

Aydın, Deniz

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Consumption Response to Credit Expansions: Evidence from Experimental Assignment of 45,307 Credit Lines*

Deniz Aydın[†]

February 2021

Abstract

I design a large-scale field experiment that constructs a randomized credit limit extension isolating selection, anticipation, wealth, and interest rate effects and study the impulse responses on spending, contract choice, and balance sheets. Participants borrow to spend 11 cents on the dollar in the quarter of the limit increase, with a cumulative difference of 28 cents by the third year. The effects extend to those far from the limit, those who had the new limits as available credit, and those with a meaningful buffer of liquid assets. Participants near their limits borrow and spend when limits are relaxed but put off spending and save out of constraints under the counterfactual when limits are tight. The findings provide strong support for a buffer-stock interpretation that emphasizes the importance of precautionary saving.

(JEL D15, E21, E51, H31)

(Keywords: consumption, credit, *MPC*, precautionary saving, randomized field experiment)

This paper reports the results of a large-scale field experiment to study how personal consumption expenditures respond to credit shocks. I design a controlled trial, implemented at a large European retail bank in Turkey. The experiment constructs a randomized credit limit increase equivalent to, on average, 145% of monthly net income. The intervention deliberately and temporarily pauses the internal underwriting process for a randomly selected subset of 45,307 customers preapproved for a lender-initiated credit limit increase, creating a counterfactual withheld from receiving the limit increases for nine months. I then use the experimental shock in conjunction with rich administrative data on spending, contract choice, and balance sheets to track the impulse responses and estimate average and heterogeneous treatment effects—marginal propensities to borrow and spend—by comparing cardholders who receive the credit line extension at different times.

From a theoretical perspective, my main object of interest, the marginal propensity to consume out of credit limits, $MPC^{\Delta L}$, is distinct from but tightly linked to the well-studied *MPC* out of a one-time asset transfer; because debt-financed spending entails interest costs and must be paid back, it provides a lower bound. Therefore, an estimate of the magnitude, heterogeneity, composition, and dynamics of the spending response to a shock to only credit limits (isolating concurrent changes in income, wealth, interest rate, and risk) attracts considerable interest and complements previously

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[†]Washington University in St. Louis, Olin Business School. Email: daydin@wustl.edu

measured $MPCs$, providing an identified moment to discipline commonly used intertemporal consumption models.¹ From a policy perspective, the interest in this object is partly inspired by salient and dramatic credit cycles worldwide and the need to understand macroeconomic fluctuations associated with credit expansions and household leveraging. However, this object is also central for the design and targeting of fiscal and macroprudential policies to offset fluctuations (e.g., *stimulus lines*, limit caps), and plays an important role in understanding the consequences of precautionary behavior on aggregate demand.²

The large-scale experiment provides an opportunity to study the effects of a truly exogenous shock to credit limits on consumption behavior using a unique randomized controlled design.³ The increases in limits are salient changes initiated and pushed by the issuer, are not preannounced, and are difficult to anticipate. Other features of the credit contract, such as the borrowing rate, remain unchanged. The nature of the variation and the econometric evaluation is in the spirit of Parker et al.'s U.S. stimulus payment study, and the experiment could be interpreted as randomizing the timing of who gets limit increases over the course of the 9-month experimental timeframe. However, it is a pure shock to the credit limit that entails no wealth effects, and the experiment also creates long-run differences in limits. The credit shock is economically significant, on average, equivalent of \$1,600, or 145% of average monthly net income. Hence the utility loss from nonoptimizing behavior is not trivial. Therefore, the intervention can be classified as an unanticipated and exogenous shock to only credit limits that isolates wealth and interest rate effects.

I organize the empirical analysis in four sections. First, using event studies, I show that a pure shock to credit limits has a precisely measured and economically significant effect on the use of credit. Using the randomized experimental assignment as an instrument for the change in credit limits, I find that borrowing rises by 11 cents per 1 Turkish lira (TRY) of limit increase in the first quarter of the limit increase, and 16 cents over the 3-quarter experimental timeframe, factoring in balance shifting. The increase in borrowing comes predominantly through increased spending, with no discernible effects on delinquencies or labor supply, and is associated with a slight positive extensive margin adjustment in big-ticket loans. In the long run, the effects are not rapidly reversed but rather build beyond the experimental timeframe. Statistically significant cumulative effects between the treatment group and control group extend to the third year, with more than one-third and about two-thirds of the 28 cents 3-year cumulative response coming after the first quarter and the first year, respectively.

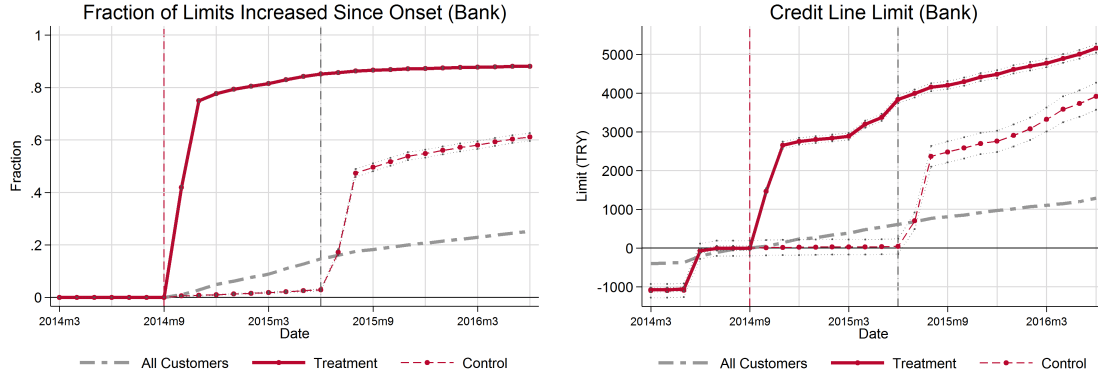
The second section analyzes in detail the heterogeneity in treatment effects. Although participants hold few liquid assets, only one-in-ten had binding constraints at the onset (i.e., utilizing more than three-quarters of their limits). For nine-in-ten, new borrowing over the short-run experimental timeframe was feasible using their baseline unused credit. Factors such as low income, high utilization of the existing limit, low nominal level of the credit buffer, low holdings of liquid assets, and the frequency with which credit constraints bind are robustly positively correlated with the marginal propensity to borrow and spend. Estimated 3-quarter $MPC^{\Delta L}$ s are the highest, at 50 cents, for participants with currently binding constraints, and the lowest, at 4 cents, for participants

¹See Nakamura and Steinsson (2018). Two aspects of disagreement regarding the $MPC^{\Delta L}$ concern heterogeneity—whether the effects of credit are confined to borrowers with a binding credit constraint who could not finance present purchases using resources that will accrue in the future—and dynamics—whether the short-run effects rapidly reverse, potentially holding back spending in the long run. An additional set of questions pertains to the mechanism through which credit expansions affect behavior, particularly what role is played by commonly invoked classical (e.g., precautionary savings) versus nonstandard ingredients that arbitrate spending through borrowing.

²Schularick and Taylor (2012) and Mian et al. (2017) argue that credit expansions are robust predictors of financial crises and subsequent declines in macroeconomic activity worldwide. Eggertsson and Krugman (2012) and Guerrieri and Lorenzoni (2017) formalize the effects of shocks to the credit limit in economies with heterogeneous agents, where the latter study the important interaction between credit constraints and precautionary behavior. Korinek and Simsek (2016) and Farhi and Werning (2016) argue for macroprudential policies to dampen credit expansions, where the tightness of the policies depend on MPC differences between borrowers and savers. See Jappelli and Pistaferri (2014) on the importance of MPC heterogeneity for the design of fiscal policy.

³Also see randomized evaluations of microcredit in low-income countries, see Banerjee et al. (2015) and Lane (2018).

Figure 1: Experimental Timeline



Note. The Figure on the left plots the fraction of participants whose limit has increased since the onset. The Figure on the right plots average credit lines at the bank, where the y-axis is normalized to display the cumulative increase relative to the onset. Vertical dashed and dash-dot lines denote the start and end dates of the experiment.

holding liquid assets worth more than 15 months of median net income. Nevertheless, the effects extend to participants with substantial ability to borrow, including those who are far from the limit, those who had the new limits as available credit, and those who hold a meaningful buffer of liquid assets.

Next, I study how participant pull consumption forward by analyzing spending patterns and the heterogeneity and dynamics of debt contract choice. In the short run, participants use flexible revolving contracts, accumulated through dynamic choice after seeing the end-of-billing-cycle balances, in tandem and in similar proportions to installment contracts, accumulated in-store and paid down over time according to a preplanned schedule. Flexible debt is primarily used to finance cash advances and open-ended spending on perishable nondurable goods, most notably groceries, gas, and services such as utilities. Installment debt is used to finance durables and services associated with future consumption in non-lumpy increments. Nontradable or discretionary nondurable spending accounts for an economically and statistically insignificant fraction of the response. Participants who are far from the limit tend to use more preplanned installment contracts, and the baseline contract share has strong explanatory power for the composition of the response. In the long run, the difference in flexible debt between the treatment and the control group attenuates, with preplanned installment debt accounting for the predominant share of the difference in debt levels.

In the final part of the empirical analysis, I turn to participants facing a binding constraint. I use event studies to analyze the dynamic interaction of constraints with precautionary behavior—the most frequently advanced explanation as to why consumption responds broadly to credit expansions. The estimated effect of the credit expansion on participants who are far from the limit is due to the treatment group increasing the pace at which they borrow. Strikingly, the estimated effect for high- MPC^{AL} participants near their limits is due to the control group's delevering. When in the treatment group, participants who are up against their limits increase borrowing and spending. Under the counterfactual, they appear to put off spending, avoid borrowing, and save their way out of binding constraints.

I view the key features of the findings as providing strong support for a buffer-stock interpretation of how consumption responds to credit expansions that emphasizes the importance of precautionary saving.⁴ Consumption behavior is sensitive to a credit expansion even for individuals

⁴ As in Imrohoroglu (1989), Deaton (1991), Carroll (1997), and Guerrieri and Lorenzoni (2017). As I describe the experimental design and present the results, I probe the assumptions and discuss the implications of commonly invoked ingredients and mechanisms that the theoretical literature on intertemporal consumption behavior emphasizes. These include the permanent income model à la Friedman (1957), in which a shock that is not net wealth and needs to be repaid has no

who are not near their constraints, as well as those who hold a meaningful buffer of liquid assets. $MPC^{\Delta L}$ is heterogeneous, negatively related to current liquidity, and positively related to the frequency with which constraints bind in the long-run. The smoking gun for precautionary behavior is the desire to build up a buffer by depressing spending and delevering when limits are tight. In line with this interpretation, although a significant sensitivity to credit is estimated broadly across the population, binding constraints—inability to finance current consumption using resources that will accrue in the future—appear to be transitory events, and those who persistently remain at their credit limit constitute a sliver of the population.

The experimental approach complements a voluminous observational literature that uses naturally occurring variation to estimate the borrowing response to changes in the credit limit and the consumption response to changes in income or wealth.⁵ The documented spending response to a shock to the credit limit that entails no wealth effects can be used to revisit $MPCs$, assuming that consumers can feasibly borrow to spend the new limits as an asset transfer, but at an interest cost proportional to the annuity factor. This calculation, discussed in Equation (1), implies $MPCs$ of 14 to 15 cents after a quarter and 29 to 34 cents after a year, assuming an MPC out of the predictable component of permanent income between two-thirds and 1.

Layout. The paper proceeds as follows. Section 1 provides details on the environment and key institutional features. Section 2 describes the experimental design and implementation. Section 3 presents the results in four subsections, with Section 3.1 reporting the event studies, balance sheet effects, and long-run responses, Section 3.2 heterogeneity in treatment effects, Section 3.3 compositional results on contract choice and spending composition, and Section 3.4 precautionary behavior and the dynamics of binding constraints. Section 4 concludes, and discusses implications for future research.

1 Environment and Institutional Details

Macroeconomic environment. The study is conducted in Turkey, an economy that has experienced a discernible household credit expansion during the 2000s. The household debt-to-GDP ratio rose from about 3% in 2000 to a peak of about 20% in 2013. The economy had been expanding between 2008 and 2018, except for declines in seasonally adjusted quarter-on-quarter GDP in Q1 2012, Q2 2014, and Q3 2016. The nominal GDP per capita based on PPP in 2014 was roughly \$23,000, or 62% of the EU-28 average. At the onset of the experiment, as of September 2014, the annual rate of inflation (CPI) was 8.9%, and the unemployment rate was 10.5%. The unit of measurement for the nominal variables is the local currency TRY with an exchange rate of 2.21 TRY-\$ at the onset. See Appendix D for details.

Credit line market. The credit lines considered here are very similar in structure to credit cards in the United States along principal dimensions. They are used as a means of payment and for liquidity within pay periods, as well as to transfer resources across pay periods. A single limit applies to all borrowing, in-store purchases, and cash advances. Those who pay the end-of-billing-cycle balance in full and on time get a float. Those who choose to not pay their balances in full accumulate interest-bearing debt equivalent to only the unpaid component of the balance.

effects in the short run and if generates interest costs has potential negative effects in the long run; two-agent spender-saver models with stylized heterogeneity in which the sensitivity to credit is driven by a small set of rule-of-thumb individuals who consume all of their disposable resources, as in Hall and Mishkin (1982) and Campbell and Mankiw (1989); models that feature simple heuristics that target credit line utilization, or quasi-rational behavior as in Cochrane (1989) and Kueng (2018); models in which a high propensity to borrow reflects a propensity to fall into delinquent status, as in Adams et al. (2009); expectations-based models focusing on the informational content of the limit increases, as in Bordalo et al. (2018); models of dynamically inconsistent repayment behavior, as in Heidhues and Köszegi (2010); and consumption models with endogenous illiquidity and kinks in the budget constraint, as in Kaplan and Violante (2014).

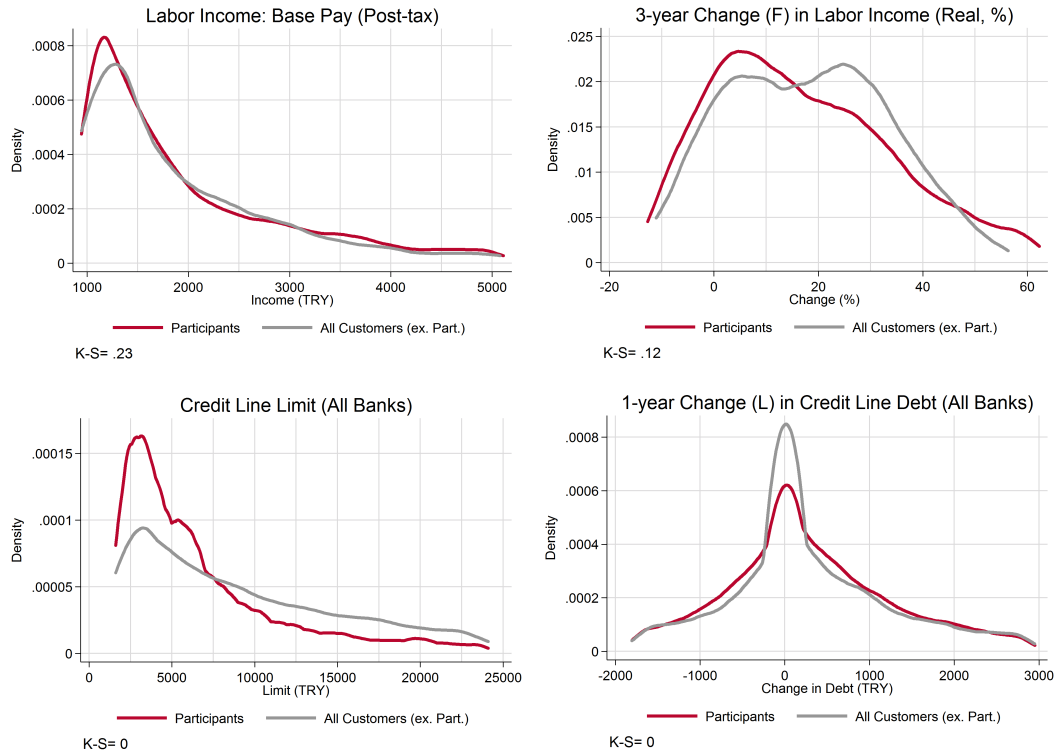
⁵See Gross and Souleles (2002) and Agarwal et al. (2015) for the former; Hall and Mishkin (1982), Johnson et al. (2006), Agarwal et al. (2007), Blundell et al. (2008), Parker et al. (2013), Berger et al. (2018), Baker (2018), Olafsson and Pagel (2018) for the latter; and Jappelli and Pistaferri (2010) for a survey.

Table 1: Summary Statistics

	N	Mean	Panel A: Participants N=45,307				Panel B: All Customers ex. Participants N=10,000	
			s.d.	p10	p50	p90	Mean	p50
Age	45,307	37	9.6	26	35	50	41	40
Labor income (TRY)	17,690	2,465	2,423	943	1,600	5,111	2,292	1,426
<i>Credit lines (Bank)</i>								
Limit (TRY)	45,307	5,111	5,653	800	3,150	12,000	7,305	3,000
Debt (TRY)	45,307	1,265	2,012	0	641	3,037	1,842	630
Flexible (TRY)	45,307	358	910	0	0	1,045	597	0
Installment (TRY)	45,307	907	1,657	0	373	2,278	1,245	212
<i>Spending (TRY)</i>								
Flexible (TRY)	45,307	874	1,577	0	387	2,151	954	201
Installment (TRY)	45,307	628	1,278	0	258	1,522	685	126
Installment (TRY)	45,307	248	757	0	0	687	273	0
<i>Credit lines (All Banks)</i>								
Limit (TRY)	45,307	10,462	17,289	1,600	5,000	24,100	20,284	8,500
Debt (TRY)	45,307	3,446	8,619	94	1,277	6,978	6,220	1,983
<i>Balance sheet</i>								
Debt (Total) (TRY)	45,307	18,463	103,847	334	6,017	49,640	20,742	5,812
Checking (Bank) (TRY)	30,796	1,011	3,269	0	4	2,153	721	0

Note. Panel A is based on $N = 45,307$ participants. Panel B is based on a random subsample of all credit line customers excluding participants ($N=10,000$). Statistics from the quarter before the experiment, June 2014. Nominal variables expressed in local currency TRY.

Figure 2: Selection: Kernel Densities



Note. Kernel densities compare $N = 45,307$ participants with a random subsample of all credit line customers excluding participants ($N=10,000$), using data from the quarter before the experiment. Densities are censored at the 10th and 90th percentiles of participants. Also displayed are the p -values from Kolmogorov-Smirnov tests for the null hypothesis of equality of distributions.

As of 2015, an annual volume equivalent to 21% of GDP flows through credit lines as in-store expenditures. It is also estimated that 40% of the working-age population have credit lines. Notably, credit lines are the predominant method for non-cash payments, with debit cards accounting for only 6% of in-store payments made using a debit or credit card.

A key feature of the credit line market is that the maximum interest rate that can be charged on any credit line or checking-linked overdraft account is capped by the regulatory authority at 24% APR, and this state-mandated maximum is binding for virtually all customers. The uniformity of borrowing rates under this cap allows me to ignore any pecking order across credit lines with potentially different rates, and focus instead on the notion of credit constraints as quantity constraints.

In addition to conventional *flexible* revolving debt, in which the borrower decides after seeing the end-of-billing-cycle balances whether to carry across pay periods or pay off in full, the credit lines also allow borrowers to finance purchases with preplanned *installments*. Similar to installment plans observed in the United States⁶ (iPhone, Affirm, Afterpay) and other countries (Mexico, Brazil, and Israel), consumers can voluntarily choose to borrow a fixed sum in an unsecured form for a predetermined term (typically 3 to 12 months) to finance in-store expenditures on a specific purchase and make preplanned payments until the loan is paid off. The installment payment due in a given month is deducted from the stock of installment debt and capitalized into end-of-billing-cycle balances alongside flexible spending. The remaining installments are reflected in installment debt, which carries specific balance calculations. The credit line yields a single consolidated statement, with total credit line debt carried across pay periods equal to the sum of the installment and flexible components.⁷

2 Experimental Design

For the purposes of the field experiment, I collaborate with a large European retail bank.⁸ The controlled trial is conducted by the financial institution as a pilot to better understand customer behavior. The exact nature of the intervention is to deliberately pause the internal credit line underwriting process 9 months for a randomly selected group of preexisting customers who would otherwise satisfy the underwriting criteria and have been pushed for credit line extensions. In this section I describe the key features of the randomized trial, with further details in Appendix A.

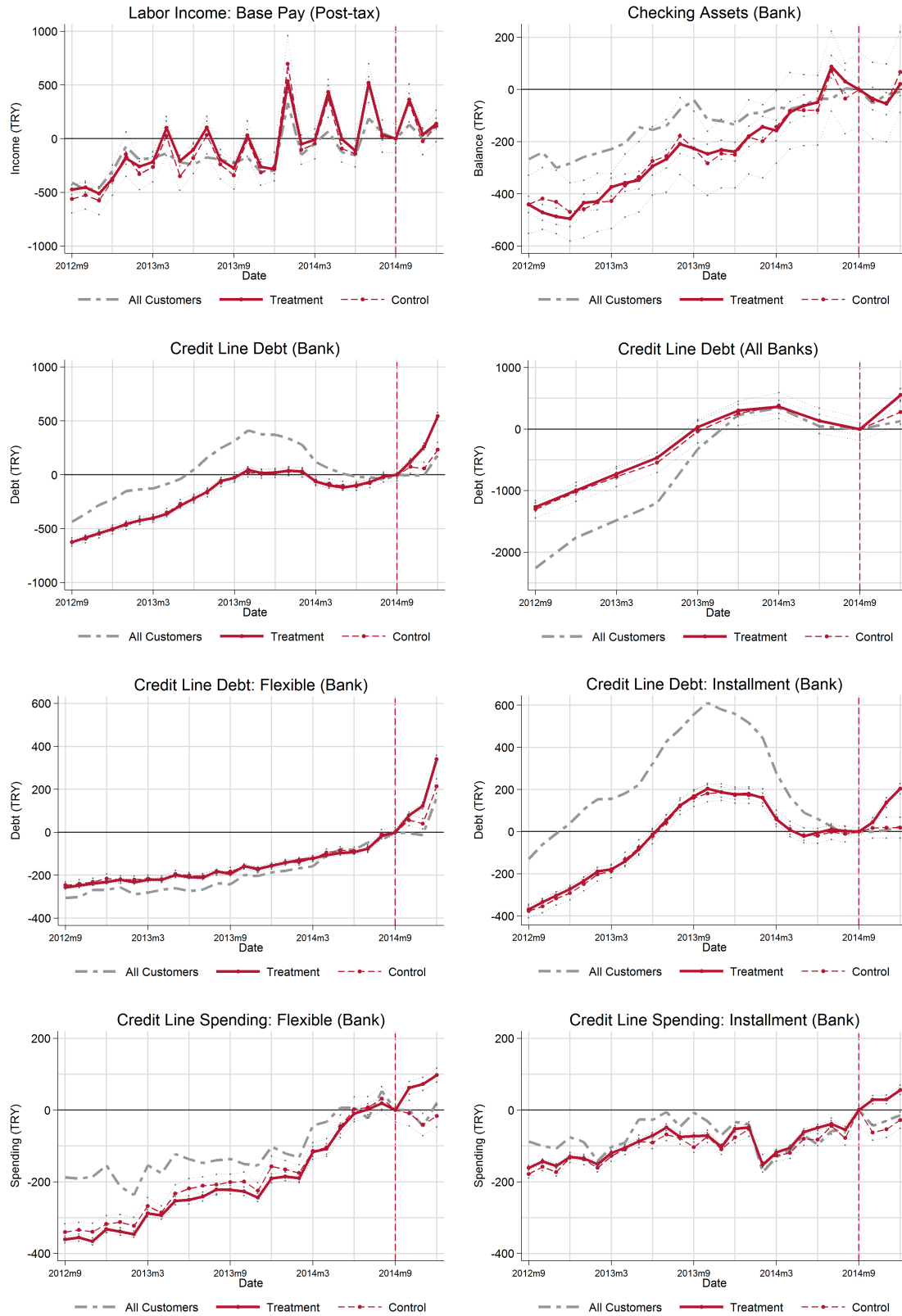
How participants are selected. The participants are not randomly selected from the broad population or the universe of cardholders, but are identified by processing active cardholders through the bank underwriting decision rule, outlined and discussed in Table A.1. Different divisions within the bank (affluent, new customer, small business owner, corporate) have different decision rules and adjust underwriting parameter thresholds at different times. The decision rule trades off the potential increase in revenue from the limit increase with the increased risk of default under the new limit, filters high-risk customers using in-house risk scores, and has built-in timing rules that make increases less likely for cardholders who have recently opened their accounts or have recently experienced credit limit increases.

⁶Persons (1930), Robinson and Stearns (1930), and Olney (1999) document that installment debt, often against a tangible asset, accounted for much of the expansion in household debt in the U.S. during the 1920s, with the collapse of consumption in 1930 following soon after. They find that over 40% of the 506 families of federal employees the BLS surveyed in 1928 used installments to finance furniture, clothing, radios, automobiles, pianos, and appliances. See Appendix C.4 for details.

⁷Installment debt is used to finance durables and semi-durables (e.g., appliances, furniture, clothing) and services (e.g., health and education expenses) and cannot be used, by law, to finance food and gas. When installment credit is available, the choice between making a flexible purchase versus using installment credit is strictly voluntary. There are no additional perks or benefits (e.g., price discounts or increased credit lines) associated with purchasing with installments, and installment purchases take up available credit. The effective rate on installment debt could be lower than the 24% APR interest rate printed on the statement due to special financing. See Appendix C.1 for a detailed discussion of installment plans.

⁸As of 2014, the financial institution is one of the 10 largest credit line platforms in Europe with a customer base that is representative of the local banked population. In addition to credit lines, it offers a multitude of financial products, including unsecured loans; mortgages; checking, savings, and overdraft accounts; as well as brokerage and insurance services.

Figure 3: Covariate Balance: Pre-trends



Note. Figures plot the levels of covariates for treatment ($Z_i = 1$) and control ($Z_i = 0$) groups by calendar month. Vertical dashed line denotes the start and end dates of the experiment. The y-axis is normalized to have levels equal to zero at the onset of the experiment. Vertical dashed line denote the start date of the experiment. Dashed lines indicate 95% confidence intervals for the estimate of the mean.

How participants compare with the typical cardholder. Table 1 displays summary statistics for the 45,307 participants in Panel A, and for a random subsample of all credit line customers excluding participants ($N=10,000$) in Panel B. Similarly, Figure 2 displays kernel densities to highlight the main differences between participants and the typical credit line customer. Participants, compared with the universe of cardholders, and on average, do not differ substantially in terms of age, labor income, spending on bank credit lines, and total debt. However, the participants are not representative of the typical cardholder on several observable dimensions, most notably, with respect to their limits on cards outside the bank, as the participants' median credit line across all banks is 30% lower than that of the typical cardholder. Hence, participants appear to be cardholders with low limits outside the bank whose incomes justify a credit limit increase and are catching up with the typical cardholder in terms of limits⁹

Randomization. Assignment of subjects to the control group is done after the customers have been preapproved for a limit extension but before the limits are pushed. Participants are first stratified into nonoverlapping and exhaustive bins with respect to their end-of-billing-cycle balances over limits. A random subsample is then drawn from each bin using a random number generator, and these participants are assigned to the treatment group. I denote this assignment $Z_i = 1$. The treatment group is then pushed downstream in the underwriting process for limit increases. The control group is withheld from lender-initiated credit line increases for 9 months starting in September 2014 by altering the decision rule governing automatic underwriting.

Covariate balance. This randomization procedure makes the exogenous variable for econometric evaluation the dummy variable for treatment group, Z_i . Figure 3 displays the pre-trends of main outcome variables, as well as other covariates that could potentially be correlated with borrowing and spending decisions, showing indistinguishable behavior between the treatment and control groups prior to the intervention. Similarly, Table A.2 performs statistical tests on these lags and finds no statistically significant differences in levels and changes in these pre-trend variables.

Timeline of limit increases. Table A.3 displays the timeline of the experiment and Figure 1 displays the timeline of limit increases. The first set of participants see the limit increases on their end-of-billing-cycle statements in September 2014. These extended limits are available for use in the first billing cycle of the experiment, October 2014. There are three impediments to immediate and perfect compliance. First, only 85% of participants in the treatment group see their limits increase, and most of this is staggered in the first two calendar months of the experiment. Second, 3% of participants in the control group excluded from lender initiated limit increases request and are granted a limit increase. Finally, starting in month 6 of the experiment, participants in the treatment group may be reevaluated and have their credit lines increased a second time¹⁰

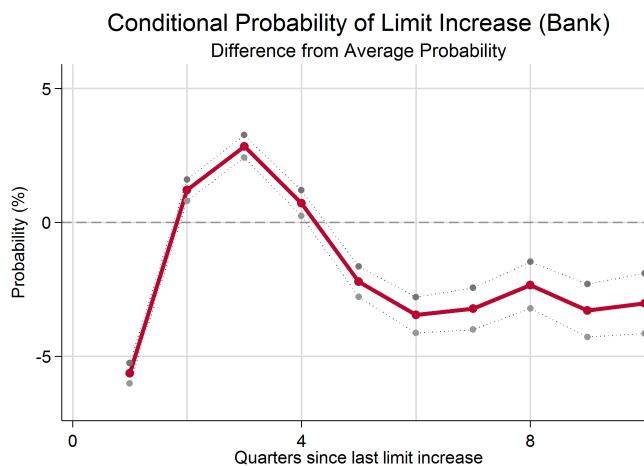
Duration of experiment. The short-run experimental timeframe concludes after 9 months, in June 2015, when the control group is allowed to proceed downstream in the underwriting process. Therefore, similar to Parker et al. (2013)'s stimulus payment¹¹ study, the experimental intervention could be interpreted as randomizing the *timing* of who gets limit increases *over the short run*. However, in contrast to that study, not everyone in the control group receives a limit increase after

⁹For applications such as an economy-wide credit expansion or fiscal policy, the object of interest is the average treatment effect on the broad population, including unqualified individuals. Comparing the banked and prequalified participants to the typical citizen (60% of which do not have access to credit lines), participants are likely to be much less credit constrained. In contrast, compared with the universe of cardholders, participants' lower baseline limits likely imply tighter credit constraints. However, low baseline limits may also indicate low credit demand, as it is possible for cardholders to request a limit increase manually.

¹⁰The primary determinant of who gets limits first in the first two months is operational constraints. For example, as limit increases are pushed right before the statement is printed, those with statement days later in the month may get the limit increases earlier. The remaining 15% of participants in the treatment group do not see their limits increase, either because of a lack of consent to automatic limit increases, or because of the regulatory cap on limit-to-income. The second limit increase for the treatment group after month 6 primarily reflects the expiration of the timing rules built into the bank's decision rule. Regarding the limit increases requested by the control group, one could assume that a similar percentage in the treatment group would request and be granted credit line extensions, given random assignment to treatment.

¹¹See Appendix A for a discussion of the parallels of the current experiment with fiscal stimulus payments.

Figure 4: Anticipation of Limit Increases



Note. Figure plots the estimated coefficients for 10 indicators for quarters since the last limit increase from a linear probability model predicting limit increases at the bank. The specification uses data on the 8 quarters prior to the start of the experiment for the $N=45,307$ participants and includes calendar quarter fixed effects.

the conclusion of the experimental timeframe. At this point, the control group is processed using different underwriting parameters, and credit may be extended to some but not necessarily all. Therefore, the short-run withholding also creates long-run differences in credit limits. I base my analysis primarily on the short-run 9-month experimental timeframe between October 2014 and June 2015 but also examine long-run responses.

Magnitude of limit increase. The credit line increases are economically significant, on average, 3,600 TRY or \$1,600, which is equivalent to about 145% of average monthly net income of the participants. Hence the utility loss from lack of consumption smoothing is not second order; see [Cochrane \(1989\)](#). However, the magnitude of the limit change conditional on a limit increase is not randomized and could potentially be correlated with characteristics such as income and base-line limit. The analysis restricts the amount of variation used only to only what is random: the assignment of a participant at the onset to control versus treatment group Z_i .

Information and salience. The experimental limit increases are *automatic*, initiated and pushed by the issuer, not requested by the customer. Subjects in the treatment group are notified through their preferred method of notification (phone call or text message), as is typical in all limit increases. They can also learn about the increased limits through their statement or through purchase receipts after using their card in-store. Importantly, the experiment takes place in a natural setting—participants are not placed on an artificial margin, but the shock is administered to preexisting customers and disbursed through an account they are familiar with and use prevalently. Moreover, there is no explicit participation choice and no lack of blinding, and therefore the cardholders are not aware that they are participants in a controlled trial.

Predictability and anticipation. In contrast to dividend payments or fiscal programs in which the details of the policy (timing, amount) is announced in advance, the typical automatic line increase is an idiosyncratic event with no bank-intermediated signal preannouncing its arrival. Moreover, randomization ensures that the treatment and control groups should have similar expectations, at least until receipt of the limit increase. Nonetheless, if the limit increases are predictable, participants may partially respond prior to the limit increase once they anticipate the increase. Moreover, after receiving (or not receiving) a limit increase, participants may believe that a limit increase is more or less likely, which could affect their behavior as well.

To address these concerns, I examine whether the limit increases are predictable. Visual inspec-

Table 2: Predictability of Limit Increases

		Panel A: Participants N=45,307						Panel B: All Customers ex. Participants N=10,000		
		In-sample			Out-of-sample			In-sample		
Actual	TP+FN	.18	.18	.17	.79	.79	.84	.12	.12	.15
Predicted	TP+FP	.57	.58	.51	.25	.33	.38	.57	.56	.53
Precision	TP / (TP+FP)	.21	.21	.21	.72	.80	.86	.14	.16	.20
Sensitivity	TP / (TP+FN)	.65	.68	.63	.22	.34	.39	.68	.73	.69
AUC		.56	.57	.61	.43	.55	.56	.55	.61	.63
Timing rule		✓	✓	✓	✓	✓	✓	✓	✓	✓
Account characteristics			✓			✓	✓		✓	
Income-based				✓			✓			✓

Note. Table reports classification accuracy of logistic regressions predicting limit increases at bank. The specification uses data on the 8 quarters prior to the start of the experiment. The out-of-sample accuracy is calculated for the treatment group in the first quarter of the experiment. See Figure A.1 for the histogram of predicted out-of-sample probabilities. Estimates using income-based right-hand-side variables for the subset of participants with labor income information. Logistic regression is assumed to predict a limit increase if the predicted probability is above the actual empirical frequency with which limits are increased over the period the logistic model is estimated. Threshold-invariant *AUC* measures the area under the receiver operating characteristic curve as the discrimination threshold is varied.

tion of the timing rule displayed in Figure 4 shows a hump-shaped pattern in the supply of credit and the conditional probability of a limit increase since the time of the last increase. However, the magnitude of the shifts induced by the timing rules is rather small. Moreover, it is difficult to accurately predict when a limit increase will occur using this timing rule, or a comprehensive econometric prediction. The kitchen-sink logit model reported in Table 2 has out-of-sample sensitivity (i.e., true positive rate) of 34% for the experimental limit increases the treatment group receives. The out-of-sample threshold-invariant area under the curve (*AUC*) of .55 (where .50 would correspond to random classification), hence points to a very low discriminatory power of the econometric specification to predict limit increases. Due to the low precision of forecasts based on repeat learning and calibration, I assume that participants are surprised by the arrival of limit increases, and the control group's not receiving a limit increase for 9 months does not have a material effect on their behavior.¹²

Effects on the interest rate and other margins. The experiment is designed to ensure the randomized assignment operates only via the impact on credit limits, hence isolating alternative causal pathways. The increase in the credit limit entails no wealth effects, and holds constant features of the credit contract such as the interest rate and non-interest perks.¹³ Moreover, vis-a-vis expectations-based models of credit cycles, the limit increases do not have informational value regarding the participants future income prospects, and almost all (97%) of the participants have previously experienced limit increases. Therefore, unlike a once-in-a-lifetime event, repeated experience potentially creates an opportunity for learning and attenuation of the informational cue effects. See Appendix B.2 for a discussion. In sum, the shock provides an opportunity to isolate and focus on the effect of a pure shock to only the quantity of available credit.

Relationship between MPC and MPC^{ΔL}. The consumption response to a change in the credit limit

¹²See Appendix B.1 for a detailed discussion and additional investigations. Importantly, the 33% of participants for whom the econometric model predicts a limit increase do not respond differently than the participants for whom the model does not predict a limit increase ($p = .31$).

¹³For example, a change in the contract interest rate that is contemporaneous with the change in the credit limit could affect spending due to an intertemporal substitution effect, as well as through an indirect wealth effect that acts through the revaluation of existing debt. The 24% APR regulatory cap on the interest rate allows me to abstract away from such interest-rate effects across time and cards. Moreover, given the uniformity of the interest rate, participants are more likely to be informed about the true cost of borrowing.

that entails no wealth effects is related to the MPC out of a one-time asset transfer through the MPC out of a change in permanent income via,

$$\frac{\Delta C^*}{\Delta L} = \frac{\Delta C^*}{\Delta A} - \frac{R}{1+R} \frac{\Delta C^*}{\Delta Y^P} \quad (1)$$

Intuitively, the consumer can feasibly borrow out of the increased credit limits to permanently increase assets, or vice versa. However, this will come at a periodic interest cost (or conversely, foregone benefit) proportional to the annuity factor¹⁴. Therefore, consumption will be sensitive to a change in the credit limits if sensitive to a one-time transfer. However, the $MPC^{\Delta L}$ will bound below MPC ; hence, evidence on high MPC s need not imply a high $MPC^{\Delta L}$. If borrowing is costless the estimated $MPC^{\Delta L}$ is likely to resemble an MPC .

2.1 Data

To track the impulse responses, I draw on administrative data on spending, contract choice, balance sheets (assets and liabilities), and labor income, with further details given in Appendix [D](#). Information on credit lines at the bank are taken from end-of-billing-cycle statements and include limits, within-cycle expenditures, and debt carried across statement periods. Credit line debt at bank can be decomposed to preplanned installment debt and flexible revolving debt. Expenditures at bank can be disaggregated into sectoral spending in 18 categories (e.g., groceries, appliances, health) mapped using a unique retailer point-of-sale machine identifier.

This information is supplemented with balance sheet and credit bureau variables, which contain detailed information on limits and debt owed both inside and outside the bank, including other types of liquid (e.g., overdraft) and illiquid debt (e.g., mortgages and unsecured loans), available on a quarterly basis.¹⁵ The data also contain limited information on the asset side of the balance sheet: checking balances at the bank and coarse indicator variables based on total liquid assets at the bank, including checking and savings accounts plus holdings of stocks, bonds, and funds. Finally, for a subset of customers whose employers have a direct deposit relationship with the financial institution, administrative data on monthly post-tax labor income is available. All of this information is consolidated for all accounts a customer has at the bank, matched with a unique citizenship number, and verified with a customer identification number to ensure perfect match quality.

3 Results

3.1 Event Study and Marginal Propensity Estimates

I begin the empirical analysis by plotting average credit line debt for the 45,307 participants, by treatment and control groups in Figure [5](#). In these event studies, the x -axis is calendar date and the dashed lines indicate the 95% confidence intervals for the estimate of the mean.

The top left panel displays the stock of debt on bank credit lines. The y -axis is normalized to display the increase in levels since the onset of the experiment. This variable measures balances carried across pay periods with the cumulative change relative to the onset tied to net cumulative spending over the same period.¹⁶ The sharp increase in credit line debt by the treatment group

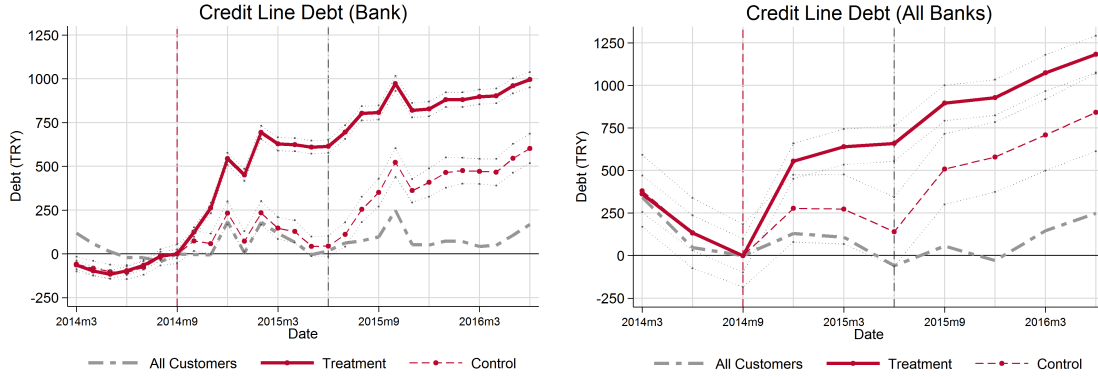
¹⁴I thank Adrien Auclert for bringing this relationship to my attention. See Appendix [A](#) for a discussion.

¹⁵Unlike bank data, which measures balances carried across pay periods, credit bureau data on credit lines is subject to potential limitations, as it is designed to measure the total liabilities at a point in time, hence, conflate float spending incurred during the cycle that is reported to the credit bureaus as a liability. In what follows, total credit line debt denotes the sum of debt carried across pay periods at bank and the total liabilities on credit lines outside the bank.

¹⁶For example, consider a state-provided stimulus line with a limit that households can spend but also top off. The *net* cumulative spent is given by the change in the balance of this account—total spent, minus topped off.

relative to the control group after the intervention is the causal effect of the increase in credit limits.

Figure 5: Event Study



Note. Figures plot average credit line debt at bank (left) and across all banks (right) for treatment ($Z_i = 1$) and control ($Z_i = 0$) groups by calendar month. The y-axis is normalized to have levels equal to zero at the onset of the experiment. Vertical dashed and dash-dot lines denote the start and end dates of the experiment.

I first focus on the short run, the 3-quarter experimental timeframe. I report first-stage, intent-to-treat, and marginal propensity (treatment effect) estimates using simple regressions of the form,

$$Y_i = \psi X_i + f_s + \varepsilon_i \quad (2)$$

where i denotes an individual and f_s stands for randomization strata fixed effects. The first-stage and intent-to-treat specifications make comparisons of the average change in credit line limits and debt between the treatment group and the control group (those withheld from the limit increases) using ordinary least squares (OLS) focusing on purely exogenous differences. The marginal propensity specification estimates the treatment effect of the change in credit limits on the change in credit line debt using two-stage-least-squares (2SLS), in which the randomized experimental assignment is used as an instrument for the change in credit limits.¹⁷

In the first-stage (FS) specification, the left-hand-side variable is $\Delta^\tau L_i$, the change in credit line limits over a period τ . Similarly, in the intent-to-treat (ITT) specification, the left-hand-side variable is $\Delta^\tau D_i$, the change in credit line debt over a period τ . In both specifications the right-hand-side variable is Z_i , the dummy variable for assignment to the treatment group. The coefficients ψ_τ^{FS} and ψ_τ^{ITT} are estimated using ordinary least squares, and measure the causal effect of assignment to the treatment group on the left-hand-side variable over a period τ , either 1 or 3 quarters. Randomization ensures orthogonality between assignment to the treatment group Z_i and all other variables, including omitted ones, and in particular the residual, which stand for shocks to consumption, income, wealth, risk, and the like.

The first row in Table 3 reports the first-stage on bank cards. The event study for this first-stage is displayed in Figure 1. The F -statistic from this first-stage regression is 1,224 and 1,407 for 1 quarter and 3 quarters. The second row reports the intent-to-treat estimates on bank cards. Focusing on 3-quarter differences, being withheld from the approved limit increase is associated with a first-stage difference in new credit lines at the bank of 3,795 TRY (with a standard error of 34 TRY) and a 3-quarter difference in new credit line debt at the bank of 571 (35) TRY. The latter number corresponds to about \$250 or about a quarter of average monthly post-tax labor income for participants. The naive Wald estimator of the marginal propensity on bank cards equals $\frac{571}{3,795} = .15$

¹⁷See Table A.4 for further details on the empirical framework used throughout the paper.

Table 3: Borrowing on Credit Lines:
First-stage, Intent-to-treat, and Marginal Propensity Estimates

	Baseline level	Panel A: Equation (2) — ψ						Panel B: Equation (3) — ϕ				
		First-stage F-stat		First-stage & Intent-to-treat		Marginal Propensity		First-stage & Intent-to-treat Cumulative		Marginal Propensity Point-in-time		Cum.
		1q	3q	1q	3q	1q	3q	1q	3q	1q	$\sum_{j=2q}^{3q}$	
Δ Limit (TRY) (Bank)	5,111	1,224	1,407	2,737 (24)	3,795 (34)			2,737 (24)	3,795 (34)			
Δ Debt (TRY) (Bank)	1,265			310 (28)	571 (35)	.113 (.010)	.150 (.009)	310 (28)	571 (35)	.114 (.010)	.049 (.015)	.162 (.012)
Δ Limit (TRY) (All banks)	10,462	933	1,114	2,589 (50)	3,554 (72)			2,589 (50)	3,554 (73)			
Δ Debt (TRY) (All banks)	3,446			278 (48)	519 (69)	.108 (.018)	.146 (.019)	278 (48)	519 (69)	.106 (.019)	.053 (.026)	.159 (.023)

Note. Baseline levels of limits and debt from the quarter before the experiment. Estimates in Panel A from Equation (2) use data on $N = 45,307$ participants. Estimates in Panel B from Equation (3) use data on $N \times T = 135,921$ participant-quarter observations, where robust standard errors are corrected for clustering at the individual level. First-stage and intent-to-treat estimates use OLS, and marginal propensity estimates use 2SLS. Cumulative effects calculated as $\Phi_\tau = \sum_{j=1}^\tau \phi_j$.

after 3 quarters.¹⁸

To ensure that the results do not represent mere balance shifting, the right panel in Figure 5 and the bottom two rows in Table 3 measure the effect across all banks.¹⁹ The 3-quarter first-stage estimates point to a difference in limits of 3,554 (72) TRY and the 3-quarter intent-to-treat estimates point to a difference in credit line debt of 519 (69) TRY implying that 6% of the difference in limits and 9% of the average increase in credit line debt is offset by adjustments on non-bank cards. The change in total credit line limit and debt, relative to baseline levels of 10,462 TRY and 3,446 TRY, represents a 34% and 15% increase respectively. For the remainder of the article, the focus is on total debt and limits across credit lines at all banks.

To estimate a treatment effect and obtain a value interpretable as marginal propensity (MP), I use as the left-hand-side variable $\Delta^\tau D_i$, and the right-hand-side variable $\Delta^\tau L_i$, again over either a 1-quarter or a 3-quarter period. I estimate ψ_τ^{MP} with 2SLS, with Z_i the dummy variable for being in the treatment group as the instrument. The estimated coefficient then gives the instrumental variables estimate of the local average treatment effect (LATE) for the participants who see limit changes induced by Z_i .²⁰ This choice of instrument limits the amount of variation to the assignment of control versus treatment group only, as, although the assignment to the treatment, Z_i , is random, the variation in *who* gets a limit increase, and *how much*, is not random, and could po-

¹⁸The Wald estimator of the marginal propensity on bank cards is calculated as the ratio of the intent-to-treat and first stage effects, i.e.,

$$\psi_\tau^{\text{Wald}} = \frac{\psi_\tau^{\text{ITT}}}{\psi_\tau^{\text{FS}}} = \frac{\mathbb{E}[\Delta^\tau D_i | Z_i = 1] - \mathbb{E}[\Delta^\tau D_i | Z_i = 0]}{\mathbb{E}[\Delta^\tau L_i | Z_i = 1] - \mathbb{E}[\Delta^\tau L_i | Z_i = 0]}$$

¹⁹At the onset of the experiment, 56% of participants had credit lines at other banks, and 47% were carrying balances on other bank cards. 23% of participants see their limits increase at other banks over the 3-quarter experimental timeframe, and this number is 3.5% lower for the treatment group. The fraction of cardholders carrying balances on cards outside the bank decreases by 0.4% for the treatment group after 3 quarters; however, this difference is not statistically significant. The response is quantitatively similar and statistically significant for the sample of cardholders that do not have a banking relationship with any other institution.

²⁰The instrument satisfies *monotonicity*, as withholding makes a participant less, not more, likely to receive a limit increase. I interpret those who request manual increases as *always-takers* and those who would bounce back from downstream underwriting processes as *never-takers*, with the rest as *compliers*.

tentially be correlated with the error term.²¹ The additional identifying assumption for the LATE interpretation is that there is no effect of experimental assignment Z_i on the outcomes studied, on average, that does not operate via the experimental assignment's impact on credit limits.

These estimates are reported in the last two columns of Panel A in Table 3. Similar to the above, the bottom two rows use total debt and limits across all banks. Results show that a unit increase in total credit lines across all cards is accompanied by an increase in total credit line debt of 10.8 (.018) cents after 1 quarter and 14.6 (.019) cents after 3 quarters. These estimates are highly statistically significant ($p < .0001$). As the specification is just identified, these marginal propensity estimates ψ_τ^{MP} are analogous with the naive Wald estimators $\frac{\psi_\tau^{\text{ITT}}}{\psi_\tau^{\text{FS}}}$.

The simple estimates obtained from Equation (2) confound the immediate effects that occur in the first quarter, versus the delayed effects that occur in the subsequent quarters. To capture dynamic effects, I also report the results of a quarterly panel regression of the form,

$$Y_{it} = \sum_{j=1}^T \phi_j X_{ij} + f_t + f_s + \varepsilon_{it} \quad (3)$$

where $t \in \{1, \dots, T\}$ stands for calendar quarters, and f_t stands for calendar quarter fixed effects. This specification uses data on $N = 45,307$ participants for the experimental timeframe of $T = 3$ quarters, totaling $N \times T = 135,921$ participant-quarter observations. Robust standard errors are corrected for clustering at the individual level.

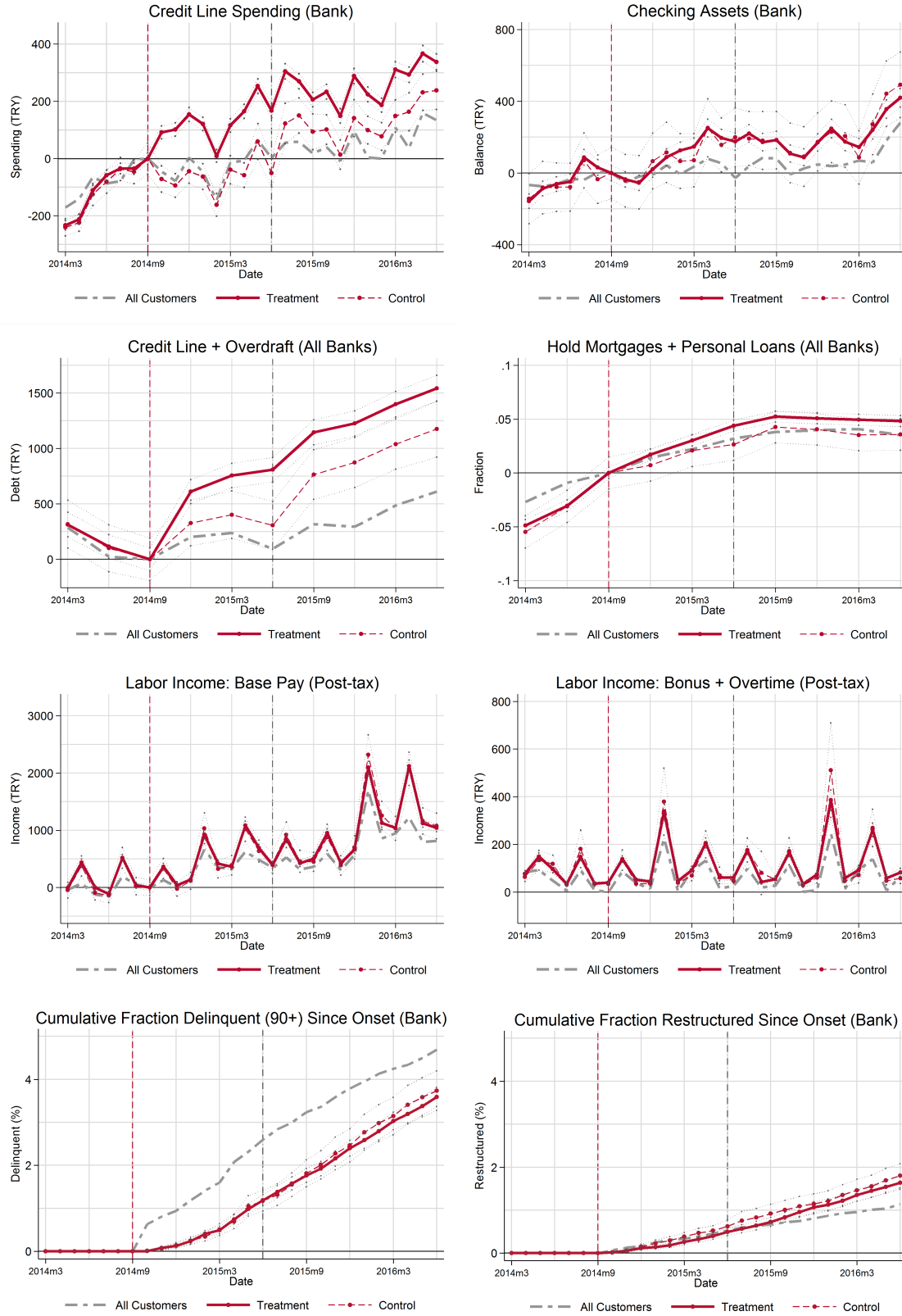
Similar to Equation (2), the dynamic specification Equation (3) can also be used to obtain first-stage, intent-to-treat, and treatment effect estimates. The first-stage and intent-to-treat specifications use OLS and focus on purely exogenous differences, in which X_{ij} is the treatment group dummy interacted with calendar quarter fixed effects, $Z_i \times f_{t=j}$. The coefficients ϕ_1^{FS} and ϕ_1^{ITT} isolate the difference in the changes in limits and debt between the treatment and control groups in the *first calendar quarter of the experiment*. The cumulative estimates, $\Phi_\tau^{\text{FS}} = \sum_{j=1}^\tau \phi_j^{\text{FS}}$ and $\Phi_\tau^{\text{ITT}} = \sum_{j=1}^\tau \phi_j^{\text{ITT}}$ add these point-in-time coefficients and yield the difference in the cumulative change in credit lines and debt over a time frame of τ calendar quarters. These dynamic cumulative estimates, displayed in the first two columns of Panel B, coincide with the simple intent-to-treat estimates ψ_τ^{ITT} obtained using Equation (2).

In the marginal propensity specification, the left-hand-side variable is the change in credit line debt, ΔD_{it} , and X_{ij} is ΔL_{it-j+1} . The coefficient ϕ_1^{MP} measures the point-in-time effect of a unit change in credit line limits on the left-hand-side variable in the *quarter of the limit increase*. The remaining coefficients ϕ_2^{MP} and ϕ_3^{MP} measure the delayed point-in-time responses that occur in the two quarters subsequent to the increase in limits. In this dynamic marginal propensity specification, the coefficients ϕ_j^{MP} are estimated using 2SLS, in which the right-hand-side variable of the intent-to-treat specification above—treatment group dummy variable interacted with calendar quarter fixed effects—is used as the instruments.

The cumulative marginal propensity, $\Phi_\tau^{\text{MP}} = \sum_{j=1}^\tau \phi_j^{\text{MP}}$, adds the point-in-time coefficients and yields the cumulative impulse response of a unit change in credit lines on the left-hand-side variable over a time frame of τ quarters since the limit increase. This is the main marginal propensity estimate I use throughout the paper. In order to measure the delayed response that occurs after the first quarter of the limit increase, I also report $\phi_2^{\text{MP}} + \phi_3^{\text{MP}}$, which measures the sum of the delayed responses that occur in the two subsequent quarters.

²¹For example, using a dummy variable for receiving a limit increase as the instrument would identify the effect from *who* gets a limit increase. Similarly, estimating this specification by ordinary least squares would identify the effect also including the variation in *how much* the limits are increased, $\Delta L_i | \Delta L_i > 0$. However, participants in the *control* group may *request*, and *receive*, limit increases; and the magnitude of the limit increase for the treatment group is not randomized.

Figure 6: Event Study: Balance Sheet Effects



Note. Figures plot the levels of covariates for treatment ($Z_i = 1$) and control ($Z_i = 0$) groups by calendar month. The y-axis is normalized to have levels equal to zero at the onset of the experiment in September 2014. Vertical dashed and dash-dot lines denote the start and end dates of the experiment.

Panel B in Table 3 displays the results obtained from the dynamic specification Equation (3). The estimates for ϕ_1^{MP} show that a unit increase in credit limits increases additional borrowing by 10.6 (.019) during the quarter in which it was received. The 3-quarter cumulative response, MP_{3q} , is estimated as 15.9 (.023) cents. These 1-quarter and 3-quarter estimates obtained from the dynamic specification reported in Panel B are broadly compatible with the simple marginal propensity estimates reported in Panel A. Borrowing rises sharply in the first quarter following a credit limit increase, and subsequent marginal coefficients decline in significance. Although the estimates for the delayed response coming after the first quarter have less precision than the contemporaneous effect, there is evidence of borrowing, 5.3 (2.6) cents, in the two subsequent quarters beyond the quarter of the increase in limits.

Balance sheet effects. The event studies in Figure 6 display the effects of the credit limit increase on credit line spending, checking assets at the bank, total liabilities, labor income, and delinquencies. Statistical estimates and tests are relegated to Table A.7.

The top left event study in Figure 6 shows that the increase in credit line limits and borrowing is associated with a large increase in spending. I analyze this spending response in detail in Section 3.3. Focusing on the checking assets on the top right, there is no evidence of crowding out on this dimension (3-quarter $p=.76$). The second row in Figure 6 separates total liabilities into smaller liabilities (credit lines and overdraft) and big-ticket loans (mortgages and unsecured personal loans). Focusing on smaller liabilities, the intent-to-treat estimates point to a 3-quarter difference of 502 (72) TRY in the sum of credit lines and overdraft, implying a 3% offset by adjustments on overdraft debt. Focusing on extensive margin adjustments on big-ticket loans, the treatment group is 1.7 percentage point more likely (off of a 60% base) to take on big-ticket loan by the end of the experimental timeframe ($p=.009$).

The event studies analyzing labor income show no discernible effects in base pay ($p=.20$), but a small increase in overtime and bonuses in the two Januaries subsequent to the onset of the experiment. The difference, 41 (74) TRY and 125 (104) TRY, correspond to about 1.5% and 5% of average monthly post-tax labor income. However these effects are not statistically significant ($p=.58$ and $p=.23$). Hence, there is little evidence that participants respond to an expansion of credit by working less.

Finally, as the credit contract contains the option to default, the sensitivity of borrowing to increased credit limits may reflect a tendency to fall into delinquent status. A credit line account is classified by the bank as nonperforming if payments on outstanding balances are past due by 90 days or more. It is also common for delinquent debt to be restructured through a maturity extension prior to falling into collection status. The event studies for these variables displayed at the bottom. By the end of the experimental timeframe 1.2% of the participants are past due by 90 days or more and .6% restructure, with no statistically significant differences between the treatment group and the control group ($p=.95$ and $p=.13$).

Long-run effects. A noteworthy feature of the experiment is that not everyone in the control group receives a limit increase after the experimental timeframe's conclusion. Hence, the short-run withholding of limits also creates long-run differences. This feature allows for an investigation of long-run effects beyond the short-term window. A priori, the sign or magnitude of the long-run impact of credit is not obvious.²²

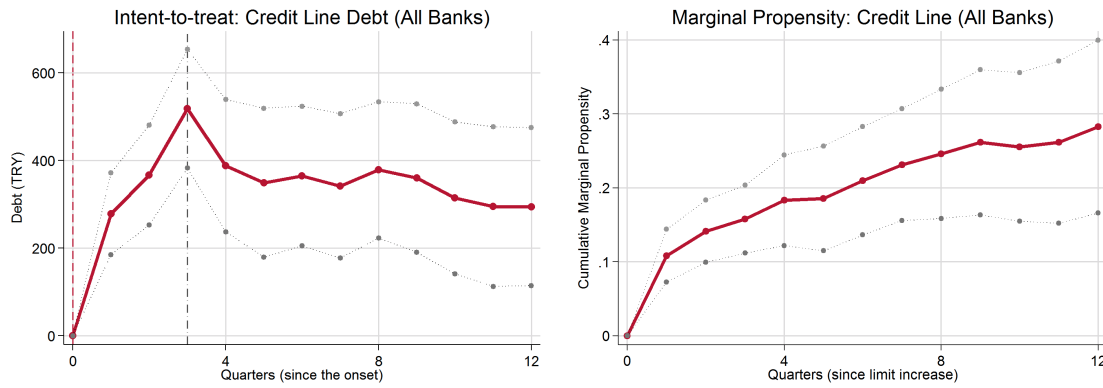
Table 4 displays the cumulative first-stage and intent-to-treat estimates obtained from Equation (3), extending the horizon to $T=12$ quarters. Displayed estimates report the average difference in the change in limits and debt between the treatment group and the control group, over a period τ of either 4, 8, or 12 calendar quarters. The intent-to-treat estimates are visually displayed on the

²²In contrast to the MPC out of assets, which should sum to one in the long-run, the marginal propensity to borrow out of credit limits should sum to zero in the long-run. However, interest costs and debt service can also hold back future borrowing and spending.

Table 4: Borrowing on Credit Lines: Long-run

	Baseline level	First-stage & Intent-to-treat Cumulative			Marginal Propensity Point-in-time				Marginal Propensity Cumulative		
		4q	8q	12q	1q	$\sum_{j=2q}^{4q}$	$\sum_{j=5q}^{8q}$	$\sum_{j=9q}^{12q}$	4q	8q	12q
Δ Limit (TRY) (All banks)	10,462	1,450 (102)	844 (141)	717 (167)							
Δ Debt (TRY) (All banks)	3,446	388 (77)	379 (79)	295 (92)	.108 (.018)	.075 (.025)	.063 (.037)	.037 (.037)	.183 (.031)	.246 (.045)	.283 (.060)

Note. Baseline levels of limits and debt from the quarter before the experiment. Estimates from Equation (3) use data on $N \times T = 543,684$ participant-quarter observations, where robust standard errors are corrected for clustering at the individual level. First-stage and intent-to-treat estimates use OLS, and marginal propensity estimates use 2SLS. Cumulative effects calculated as $\Phi_\tau = \sum_{j=1}^\tau \phi_j$.

Figure 7: Borrowing on Credit Lines:
Long-run Intent-to-treat and Marginal Propensity Estimates

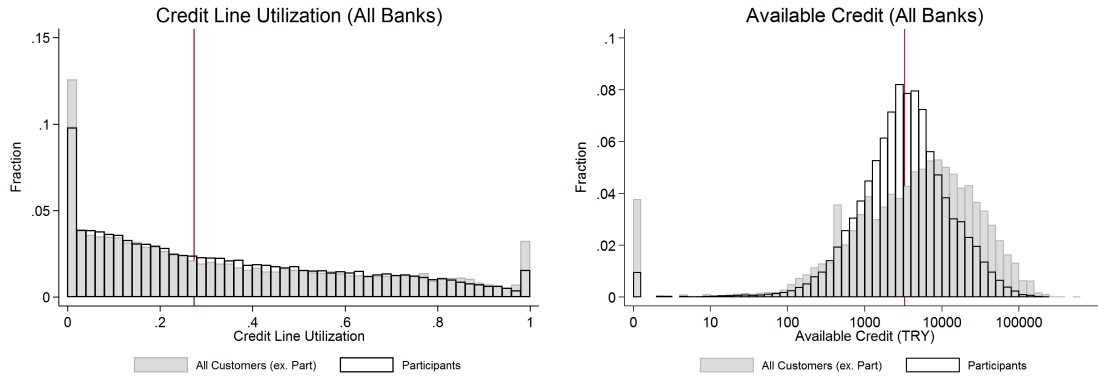
Note. Figure plots the long-run cumulative intent-to-treat and marginal propensity estimates. Estimates from Equation (3) and use data on $N \times T = 543,684$ participant-quarter observations, where robust standard errors are corrected for clustering at the individual level.

left in Figure 7. Over time the difference in credit limits between the treatment and control groups attenuates as credit limits are increased for participants in the control group who were withheld from the limit increases. After 12 quarters, 89% of the participants in the treatment group and 70% of the participants in the control group see their limits at bank increased since the onset, with a difference in the change in total credit limits of 717 (167) TRY. The difference in the change in total credit line debt is 295 TRY (92), which correspond to a naive Wald estimate of the 3-year marginal propensity of 41 cents.

The remaining columns report the results of the marginal propensity specification. This specification also extends the horizon to $T=12$ quarters, and makes use of the changes in limits that occur after conclusion of the experimental timeframe when some, but not all, of the participants in the control group see their limits increased. Similar to the short-run specification, the coefficients are estimated using 2SLS, in which the right-hand-side variable of the intent-to-treat specification—treatment group dummy variable interacted with calendar quarter fixed effects—is used as the instrument.

The object of interest, the cumulative marginal propensity, is displayed on the right in Figure 7. In this figure, the x -axis represents quarters since the limit increase. The estimates obtained from this long-run specification are quantitatively consistent with the estimates reported in the

Figure 8: Baseline Distance-to-limit



Note. Histograms based on data from the quarter before the experiment. Vertical lines indicate the median.

short-run specification in Table 3. When the credit limit is increased, participants borrow 10.8 (1.8) cents in the quarter they are received, and 18.3 (3.1) cents in the first year of the limit increase. Although the point-in-time effects decay over time, borrowing continues beyond the first year, with statistically significant and economically large cumulative effects extending to the second and third years ($p < .001$). The cumulative response is 24.6 (4.5) cents after 2 years and 28.3 (6.0) cents after 3 years, which indicates more than one-third and about two-thirds of the 3-year cumulative response occur after the first quarter and the first year, respectively. For the 3-year horizon, the effect does not exhibit a reversal but instead builds up over time, implying that the short-run borrowing response to the credit shock does not merely reflect a transitory surge of borrowing that is rapidly reversed.

3.2 Heterogeneity by Baseline Distance-to-limit and Liquid Assets Holdings

A clear picture emerging from the previous section is that a pure shock to the credit limit has discernible effects on the borrowing and spending behavior of participants. However, the average treatment effect estimates mask substantial heterogeneity and do not distinguish whether the response is driven by participants across the distance-to-limit and liquid assets distribution, or by the part of the sample that has no liquid assets, face binding constraints, or could not have feasibly financed the new borrowing with existing available credit.

I examine in detail the heterogeneity of the response, using various baseline (pre-experiment) measures of distance-to-limit and liquid assets. First, consistent with commonly used in the literature, and models in which precautionary mechanisms are in play and a normalized measure of distance-to-limit is key, I use total credit line utilization, defined as the ratio of total credit line debt to the total limit. Second, I group participants by available credit (unused limits) in levels, defined as the difference in the total credit line limit and the total credit line debt. Finally, to study the effects of credit by liquid financial asset holdings, I use coarse classifications based on the balances of total liquid assets at bank.

Figure 8 displays the histogram of credit line utilization and unused limits in levels across all cards and banks in the quarter before the experiment. Perhaps surprisingly, median credit line utilization is .27 with only 11% of the participants utilizing more than 75% of their credit lines; and the median available credit is 3,284 TRY, about \$1,500, or 1.33 times the average income.²³ For 89% of the participants the debt levels by the end of the experimental timeframe was feasible using their

²³Nevertheless, 39% face a binding constraint in the following 3-year period. Conditional on having a liquid asset account, 48%, 22% and 5% of the participants hold liquid assets at bank more than 250 TRY, 2,500 TRY, and 25,000 TRY (corresponding to about \$100, \$1,000, \$10,000), respectively. For the U.S. see Board of Governors of the Federal Reserve System (2015), who report that 47% of adults in the could not cover a \$400 unexpected expense with cash or it's equivalent. Also see Ogden (2018) for a cross-country comparison.

unused credit at the onset. Moreover, 72% of the participants had the additional limits they receive as available credit.

For each of these variables, participants are sorted by their baseline values in the quarter before the experiment and placed into K bins. I then estimate a nested variant of the dynamic specification in Equation (3),

$$Y_{it} = \sum_{k=1}^K \sum_{j=1}^T \phi_{jk} \cdot X_{ij} \times f_k + f_{kt} + f_s + \varepsilon_{it} \quad (4)$$

where k denotes a bin and f_{kt} stands for calendar quarter-bin fixed effects. This specification uses data on the $N=45,307$ participants for the experimental timeframe of $T = 3$ quarters, totaling $N \times T = 135,921$ participant-quarter observations. I report robust standard errors that are corrected for clustering at the individual level.

Table 5 displays the estimates. In this table, panels are organized by the baseline distance-to-limit or liquid assets measure. L and D stand for total credit line limit and debt across all banks. In each panel, the first column displays the average available credit, $L - D$ across all banks in the quarter before the experiment. The second and third columns display the 3-quarter cumulative first-stage (FS) and intent-to-treat (ITT) effects on the changes in limits and credit line debt. Finally, the fourth column reports the object of interest, the cumulative marginal propensity (MP) over 3 quarters²⁴

Panel A in Table 5 reports the heterogeneity of the response by credit line utilization. Participants who were not carrying any balances across pay periods are in the bottom bin. Those that were carrying some balances but were using less than 25% of their existing limit are in the second-to-bottom bin, and so on. This normalized distance-to-limit measure directly identifies participants for whom the constraint is binding.

Panel A points to substantial heterogeneity based on participants' disposable resources. Unsurprisingly, baseline demand out of the existing supply of credit limits is associated with a high marginal propensity to borrow, and the estimated responses are largest for participants who are closer to their constraints. For example, the 11% of participants who use more than 75% of their existing credit lines borrow 50 (6.7) cents, corresponding to more than three times the average response. These participants are arguably at a corner solution to their intertemporal problem with little to no credit available to spend out of resources that will accrue to them in the future. However, the 95% confidence interval for the response lies between .37 and .63 rejecting the null of literal hand-to-mouth behavior where participants at this kink against their limits follow a rule-of-thumb and simply borrow all their newly available credit. For the remainder of the article, using more than three-quarters of credit lines is equated to binding constraints.

At the other extreme, the small group (6%) of participants not carrying any balances are induced by the limit increase to borrow 7.3 (2.6) cents. Similarly, those with some balances but less than 25% of their limits across all cards accumulate 7.3 (3.0) cents. This utilization group had an average available credit of about 10,000 TRY prior to the intervention, and see an increase in limits of about 4,000 TRY on top of the control, accompanied by an increase in debt of 247 (102) TRY. 95% of this group, including the control group, could have feasibly financed their debt levels by the end of the experimental timeframe using their unused credit at the onset. Moreover, 81% of this group had the additional limits they receive as available credit. Table A.8 shows that the effects extend to those who had the additional limits they receive as available credit as well.

²⁴As in the dynamic specification in Equation (3), in the first-stage and intent-to-treat specifications X_{ij} is $\mathbb{Z}_i \times f_{t=j}$, the treatment group dummy interacted with calendar quarter fixed effects. In the marginal propensity specification, X_{ij} is ΔL_{it-j+1} . The coefficients are estimated using two-stage least squares, in which the right-hand-side variable of the intent-to-treat specification above—treatment group dummy interacted with bin fixed effects and calendar quarter fixed effects—is used as the instruments.

Table 5:
Borrowing on Credit Lines:
Heterogeneity by Baseline Distance-to-limit and Liquid Asset Holdings

Panel A: Credit Line Utilization						Panel B: Available Credit Quintiles					Panel C: Total Liquid Assets at Bank				
			FS _{3q}	ITT _{3q}	MP _{3q}			FS _{3q}	ITT _{3q}	MP _{3q}			FS _{3q}	ITT _{3q}	MP _{3q}
	<i>D/L</i>	<i>L - D</i>	ΔL	ΔD	ΔD	$QU_{(L-D)}$	<i>L - D</i>	ΔL	ΔD	ΔD	A^{Liq}	<i>L - D</i>	ΔL	ΔD	ΔD
		(TRY)	(TRY)	(TRY)			(TRY)	(TRY)	(TRY)		(Bank)	(TRY)	(TRY)	(TRY)	
<i>Less liquidity</i>	[0.75, 1]	1,278	2,580 (210)	1,227 (182)	.503 (.067)	QU_1	557	2,466 (91)	713 (43)	.298 (.022)	\emptyset	6,808	2,485 (112)	459 (120)	.200 (.052)
	[0.50, 0.75)	3,957	2,707 (139)	1,010 (251)	.403 (.096)	QU_2	1,767	2,616 (86)	658 (78)	.266 (.033)	[0, 250)	5,626	2,565 (111)	663 (137)	.269 (.058)
	[0.25, 0.50)	6,690	3,313 (156)	403 (110)	.121 (.040)	QU_3	3,328	2,621 (117)	405 (72)	.166 (.030)	[250, 2,500)	6,698	4,079 (179)	434 (102)	.119 (.031)
	(0, 0.25)	10,266	4,262 (121)	247 (102)	.073 (.030)	QU_4	6,181	3,452 (133)	437 (103)	.144 (.035)	[2,500, 25,000)	9,951	6,488 (290)	505 (175)	.096 (.039)
<i>More liquidity</i>	= 0	4,834	3,813 (182)	275 (69)	.073 (.026)	QU_5	23,252	6,615 (286)	385 (309)	.062 (.054)	25,000+	15,233	10,899 (407)	204 (484)	.041 (.067)

Note. In this table, *L* and *D* stand for total credit line limit and total credit line debt across all banks. Estimates are obtained from the nested dynamic specification Equation (4), using data on 45,307 participants for the experimental timeframe of $T=3$ quarters, totaling $N \times T = 135,921$ individual-quarter observations. Robust standard errors are corrected for clustering at the individual level. FS_{3q}, ITT_{3q}, and MP_{3q} stand for the 3-quarter cumulative first-stage, intent-to-treat, and marginal propensity estimates. First-stage and intent-to-treat estimates use OLS, and marginal propensity estimates use 2SLS. Cumulative effects calculated as $\Phi_\tau = \sum_{j=1}^{\tau} \phi_j$. Participants are sorted by values in the quarter before the experiment. In each panel, the first column, *L - D*, displays the average baseline available credit.

The adjacent bin, those using between 25% and 50% of total credit lines, accumulate 12.1 (4) cents of debt on the dollar. This group also has substantial available credit relative to the limit increase they receive. The null hypothesis that the response is zero for these low utilization groups, as well as the null that the difference in response of .43 (.07) is equal between the top and bottom utilization groups is decisively rejected.

To isolate situational factors from fixed and persistent spurious effects, Panel A2 in Table 6 splits the participants into four groups based on pre-experiment utilization versus typical utilization (average in quarters -12 to -5 relative to the onset). For participants utilizing more than half of their credit line, the estimated response is between 40 to 45 cents irrespective of their long-run utilization levels. Therefore, current distance-to-limit is a first-order determinant of the sensitivity to credit. In contrast, focusing on cardholders utilizing less than half of their credit lines, the estimated response is the sensitivity to credit is related to the long-run utilization levels. Panel A3 in Table 6 corroborates these findings, showing a tight link between the estimated sensitivity to credit and the number of times constraints bind.

The heterogeneity of the response by credit line utilization is directionally compatible with models featuring simple utilization-targeting heuristics. The response is larger for customers with higher utilization with this normalized measure of the distance to limit at the onset a very strong predictor the magnitude of the response. However, the strict form of this hypothesis leads to several testable predictions rejected in the data. First, the average 3-quarter response of .162 (.012) is significantly smaller than the average baseline utilization of .34. Second, participants in the treatment group reduce their average utilization rate, by .041 (.004) after 3-quarters, but also at all baseline utilization levels, as displayed and discussed later on in Section 3.4 and Figure 13. Finally, the heterogeneity of the estimated effect by baseline utilization is significantly flatter than the 45-degree line. Participants at their constraints reduce their utilization, while those not utilizing at the onset do respond. Each of these implications is decisively rejected with $p < .001$.

The commonly used credit line utilization metric correctly classifies a high-income and high-utilization individual as constrained, but a low-income and low-utilization individual as liquid, despite potential inability of the latter to cope with an expense or financial disruption (car repair, appliance replacement, or medical bill) of a moderate nominal amount. To address this issue, Panel B in Table 5 examines the heterogeneity of the response by quintiles of unused available credit in levels.

Similar to the results based on credit line utilization, participants who have lower unused available credit exhibit higher marginal propensities. For example, the estimated treatment effect for the first quintile is 29.8 (2.2) cents, which drops to 14.5 (3.5) for the fourth quintile. The fourth quintile, on average, had available credit of 6,181 TRY, receives credit line increases of 3,452 TRY, and borrows 437 (103) TRY. This group has substantial ability to borrow, as for 96% of this group (including the control group) the debt levels by the end of the experimental timeframe were feasible given unused credit at the onset; and 85% of the participants in this group, had the additional limits they receive as available credit.

For participants in the fifth quintile, with an average available credit of about 25,000 TRY or about 15 months of median net income, the estimated effect is 6.2 (5.4) cents ($p=0.25$). This group displays a statistically significant borrowing response of 10.3 (2.8) on bank credit lines that is offset by a decrease in balances on other bank cards—which could reflect a decrease in balances carried across pay periods or a decrease in float spending reported as a liability at that point in time.^{25,26}

²⁵Panel A1 in Table 6 reports the heterogeneity of the estimated treatment effects by income quartiles, which range between 16.6 (3.9) cents and 6.2 (3.0) cents. In unreported results, I do not detect a meaningful relationship between the estimated treatment effects and past or future income growth over various horizons; hence there is no evidence that borrowing presages high income growth.

²⁶Panel A4 in Table 6 splits the participants into four groups based on credit line utilization (debt-to-limit) at the onset versus available credit (limit minus debt) at the onset. For low utilization participants, the effects extend beyond those with

Finally, Panel C in Table 5 measures the heterogeneity of the response by liquid assets. These coarse categories are based on total liquid assets at the bank, including the financial assets at the in-house brokerage in liquid form. Consistent with what is commonly used in the literature (e.g., Kaplan et al. (2014)), this includes checking and savings accounts, and holdings of stocks, bonds, and funds; excludes housing, illiquid retirement accounts, and life insurance policies, see Appendix D. Participants with no active accounts with assets (32%) are placed in the first bin. Those with assets are grouped based on averages in the two quarters before the experiment cut at 250 TRY, 2,500 TRY and 25,000 TRY, roughly corresponding to about one-tenth, one, and 10 times the average labor income. 11% and 4% of the participants are in the latter two bins.

Similar to above, Panel C in Table 5 points to a negative relationship between the propensity to borrow and liquid asset holdings. For example, participants with an account but next to no assets spend 26.9 (5.8) cents with a 95% confidence interval of .16 and .38. For the adjacent group, with an average level of assets between 250 TRY and 2,500 TRY, the marginal propensity drops to 11.9 (3.1) cents. Although participants in these two groups could potentially tap into their available credit to pull spending forward, they have no meaningful buffer of liquid assets. Moreover, the wedge between the return on liquid assets and the interest on unsecured credit may create a kink in the budget constraint. Hence, some definitions would also categorize these participants as constrained. Similar to participants up their limits, however, strict hand-to-mouth behavior for these two groups is also decisively rejected.

The effect of credit, however, extends even to those with a meaningful buffer of assets. Participants in the next group, those holding between 2,500 TRY and 25,000 TRY—about \$1,000 to \$10,000—in liquid assets, spend 9.6 (3.9) cents out of the credit limit increase. The increase in credit line debt for this subgroup is 505 (175) TRY. In comparison, their average available credit at the onset is about 10,000 TRY. Since these participants also had at least 2,500 TRY of liquid assets, they likely had the necessary disposable resources in both available credit and liquid assets to finance the new marginal increase in debt. In fact, only 6% of these participants, including the control group, borrow beyond the limit available to them at the onset. Moreover, roughly 30% of this group carry revolving balances at 24% APR, implying they could have repaid some of this debt using liquid assets. Therefore, not only do participants simultaneously hold liquid assets and credit line debt, but also participants who carry a buffer of liquid assets exhibit statistically significant propensities to spend out of additional credit.

For the 4% of participants in bin (5) who have more than 25,000 TRY of total assets, the null that credit has no effect on their borrowing and spending behavior is not rejected ($p=.53$). The estimated effect for this group is 4.1 (6.7) cents. This group also does not display a statistically significant borrowing response on bank credit lines, with a response of 6.2 (6.2) cents. The loss of precision might also reflect the small sample size in this bin. This group, on average, had about four times the disposable resources at the onset relative to the limit increase they get over the experiment. However, 9% of this group face a binding constraint in the 3 years after the onset. The latter number is about 45% for the second bin and 21% for the fourth bin. I nevertheless reject the null hypothesis that the response is equal between the second bin and the bottom bin ($p=.016$).

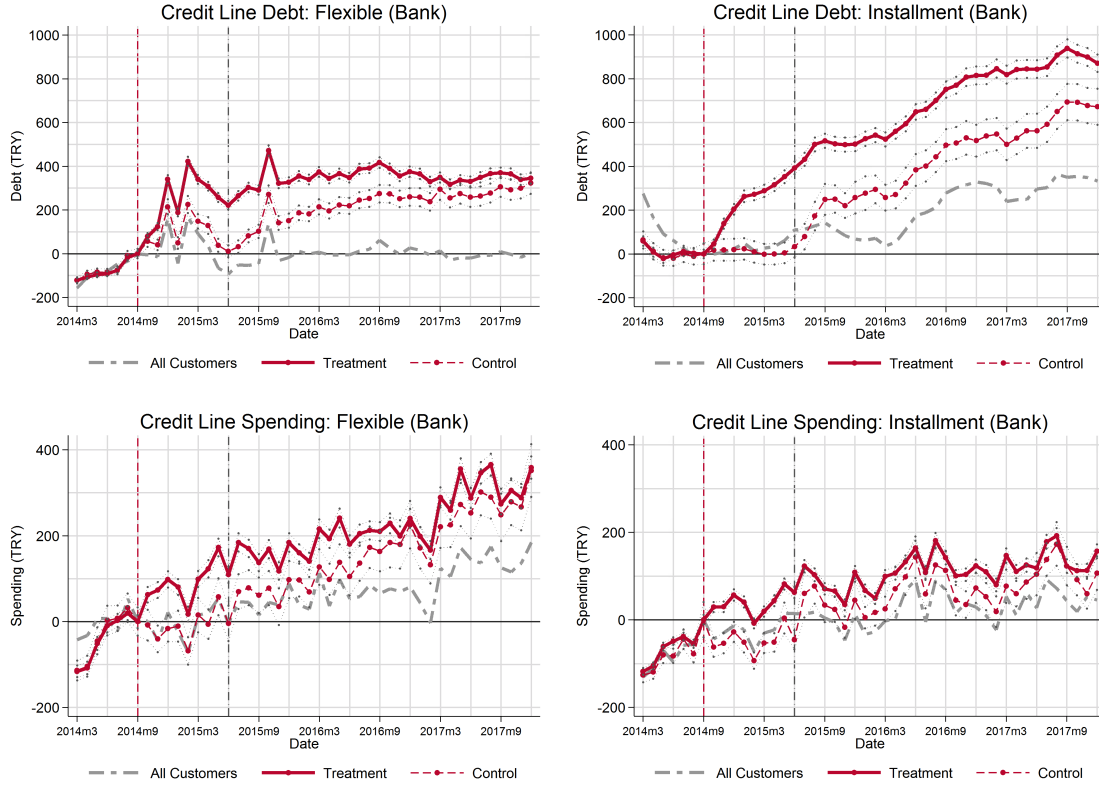
a very low available credit, i.e., 1,500 TRY, which correspond to the 25th percentile of unused limits or the median post-tax labor income. On the other hand, for high utilization participants, the effect is larger for participants with a *higher* available credit in levels, indicating that having very low available credit is not as good a predictor of the propensity to spend out of arrival as high demand out of existing supply of credit.

Table 6:
Borrowing on Credit Lines: Heterogeneity—Additional Splits

Panel A: Additional Heterogeneity										Panel B: Heterogeneity and Composition												
Panel A1: Income Quartiles			Panel A2: Times Binding Past 12q			Panel A3: Curr. vs. Typic. Utilization				Panel A4: Utilization vs Available Credit				Panel B1: Credit Line Utilization				Panel B2: Baseline Contract Preference				
ITT _{3q}		MP _{3q}	ITT _{3q}		MP _{3q}	ITT _{3q}		MP _{3q}	ITT _{3q}		MP _{3q}	MP _{3q}		MP _{3q}	MP _{3q}		MP _{3q}	MP _{3q}				
ΔD	ΔD		ΔD	ΔD		Curr.	Typic.	ΔD	ΔD	Util.	Avail. Credit	ΔD	ΔD	D / L	ΔD	ΔD	Flex Share	Flex?	Inst?	ΔD	ΔD	Flex Share
(TRY)			(TRY)					(TRY)			(TRY)	(TRY)		(All Banks)	(Inst, Bank)	(Flex, Bank)	(Bank)	(Bank)	(Bank)	(Inst, Bank)	(Flex, Bank)	(Bank)
Q ₁	319 (71)	.166 (.039)	6+	1,012 (268)	.572 (.148)	[0.50, 1]	[0.25, 1]	1,159 (226)	.454 (.083)	[0.50, 1]	[0, 1,500]	779 (53)	.327 (.026)	[0.75, 1]	.280 (.038)	.193 (.032)	.41	×	×	.022 (.033)	.072 (.036)	.77
Q ₂	321 (124)	.124 (.049)	3 to 6	1,174 (173)	.451 (.063)		[0, 0.25]	997 (259)	.402 (.117)		1,500+	1,550 (417)	.515 (.128)	[0.50, 0.75]	.203 (.025)	.160 (.022)	.44	×	✓	.091 (.019)	.014 (.008)	.13
Q ₃	476 (107)	.128 (.035)	1 or 2	630 (143)	.256 (.058)	[0, 0.5]	[0.25, 1]	367 (137)	.120 (.050)	[0, 0.5]	[0, 1,500]	514 (41)	.218 (.023)	[0.25, 0.50]	.099 (.020)	.071 (.014)	.42	✓	✓	.128 (.014)	.093 (.010)	.42
Q ₄	577 (230)	.062 (.030)	= 0	297 (92)	.073 (.027)		[0, 0.25]	270 (79)	.071 (.023)		1,500+	272 (80)	.072 (.024)	(0, 0.25)	.078 (.018)	.024 (.008)	.24	✓	×	.176 (.031)	.121 (.037)	.41
														= 0	.059 (.018)	.012 (.012)	.17					

Note. Estimates are obtained from the nested dynamic specification Equation (4), using data on 45,307 participants for the experimental timeframe of $T=3$ quarters, totaling $N \times T = 135,921$ individual-quarter observations. Robust standard errors are corrected for clustering at the individual level. ITT_{3q} and MP_{3q} stand for the 3-quarter cumulative intent-to-treat and marginal propensity estimates. Intent-to-treat estimates use OLS, and marginal propensity estimates use 2SLS. Cumulative effects calculated as $\Phi_\tau = \sum_{j=1}^{\tau} \phi_j$. *Flex* and *Inst* indicate whether a participant is holding flexible debt or installment debt on bank credit lines in the quarter before the experiment. In Panels A1, A4, B1, and B2 participants are sorted by their values in the quarter before the experiment. In Panel A2, participants are sorted by the number of times they face a binding constraint in the 12 quarters prior to the experiment. In Panel A3, typical utilization is the average in quarters -12 to -5 relative to the onset.

Figure 9: Event Study: Contract Choice and Spending Patterns



Note. Figures plot the nominal levels of covariates for treatment ($Z_i = 1$) and control ($Z_i = 0$) groups by calendar month. The y-axis is normalized to have levels equal to zero at the onset of the experiment in September 2014.

3.3 Contract Choice and Spending Patterns

The analysis and discussion based on total borrowing behavior remains silent regarding how borrowers pull consumption forward and why behavior is sensitive to credit. To better understand these issues, I first study in detail how contracts arbitrate spending through borrowing, and how the additional credit is spent.

Contract choice. The event studies displayed in Figure 9 decompose the choice of debt contracts. These figures use data from the bank, which is available on a monthly basis, allowing for higher-frequency analysis.²⁷

The top left figure displays *flexible* debt or conventional revolving borrowing that represents unpaid end-of-billing-cycle balances accrued as a result of open-ended transactions. Borrowers accumulate flexible debt through dynamic choice, deciding after seeing the balances whether to carry across pay periods or pay off in full. In contrast, *installment* debt, displayed top right, is incurred at the time of purchase to finance in-store expenditures, and then paid down according to a preplanned schedule with a fixed nominal payment every month. Total credit line debt carried across pay periods at bank equal to the sum of the installment and flexible components. Similarly, the bottom figures display flexible and installment spending, where total credit line spending at bank equal to the sum of the installment and flexible components. The change in the stock of flexible and installment debt carried across pay periods relative to the onset (displayed at the top) is tied to net flexible and installment spending (displayed at the bottom) over the same period, after

²⁷Credit bureau data only reports total liabilities for credit lines at other banks, and does not allow for a disaggregation of flexible debt, revolving debt, and float spending that is not carried across pay periods. However, after 12 quarters, only 5% of the change in credit line debt at the bank cards (14 TRY) is offset on liabilities on credit lines at other banks, and this effect is not statistically significant ($p=.84$), see Table A.7.

Table 7: Borrowing on Credit Lines: Contract Choice

	Baseline Level	Intent-to-Treat							Marginal Propensity (Monthly)				
		1m	2m	1q	3q	8q	12q	13q	Point-in-time	$\Sigma_{j=2m}^{3m}$	$\Sigma_{j=4m}^{9m}$	Cumulative	
ΔD Flex. (TRY) (Bank)	358	23 (11)	84 (12)	126 (18)	211 (17)	143 (25)	64 (27)	22 (28)	.017 (.008)	.032 (.011)	.003 (.016)	.049 (.012)	.052 (.012)
ΔD Inst. (TRY) (Bank)	907	28 (14)	119 (20)	185 (23)	360 (31)	255 (39)	245 (47)	198 (47)	.019 (.010)	.052 (.010)	.034 (.017)	.072 (.012)	.105 (.012)
Flex Share (Bank)		.45	.41	.41	.37	.36	.21	.10	.47	.38	.08	.40	.33

Note. Intent-to-treat estimates from Equation (2) use data on $N = 45,307$ participants. Marginal propensity estimates from Equation (3) use data on $N \times T = 407,763$ participant-month observations, using as the right-hand-side variable the change in credit limits at bank, where robust standard errors are corrected for clustering at the individual level. Intent-to-treat estimates use OLS, and marginal propensity estimates use 2SLS. Flex share is defined as the ratio of flexible borrowing to the sum of flexible and installment borrowing.

factoring in payments made toward balances.

The dynamics of contract choice displayed in the event studies in Figure 9 point to several discernible patterns. The immediate response in the month limits are increased is relatively modest, and over the course of short-run experimental timeframe flexible contracts are used in tandem and in similar proportions to preplanned contracts. For example, after the first month the new limits are available, the intent-to-treat difference in flexible and installment debt, displayed in Table 7 is only 23 (11) TRY and 28 (14) TRY, corresponding to .9% and 1.1% of average monthly net income of the participants. Over the 3-quarter experimental timeframe, the intent-to-treat difference in flexible and installment borrowing at the bank is 211 (17) TRY and 360 (31) TRY with flexible debt accounting for 37% of the additional borrowing.

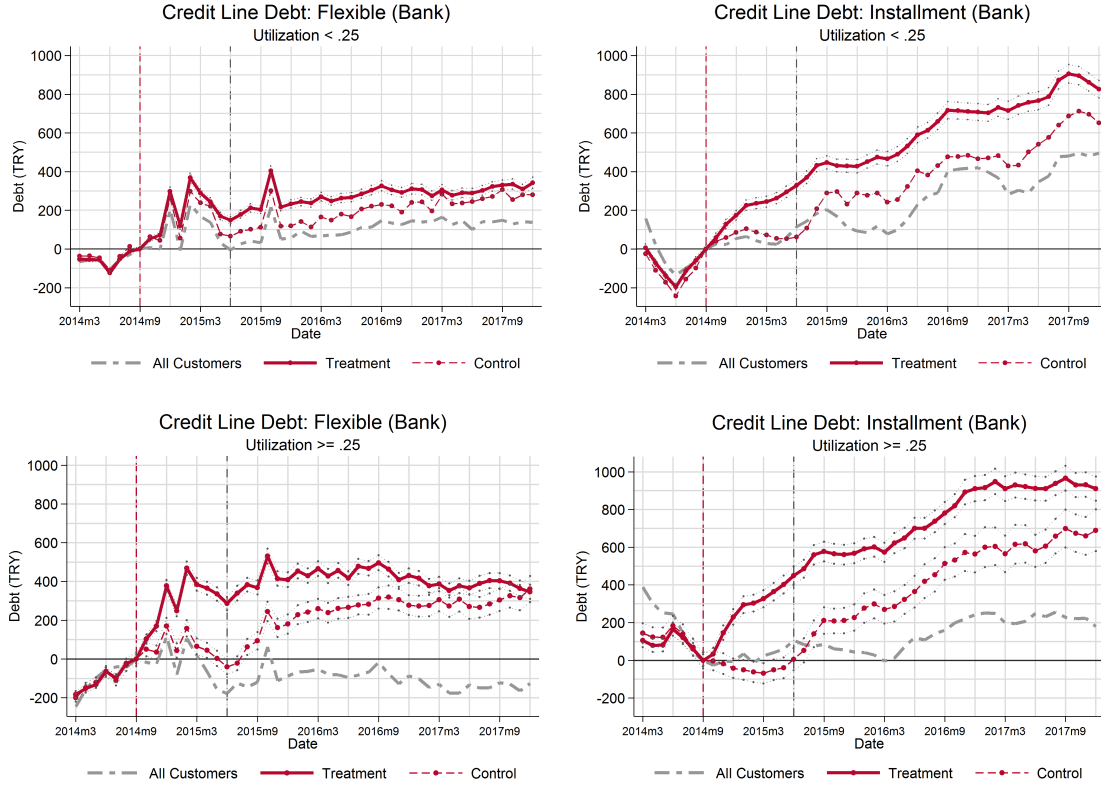
Monthly dynamic marginal propensity estimates obtained using Equation (3), which use as the right-hand-side variable the change in credit limits at the bank, is reported in Table 7. These estimates decompose, in higher frequency, the immediate effects in the month of the limit increase versus the delayed effects that occur in the subsequent months. These estimates point to a similarly modest point-in-time response of 1.7 (.8) and 1.9 (1) cents in the month limits are received for flexible and installment borrowing, and an additional 3.2 (1.1) and 5.2 (1) cents in the second and third months. After the third month, flexible borrowing comes to a pause ($p=.87$). The second and the third quarters subsequent to the limit increase bring an additional 3.4 (1.7) cents of installment debt. The 9-month marginal propensity to borrow is 5.2 (1.1) and 10.5 (1.2) cents, respectively, with flexible debt accounting for 33% of the additional borrowing.

In the long run, the difference in flexible debt between the treatment and control group attenuates, with preplanned installment debt accounting for the predominant share of the difference in debt levels. Focusing on installment spending and debt displayed on the right in Figure 9 participants in the treatment group, compared with those in the control group, spend more both throughout the experimental timeframe as well as the subsequent follow-up period, stacking concurrent loans, creating new debts beyond matching installments due.²⁸ Nevertheless, the net difference in debt levels between the treatment and control groups is stable over the follow-up period, with no discernible attenuating pattern for the differences in installment debt between the treatment and control groups.

Focusing on flexible debt and spending on the left in Figure 9 the treatment group similarly spends more throughout the short-run experimental timeframe, as well as the subsequent follow-up period. In the short-run, the difference in debt increases sharply, indicating an increase in payments by the treatment that is short of the increase in spending. The nominal level of flexible debt

²⁸This pattern is reminiscent of the *saving down* behavior discussed in Rutherford (2000) and Bauer et al. (2012).

Figure 10: Event Study: Contract Choice by Distance-to-limit



Note. Figures plot the nominal levels of covariates for treatment ($Z_i = 1$) and control ($Z_i = 0$) groups by calendar month. The y-axis is normalized to have levels equal to zero at the onset of the experiment in September 2014. Top (bottom) Figure focuses on participants with a baseline utilization rate of less than (greater than or equal to) .25.

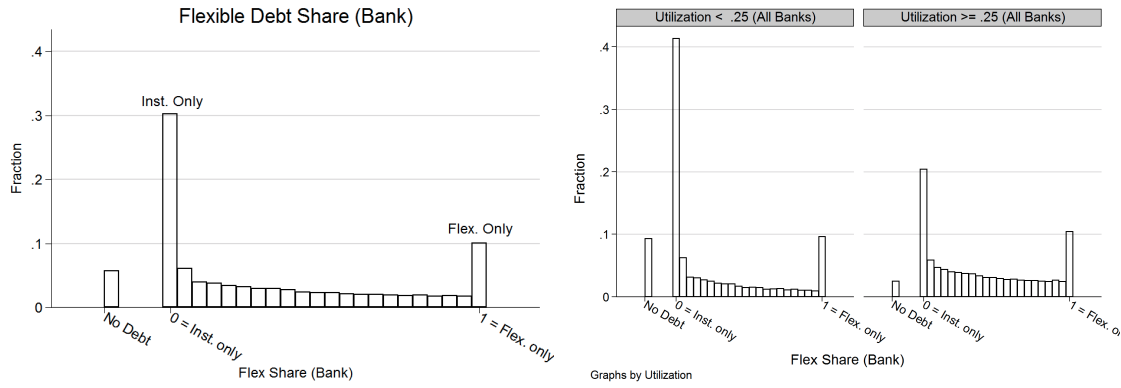
levels peak after about 8 quarters, and higher spending later on is not associated with an increase in flexible debt levels for the treatment group. In the long-run, the differences in flexible debt decreases, and it follows that payments by the treatment group (relative to control) increase to an extent as to offset the increased spending. In the last quarter of the follow-up, flexible debt accumulated through dynamic choice accounts for only 10% of the borrowing response at the bank credit lines, and the difference of 22 (28) TRY in flexible debt between the treatment and control groups is not statistically significant ($p=.43$).²⁹

In order to analyze the heterogeneity in contract choice, Figure 10 displays the contract choice event studies separately for high and low distance-to-limit groups. These event studies corroborate the qualitative attenuating long-run flexible debt dynamics, and provide evidence of a negative relationship between distance-to-limit and the use of flexible contracts. Similarly, Panel B1 in Table 6 estimates heterogeneous treatment effects using the nested specification Equation (4). The predominant component of the increase in borrowing by the participants that are far from the limit is not through dynamic choice but using installment type debt contracts with preplanned repayment schedules. For example, for participants who do not have any debt at the onset, flexible debt accounts only for 17% of the response. This ratio levels between 41% to 44% for participants closer to their constraints.

Much of the heterogeneity in the composition of the response is associated with the baseline contract share. Figure 11 shows that there exists a mass of participants (about 30%) holding only

²⁹Figure A.3 displays these event studies in real terms, deflated using the implicit price deflator for personal consumption expenditures. Estimating the intent-to-treat and treatment effect specifications in real vs nominal terms do not lead to a meaningful difference in the results.

Figure 11: Baseline Contract Share



Note. Figure displays the histogram of flexible debt share—the ratio of flexible debt at bank credit lines to the sum of flexible and installment debt at bank credit lines—in the quarter before the experiment. Three mass points stand for participants with no debt, participants with only installment debt, and participants with only flexible debt.

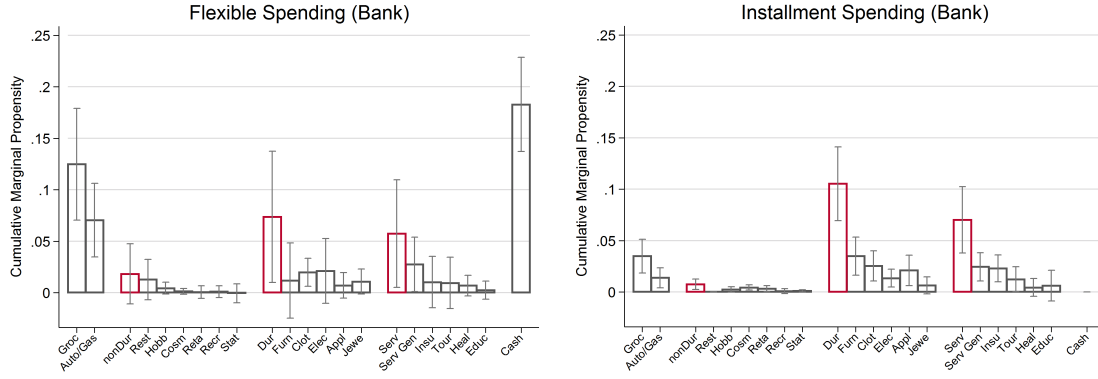
installment debt, and participants who are far from the constraint are more likely to only hold installment debt. Moreover, Panel B2 in Table 6 shows that this baseline preplanned contract share has strong predictive power for the magnitude and the composition of the response. For the mass of participants who only hold installment debt, the flexible debt share is 13%, and the flexible debt response is not statistically significant. For participants who hold flexible debt, the flexible debt share is about 40%, irrespective of whether they hold installment debt or not.

The voluntary choice of preplanned installment contracts over flexible ones is broadly compatible with several prominent explanations. First is a simple pecking-order argument, as installment debt is often cheaper. Hence, the dynamics of contract choice may reflect a temporary deviation from the optimal leverage target, in which borrowers use relatively expensive flexible debt in the short run and converge to their target in the long run. Second, this choice could also reflect differences between these contracts regarding flexibility. In contrast to flexible debt accumulated through dynamic choice, installment contracts are one-time preplanned arrangements. Despite not being *pure* or *hard* commitments³⁰, installment contracts preclude the possibility of dynamic revisions of the repayment plans made initially. Hence, borrowers who have a time-inconsistent taste for immediate gratification in *repayment* a la Heidhues and Kőszegi (2010), but manage the repayment process with a degree of sophistication, may use preplanned installment contracts as a *meaningfully binding* commitment device to prevent overborrowing a naif that underestimates the extent of this taste might do. Finally, it is also plausible that precautionary behavior leads to a positive relationship between distance-to-limit and the use of preplanned contracts. Unfortunately, the data does not contain interest charges for installment debt—to test the pecking order theory—or repayment plans for flexible borrowing—to investigate whether flexible borrowing represents dynamically inconsistent revisions of these initial plans that occur more often than borrowers predict or prefer, or whether the participants potentially learn and anticipate their dynamic inconsistencies, where earlier present bias predicts later contract choice.³¹

³⁰Unlike a *pure* commitment, installment contracts are not strictly dominated in terms of costs. Instead, they weakly dominate flexible debt, as there could be interest-rate reductions. Moreover, although it is not possible to accumulate installment debt as a consequence of a dynamically inconsistent revision of a contingent plan, borrowers do not entirely forgo repayment flexibility. Due to a simple arbitrage argument, borrowers with available credit can always unexpectedly change their behavior at will and revolve the seemingly cheap installment payments due, accompanied by a 24% APR interest rate for falling behind schedule. Nevertheless, installment contracts may, in effect, make future revisions relatively more expensive and altering behavior to pay back debt on time; but also lock in certain consumption and shrink future flexibility. See Ashraf et al. (2006) and Kaur et al. (2015) for examples of pure commitment contracts; and Bryan et al. (2010) for a discussion of commitment contracts.

³¹A unified theory of contract choice needs to explain the short-run heterogeneity, long-run dynamics, and the baseline contract share. However, the theoretical relationship between consumer credit constraints and a pecking order, or a preference for commitment/flexibility, remain largely unexplored. The latter is partly due to the difficulty in characterizing the

Figure 12: Spending Patterns



Note. Figure reports the categorical 3-quarter cumulative marginal propensity to spend on bank cards. Estimates obtained using Equation (3) on a sample of $N = 45,307$ participants, focusing on the experimental timeframe of $T = 3$ quarters, using as the right-hand-side variable the change in credit limits at bank. Red bars correspond to the total increase in spending on three main subcategories—nondurables, durables, and services. The upper and lower shadows indicate 99.8% confidence intervals for the estimate of the mean, to account for Bonferroni correction to handle many outcomes, and clustering at the individual level.

Spending patterns. Figure 12 displays the spending patterns by category. These estimates are obtained from Equation (3) and report the cumulative 3-quarter marginal propensity to spend. Since spending is a flow variable linked to the change in debt via an accounting identity, it is analyzed in levels. This Figure then displays the estimates separately for flexible and installment spending and each category within. I separate cash advances and transactions for groceries and auto/gas, and compartmentalize the remaining transactions into three groups: nondurables, durables, and services.³² The corresponding sums are displayed using red bars. The upper and lower shadows indicate 99.8% confidence intervals for the estimate of the mean, to account for Bonferroni correction to handle many outcomes.

Note that the results on flexible spending patterns displayed on the left in Figure 12 are subject to potential limitations and require careful interpretation. Credit lines are used as a means of payment and for transactional liquidity, with a float benefit of about 2% per dollar spent.³³ For borrowers who do not carry balances across pay periods, these transactions are not associated with an increase in flexible debt carried across pay periods but entirely pulled forward from the end of the month. Although, on average, the increase in flexible debt is associated with a much larger increase in flexible spending, it is not possible to know which of these flexible transactions lead to the additional flexible borrowing, or in what way these transactions may be crowded out from transactions in cash.

The panel on the left displays the gross flexible spending by category. Over the 3-quarter experimental timeframe, participants in the treatment group, relative to the control and on average, increase their flexible debt by 5.2 (1.2) cents. This debt is accumulated by making flexible transactions worth 34 cents and taking out cash advances worth 18 cents. Detailed analysis of this 34 cents' worth of spending into sectoral categories shows that 36% of the total increase in flexible

equilibria for time-inconsistent consumption/repayment models with credit constraints. Hence, theory offers little guidance on how contract choice and the ability to commit to repayment plans are complicated by factors such as liquidity and precautionary behavior, see and Bernheim et al. (2015). See DeAngelo et al. (2011) for a model of corporate capital structure dynamics under a pecking order predicting the issuance of transitory debt.

³²See Table A.12 for more details on these categories.

³³For example, participants on average spend 628 TRY in flexible form, and revolve 358 TRY using flexible debt in the quarter before the experiment. The interest cost comes to about $358 \text{ TRY} \times 2\% = 7.2 \text{ TRY}$ per month at fixed 24% APR on the flexible debt. However, flexible spending also comes with a float benefit of about $628 \text{ TRY} \times 2\% \times \frac{1}{2} = 6.3 \text{ TRY}$ per month, where the $\frac{1}{2}$ assumes purchases are uniformly placed in between billing cycles. Focusing on flexible debt, participants in the treatment group pay 27 TRY of additional interest expense over the experimental timeframe of 9 months on the additional flexible debt, with a float benefit of 11 TRY. This calculation excludes points earned or other perks.

transactions is directed toward groceries, a category in which expenditures and consumption are relatively more tightly linked.³⁴ The remaining flexible spending is about evenly accounted for by increases in vehicle expenses, durables, and services. Vehicle spending primarily consists of gas, but also includes parts and repairs. For nontradables that affect demand within their region, such as restaurants and recreation (e.g., entertainment, sports activities, and theatre) and discretionary nondurables (e.g., cosmetics, hobbies) the response is small and indistinguishable from zero. Focusing on services, the effect is mostly driven by a statistically significant response in general services, which primarily consists of utility bills.

The panel on the right decomposes installment spending. In contrast to flexible spending, spending in installment form is always associated with installment debt carried across pay periods. Over the 3-quarter experimental timeframe, participants in the treatment group, relative to the control and on average, increase their installment debt by 10.5 (1.2) cents. This debt is accumulated by making installment transactions worth 23 cents. The composition of installment spending is much more skewed toward durables, as well as a discernible increase in services spending, both associated with future consumption. The durables response, which accounts for 45% of the total increase in installment transactions, is primarily driven by consumer durables and semi-durables associated with extensive margin adjustments, most notably furniture, clothing, and appliances. The services response, which accounts for 30% of the total increase in installment transactions, is directed to general services, insurance, and tourism. In Table A.8, I show that only a negligible share of these installment purchases are made in lumpy increments. By law, installment contracts cannot be used to finance food and gas. The modest installment expenditures under groceries and auto/gas reflect the purchases of small durables (toasters, tires, spare parts) from grocery stores or auto shops/gas stations.³⁵

Figure A.5 further decomposes the contract choice event studies and spending patterns estimates by distance-to-limit. The (gross) spending response is higher across all categories for both flexible and installment spending for participants closer to the limit. For installment spending, the fraction directed to each category is alike for those who are close or far from the limit, concentrated in durables and services. For flexible spending, participants closer to their limits are much more likely to take out cash advances, direct a larger share of spending to durables and eating out, but direct a lesser share to services and auto/gas.

3.4 Dynamics of Binding Constraints and Precautionary Behavior

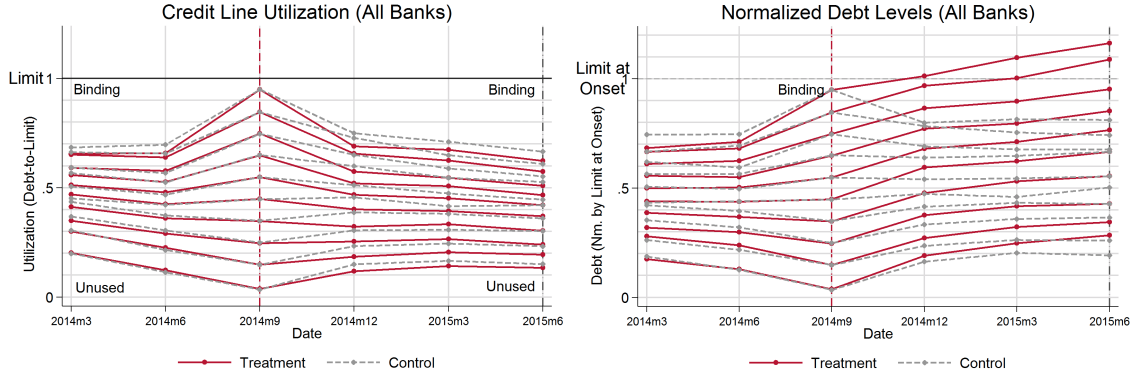
I now turn to participants facing a binding constraint and use the sharp counterfactual to analyze the dynamic interaction of constraints with precautionary saving—the most frequently advanced explanation as to why consumption is sensitive broadly to credit expansions. The smoking gun for precautionary behavior, the importance of which is ultimately difficult to disentangle, is a tendency to put off spending and build up a buffer near the credit limit.

To empirically illustrate this idea, I study the debt dynamics for treatment and control groups at different distance-to-limit levels using event studies in Figure 13. In these figures, the x -axis indicates the calendar date. Participants are sorted into ten nonoverlapping equal-width bins with respect to their total credit line utilization at the onset: 0 to .1; .1 to .2, and so on. The panel on the left then plots credit line utilization—the ratio of credit line debt to the credit limit, L_{it} —which

³⁴ Additional spending on groceries represent a 26% increase, and the additional cash advances represent a 56% increase compared with the control group over the experimental timeframe.

³⁵For many of the expenditures on nonperishable durables and services that yield benefits over time, consumption and expenditure are less tightly linked, a point made by Hayashi (1985). Hence, the sensitivity to credit could partly be accounted for by the durability of the goods. However, it is also likely prevalent that impatient or present-biased participants make spending in categories such as durables (e.g., refrigerators) or necessities (e.g., groceries), and the composition of expenditures is not informative as to whether an individual is myopic. The ratio of cumulative installment spent (23 cents) to borrowed (10.5) over 9-months is compatible with a back of the envelope calculation where borrowers take 4-month installment loans.

Figure 13: Dynamics of Binding Constraints and Precautionary Behavior



Note. In these figures the x -axis indicates the calendar date. The dashed and dash-dot lines denote the start and end dates of the experiment. Participants are sorted at the onset into ten nonoverlapping equal-width bins with respect to their total credit line utilization. The panel on the left then plots credit line utilization—the ratio of credit line debt to the credit limit, L_{it} . The panel on the right plots a normalized measures of debt levels—the ratio of credit line debt to the credit limit at the onset, $L_{it=0}$. The measures overlap at the onset.

measures the (normalized) distance-to-limit at a given date. The panel on the right plots a normalized measure that allows me to compare the debt levels for the treatment before and after the limit increase—the ratio of credit line debt to the credit limit at the onset, $L_{it=0}$. These measures overlap at the onset. For the participants who did not experience a limit increase in the three quarters before the experiment, these measures also overlap in the Figure before the onset.

The first discernible feature in the naturally occurring borrowing behavior of the control group is mean-reverting debt dynamics. For example, participants with a binding constraint at the onset spend very little time at the limit. Instead, they tend to quickly save their way out of strict constraints and build a buffer of available credit by reducing their utilization (left) and debt levels (right), and do so after only one quarter. These participants were also utilizing only about two-thirds of their limits in the quarter prior. Therefore, binding credit constraints appear to be a transitory event. Surprisingly, those who persistently remain at the credit limit—hence could not finance present purchases using resources that will accrue to him in the future—constitute a sliver of the population.³⁶

Second, comparing the behavior of the treatment group with that of the control group, the Figures show that the effect of a credit expansion on participants who are far from the limit is due to the treatment group increasing the pace at which they borrow and raising their debt levels (right), meanwhile reducing their utilization and moving further away from the limit (left). Strikingly, the estimated high $MPC^{\Delta L}$ for participants at their constraints is partly due to the constrained control group reducing their debt levels and delevering when limits are tight. When in the treatment group, participants up against their limits borrow and lever up (right), meanwhile reducing their utilization, moving further away from the limit and leaving a buffer (left). Under the counterfactual behavior that would have prevailed under the old limit, the precautionary saving motive outweighs, and these borrowers enter the process of building up their buffer stock of savings by deaccumulating debt. The main mechanism that determines high $MPC^{\Delta L}$ levels and consump-

³⁶Analysis of the utilization transition matrix in Table A.9 corroborates these findings. For example, of the 11% (4,786) of participants utilize more than 75% of their existing credit lines in the quarter before the experiment only 28% (1,344) of this group was in a similar position 1 year prior and only 13% (644) 3 years prior. Hence, over a 3-year horizon, no more than 1.4% of the subjects remain persistently stuck near constraints, as a hand-to-mouth or rule-of-thumb individual who consume all of their disposable resources would. Despite having no available credit, participants with binding constraints do not appear to borrow as much as possible and push themselves to a strict corner solution to their intertemporal problem but rather converge to interior utilization rates. These utilization rates also appear to be persistent, see Fulford and Schuh (2019). These patterns also hold for the universe of active cardholders, as well as alternative measures of buffers, such as available credit to income ratio or log available credit.

tion dynamics around binding constraints appear to be a precautionary savings effect by which constrained households depress spending and delever under tight constraints.

4 Conclusions

Using a randomized controlled experiment design, I studied the magnitude, heterogeneity, composition, and dynamics of the consumption response to an expansion of credit. Using the experimental assignment as an instrument, I find a precisely measured and economically meaningful effect of a pure shock to the limit—11 cents in the quarter of the limit increase, with a cumulative difference of 28 cents by the third year. The effects extend beyond a small set of participants who hold no assets or face binding constraints. $MPC^{\Delta L}$ is heterogeneous, negatively related to current liquidity, and positively related to the frequency with which constraints bind in the long-run. Borrowers near their constraints spend out of credit availability when limits are relaxed but delever and save their way out of constraints when limits are tight. The key features of the reduced form findings provide strong support for an explanation that embraces a precautionary saving, as in Deaton (1991), Carroll (1997), and Guerrieri and Lorenzoni (2017), in which the defining tension appears to be a desire to shift consumption forward in time versus the desire to create a buffer.³⁷

The findings raise several questions for further research. One avenue is to directly test the relationship between the MPC and $MPC^{\Delta L}$. From a policymaking perspective, understanding this relationship would provide important insights into the relative efficacy and applicability of low-cost credit instruments (e.g., *stimulus lines*) to help households weather recessions or to stimulate aggregate demand.³⁸ From a theoretical perspective, this relationship could be useful to test between models of consumption behavior. Equation (1) makes a direct prediction on how borrowers would spend out of such credit lines, which should be similar to a one-time transfer if the interest rate on these lines is low.

A second avenue pertains to the modeling and design of the contractual features of credit markets. The small-dollar installment credit contracts studied here were quite common in the U.S.—and accounted for much of the expansion in household debt—during the 1920s. However, they largely disappeared with the Great Depression. On the empirical front, little is known as to whether preplanned installment contracts are a device to compensate for self-control difficulties and prevent overborrowing by those who manage the repayment process with a degree of sophistication, or lead to plans and subsequent repayment that differs from the counterfactual flexible repayment that would prevail in the absence of installment contracts. This is perhaps surprising given the salient pattern in credit markets with flexible contracts in which borrowers carry large balances for extended periods, potentially due to present bias leading to dynamic inconsistencies in debt repayment.³⁹

Finally, several behavioral explanations that have not been put to empirical scrutiny could be important contributing factors to the consumption response documented here, such as those based on *cue-triggered* consumption. An environmental cue such as the limit increase could directly raise the marginal utility derived from consumption, which would lead to a mechanical and spontaneous increase in spending. Moreover, repeated past pairings of consumption with the cue could

³⁷In line with this interpretation, naturally occurring variation provides suggestive evidence in favor of the first-order model implication that those facing greater income risk desire larger buffers. Table A.5 shows that participants with high income risk have larger credit limits and hold higher available credit. Naturally, the buffers' level in nominal terms is strongly related to the income level, with average available credit to monthly income ratio of 2.6 to 2.8. The data also show a statistically significant and economically meaningful relationship between income risk and available credit to monthly income ratio. For example, a one standard deviation increase in future (past) income risk is associated with an increase in available credit of 57% (33%) of monthly income. Similarly, going from the 10th-to-90th percentile of future (past) income risk is associated with an increase in available credit of 108% (62%) of monthly income.

³⁸See Kimball (2012) for a proposal along these lines.

³⁹See Skiba and Tobacman (2008) on the relationship between naive quasi-hyperbolic discounting and patterns of borrowing, repayment, and default, and Kuchler and Pagel (2018) on the role of present bias for credit card paydown.

create cue-based complementarities. However, borrowers with a systematic bias toward an over-optimistic reading could also perceive the cue as informational about future income prospects.⁴⁰ Understanding the importance of these behavioral effects is a promising avenue for future research.

⁴⁰See Laibson (2001) and Bernheim and Rangel (2004) for cue effects, and Brunnermeier and Parker (2005) for optimism. In the current context, the high sensitivity of spending behavior in response to an *unanticipated increase* to the credit limit is compatible with several alternative hypotheses, including impatience, present bias, and high elasticity of intertemporal substitution. Two recent consumption papers by Ganong and Noel (2019) and Gerard and Naritomi (2019) provide examples of designs that are set up to test standard models of consumption versus high-impatience/present-bias alternatives, whereby what drives a high estimated degree of impatience is the fact that individuals do *not* build a buffer of savings in response to an *anticipated decrease*.

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