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Contract Terms, Employment Shocks, and Default in Credit Cards

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

Key words: Credit default; job loss; negative life events; minimum payment; interest rates; regulation; financial inclusion; moral hazard; credit cards.

JEL Codes: O16, G21, D14, D82.

1. INTRODUCTION

Policymakers in developing countries pursue two goals often perceived to be in tension with each other. Limiting credit market default is viewed as key to financial stability, while expanding formal credit to under-served populations is seen as critical for growth and welfare. The tension arises because new borrowers typically default at higher rates

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than established ones. This is particularly evident in policy discussions about credit-card borrowing—the most common way for new borrowers to access formal credit in many countries.[1] Concerns over new borrower card default have led several countries (e.g., Canada, Chile, Mexico, Taiwan, Turkey, and Indonesia) to mandate minimum payment floors or interest rate ceilings.[2]

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

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

We examine a popular Mexican credit card (hereafter the "study card") that was at the center of national financial inclusion efforts and regulatory concerns. Issued by a large commercial bank ("Bank A") targeted specifically at borrowers with limited or no formal credit histories, the study card accounted for approximately 15% of all first-time formal sector loan products in the country by 2010. Default rates were higher for the newest borrowers—borrowers who had been with the bank for 6–11 months (at the start of the experiment) defaulted at twice the rate of those who had been with the bank for at least two years (36% versus 18%). Such default rates among financially inexperienced populations attracted regulator attention because of their implications both for systemic risk and defaulters' subsequent access to formal credit (see, for instance, the discussions

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

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

in Banco de México, 2008, 2009a, 2010a).[3] This attention led, most prominently, to national legislation restricting credit card contract terms in 2010.

The experiment allocated a large nationwide stratified random sample of 144,000 *pre-existing* study card borrowers (hereafter "the study sample") to 8 treatment arms that varied annual interest rates between 15%, 25%, 35%, and 45%, and monthly minimum payments between 5% and 10%, for 26 months, from March 2007 to May 2009. The experimental variation is substantially larger than typical policy interventions, providing a strong test of contract terms' potential for limiting default.[4] The large sample size enables precise estimation of treatment effects across a range of contract terms and population strata (in our empirical results we use three asterisks to denote significance at the 0.001 level). The sampling scheme ensures the experimental results are representative of the bank's population of study card customers (about 1.3 million). We follow participants for five years after the intervention ended and examine their behavior across all formal financial institutions. In addition, we match the experimental sample to its monthly employment histories in the Mexican Social Security database (the Instituto Mexicano del Seguro Social or IMSS).

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

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

Our third result is that doubling the minimum payment requirements does not reduce default during the experiment—the point estimate is a 0.8 pp increase in default, with an

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

4. Policy changes have been much smaller—the mandated increases in minimum payments in Mexico was 1.5%, while interest rate caps considered in Turkey and Indonesia involved changes of no more than 5–10 pp. See, e.g., Moroglu (2018) for Turkey and Rossiana and Bisara (2016) for Indonesia.

5. See, e.g., Bar-Gill (2003); Warren (2007); Office of the Controller of the Currency (2003); Financial Conduct Authority (2015), and this circular from the Mexican Central Bank (Banco de México, 2011). 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 (Heidhues and Kőszegi, 2010; Heidhues and Kőszegi, 2016; DellaVigna and Malmendier, 2004; Gabaix and Laibson, 2006). There is some evidence that time-inconsistent preferences play a role in credit card debt accumulation (Meier and Sprenger, 2010; Laibson et al., 2003; Shui and Ausubel, 2005) and that minimum payments serve as an anchoring device (Stewart, 2009).

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

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

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

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

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

What might explain the larger effect of job loss? One possibility is that unemployment shocks have a larger impact on cash flow relative to the debt-servicing requirements of a 30 pp increase in the interest rate or a doubling of the minimum payment. Another possibility is that unemployment has a larger effect on default on a per-peso basis. A back-of-the-envelope calculation suggests that all three shocks have similar per-peso effects—a shock-induced cash flow decrease of 1,000 MXN pesos (over 18 months) is associated with default increases of 0.36–0.51 pp for all three "shocks," and we cannot reject the null hypothesis that all three parameters are equal ($p = 0.78$). These results are consistent with cash flow impacts being an important determinant of default, regardless of source. The smaller effects of contract terms can thus be rationalized by their smaller effects on cash flow, despite the changes themselves being quite substantial.

6. For instance, using Mexico's National Employment Survey (INEGI, 2015), we find that more than four-fifths of workers who lose formal employment are informally employed in the next quarter.

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

Related Literature

We connect with several strands in the literature on credit markets. First, a vibrant financial inclusion literature diagnoses low financial services penetration and advocates supply-side interventions to increase financial inclusion (Demirgüç-Kunt and Klapper, 2012; Dabla-Norris et al., 2021; Dupas et al., 2018). We examine Mexico's most popular financial inclusion loan product and provide evidence on the effectiveness of contract terms in limiting default. Second, we contribute to research on credit cards in developing countries—a relatively under-studied topic despite cards' increasingly important role as the source of entry into the formal credit sector for new borrowers (Ponce et al., 2017; De Giorgi et al., 2023). Third, we contribute to the literature evaluating the effect of changes in minimum payment terms—Keys and Wang (2019) study anchoring on minimum payments using an event-study design, while d'Astous and Shore (2017) use a difference-in-differences approach on a non-experimental change in minimum payments (both in the United States).[7] Fourth, a substantial literature has focused on the importance of contract terms and interest-rate-driven moral hazard (e.g., Karlan and Zinman, 2009; Banerjee and Duflo, 2010), though not focusing specifically on new borrower populations.

Fifth, we add to the fledgling literature analyzing the credit market consequences of job loss. Keys (2018) analyzes the effect of job loss and bankruptcy filing in the U.S. using a selection on observables assumption. Gerardi et al. (2018) use an instrumental variable approach to estimate the effect of income and housing equity on mortgage default using the Panel Study of Income Dynamics. Our contribution is to use individual-level administrative employment data matched with our experimental sample to estimate an event study design using mass layoffs as a source of exogenous job separation. More generally, we complement research studying the connections between labor and credit markets and social insurance (which is primarily U.S. focused). For instance, Herkenhoff (2019) studies the effect of credit markets on the labor market in the U.S., while we study the reverse causal relationship. Hsu et al. (2018) and Bornstein

7. There is an active literature examining credit cards in the U.S. (e.g., Agarwal et al., 2010; Ausubel, 1999; Agarwal et al., 2015, 2017). 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.

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

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

2. DATA AND SUMMARY STATISTICS

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

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

2.1. *Study Card and Bank Data (Experimental Sample)*

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

8. 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:**

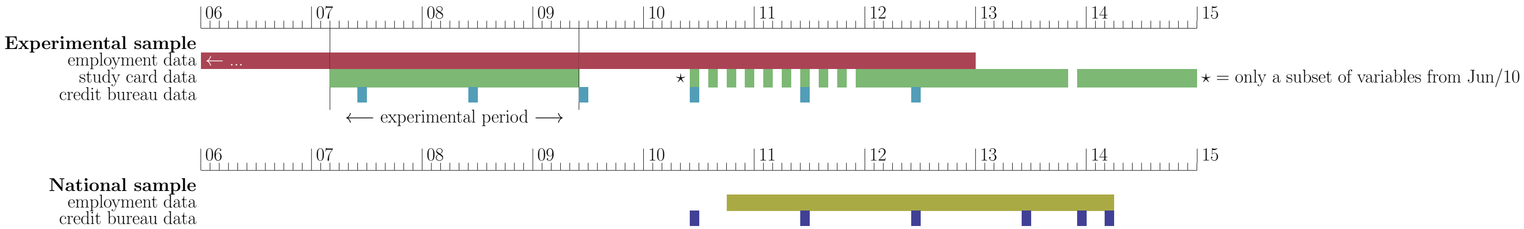
   Loan-level data matched to the experimental sample for Jun/07 to Jun/12, annually.

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

3. **Social security employment data:**

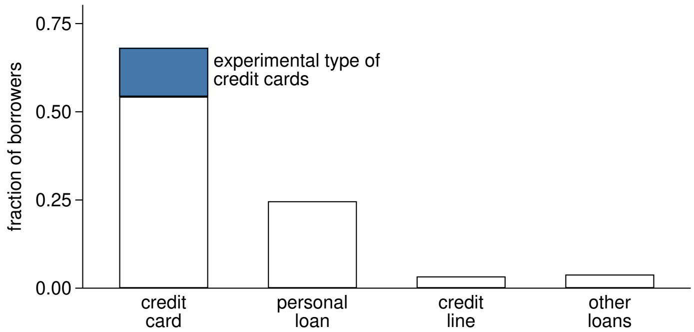
   Individual-level data matched to the experimental sample, monthly information from Jan/04 to Dec/12.

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



Notes: This figure presents a timeline for the experiment. The data used to define the 9 experimental strata was recorded in January 2007. Data from the experiment is provided monthly for each card from March 2007 to May 2009, bimonthly from June 2010 to December 2011, and monthly from January 2012 to December 2014 (with the exception of November 2013). Starting in June 2010, we only observe a limited set of variables that includes default and payments. We use CB information for the experimental sample, which is provided to us in 6 snapshots, every June from 2007 to 2012. National sample: we also have data unrelated to the experiment. This comprises random sample of 1 million borrowers from credit bureau data, and the universe of Mexico's social security data.

FIGURE 2
First Time Loans, by Type



Notes: This figure is constructed using a representative sample of one million borrowers in the credit bureau (i.e., those with formal sector loans) in 2010. For each individual, we identify the oldest loan and record its type (e.g., credit cards, personal loans, credit lines, auto loans, real estate loans). We then plot the fraction of first loans by type. The blue area represents the Study Card (described in Section 2). The Study card represents 15% of all first time loans in Mexico.

Borrowers new to formal credit market. The card was specifically targeted at low-income borrowers with no or limited credit histories (internally, the bank referred to them as the "C, C- and D" customer segments). Consistent with this, the study card was the first formal loan product for 47% of our study sample, and was the first credit card for 57%. Customers for the study card approached bank kiosks in supermarkets nationwide and completed a brief paper application. The card had an initial credit limit of approximately 7,000 pesos, an annual interest rate of 55 basis points over the inter-bank interest rate (the TIIE from Banco de México, 2015), and a monthly minimum payment of 4% of the total amount outstanding. The card was initially offered in 2003, and by 2009 it had approximately 1.3 million users——a substantial financial inclusion effort in a country with approximately 11 million cards at the time (Banco de México, 2010b).

Sample. The sampling frame consisted of all study card holders who had paid at least the minimum amount due in each of the last six months through January 2007, and our results are representative of this population. Using data from the credit bureau, we find that the minimum payment eligibility restriction removed 6.7% of study card holders from the sampling frame. When we construct weights to attempt to make the experimental sample representative of the population without the eligibility criterion, we obtain treatment effect elasticities that are virtually the same as those reported here (results available upon request). The frame was partitioned into nine strata based on tenure with the bank and payment behavior (each taking on three values), both of which the bank uses internally as predictors of default. The bank then selected a random sample of 18,000 clients per stratum. We use stratum weights (see Supplementary Appendix Table OA-1) in all of our analysis to ensure our results are representative of the sampling frame. Table 1 compares our sample with the national population of new borrowers in Mexico.

Variables. We have monthly data on purchases, debt, credit limits, and cancelations from March 2007 to May 2009. We observe default and payments from March 2007 to December 2014 but at different frequencies and with one gap (from June 2009 to June 2010): monthly for the duration of the experiment (March 2007 to May 2009), every two months from June 2010 through December 2011, and then monthly again through December 2014. We observe a limited set of demographic variables—age, gender, marital status and residential zip code.

Throughout the paper, we focus on default because it is the focus of a significant literature on credit markets, a key outcome of interest for lenders and regulators, and we observe it over a long horizon.[9] Since it is a key outcome, we describe it in some detail here. In keeping with the legal definition, default is defined as three consecutive monthly payments that are each less than the minimum payment due. In such instances, it is Bank A's policy to revoke the study card automatically (there is no appeal procedure). Our default measure at time t is cumulative: i.e., $Y_{it}=1$ if i defaulted in any month $s \leq t$ and 0 otherwise. This allows us to perform the default analysis on an unchanging sample. By contrast, defining $Y_{it}=1$ if i defaults in t conditional on not defaulting in $t-1$ and dropping i from estimation for $t' > t$ implies that the sample changes from month to month, with attrition potentially driven by the treatment, making the estimands difficult to interpret.

2.2. *Credit Bureau Data (Banco de México, 2014)*

2.2.1. Matched to the Experimental Sample.

A borrower appears in the credit bureau if they have had a loan with a formal financial intermediary.[10] For each loan, we observe the date of initiation and closing, the source and type of loan, monthly delinquency, and default history. We observe the credit score, but we do not observe interest rates, debt, or contract terms, except for credit limits. We matched the study sample to the credit bureau (*Buró de Crédito*) data once each year from June 2007 to June 2012. This match enables us to observe all other formal sector loans and their default status for these borrowers, allowing us to measure effects on non-Bank A related outcomes. We refer to this data as the *matched* CB data.

2.2.2. Representative Cross-Sections.

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

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

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

2.3. *Social Security Employment Data (IMSS, 2012)*

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

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

2.4. *Additional Datasets*

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

2.4.2. 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 from INEGI, 2010) to measure credit card penetration in the country and the Mexican Family Life Survey (Rubalcava and Teruel, 2006, 2008, 2013) to measure loan terms for both formal and informal loans.

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

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TABLE 1
Summary Statistics and Baseline Characteristics

	Experimental sample	Experimental sample	Credit bureau sample		
			$\geq$1 Card Holders	New borrowers (matched)	Experienced borrowers
	(1)	(2)	(3)	(4)	(5)
Panel A. Information from the experimental sample dataset					
Month of measurement	March 2007	May 2009			
Payments	711	734	-	-	-
	(1,473)	(1,375)			
Purchases	338	550	-	-	-
	(1,023)	(1,438)			
Debt	1,198	1,799	-	-	-
	(3,521)	(4,804)			
Credit limit	7,879	11,823	-	-	-
	(6,117)	(10,101)			
Credit score	645	-	-	-	-
	(52)				
(%) Consumers for whom experiment is their first card	57	-	-	-	-
(%) Consumers who default between Mar/07 - May/09	19	-	-	-	-
Panel B. Information from the credit bureau dataset					
Month of measurement	June 2007	June 2010	June 2010	June 2010	June 2010
Mean card limit (all cards)	13,987	18,579	23,572	20,499	50,369
	(10,760)	(15,473)	(31,471)	(27,395)	(42,397)
Total credit line (all cards)	51,542	54,305	52,500	44,823	124,333
	(54,056)	(55,765)	(98,149)	(83,399)	(152,130)
Tenure of oldest credit	71	103	85	72	194
	(54)	(51)	(81)	(56)	(81)
Panel C. Demographic information					
Month of measurement	June 2007	June 2010	June 2010	June 2010	June 2010
(%) Male	53	-	50	49	56
(%) Married	65	-	52	52	51
Age (in years)	39	42	42	41	50
	(6)	(6)	(12)	(12)	(11)
Monthly income	10,065	11,951	-	-	-
	(8,345)	(10,143)			
Panel D. Comparable income estimates					
Month of measurement	October 2011	-	October 2011	October 2011	October 2011
Monthly Income‡	13,849	-	14,500	14,113	22,286
	(11,246)		(12,730)	(12,431)	(15,803)
Observations	162,000	97,248 (Panel A) 155,945 (Panel B & C)	415,793	379,310	110,904

Notes: This table presents means and standard deviations for selected variables from the experimental sample and three different credit bureau sub-samples. Panel A shows statistics for the experimental sample (what we called "Study Card and Bank Data (Experimental Sample)" in the data section). Panels B and C use different data sources. For columns 1 and 2 they use the "Study Card and Bank Data (Experimental Sample)". For columns 3,4,5 they use what we called "Credit Bureau Data (Representative Cross-Sections)". Panel C uses "Study Card and Bank Data (Experimental Sample)" in columns 1 and 2, and "Credit Bureau Data (Representative Cross-Sections)" for columns 3,4,5. Finally Panel D uses "IMSS Employment Data (Matched to Experimental Sample)" for columns 1 and 2, and "IMSS Employment Data (Matched to the CB)" for columns 3,4,5. Columns 1 and 2 are computed using strata weights. Column 3 presents summary statistics for the credit bureau sub-sample restricted to borrowers with at least one credit card in June 2010. Column 4 selects a sub-sample from the Column 3 sample that mimics the distribution of card tenure for the experimental sample (see Supplementary Appendix Section 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.

2.5. *Summary Statistics*

Table 1 presents summary statistics for the experimental sample in columns 1–2 and comparisons with samples representative of Mexican borrowers in columns 3–5. Column 3 is a nationally representative sample of borrowers with at least one credit card in 2010. Column 4 is a sample of borrowers in the CB data that matches the tenure of the experimental sample in the formal credit market (measured by the year of the first loan of the experimental sample; see Supplementary Appendix Section 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 13,849 MXN pesos compared to 14,500 MXN pesos for recent and 22,286 MXN pesos for experienced borrowers.[12] The proportion of study card borrowers we could match in the IMSS data (i.e., those that held a formal sector job for at least one month between January 2004 and December 2012) is 59%, roughly similar to the fraction of the formal labor force in the country. 41% of study card borrowers were employed in the formal sector in March 2007, when the experiment began.

3. CONTEXT

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

Rapid Card Expansion Among Low-Income Individuals. The number of credit card accounts in Mexico grew by 28% from 2006 to 2011 (Banco de México, 2010b), with a substantial part of the growth being concentrated among lower-income individuals (Supplementary Appendix Figure OA-1 using data from INEGI, 2010). As noted above, the study card played a vital role in this expansion. This pattern is typical throughout Latin America, as many borrowers use only credit cards in their formal loan portfolio (Supplementary Appendix Figure OA-2 using data from World Bank, 2017).

Distance Lending and Default Mitigation. Bank A's initiatives, and those of other large traditional commercial banks, to pursue low-income clients with limited credit histories appear to have been partly inspired by the success of Banco Compartamos and Banco Azteca.[13] However, Compartamos and Azteca pursue markedly different strategies than those pursued by Bank A. Compartamos is a micro-finance lender, primarily using joint liability via group lending, while Azteca requires collateral, typically household durables. Both lenders expend considerable resources on face-to-face interactions and home visits for loan collection.[14] In contrast, Bank A relies on traditional credit

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

13. See e.g., Consultative Group to Assist the Poor (CGAP) (2021).

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

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 Supplementary Appendix Figure OA-3). Whether these lower-cost distance-lending methods are sustainable with new-to-banking borrower populations remains an open question—the concern is that default may be substantially higher with such methods for these populations. In this context, it is important to understand the causes of default and the extent to which contract terms could mitigate default.

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

Default is High for New Borrowers and Declines with Tenure. During our 26-month study, approximately 19% of the control group defaulted on their card (computed using stratum weights), compared to an average cumulative 26-month default rate of 12% for a random sample of cards in the credit bureau during the same period. As a further point of comparison, default rates for the micro-lender Compartamos are less than 1% (Karlan and Zinman, 2019). Figure 3 shows that newer borrowers in the study card sample are indeed riskier: default rates are 36% during the experiment in the control group for the newest borrowers (those who had been with the bank for 6–11 months when the experiment began) and 18% for the oldest borrowers (those with tenure greater than two years).[15]

Figure 3 plots default rates for three different card groups: the study card (pink diamonds), all credit cards offered by Bank A (blue squares), and all cards in the credit bureau (green circles).[16] Default on the study card is twice as high as that on Bank A's other cards—consistent with the study card being a "financial inclusion" product targeted at those with lower incomes and limited credit histories. Default rates for Bank A's other cards are similar to those at other banks.

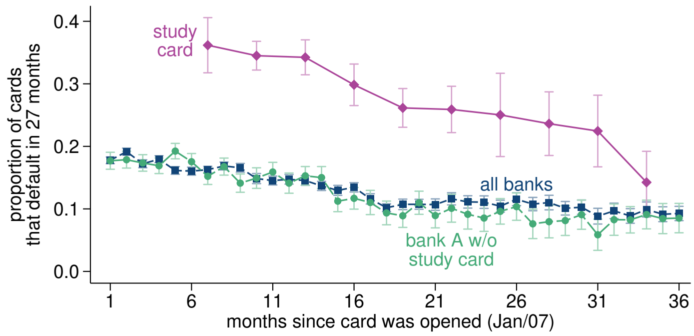
In principle, high default rates could reflect a low default cost or limited benefits from keeping the card current. We provide evidence against this below by (a) documenting that card default substantially reduces access to formal credit, (b) providing revealed preference estimates of the value of formal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

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

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

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 (pink diamonds). The figure plots the probability that a credit card defaults in the 26-month period from March 2007 to May 2009 (y-axis) against card tenure as of January 2007 (x-axis). The red diamonds show, for the control group of our study card, the proportion of cardholders that default by the months since the card was opened (binned into quarters). The control group averages are constructed using stratum weights. The blue squares and green circles use the same sampling design used to generate the experimental sample (but in the credit bureau data). The blue squares use all cards, whereas the green circles restrict attention to Bank A cards that are not the same type as the card we study.

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

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

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

Informal Terms are Worse Than Formal Terms. We use the Mexican Family Life Survey (MxFLS) to compare interest rates, loan amounts, and loan durations for formal and informal loans.[17] We find that informal loan terms are significantly worse than formal terms. Supplementary Appendix Table OA-2 shows results from regressing contract terms on an indicator for a formal loan and controls. First, the average annual informal loan interest rate is 291%, while the corresponding rate for formal loans is 86 points lower (col. 1). The average loan size is 3,658 MXN pesos for informal loans and 8,968 MXN pesos for formal loans (col. 4). The average term for informal loans is 0.52 years versus 1.03 years for formal loans (col. 9). Supplementary Appendix Figure OA-4 shows that the distribution of interest rates for informal loans first-order stochastically dominates that for formal loan rates, while the opposite is true for loan terms and amounts. These results are robust to controlling for income and wealth proxies (columns 2, 4, and 7), and the loan term and duration results hold even with household fixed effects.[18] While not dispositive, these results suggest that informal loan terms are onerous compared to formal terms, incentivizing borrowers to maintain access to formal credit.

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

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

4. EXPERIMENT OVERVIEW

The bank partitioned its sample frame of eligible study card clients into nine different strata based on the length of tenure with the bank and repayment history over the past year (both measured in January 2007).[19] Each borrower was classified into one of three

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

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

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

categories of tenure with the bank: (a) a long-term customer who had been with the bank for more than two years, (b) a medium-term customer who had been with the bank for more than one but less than two years, and (c) a new customer, who had been with the bank for more than six months but less than a year. Each borrower was also classified into one of three categories based on their repayment behavior over the past 12 months: (i) a "full payer," who had paid their bill in full in each of the previous 12 months and hence accrued no debt, (ii) a "partial payer," whose average monthly payment over the past 12 months was greater than 1.5 times the average of the minimum payments required from them during this time, and (iii) a "poor payer," whose average monthly payment over the past 12 months was less than 1.5 times the average of the minimum payments required from them during this time. These two variables were used to define nine strata, and 18,000 borrowers were randomly selected from each stratum. The resulting sample is geographically widespread—covering all 32 states, 1,360 municipalities, and 12,233 zip codes.

4.1. *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 Supplementary Appendix Figure OA-5). The minimum payment prior to the study was 4%. The interest rate (expressed as the annual percentage rate or APR) could take one of four values: 15%, 25%, 35%, or 45%. The interest rate for the study card prior to the study was approximately 55%, so all the experimental interest rates are reductions relative to the status quo (as in Karlan and Zinman, 2009). The new interest rate was applied to all new debt incurred going forward and to debt outstanding. Thus, the rate changes include both a forward-looking component as well as a current component (in contrast with e.g. Karlan and Zinman, 2009, who vary both components independently).

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

The two different minimum payments and four different interest rates yield eight unique contract terms (see Supplementary Appendix Table OA-3). We were informed that the minimum payment for the hold-out arm was 4%. However, the interest rate

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

varied across clients, and, unfortunately, we do not observe this rate.[21] Consequently, we do not use the hold-out group as a contrast. We use the 45% interest rate and the 5% minimum payment group (abbreviated to (45,5) when useful) as the comparison group and refer to it as the base arm or excluded group. Panel A of Supplementary Appendix Table OA-4 tests the randomization procedure and shows that treatment assignment is uncorrelated with baseline observables for the initial sample, as well as for the sample that did not attrit for the entire duration of the experiment.

Figure 1 shows the experiment's timeline and measurement dates. The bank mailed each study client a letter in March 2007 stating the new contract terms in force starting in April 2007. Clients were not informed that they were part of a study or of any timelines for when the new contract terms would change. The measurement of experimental outcomes began in March 2007 and lasted through May 2009. During this period, the interest rate and the minimum payment were fixed at their experimentally assigned levels. Internally, the experimental terms were not revealed to the risk department in charge of determining credit limits. We cannot reject the null of no differences in credit limits across treatment arms at baseline and end-line (Supplementary Appendix Table OA-5 and Figure OA-6).[22] The experiment ended in May 2009, when all participants received a letter stating their new contract terms. The new contract terms were the standard conditions with an APR of approximately 55% and a minimum payment of 4%.

5. FROM CONTRACT TERMS AND INCOME SHOCKS TO DEFAULT: A FRAMEWORK

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

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

If the agent does not default, they make net purchases P (i.e., purchases minus any payments in excess of the minimum payment) on the card and therefore their total debt at the end of the period is given by $C_1 = P + (1-m_1) R C_0$. Supplementary Appendix Section B.4 verifies the multi-period analog of this identity for our experimental data. In principle, P can be negative so borrowers can choose to repay more than the minimum amount. We will assume that borrowers (if they pay) pay strictly less than what they owe,

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

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

so that they carry positive debt into the second period (i.e., $C_1 > 0$). This is consistent with our setting where the vast majority of borrowers pay close to the minimum payment. Since interest is accumulated on previously accumulated debt C_0, interest rate increases will automatically increase one component of total debt. This feature will be useful in understanding the effect of interest rate changes on debt.

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

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

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

In the second period the borrower realizes exogenous income $y_2 \in \{y_L, y_H\}$. If the income realization is low—y_L which occurs with probability q—the borrower defaults and earns utility $u(y_L)$. If the realization is high, the borrower chooses whether to default (and consume income y_H) or make the minimum payment m_2RC_1 and continue using the card in the future. Thus, conditional on the high-income realization, the second period utility is given by:

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

where v is the additional utility derived from the continued access to credit, and which can be interpreted as a reduced form parameter capturing the future flow of card benefits, a warm glow from card ownership, or the option value of having a card in the future.[24] Consistent with our context, v is only experienced if the card is not in default (defaulted cards are closed by the bank). We assume that the high-income realization is high enough to cover the minimum amount due ($y_H > m_2RC_1$). The agent will not choose to pay more than the minimum in period 2 when income is high since there is no benefit to doing so in the model. This reduces the agent's period 2 decision to either default or make the minimum payment (and remain in good standing).

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

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

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

$q \equiv P(y_2 = y_L)$. To ease notation, we define θ as the entire vector of exogenous variables $\theta \equiv (C_0, R, m_1, m_2, \delta, v, y_1, y_L, y_H, q)$.

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

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

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

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

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

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

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

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

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

6. THE EFFECTS OF CONTRACT TERMS ON DEFAULT

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

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

$$Y_{it} = \alpha_t + \beta_t \cdot \mathbb{1}\{MP_i = 10\%\} + \gamma_t \cdot (45\% - r_i)/30\% + \varepsilon_{it} \tag{6.1}$$

estimated on the sample of 144,000 individuals in the eight treatment arms using stratum weights (as defined in Supplementary Appendix Table OA-1). Y_{it} is the dependent variable for borrower i in month t, $\mathbb{1}\{MP_i = 10\%\}$ indicates assignment to the 10% minimum payment arms, and r_i is the experimentally assigned interest rate. The main dependent variable in the paper is cumulative default, as explained in Section 2, but we also use this specification to study additional variables such as debt and purchases.

We interpret α_t as the mean value of Y_{it} in month t for the excluded group (i.e., the $r = 45\%$ and $MP = 5\%$ arm), β_t as the average treatment effect of increasing the minimum payment to 10%, and γ_t as the effect of decreasing interest rates to 15%. We estimate Equation 6.1 month-by-month with heteroscedasticity robust standard errors, which is equivalent to estimating a single equation, i.e., pooled OLS, that fully interacts the intervention variables with month dummies, along with month-specific intercepts with robust standard errors clustered at the borrower level. We estimate the equation with and without stratum-by-month fixed effects and find almost identical results for β_t and γ_t.

Given the large number of estimated monthly treatment effects $\{\beta_t, \gamma_t\}_t$ over seven years, we present the results succinctly in two ways. First, we present the estimates graphically in Figure 4, plotting monthly means and treatment effects from March 2007 through December 2014, along with their corresponding confidence intervals. The estimated means and treatment effects for the interest rate arms are in pink (left side), while those for the minimum payment arms are in blue (right side). Second, we present point estimates in tabular form at a set of (nine) time points in Supplementary Appendix Table OA-6.

Equation 6.1 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 Supplementary Appendix Table OA-6, which yield similar estimates. We also use the fully saturated model to test the separability and linearity assumptions and cannot statistically reject them.[25] For these reasons and because of their interpretability, we only discuss estimates from Equation 6.1. Additionally, we assess the sensitivity of our results to using cumulative default as the main dependent variable by estimating duration models in Supplementary Appendix Section F and find that they yield nearly identical treatment effects.

6.1. *Main Results*

We begin by plotting the evolution of default for the excluded group (i.e., the (45,5) arm) with grey dots in Figures 4(a) and 4(b) using the estimates of $\{\alpha_t\}_t$ from March 2007 until December 2014 from Equation (6.1). 19% of the base arm had defaulted by the end of the 26 month experiment, and this figure rose to 41% by the end of the 93-month study period.

Result 1. *Decreasing interest rates by 30 pp for 26 months causes a 2.5 pp decrease in default.*

Figure 4(c) plots the estimated treatment effects corresponding to a 30 pp decrease in the annual interest rate (i.e., plotting the estimates of $\{\gamma_t\}_t$ from eq. 6.1). Default declines gradually in response to the interest rate decrease. By the end of the experiment, default fell by 2.5 pp ($p < 0.001$). Policy-relevant interest rate changes (of, e.g., 10 pp) result in correspondingly smaller effects (0.84 pp).

We benchmark this result in two ways. First, our 26-month default elasticity of +0.20 is considerably lower than in Karlan and Zinman (2019) (1.8) and Adams et al. (2009) (2.2), though in the same range as Karlan and Zinman (2009) (0.27) and DeFusco et al. (2022) (0.01)—see Figure 4(e) for a graphical comparison accounting for intervention length. The variation in elasticities across studies could reflect variation in borrower tenure, and we explore this in Result 2 below.[26] Second, we compare the estimated effects with senior Mexican regulator predictions as well as 72 incentivized responses on the Social Science Prediction Platform (SSPP). Among regulators, the average predicted decline in default from a 30 pp decrease in interest rates was 8.6 pp (at the 18-month horizon), while the corresponding figure for SSPP respondents was 5 pp. Both estimates are considerably higher than the estimated ATE of 1.03 pp.[27]

All study borrowers were returned to the same contract terms after the end of the experiment in May 2009. Figure 4(c) displays the differences in default across study arms

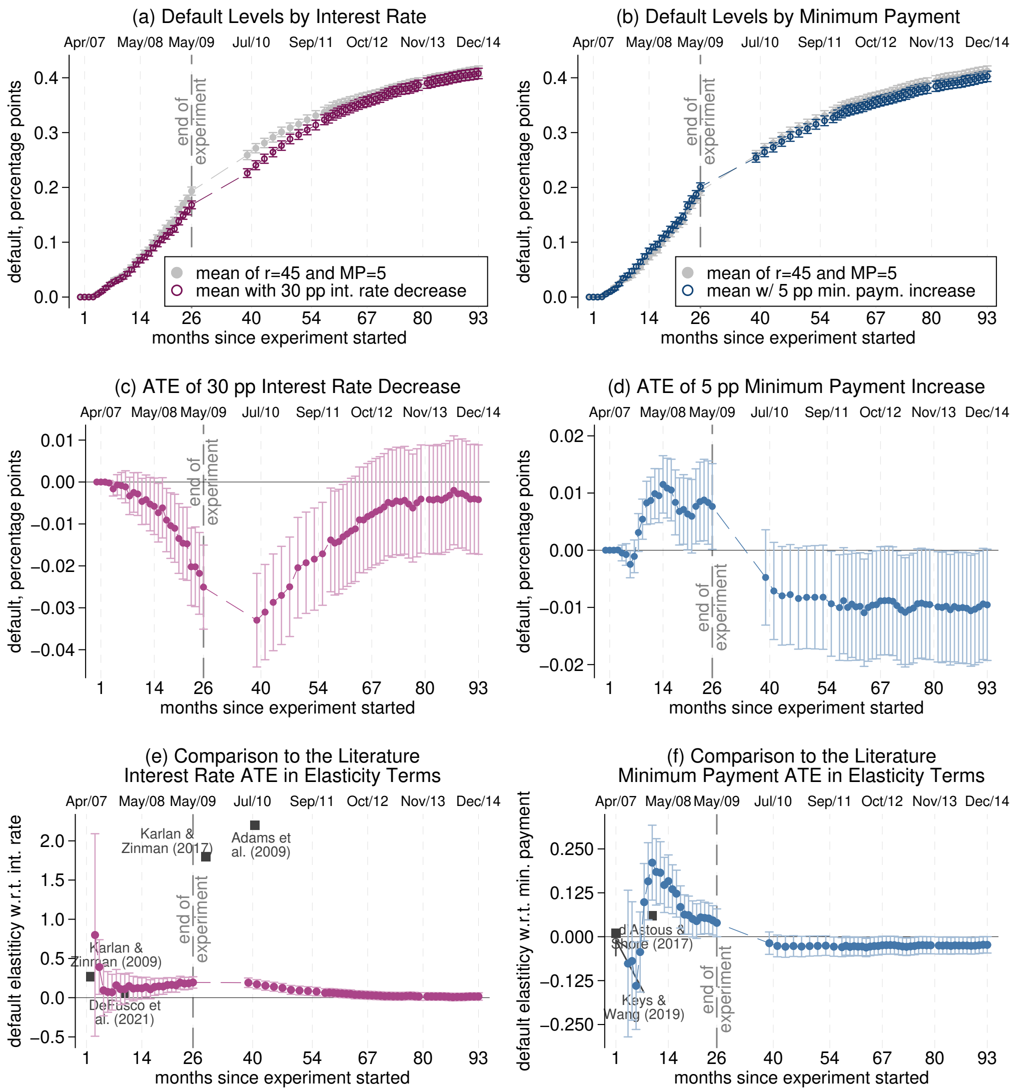
25. For example, we use the fully saturated model to test whether the minimum payment effect is different across interest rate treatment arms and cannot reject the null that they are equal. Similarly, we test whether the interest rate effect differs in the low and high minimum payment groups and cannot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

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

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

FIGURE 4

Treatment Effects of Contract Terms on Default



Notes: These figures plot the causal effect of interest rate and minimum payment changes on default in the Study card. Figures on the left examine interest rate changes, and figures on the right examine minimum payment changes. The grey dots in Panels (a) and (b) plot the share of cardholders that default over time in the ($r=45\%, MP=5\%$) group. The pink dotted line in Panel (a) plots the share of cardholders that default over time when the interest rate is decreased by 30 pp from 45% to 15%. The difference between the two lines in Panel (a) is plotted in Panel (c) and corresponds to the average treatment effect of a 30 pp interest rate decrease from 45% to 15%. Panel (e) computes the elasticity of default by computing the average treatment effect in percent terms (i.e., γ_t/α_t in Equation 6.1) and dividing it by $(45-15)/45$. The first estimate is particularly high because the elasticity involves a term in the denominator very close to zero. Panel (b) plots the comparison of the share of cardholders that default when the minimum payment increases by 5 pp relative to the ($r=45\%, MP=5\%$) group; Panel (d) computes the average treatment effect of a 5 pp minimum payment increase, and Panel (f) computes the elasticity of default (i.e., β_t/α_t in Equation 6.1, divided by $(10-5)/5$) with respect to a minimum payment increase from 5% to 10%.

through December 2014. Default continues to be lower in the 15% rate arm for about three years after the experiment ends. The effects attenuate over time, reaching -1 pp by March 2012, after which they become statistically indistinguishable from zero. The 26-month reduction in interest rates thus decreased post-experiment default for nearly three years after the intervention ended, with elasticities ranging between 0.1 and 0.2.

Result 2. *Decreasing interest rates by 30 pp has no effect on default for the newest borrowers.*

The large sample size and explicit stratification allow us to focus on newer borrowers—a population of interest since they most starkly reflect the challenges of financial inclusion. We estimate eq. 6.1 separately for the newest (6–11 months with the study card) and oldest strata (24+ months) and plot our results in Figure 5. Figure 5(a) shows that newer borrowers default at roughly twice the rate of older borrowers by the end of the experiment, with a level difference that persists through the study period. Figure 5(b) displays the ATEs separately for the newest and oldest strata, showing that new borrowers do not respond to a 30 pp decrease in interest rates, with point estimates remaining consistently small and statistically indistinguishable from zero (in contrast to older borrowers, who are much more responsive). This is in stark contrast to the elasticities reported in the literature above. The unresponsiveness of newer borrowers to large changes in interest rates is striking, as asymmetric information problems are likely most severe for this population.[28]

Result 3. *Increasing minimum payments does not reduce default during the experiment, but reduces default by 1 pp in the long run.*

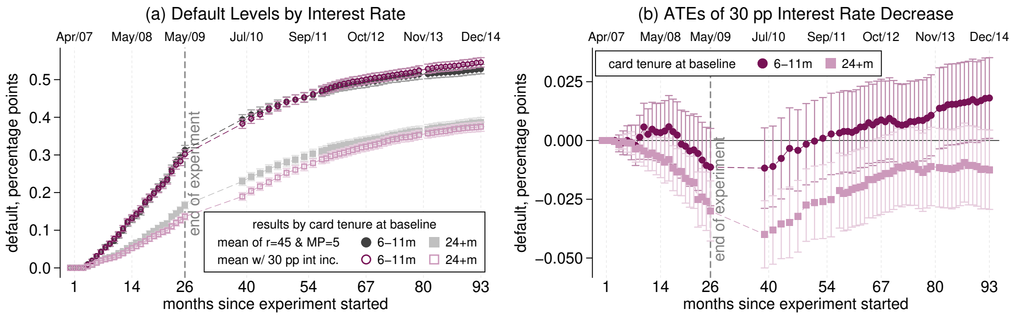
Figure 4(d) plots the estimated treatment effects corresponding to a doubling of the minimum payment (i.e., plotting the estimates of $\{\beta_t\}_t$ in Equation 6.1) from the base rate of 5%. The treatment effect is indistinguishable from zero for the first eight months of the study. Although we observe a slight decline in months 7 and 8, the point estimates are small (about 0.001 or a tenth of 1 percent) and not statistically significant (the smallest p-value is 0.46). For these reasons, we view these declines as indistinguishable from zero. Default rose sharply starting at month 9 and peaked at approximately 1 pp about 14 months into the intervention. The ATE then hovers around this point, and by the end of the experiment, the minimum payment increase had increased default by 0.8 pp ($p = 0.016$).

Given the cumulative nature of our default measure, this rough constancy of the ATE after month 14 implies very little subsequent differential default by treatment arm for the rest of the experimental period. Therefore, the increase in default due to higher minimum payments during the 26-month experiment predominantly arises from default in the first year. The implied 26-month elasticity is +0.04, and the confidence intervals rule out negative values. There are relatively few studies on the effect of minimum payments on default to benchmark our results. Our estimated elasticity is similar to those found in earlier non-experimental work (Keys and Wang, 2019; d'Astous and Shore, 2017) that documents estimates in the 0.01–0.06 range (see Figure 4(f) and Supplementary Appendix Table OA-7 for more details), and considerably different from Mexican

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

FIGURE 5

Effects of Interest Rates on Default by Tenure with Card



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

regulator and SSPP respondent predictions. On average, regulators predicted a decrease in default of 0.4 pp in response to an 18-month doubling of the minimum payment (from 5%) compared to the actual increase of 0.8 pp. Three-quarters of SSPP respondents predicted an increase in default from increasing minimum payments (consistent with our findings), but the mean predicted increase in default was substantially larger (6.4 pp).

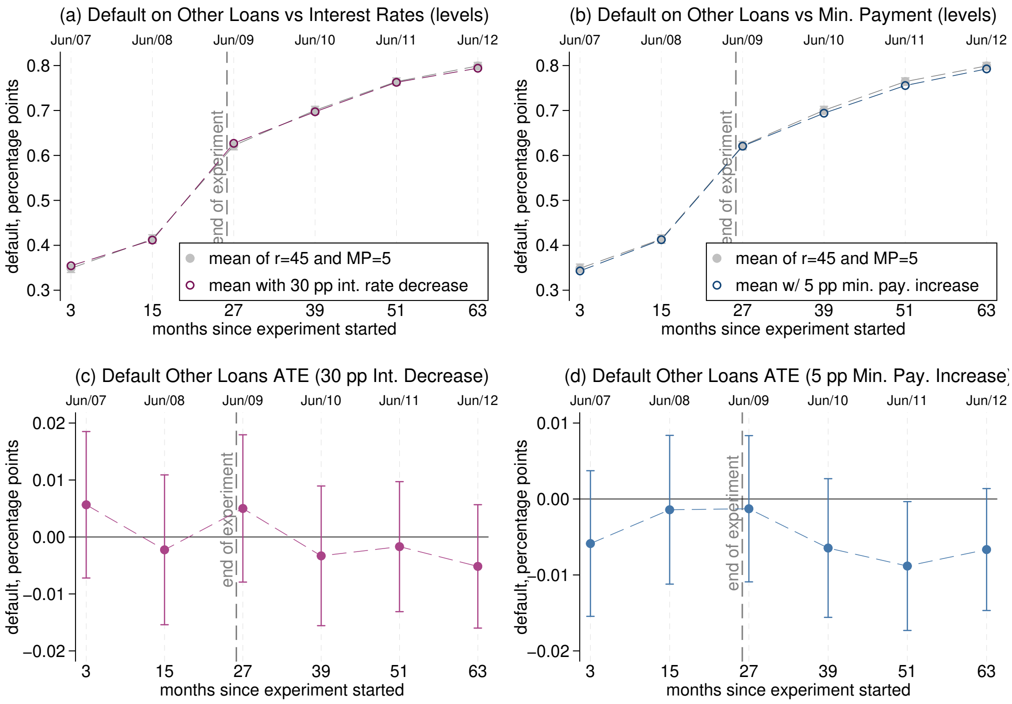
In contrast to the work cited above, the length of our panel and the experiment's timing allow us to estimate the post-intervention effects of the higher minimum payment. We find that the post-experimental long-run effects of the increase in minimum payments are opposite in sign to the effects in the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

6.1.1. Secondary Results.

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

FIGURE 6

Spillovers: Default on Other Loans



Notes: These figures plot the causal effect of interest rates and minimum payment changes on default in other loans and on new bank loan issuance. The dependent variable is default on any loan in the credit bureau except for the experiment credit card. The data source for the dependent variables is the credit bureau. The figures on the left examine interest rate changes. The figures on the right examine minimum payment changes.

rates is constant across both minimum payment arms in May 2009 ($p = 0.08$) and five years after ($p = 0.411$).

No Spillover Effects. The considerable variation in contract terms could also have affected behavior with other lenders, which we observe using the credit bureau data. For example, reductions in the study card interest rate could have decreased default on other loans. Additionally, higher minimum payments could have driven borrowers to other lenders, while lower interest rates may have had the opposite effect. We find no evidence of spillovers in terms of default, both during the experiment and after it ended (Figure 6).[29] We also find no evidence of crowd-out or crowd-in from other lenders in terms of new loans or loan closures (Supplementary Appendix Figure OA-8). This

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

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

6.2. *Mechanisms and Interpretation*

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

Mechanisms for Result 1. The model predicts that decreasing interest rates will decrease default (Prediction 1) and Supplementary Appendix Sections E.1.3 and E.1.5 clarify the role played by debt. While default is increasing in debt, the model demonstrates that debt *can be* increasing in the interest rate. Supplementary Appendix Equation 12 shows that a decline in the interest rate affects debt in two ways. First, debt increases as individuals purchase more in response to lower interest rates (the usual price effect). Second, debt declines since a lower interest rate is applied to the stock of previously accumulated debt. Thus, debt will *decline* in response to interest rate declines if the decrease in overall debt due to the latter exceeds the increases due to the former.

These patterns are indeed what we observe in the experiment. First, we document that purchases-net-of-payments in Figure 7(a) (and purchases in Supplementary Appendix Figure OA-20(e)) increase in response to interest rate declines, consistent with downward-sloping demand. Despite these increases, overall debt *declines* as Figure 7(b) shows.[31] Supplementary Appendix Section B.4 empirically verifies that current debt can be decomposed into past debt and current net purchases as in our model, and Supplementary Appendix Equation 36 shows that this decomposition implies that debt will increase with the interest rate, as we show empirically, if and only if the (mechanical) interest compounding effect exceeds the (behavioral) new purchase response. Finally, consistent with this line of argument, Figure 7(c) shows that the declines in default are concentrated among borrowers with the highest baseline debt utilization levels, i.e., those for whom the debt compounding effect is likely the strongest.

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

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

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

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

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

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

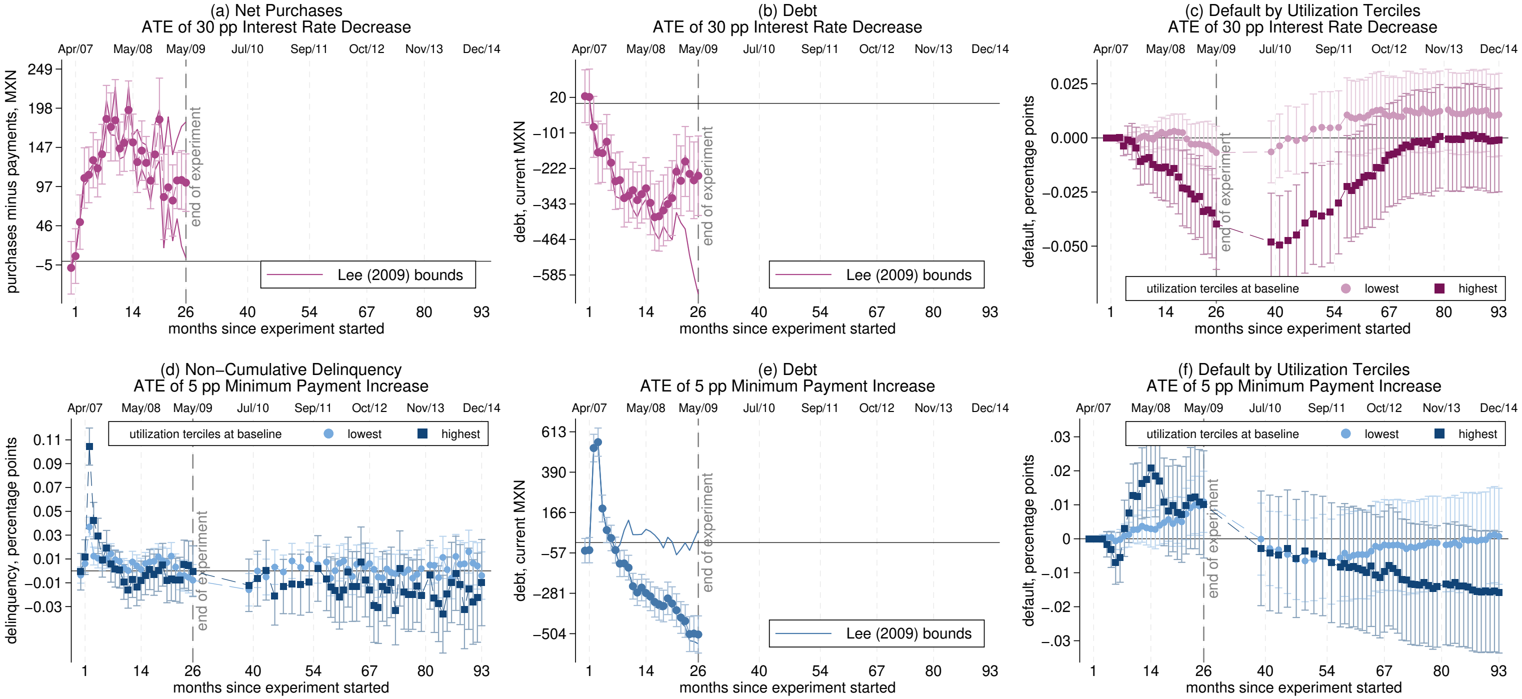
However, newer borrowers may also vary in other important dimensions from older borrowers. To study whether other baseline covariates may explain the difference between the treatment effect elasticities of new and older borrowers, we re-estimate the treatment effects at endline including a range of baseline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

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

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

FIGURE 7

Mechanisms: Contract Term Effects on Intermediate Variables



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

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

The empirical patterns we observe align with this interpretation. First, Figure 7(f) shows that the increase in default is entirely concentrated among borrowers with the highest debt utilization rates at baseline, who are arguably more liquidity-constrained than borrowers with lower utilization rates (see the discussion in Section 3). Second, the increase in default is preceded by a sharp rise in delinquencies (i.e., failure to make the minimum payment), particularly in months 3, 4, and 5 (Supplementary Appendix Figure OA-9(b)). This increase in the delinquency ATE only occurs among borrowers with the highest debt utilization rates (Figure 7(d)). Each such delinquency incurs a fee of 350 MXN pesos, further exacerbating repayment concerns. Indeed, we find a sharp rise in debt that mirrors the rise in delinquencies, suggesting that delinquency fees contributed to an increased repayment burden during this period and thus can also be attributed to liquidity constraints.[34]

Third, Supplementary Appendix Figures OA-15(d) and OA-9(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 stratum (the baseline debt utilization rate for minimum payers is 85%, more than twice the rate for full-payers). Furthermore, the constancy of the ATE after the first year implies there was very little differential default by the treatment arm after the first year during the experiment.

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

The long-run decrease in default from raising minimum payments during the experiment is consistent with regulators' safety-and-soundness concerns cited in the introduction (see fn. 2). For instance, the Office of the Comptroller of the Currency in the United States has been concerned that minimum payments are too low, thus

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

35. One plausible alternative to this debt-driven explanation is that borrowers in the higher minimum payment arm changed their payment behavior permanently in response to the experimental intervention e.g., through habit formation. Supplementary Appendix 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.

leading to negative amortization, debt accumulation, and elevated default risk.[36] Result 3 provides some evidence that higher minimum payments can decrease default over the longer run possibly by reducing debt (which may be a normative goal for paternalistic policy makers and/or in the presence of behavioral consumers, e.g., see fn. 5).

6.3. *Benchmarking Contract Terms Effects via Cash Flow Changes*

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

We estimate the total reduced form effect of contract term changes on *mpd* using the same specification as Equation 6.1 and estimate:

$$mpd_{it} = \rho_t + \mu_t \cdot \mathbb{1}\{MP_i = 10\%\} + \kappa_t \cdot (45\% - r_i)/30\% + \nu_{it}. \tag{6.2}$$

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

Normalized Default. We define the per-peso effect of each intervention on default at month t as $\lambda_t^{IR} \equiv \gamma_t / \sum_{j=1}^{t} \kappa_j$ and $\lambda_t^{MP} \equiv \beta_t / \sum_{j=1}^{t} \mu_j$. [38] We scale each estimate

36. The OCC (Williams, 2005; Hsu, 2021) wrote that "Finally, over the past several years, examiners observed declining minimum payment requirements for credit card accounts. During the same period, credit lines, account balances, and fees all have increased. As a result, borrowers who make only minimum payments have been unable to meaningfully reduce their credit card balances. From a safety-and-soundness standpoint, reductions in minimum payment requirements can enable borrowers to finance debts beyond their real ability to repay, thus increasing credit risk to the bank."

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

38. (γ_t, β_t) are defined in Equation 6.1. Recall that default is defined cumulatively when estimating Equation 6.1, so the numerator in each term captures the cumulative effect of the respective intervention on default while the denominator captures its cumulative effect on the minimum payment due.

so that $\lambda_t^{\cdot}$ can be interpreted in terms of percentage points per 1,000 MXN pesos of additional required payments. We find $\hat{\lambda}_{18}^{IR} = 0.36$ and $\hat{\lambda}_{18}^{MP} = 0.51$. A 1,000 MXN peso increase in the minimum payments due from an increase in the minimum payment (interest rate) is associated with a 0.51 pp (0.36 pp) increase in default over 18 months. The point estimates suggest that minimum payments have a stronger effect on default per-peso increment to debt servicing, consistent with minimum payments lowering the denominator for λ via reduced debt in the longer run. However, the two estimates are statistically indistinguishable (Supplementary Appendix Table OA-22), so based on this analysis, we conclude that both interventions had similar *per-peso* effects on default.

7. THE EFFECT OF JOB LOSS ON DEFAULT

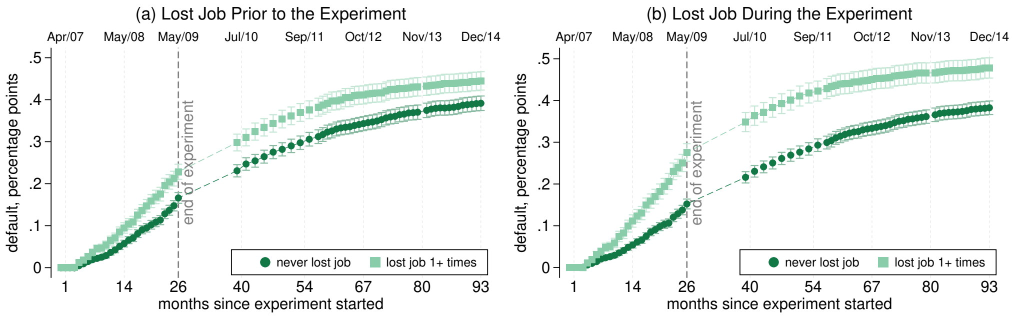
Despite the value of formal credit and the high cost of default documented in Section 3, default among new borrowers is substantial. Section 6 documents that even significant contract term changes have limited effects on default. In this section, we provide evidence that new borrowers are vulnerable to frequent, large shocks that precipitate default. This is a simple but relatively unexplored hypothesis in the financial development literature, which has typically focused on asymmetric information and high fixed lending costs. We focus on one particular shock—job separation in the formal sector—which we observe using our matched borrower-employee data. We can think of job loss in the framework of Section 5 by viewing it as generating a first-order stochastically dominated period-two income distribution. Prediction 4 would then imply that job loss increases default.

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

7.1. *Quantifying the Default Effect of Formal Job Loss*

The magnitude of the effect of job loss on default is important yet understudied. On the one hand, limited unemployment insurance in Mexico suggests that formal unemployment shocks could strongly increase default. On the other hand, informal employment and informal insurance are common in Mexico (see Section 3 for the former and, e.g., Morduch, 2004; Ohnsorge and Yu, 2022, for the role of informal insurance as a buffer from economic disruptions) and could mitigate the effects of formal sector job loss on default. In addition, default may be further mitigated as access to credit may be

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

particularly valuable during unemployment spells. Thus, even if the sign of the effect of job loss on default may not be controversial, its magnitude (and magnitude relative to, e.g., contract term effects) remains largely an open empirical question.

Given the difficulty of explicit randomization, work on the effects of job loss has focused on quasi-experimental methods. Jacobson et al. (1993) use mass layoff events, defined as significant net contractions in firm employment, to deal with the endogeneity of job loss. This approach has become increasingly common and has been used, inter alia, by Sullivan and von Wachter (2009); Couch and Placzek (2010); Flaaen et al. (2019). The key idea is that job loss during mass layoff events—referred to as *displacement*—is more likely to be an involuntary separation and, thus, potentially orthogonal to displaced worker characteristics.

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

We focus on firms with more than 50 employees and use the universe of formal employment data from the IMSS to define a mass layoff month as the first month in which the year-on-year employment decrease at a firm exceeds 30 percent of average employment in the 12 months prior to the experiment. These definitions (for firm size and layoffs) are standard in the literature (see, e.g., Davis and Von Wachter, 2011;

Flaaen et al., 2019) and yield 872 mass layoff events for the experimental sample during the experiment. At the firm level, mass layoffs decrease employment by 60 employees on average (about 27% of the average number of employees in a firm) and the wage bill by 424,000 MXN pesos (about 20% of the average wage bill).

We define an individual as displaced if they lost employment in the same quarter as the mass layoff event at their firm (i.e., in the month of the layoff and the preceding and succeeding months). Supplementary Appendix Figure OA-10 shows event study graphs for total employees and the wage bill using the estimation approach in de Chaisemartin and D'Haultfoeuille (2024), which confirm the substantial effects of mass layoffs on firm outcomes.

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

$$\text{default}_{it} = \alpha_i + \gamma_t + \sum_{k \neq 0} \beta_k \times \mathbb{1}\{t - \tau_i = k\} + \varepsilon_{it}, \tag{7.3}$$

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

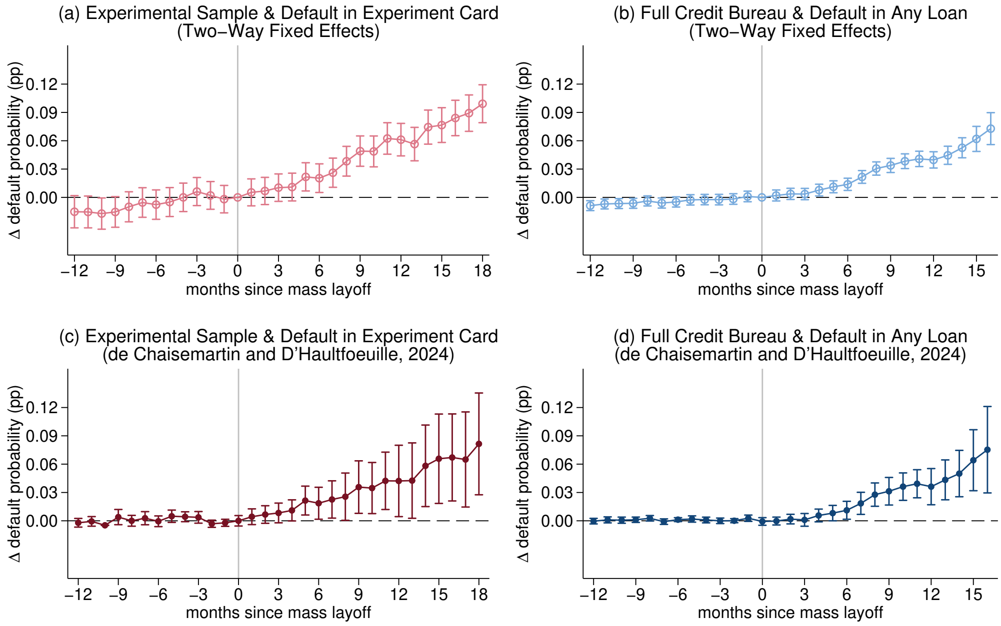
Result 4. *One year after separation, borrowers are 4.8 pp more likely to default on the study card, and this figure increases to 7.6 pp after eighteen months.*

Figure 9(a) and 9(b) show the effect of job displacement on default for our experimental sample during the experiment. The dependent variable is cumulative default on the study card—the same outcome as in the previous sections. We estimate

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

FIGURE 9

Job Displacement and Default



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

no differential pre-trends in default between displaced and non-displaced workers before separation, suggesting that their behavior in the credit market was similar prior to separation.

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

7.2. *Comparing the Default Effects of Displacement and Contract Terms*

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

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

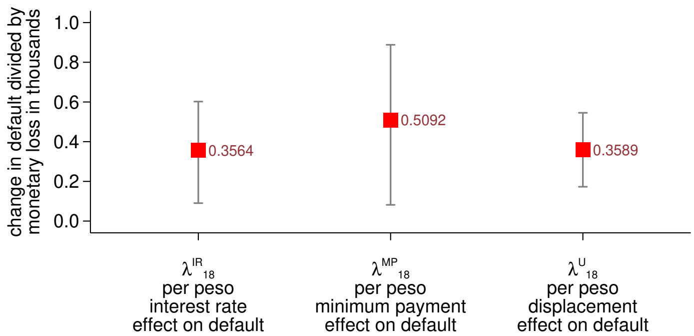
We proceed in two steps. First, we compute the loss in *formal sector earnings* arising from a displacement shock using the same econometric design as in Equation 7.3 but with formal sector monthly earnings (from the IMSS) as the dependent variable. We find that the change in formal sector earnings over the 18 months following formal job loss is 77,555 MXN pesos (Supplementary Appendix Figure OA-23). Second, we adjust this amount by accounting for income replaced via informal employment (which is not recorded in the IMSS). Ignoring this could seriously overestimate income losses given the fluid transitions between formal and informal jobs (see Section 3). Using the Mexican labor force survey (the ENOE), we find that 82% of all workers who lose formal employment in a given quarter are informally employed in the following quarter. In Supplementary Appendix Section J.1.2, we estimate that that fall in total labor earnings (after a formal unemployment event) is only 27.5% of the fall in formal earnings. We therefore calculate the total loss in income due to formal sector job loss as 21,328 MXN pesos ($\approx 0.275 \times 77{,}555$). Encouragingly, accounting for informal earnings also brings our magnitudes closer to previous work documenting earnings losses due to job displacement in other settings (see Supplementary Appendix Table OA-23). Finally, we normalize the default effect of job loss by dividing it by the estimated income loss:

$$\lambda_{18}^{U} \equiv \frac{\text{18-month default from formal job loss}}{(\text{18m formal income loss ('000 pesos)}) \times (\text{\% formal income not replaced})}. \quad (7.4)$$

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

FIGURE 10

Effect Sizes After Normalizing Default by Cash Flow Shock



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

7.3. *Discussion*

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

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

A common thread linking our findings, consistent with our framework, is that cash flow shocks and their impacts on debt are important determinants of default. A speculative narrative combining the stylized facts about the Mexican credit card sector and our estimates could be the following: new borrowers are credit-constrained with limited access to cheaper formal credit. This makes the study card valuable, and

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

borrowers have an incentive to avoid default when cash flow permits (interest-rate-driven moral hazard appears to be less important in this context). Given initial debt burdens, even large contract term changes have relatively modest effects on free cash flow and, consequently, modest effects on default. However, the frequency and consequences of job loss on cash flow are harder to mitigate, resulting in default.

8. CONCLUSION

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

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

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

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

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Data Availability

All code necessary to replicate the analyses results in this paper is publicly available at Zenodo: https://doi.org/10.5281/zenodo.15569935. The main datasets used (bank records, credit bureau data, and social security records) are confidential and thus cannot be shared publicly as they are protected under Mexican privacy and confidentiality laws. Instructions for researchers on how to request access to these restricted datasets are provided at the Zenodo link above. Additional data sources used in the analysis are publicly accessible and included in our replication package.

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