methods and use the estimated parameters to estimate the effect of differing debt profiles on the likelihood of default over any time-period of interest.

There are several assumptions behind such a calculation. First, an exclusion restriction (as above): that interest rates affect default only through debt— this could be violated if, for instance, higher interest rates lead borrowers to dislike the card (e.g., lowers  $v$  in terms of the model). Second, mechanical debt is distinct from actual debt—though as argued above it seems to be a substantive component of it. Third, the parametric choice of hazard function imposes a specific functional form on default probabilities and their dependence on the debt profile. We show in [Appendix F](#) that the broader parametric specification is reasonable in the sense that it approximates the treatment effects estimated using linear specifications (either the fully saturated specification or those based on [Equation \(1\)](#)).

Using the estimated model we consider the effect on default from three different scenarios: (i) a “control” scenario, where the mechanical debt profile of individuals in the excluded group ( $r = 45\%$  and  $MP = 5\%$ ), (ii) a “flat effect” scenario, equalling the control scenario profile plus an immediate, flat and permanent increase of \$1,000 pesos over the 18-month horizon. (iii) an “equivalent interest” scenario, where starting with the same initial debt  $d_{i0}$  we ask what interest rate  $r$  would be needed to generate the same default rates at month 18 as

they operate through other channels other than debt – see [footnote 73](#) below.

<sup>72</sup>This correlation is perhaps not surprising given the evidence in [Appendix G.1](#) that debt responses to interest rate changes are substantially driven by the mechanical accrual of interest on previously accumulated debt.

that arising from displacement induced debt (i.e. the 6.1 pp. computed above). We will compare the control scenario vs the two other treatment scenarios.

For all three scenarios we use the hazard model to estimate the fraction of cardholders that would default by month 18 for each debt profile—the 18 month horizon is chosen to make the period for the two shocks (income shocks and interest rates) comparable. For the third scenario we find the interest rate such that the predicted default in month 18 equals 6.1 pp. We find that (a) relative to the control scenario debt profile scenario (ii) increases default by 0.91 pp. (b) interest rates would have to be set at 228% to generate the same default at 18 months as that arising from displacement induced debt.

### I.2.3 Conclusion from Debt as an Intermediate Outcome

As noted above, we calculate that we would need an 18 month increase in interest rates from 45% to 228% to generate the same increase in default as that arising displacement induced debt estimated in [Appendix I.2.1](#). Thus, interest rates would have to rise by a factor of 5 (from 45%) to match the displacement induced default via debt. Second, from another perspective, a \$1,000 peso increase in debt arising from job loss is associated with a 7.5 times larger increase in default compared to a \$1,000 pesos increase in debt arising from raising interest rates (6.8 pp. vs. 0.91 pp.).

One reasonable conclusion from these comparisons is that the larger estimated effects on default due to debt arising from displacement (relative to those driven by debt arising purely from interest rate changes) implies that the effect of displacement on default does not operate only through debt.<sup>73</sup> Thus our conclusion is that job-displacement shocks are much larger than changes in interest rates even after normalization because they affect borrowers in a myriad of ways (as noted in the text).

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<sup>73</sup>Indeed, in a constant treatment effect model with  $Y = \beta_0 + \beta_1 X + \beta_2 Z + u$  and  $X = \pi_0 + \pi_1 Z + \epsilon$  where  $Z$  is uncorrelated with both error terms, the Wald-IV estimate using  $Z$  as an instrument is consistent for  $\beta_1 + \frac{\beta_2 \sigma_z^2}{\pi_1} > \beta_1$  if  $\beta_2, \pi_1 > 0$ . If we view the results from the hazard model as providing us with a measure of  $\beta_1$  then a comparison to the Wald-IV estimand provides us with a measure of the non-debt channels through which job displacement affects default (i.e.  $\frac{\beta_2 \sigma_z^2}{\pi_1}$ ).