American Economic Review 2022, 112(1): 1–40
https://doi.org/10.1257/aer.20191178

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

By Deniz Aydin*

In a field experiment that constructs a randomized credit limit shock, participants borrow to spend 11 cents on the dollar in the first quarter and 28 cents by the third year. Effects extend to those far from the limit, those who had the new limits as available credit, and those with a liquid asset buffer. In the short-run, flexible and installment contracts are used in tandem, with unconstrained using installments more. Long-run borrowing is predominantly using installments. Near limits, participants borrow when credit expands but save out of constraints when limits are tight. Findings support a buffer-stock interpretation emphasizing precautionary saving. (*JEL* C93, E21, G21, G51, O12, O16)

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 percent of monthly posttax 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.

* Washington University in St. Louis, Olin Business School. (email: daydin@wustl.edu). Emi Nakamura was the coeditor for this article. I am grateful to my advisers, Douglas Bernheim, John Beshears, John Cochrane, and Luigi Pistaferri, for invaluable guidance and continuous support. I thank Adrien Auclert, Cevdet Akcay, Chris Carroll, Raj Chetty, Bob Hall, Ayşe Imrohoroğlu, Jonathan Parker, Amit Seru, Alp Simsek, and Aiga Stokenberga, whose thoughtful comments have dramatically improved this paper. I also thank Sumit Agarwal, Scott Baker, Radha Gopalan, Tullio Jappelli, Lorenz Kueng, Pablo Kurlat, Jonathan Levin, Davide Malacrino, Eren Ocakverdi, and Amir Sufi, as well as numerous seminar and conference participants for helpful suggestions. Oguz Caylan, Wonjin Lee, Jorge Sabat, and Tatiana Vdovina provided excellent research assistance. This paper is a revised version of chapter 1 of my Stanford PhD dissertation. The field experiment was conducted within the guidelines of a Stanford IRB approved human subjects protocol (29,432) and was submitted to the American Economic Association Randomized Controlled Trial Registry on February 2, 2015 (608). Financial support from the National Bureau of Economic Research through the Alfred P. Sloan Foundation (22-2382-12-1-33-003) and the B. F. Haley and E. S. Shaw Fellowship for Economics through the Stanford Institute for Economic Policy Research is greatly appreciated.

† Go to https://doi.org/10.1257/aer.20191178 to visit the article page for additional materials and author disclosure statement.

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 measured *MPC*s, 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. 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 (2013) US stimulus payment study, and the experiment could be interpreted as randomizing the timing of who gets limit increases over the course of the nine month experimental time frame. 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 experimental assignment induces a statistically strong instrument, and an economically large credit shock—on average, the equivalent of about $1,600, hence the utility loss from nonoptimizing behavior is not trivial—and is associated with an almost equally strong and large first-stage effect on total credit limits. 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 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

[1] See 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.

[2] Schularick and Taylor (2012) and Mian, Sufi, and Verner (2017) argue that credit expansions are 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.

16 cents over the three-quarter experimental time frame, 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 time frame. 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 three-year cumulative response of 28 cents 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 time frame 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 three-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 holding liquid assets worth more than 15 months of median posttax 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 participants 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-$MPC^{\Delta L}$ 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

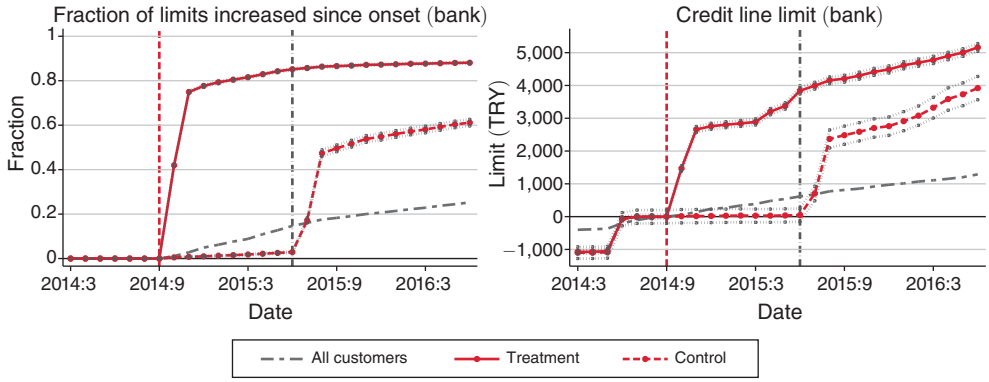

Figure 1. Experimental Timeline

Notes: The panel on the left plots the fraction of participants whose limit has increased since the onset. The panel 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 dotted-and-dashed lines denote the start and end dates of the experiment.

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.[3] Consumption behavior is sensitive to a credit expansion even for individuals 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, those facing greater income risk desire larger buffers, and 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, with those persistently remaining at their credit limit constituting 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

[3] As in Imrohoroğlu (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 has no effects in the short run, and if generates interest costs and needs to be repaid, then can hold back spending 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 tendency to fall into delinquent status, as in Adams, Einav, and Levin (2009); expectations-based models focusing on the informational content of the limit increases, as in Bordalo, Gennaioli, and Shleifer (2018); models of dynamically inconsistent *repayment* behavior, as in Heidhues and Kőszegi (2010); and consumption models with endogenous illiquidity and kinks in the budget constraint, as in Kaplan and Violante (2014).

in the credit limit and the consumption response to changes in income or wealth.[4] The documented spending response to a shock to the credit limit that entails no wealth effects can be used to revisit *MPC*s, 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 *MPC* s of 14 to 15 cents after a quarter and 28 to 33 cents after a year, assuming an *MPC* out of the predictable component of permanent income between two-thirds and one.

Layout.—The paper proceeds as follows. Section I provides details on the environment and key institutional features. Section II describes the experimental design and implementation. Section III presents the results in four subsections, with Section IIIA reporting the event studies, balance sheet effects, and long-run responses, Section IIIB heterogeneity in treatment effects, Section IIIC compositional results on contract choice and spending, and Section IIID precautionary behavior and the dynamics of binding constraints. Section IV concludes, and discusses implications for future research.

I. 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 percent in 2000 to a peak of about 20 percent in 2013. The economy had been expanding from 2010 through 2017, except for declines in seasonally adjusted quarter-on-quarter GDP in 2012:I, 2014:II, and 2016:III. The nominal GDP per capita based on purchasing power parity in 2014 was roughly two-thirds of the EU average. At the onset of the experiment, as of September 2014, the annual rate of inflation was 8.9 percent, and the unemployment rate was 10.5 percent. The unit of measurement for the nominal variables is the local currency TRY with an exchange rate of 2.28 TRY to US$ at the onset. See online 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.

As of 2014, 39 percent of adults aged 25 and over own credit cards (compared with 44 percent and 67 percent in the European Union and United States; see Demirgüç-Kunt et al. 2020), and an annual volume equivalent to 21 percent of GDP flows through credit lines as in-store expenditures (compared with 17 percent for

[4] See Gross and Souleles (2002) and Agarwal et al. (2018) for the former; Hall and Mishkin (1982); Johnson, Parker, and Souleles (2006); Agarwal, Liu, and Souleles (2007); Blundell, Pistaferri, and Preston (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. For randomized evaluations of microcredit in low-income countries, see Karlan and Zinman (2010), Banerjee et al. (2015), and Lane (2018).

the European Union in 2014 and United States in 2015). Notably, credit lines are the predominant method for noncash payments, with debit cards accounting for only 6 percent 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 percent APR (annual percentage rate), 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 States[5] (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.[6]

II. Experimental Design

For the purposes of the field experiment, I collaborate with a large European retail bank.[7] 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 nine months for a randomly selected group of pre-existing customers who would otherwise satisfy the underwriting criteria and would have been pushed for credit line extensions. In this section I describe the key features of the randomized trial, with further details in online Appendix A.

[5] Persons (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 United States during the 1920s, with the collapse of consumption in 1930 following soon after. They find that over 40 percent of the 506 families of federal employees the US Bureau of Labor Statistics surveyed in 1928 used installments to finance furniture, clothing, radios, automobiles, pianos, and appliances. See online Appendix C.4 for details.

[6] Installment debt is used to finance durables and semidurables (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 percent APR interest rate printed on the statement due to special financing. See online Appendix C.1 for a detailed discussion of installment plans.

[7] As of 2014, the financial institution is one of the ten largest credit card 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, overdraft, and retirement accounts; as well as brokerage and insurance services.

Table 1—Summary Statistics

	Panel A. Participants						*Panel B. Universe*	
	N	Mean	SD	*p*10	*p*50	*p*90	Mean	*p*50
Age	45,307	37	10	26	35	50	41	40
Labor income (TRY)	17,690	2,465	2,423	943	1,600	5,111	2,292	1,426
Credit lines (*bank*)								
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
Spending (TRY)	45,307	874	1,577	0	387	2,151	954	201
Flexible (TRY)	45,307	628	1,278	0	258	1,522	685	126
Installment (TRY)	45,307	248	757	0	0	687	273	0
Credit lines (*all banks*)								
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
Balance sheet								
Checking (Bank) (TRY)	30,796	1,011	3,269	0	4	2,153	721	0
Debt (Total) (TRY)	45,307	18,463	103,847	334	6,017	49,640	20,742	5,812

Notes: Panel A is based on $N = 45{,}307$ participants. Panel B is based on a random subsample of the universe 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.

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 online Appendix 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.

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 about 40 percent lower than that of the typical cardholder. Hence, participants appear to be cardholders

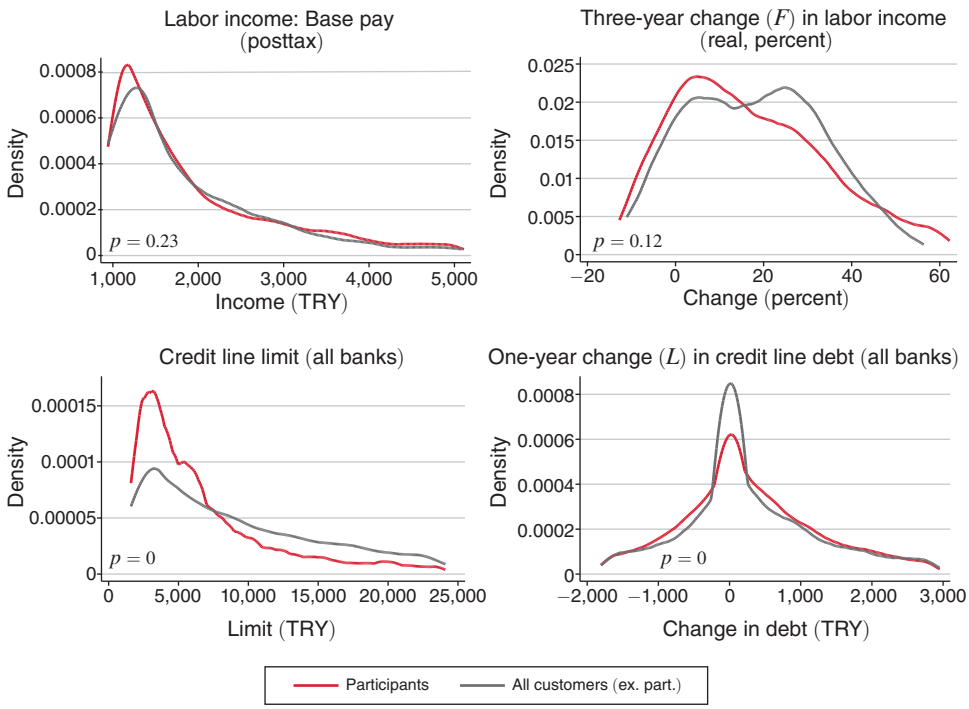

FIGURE 2. SELECTION: KERNEL DENSITIES

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

with low limits outside the bank whose incomes justify a credit limit increase and are catching up with the typical cardholder in terms of limits.[8]

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 $\mathbb{Z}_i = 1$. 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 nine months starting in September 2014 by altering the decision rule governing automatic underwriting.

[8] For 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 (about 60 percent 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.

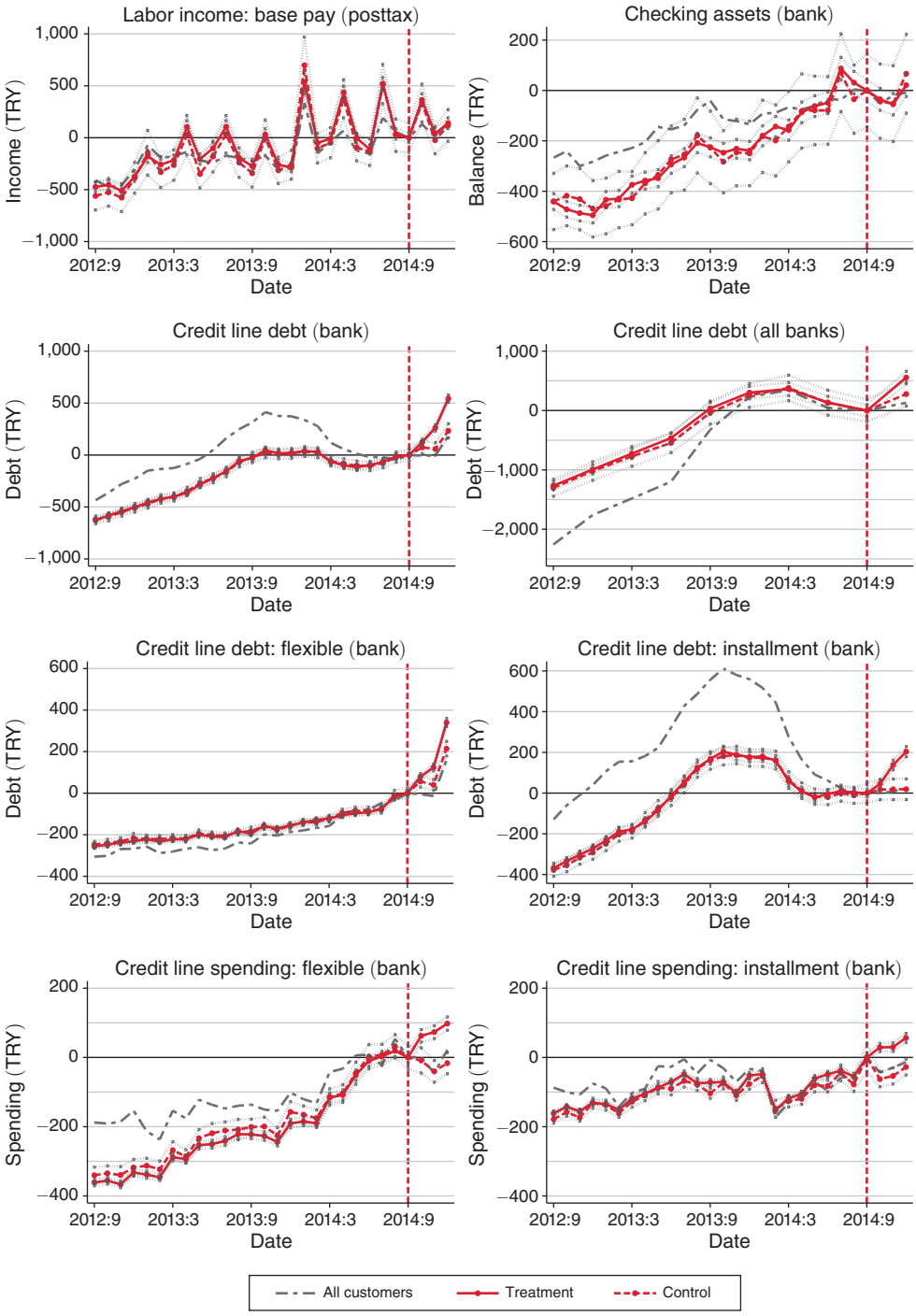

FIGURE 3. COVARIATE BALANCE: PRE-TRENDS

Notes: Panels plot the levels of covariates for treatment ($\mathbb{Z}_i = 1$) and control ($\mathbb{Z}_i = 0$) groups by calendar month. The vertical line denotes the start date of the experiment. The y-axis is normalized to have levels equal to zero at the onset of the experiment. Dashed lines indicate 95 percent confidence intervals for the estimate of the mean.

Covariate Balance.—This randomization procedure makes the exogenous variable for econometric evaluation the dummy variable for treatment group, $\mathbb{Z}_i$. 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, online Appendix 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.—Online Appendix 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 percent of participants in the treatment group see their limits increase over the first nine months, and most of this is staggered in the first two calendar months of the experiment. Second, 3 percent of participants in the control group excluded from lender initiated limit increases request and are granted a limit increase. Finally, starting in month six of the experiment, participants in the treatment group may be reevaluated and have their credit lines increased a second time.[9]

Duration of Experiment.—The short-run experimental time frame concludes after nine months, in June 2015, when the control group is allowed to proceed downstream in the underwriting process. Therefore, similar to Parker et al.'s (2013) stimulus payment study,[10] 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 the conclusion of the experimental time frame. 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 nine month experimental time frame 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 about $1,600, which is equivalent to about 145 percent of average monthly posttax income of the participants. Hence the utility loss from lack of consumption smoothing is not second order; see Cochrane (1989).

[9] The 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 percent 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.

[10] See online Appendix A for a discussion of the parallels with fiscal stimulus payments.

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 baseline 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 $\mathbb{Z}_i$.

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. 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, such as the timing and the 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 inspection 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 based on the timing rule and account usage reported in Table 2 has out-of-sample sensitivity of 34 percent for the experimental limit increases the treatment group receives. The out-of-sample threshold-invariant area under the curve (AUC) is 0.55 (where 0.50 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 nine months does not have a material effect on their behavior.[11]

[11] See online Appendix B.1 for a detailed discussion and additional investigations. Importantly, the econometric model predicts a limit increase for one in three participants. However, these participants do not respond differently than the participants for whom the model does not predict a limit increase ($p = 0.30$).

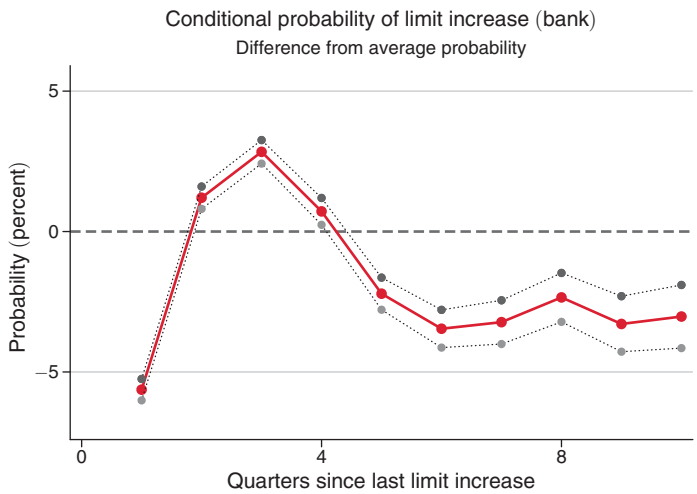

FIGURE 4. ANTICIPATION OF LIMIT INCREASES

Notes: Figure plots the estimated coefficients for ten 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 eight quarters prior to the start of the experiment for the $N = 45{,}307$ participants and includes calendar quarter fixed effects.

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.[12] Moreover, vis-à-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 percent) 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 online 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 $MPC^{\Delta L}$.—The consumption response to a change in the credit limit that entails no wealth effects is related to the *MPC* out of a one-time asset transfer through the interest rate R and the MPC out of a change in permanent income via

$$\frac{\Delta C^*}{\Delta L} = \frac{\Delta C^*}{\Delta A} - \frac{R}{1+R}\frac{\Delta C^*}{\Delta Y^P}. \tag{1}$$

[12] For 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 percent 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.

Table 2—Predictability of Limit Increases

	Panel A. Participants						*Panel B. Universe*	
	In sample			Out of sample			In sample	
Actual TP + FN	0.18	0.18	0.17	0.79	0.79	0.84	0.12	0.15
Predicted TP + FP	0.57	0.58	0.51	0.25	0.33	0.38	0.56	0.53
Precision TP/(TP + FP)	0.21	0.21	0.21	0.72	0.80	0.86	0.16	0.20
Sensitivity TP/(TP + FN)	0.65	0.68	0.63	0.22	0.34	0.39	0.73	0.69
AUC	0.56	0.57	0.61	0.43	0.55	0.56	0.61	0.63
Timing rule	✓	✓	✓	✓	✓	✓	✓	✓
Account usage		✓	✓		✓	✓	✓	✓
Income based			✓			✓		✓

Notes: Table reports classification accuracy of logistic regressions predicting limit increases at bank .TP, FP, TN, and FN stand for true positive, false positive, true negative, and false negative. Panel A is based on $N = 45{,}307$ participants. Panel B is based on a random subsample of the universe of all credit line customers excluding participants ($N = 10{,}000$). The specifications use data on the eight 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 online Appendix 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.

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 factor.[13] 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 low cost (R\approx 0) the estimated $MPC^{\Delta L}$ will resemble an *MPC*.

A. *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 online Appendix D. Information on credit lines at the bank is taken from end-of-billing-cycle statements and includes 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 small (e.g., overdraft) and big-ticket debt (e.g., mortgages and unsecured loans), available on a quarterly basis.[14] The data

[13] See Guerrieri and Lorenzoni (2017) and online Appendix A for a discussion.

[14] Unlike bank data, which measure balances carried across pay periods, credit bureau data on credit lines are designed to measure the total liabilities at a point in time, and 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.

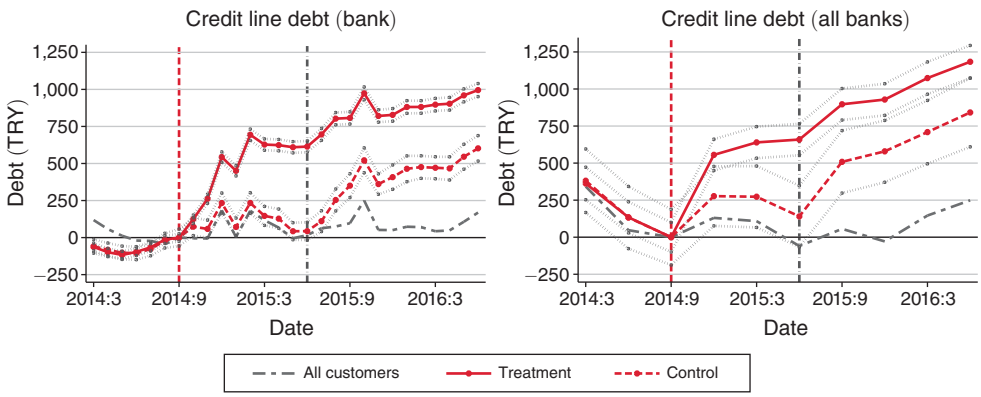

FIGURE 5. EVENT STUDY

Notes: Panels plot average credit line debt at bank (left) and across all banks (right) for treatment ($\mathbb{Z}_i = 1$) and control ($\mathbb{Z}_i = 0$) groups by calendar month. The y-axis is normalized to have levels equal to zero at the onset of the experiment. Vertical dashed and dotted-and-dashed lines denote the start and end dates of the experiment.

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 posttax 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.

III. Results

A. *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 percent 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.[15] The sharp increase in credit line debt by the treatment group relative to the control group after the intervention is the causal effect of the increase in credit limits.

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

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

$$Y_i = \psi X_i + f_s + \varepsilon_i, \tag{2}$$

where i denotes an individual and f_s stands for randomization strata fixed effects.

The first-stage (FS) and intent-to-treat (ITT) 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 (MP) 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.[16]

In the first-stage 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 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 $\mathbb{Z}_i$, the dummy variable for assignment to the treatment group. The coefficients ψ_τ^{FS} and ψ_τ^{ITT} are estimated using OLS, and measure the causal effect of assignment to the treatment group on the left-hand-side variable over a period τ, either one or three quarters. Randomization ensures orthogonality between assignment to the treatment group $\mathbb{Z}_i$ 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 1,216 and 1,397 for one quarter and three quarters. The second row reports the intent-to-treat estimates on bank cards. Focusing on three-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 three-quarter difference in new credit line debt at the bank of 571 (SE 35) TRY. The latter number corresponds to \$250 or about a quarter of average monthly post-tax labor income for participants. The naïve Wald estimator of the marginal propensity on bank cards equals $571/3{,}795 = 0.15$ after three quarters.[17]

To ensure that the results do not represent mere balance shifting, the right panel in Figure 5 and the bottom three rows in Table 3 measure the effect across all banks. The three-quarter first-stage estimates point to a difference in limits of 3,554 (SE 73) TRY and the three-quarter intent-to-treat estimates point to a difference in credit line debt of 519 (SE 69) TRY implying that 6 percent of the difference in limits and 9 percent of the average increase in credit line debt is offset by adjust-

[16] See online Appendix Table A.4 for further details on the empirical framework used throughout the paper.

[17] The 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.,

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

Table 3—Borrowing on Credit Lines: First-Stage, Intent-to-Treat, and Marginal Propensity Estimates

	Baseline level (TRY)		*Panel A. Equation* (2) — ψ			*Panel B. Equation* (3) — ϕ		
			F-stat			Point in time		Cumul.
			$3q$	$1q$	$3q$	$1q$	$\sum_{j=2q}^{3q}$	$3q$
Δ Limit (TRY) (Bank)	5,111	First stage (OLS)	1,397	2,737	3,795	2,737	1,058	3,795
				(24)	(34)	(24)	(23)	(34)
Δ Debt (TRY) (Bank)	1,265	Intent-to-treat (OLS)		310	571	310	260	571
				(28)	(35)	(28)	(35)	(35)
		Marginal propensity (2SLS)		0.113	0.150	0.114	0.049	0.162
				(0.010)	(0.009)	(0.010)	(0.015)	(0.012)
Δ Limit (TRY) (All banks)	10,462	First stage (OLS)	1,110	2,589	3,554	2,589	965	3,554
				(50)	(73)	(50)	(64)	(73)
Δ Debt (TRY) (All banks)	3,446	Intent-to-treat (OLS)		278	519	278	240	519
				(48)	(69)	(48)	(61)	(69)
		Marginal propensity (2SLS)		0.108	0.146	0.106	0.053	0.159
				(0.018)	(0.019)	(0.019)	(0.026)	(0.023)

Notes: Estimates in panel A from equation (2) use data on $N = 45{,}307$ participants. Estimates in panel B from equation (3) use data on $N \times T = 45{,}307 \times 3$ participant-quarter observations, where robust standard errors are corrected for clustering at the individual level. Cumulative effects calculated as $\Phi_\tau = \sum_{j=1}^{\tau} \phi_j$.

ments on nonbank cards.[18] The change in total credit line limit and debt, relative to baseline levels of 10,462 TRY and 3,446 TRY, represents a 34 percent and 15 percent 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, I use as the left-hand-side variable $\Delta^\tau D_i$, and as the right-hand-side variable $\Delta^\tau L_i$, again over either a one-quarter or a three-quarter period. I estimate ψ_τ^{MP} with 2SLS, with $\mathbb{Z}_i$ 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 $\mathbb{Z}_i$.[19] 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, $\mathbb{Z}_i$, is random, the variation in *who* gets a limit increase, and *how much*, is not random, and could potentially be correlated with the error term.[20] The additional

[18] At the onset of the experiment, 56 percent of participants had credit lines at other banks, and 47 percent were carrying balances on other bank cards. Twenty-three percent of participants see their limits increase at other banks over the three-quarter experimental time frame, and this number is 3.5 percent lower for the treatment group. The fraction of cardholders carrying balances on cards outside the bank decreases by 0.4 percent for the treatment group after three 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. Previously, Gross and Souleles (2002) investigated balance shifting using naturally occurring variation and found that the increase in limits on the treated credit card is associated with a positive but statistically insignificant change in balances on other cards.

[19] The 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*.

[20] For 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 OLS 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.

identifying assumption for the LATE interpretation is that there is no effect of experimental assignment $\mathbb{Z}_i$ on the outcomes studied, on average, that does not operate via the experimental assignment's impact on credit limits.

These marginal propensity estimates are reported in the third and the last rows of panel A in Table 3. Similar to the above, the last row uses total debt and limits across all banks. Results show that a unit increase in total credit lines across all banks is accompanied by an increase in total credit line debt of 10.8 (SE 0.018) cents after one quarter and 14.6 (SE 0.019) cents after three quarters. These estimates are highly statistically significant ($p < 0.0001$). As the specification is just identified, these marginal propensity estimates ψ_τ^{MP} are analogous with the naïve Wald estimators $\psi_\tau^{ITT}/\psi_\tau^{FS}$.

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

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

where $t \in \{1, \ldots, T\}$ 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 time frame of $T = 3$ quarters, totaling $N \times T = 45{,}307 \times 3$ 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, $\mathbb{Z}_i \times f_{t=j}$. 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, $\Phi_\tau^{FS} = \sum_{j=1}^{\tau} \phi_j^{FS}$ and $\Phi_\tau^{ITT} = \sum_{j=1}^{\tau} \phi_j^{ITT}$ 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 first-stage and intent-to-treat estimates obtained using equation (3) displayed in panel B coincide with the simple estimates ψ_τ^{FS} and 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, $\Phi_\tau^{MP} = \sum_{j=1}^{\tau} \phi_j^{MP}$, 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 used throughout the paper. In order to measure the delayed response that occurs after the first quarter of the limit increase, I also report $\phi_2^{MP} + \phi_3^{MP}$, which measures the sum of the delayed responses that occur in the two subsequent quarters.

The bottom row of panel B in Table 3 displays the marginal propensity estimates across all banks 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 (SE 1.9) cents during the quarter in which it was received. The three-quarter cumulative response is estimated as 15.9 (SE 2.3) cents. These one-quarter and three-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 (SE 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 online Appendix Table A.6.

The top left event study in Figure 6 shows that the increase in limits and borrowing is associated with a large increase in spending. I analyze this spending response in detail in Section IIIC. Focusing on the checking assets on the top right, there is no evidence of crowding out on this dimension (three-quarter $p = 0.76$).

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 three-quarter difference of 502 (SE 72) TRY in the sum of credit lines and overdraft, implying a 3 percent offset by adjustments on overdraft debt. Focusing on extensive margin adjustments on big-ticket loans, the treatment group is 1.7 percentage points more likely (off of a 60 percent base) to take on big-ticket loans by the end of the experimental time frame ($p = 0.009$).

The event studies analyzing labor income show no discernible effects in base pay ($p = 0.20$), but a small increase in overtime and bonuses in the two Januaries subsequent to the onset of the experiment. The differences, 41 (SE 74) TRY and 125 (SE 104) TRY, correspond to about 1.5 percent and 5 percent of average monthly posttax labor income. However these effects are not statistically significant ($p = 0.58$ and $p = 0.23$). 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 time frame 1.2 percent of the participants are past due by

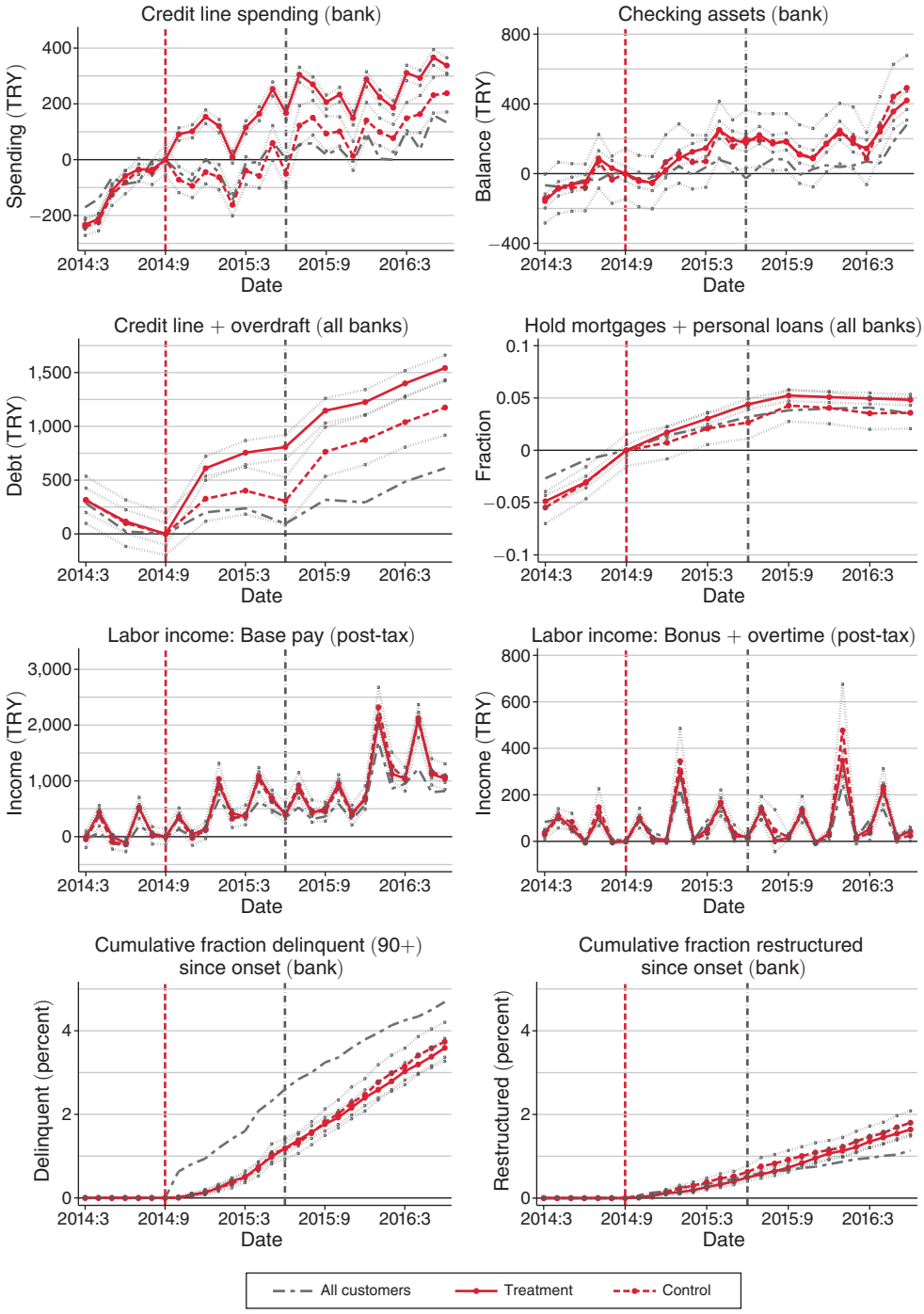

FIGURE 6. EVENT STUDY: BALANCE SHEET EFFECTS

Notes: Panels plot the levels of covariates for treatment ($\mathbb{Z}_i = 1$) and control ($\mathbb{Z}_i = 0$) groups by calendar month. The y-axis is normalized to have levels equal to zero at the onset of the experiment. Vertical dashed and dotted-and-dashed lines denote the start and end dates of the experiment.

Table 4—Borrowing on Credit Lines: Long Run

		Cumulative			Point in time			
		$4q$	$8q$	$12q$	$1q$	$\sum_{j=2q}^{4q}$	$\sum_{j=5q}^{8q}$	$\sum_{j=9q}^{12q}$
Δ Limit (TRY)	First stage (OLS)	1,450	844	717	2,589	−1,139	−606	−127
		(102)	(141)	(167)	(50)	(96)	(100)	(100)
Δ Debt (TRY)	Intent-to-treat (OLS)	388	379	295	278	110	−10	−84
		(77)	(79)	(92)	(48)	(74)	(74)	(74)
	Marginal propensity (2SLS)	0.183	0.246	0.283	0.108	0.075	0.063	0.037
		(0.031)	(0.045)	(0.060)	(0.018)	(0.025)	(0.037)	(0.037)

Notes: Estimates from equation (3) use data on $N \times T = 45{,}307 \times 12$ participant-quarter observations, where robust standard errors are corrected for clustering at the individual level. Cumulative effects calculated as $\Phi_\tau = \sum_{j=1}^{\tau} \phi_j$. Limit and debt stand for totals across all banks.

90 days or more and 0.6 percent restructure, with no statistically significant differences between the treatment group and the control group ($p = 0.95$ and $p = 0.13$).

Long-Run Effects.—A noteworthy feature of the experiment is that not everyone in the control group receives a limit increase after the experimental time frame'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.[21]

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 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 percent of the participants in the treatment group and 70 percent 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 (SE 167) TRY. The difference in the change in total credit line debt is 295 TRY (SE 92), which corresponds to a naïve Wald estimate of the three-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 time frame 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.

[21] In contrast to the *MPC* out of assets, which should sum to one in the long-run, the *MPB* out of credit limits should sum to zero. However, interest costs and debt service can also hold back future borrowing and spending.

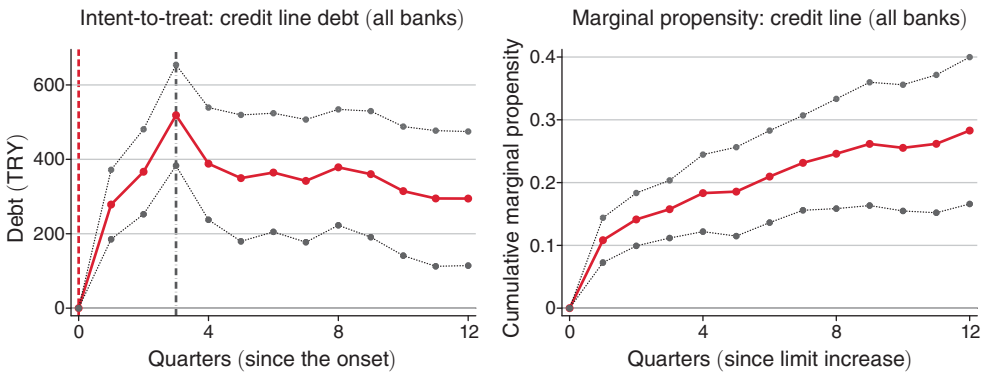

FIGURE 7. BORROWING ON CREDIT LINES: LONG-RUN INTENT-TO-TREAT AND MARGINAL PROPENSITY ESTIMATES

Notes: Figure plots the long-run cumulative intent-to-treat and marginal propensity estimates. Estimates from equation (3) use data on $N \times T = 45{,}307 \times 12$ participant-quarter observations, where robust standard errors are corrected for clustering at the individual level.

The estimates obtained from this long-run specification are quantitatively consistent with the estimates reported in the short-run specification in Table 3. When the credit limit is increased, participants borrow 10.8 (SE 1.8) cents in the quarter they are received, and 18.3 (SE 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 ($p < 0.001$). The cumulative response is 24.6 (SE 4.5) cents after two years and 28.3 (SE 6.0) cents after three years, which indicates more than one-third and about two-thirds of the three-year cumulative response occur after the first quarter and the first year, respectively. For the three-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.

B. *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, faces 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 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

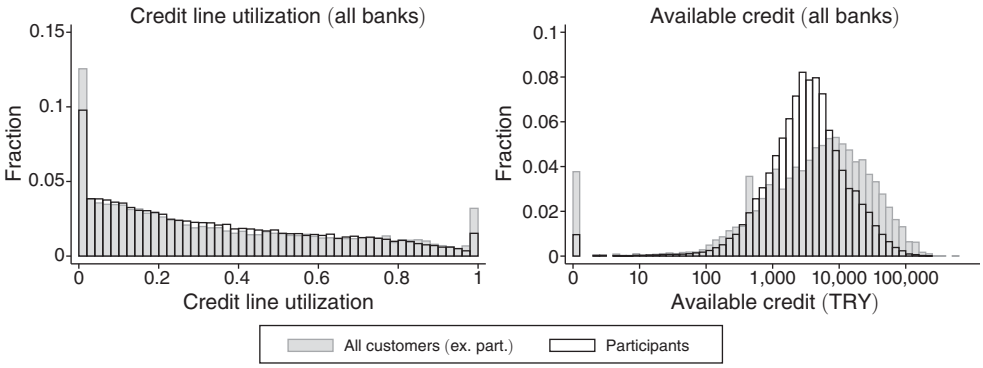

Figure 8. Baseline Distance-to-Limit

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 banks in the quarter before the experiment. The median and average credit line utilization are 0.27 and 0.34, respectively, with only one in ten of the participants utilizing more than 75 percent of their credit lines. The median and average available credit are 3,284 TRY and 7,016 TRY, corresponding to 1.33 and 2.85 times the average monthly posttax labor income. For 89 percent of the participants, the debt levels by the end of the experimental time frame were feasible using their unused credit at the onset. Moreover, 73 percent of the participants had the additional limits they receive as available credit.[22]

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),

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

where k denotes a bin and f_{kt} stands for calendar quarter-bin fixed effects. This specification uses data on $N \times T = 45{,}307 \times 3$ 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. Limit and debt stand for totals across all banks. In each panel, the first column displays the average available credit across all banks in the quarter before the experiment. The second and third columns display the three-quarter cumulative first-stage and intent-to-treat effects on the changes in

[22] Thirty-nine percent of the participants face a binding constraint in the following three-year period. Conditional on having a liquid asset account, 48 percent, 22 percent, and 5 percent of the participants hold liquid assets at the bank more than 250 TRY, 2,500 TRY, and 25,000 TRY (corresponding to about \$100, \$1,000, \$10,000), respectively. For the United States, Fulford and Schuh (2020) report a stable average utilization of about 0.3 using Federal Reserve Bank of New York Consumer Credit Panel data.

Table 5—Borrowing on Credit Lines: Heterogeneity by Baseline Distance-to-Limit and Liquid Assets

Panel A. Credit line utilization				
Utilization	Available credit (TRY)	Δ Limit (TRY) First stage (OLS)	Δ Debt (TRY) Intent-to-treat (OLS)	Δ Debt (TRY) Marginal propensity (2SLS)
[0.75, 1]	1,278	2,580	1,227	0.503
		(210)	(182)	(0.067)
[0.50, 0.75)	3,957	2,707	1,010	0.403
		(139)	(251)	(0.096)
[0.25, 0.50)	6,690	3,313	403	0.121
		(156)	(110)	(0.040)
(0, 0.25)	10,266	4,262	247	0.073
		(121)	(102)	(0.030)
= 0	4,834	3,813	275	0.073
		(182)	(69)	(0.026)
Panel B. Available credit quintiles				
Quintile	Available credit (TRY)	Δ Limit (TRY) First stage (OLS)	Δ Debt (TRY) Intent-to-treat (OLS)	Δ Debt (TRY) Marginal propensity (2SLS)
QU_1	557	2,466	713	0.298
		(91)	(43)	(0.022)
QU_2	1,767	2,616	658	0.266
		(86)	(78)	(0.033)
QU_3	3,328	2,621	405	0.166
		(117)	(72)	(0.030)
QU_4	6,181	3,452	437	0.144
		(133)	(103)	(0.035)
QU_5	23,252	6,615	385	0.062
		(286)	(309)	(0.054)
Panel C. Total liquid assets at Bank				
Assets (TRY)	Available credit (TRY)	Δ Limit (TRY) First stage (OLS)	Δ Debt (TRY) Intent-to-treat (OLS)	Δ Debt (TRY) Marginal propensity (2SLS)
∅	6,808	2,485	459	0.200
		(112)	(120)	(0.052)
[0, 250)	5,626	2,565	663	0.269
		(111)	(137)	(0.058)
[250, 2,500)	6,698	4,079	434	0.119
		(179)	(102)	(0.031)
[2,500, 25,000)	9,951	6,488	505	0.096
		(290)	(175)	(0.039)
25,000+	15,233	10,899	204	0.041
		(407)	(484)	(0.067)

Notes: Estimates from equation (4) use data on $N \times T = 45{,}307 \times 3$ participant-quarter observations, where robust standard errors are corrected for clustering at the individual level. Participants are sorted by values in the quarter before the experiment. First-stage, intent-to-treat, and marginal propensity estimates are three-quarter cumulative effects, calculated as $\Phi_\tau = \sum_{j=1}^{\tau} \phi_j$. Limit and debt stand for totals across all banks.

limits and credit line debt. Finally, the fourth column reports the object of interest, the cumulative marginal propensity over three quarters.[23]

[23] As 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

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 percent 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, one in ten of participants who use more than 75 percent of their existing credit limits borrow 50 (SE 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 percent confidence interval for the response is (0.37, 0.63) 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 percent) of participants not carrying any balances are induced by the limit increase to borrow 7.3 (SE 2.6) cents. Similarly, those carrying some balances but were using less than 25 percent of their limits across all banks accumulate 7.3 (SE 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 (SE 102) TRY. Ninety-five percent of this group, including the control group, could have feasibly financed their debt levels by the end of the experimental time frame using their unused credit at the onset. Moreover, 81 percent of this group had the additional limits they receive as available credit. Online Appendix Table A.7 shows that the effects extend to those who had the additional limits they receive as available credit as well.

The adjacent bin, those using between 25 percent and 50 percent of total credit lines, accumulate 12.1 (SE 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 0.43 (SE 0.07) is equal between the top and bottom utilization groups is decisively rejected.

To isolate situational factors from fixed and persistent ones, panel A1 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

specification, X_{ij} is ΔL_{it-j+1}, and the coefficients are estimated using 2SLS, in which the right-hand-side variable of the intent-to-treat specification above is used as the instruments.

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

Panel A. Additional heterogeneity

Utilization curr. versus typic.		*Panel A1. Current versus typical utilization* Intent-to-treat (OLS)	Marginal propensity (2SLS)	*Panel A2. Times binding past 12q* Times binding	Intent-to-treat (OLS)	Marginal propensity (2SLS)	*Panel A3. Income quartiles* Quartile	Intent-to-treat (OLS)	Marginal propensity (2SLS)
[0.50, 1]	[0.25, 1]	1,159	0.454	6+	1,012	0.572	Q_1	319	0.166
		(226)	(0.083)		(268)	(0.148)		(71)	(0.039)
	[0, 0.25)	997	0.402	3 to 6	1,173	0.451	Q_2	321	0.124
		(259)	(0.117)		(173)	(0.062)		(124)	(0.049)
[0, 0.5)	[0.25, 1]	367	0.120	1 or 2	633	0.257	Q_3	476	0.128
		(137)	(0.050)		(142)	(0.057)		(107)	(0.035)
	[0, 0.25)	270	0.071	= 0	295	0.073	Q_4	577	0.062
		(79)	(0.023)		(92)	(0.027)		(230)	(0.030)

Panel B. Heterogeneity and Composition

Utilization	*Panel B1. Composition by utilization* Δ Debt installment (Bank) Marginal propensity (2SLS)	Δ Debt flexible (Bank) Marginal propensity (2SLS)	Flex share	*Panel B2. Composition by baseline contract preference* Flex? (bank)	Inst? (bank)	Δ Debt installment (Bank) Marginal propensity (2SLS)	Δ Debt flexible (Bank) Marginal propensity (2SLS)	Flex share
[0.75, 1]	0.280	0.193	0.41	×	×	0.022	0.072	0.77
	(0.038)	(0.032)				(0.033)	(0.036)	
[0.50, 0.75)	0.203	0.160	0.44	×	✓	0.091	0.014	0.13
	(0.025)	(0.022)				(0.019)	(0.008)	
[0.25, 0.50)	0.099	0.071	0.42	✓	✓	0.128	0.093	0.42
	(0.020)	(0.014)				(0.014)	(0.010)	
(0, 0.25)	0.078	0.024	0.24	✓	×	0.176	0.121	0.41
	(0.018)	(0.008)				(0.031)	(0.037)	
= 0	0.059	0.012	0.17					
	(0.018)	(0.012)						

Notes: Estimates from equation (4) use data on $N \times T = 45{,}307 \times 3$ participant-quarter observations, where robust standard errors are corrected for clustering at the individual level. Intent-to-treat and marginal propensity estimates are three-quarter 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 panel A1, typical utilization is the average in quarters −12 to −5 relative to the onset. 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 panels A3, B1, and B2 participants are sorted by values in the quarter before the experiment.

response is the sensitivity to credit is related to the long-run utilization levels. Panel A2 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 three-quarter response of 0.162 (0.012) is significantly smaller than the average baseline utilization of 0.34. Second, participants in the treatment group reduce their average utilization rate, by 0.041 (0.004) after three quarters, but also at all utilization levels, as displayed and discussed later on in Figure 13 and Section IIID. 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 predictions is decisively rejected with $p < 0.001$.

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 (SE 2.2) cents, which drops to 14.4 (SE 3.5) for the fourth quintile. The fourth quintile, on average, has available credit of 6,181 TRY, receives credit line increases of 3,452 TRY, and borrows 437 (SE 103) TRY. This group has substantial ability to borrow, as for 96 percent of this group (including the control group) the debt levels by the end of the experimental time frame were feasible given unused credit at the onset; and 85 percent 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 posttax income, the estimated effect is 6.2 (SE 5.4) cents ($p = 0.25$). This group displays a statistically significant borrowing response of 10.3 (SE 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.[24]

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, Violante, and Weidner 2014), this includes checking and savings accounts, and holdings of stocks, bonds, and funds; and excludes housing, illiquid retirement accounts, and life insurance policies; see online Appendix D. Participants with no active asset accounts (32 percent) are placed in the first bin. Those with active asset accounts 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 ten times the average labor income; 11 and 4 percent of the participants are in the latter two bins.

Similar to above, panel C in Table 5 points to a negative relationship between liquid asset holdings and the propensity to borrow. For example, participants with an account but next to no assets borrow 26.9 (SE 5.8) cents with a 95 percent confidence interval of 0.16 and 0.38. For the adjacent group, with an average level of assets between 250 TRY and 2,500 TRY, the marginal propensity drops to 11.9 (SE 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

[24] Panel A3 of Table 6 displays the estimated treatment effects by income quartiles, which range between 16.6 (SE 3.9) cents and 6.2 (SE 3.0) cents. There exists no meaningful relationship between the estimated treatment effects and past or future income growth over various horizons; hence there is no evidence that borrowing presages income growth.

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, borrow 9.6 (SE 3.9) cents out of the credit limit increase. The increase in credit line debt for this subgroup is 505 (SE 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 percent of these participants, including the control group, borrow beyond the limit available to them at the onset.

For the 4 percent 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 ($p = 0.54$). The estimated effect across all banks for this group is 4.1 (SE 6.7) cents. This group also does not display a statistically significant borrowing response on bank credit lines, with a response of 6.2 (SE 6.2) cents. The loss of precision might also reflect the small sample size in this bin.[25] 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 percent of this group face a binding constraint in the three years after the onset. The latter number is 45 percent for the second bin and 21 percent for the fourth bin. I nevertheless reject the null hypothesis that the response is equal between the second bin and the bottom bin ($p = 0.016$).

C. *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 then 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 are available on a monthly basis, allowing for higher-frequency analysis.[26]

[25] In sum, borrowing on bank credit lines increases for all subgroups in Table 5 and panel A of Table 6 except the highest asset group (more than 25,000 TRY), and credit line debt across all banks increases for all subgroups except the highest asset group and the highest available credit group (highest quintile in levels). Previously, Agarwal et al. (2018) investigated heterogeneity in balance shifting by credit score using naturally occurring variation and found that the increase in balances on the treated credit card corresponds to an increase in overall balances for all but the highest credit score group.

[26] Credit bureau data on credit lines only report total liabilities at other banks, and do 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 percent of the change in credit line debt at the bank cards (14 TRY) is offset on liabilities on credit

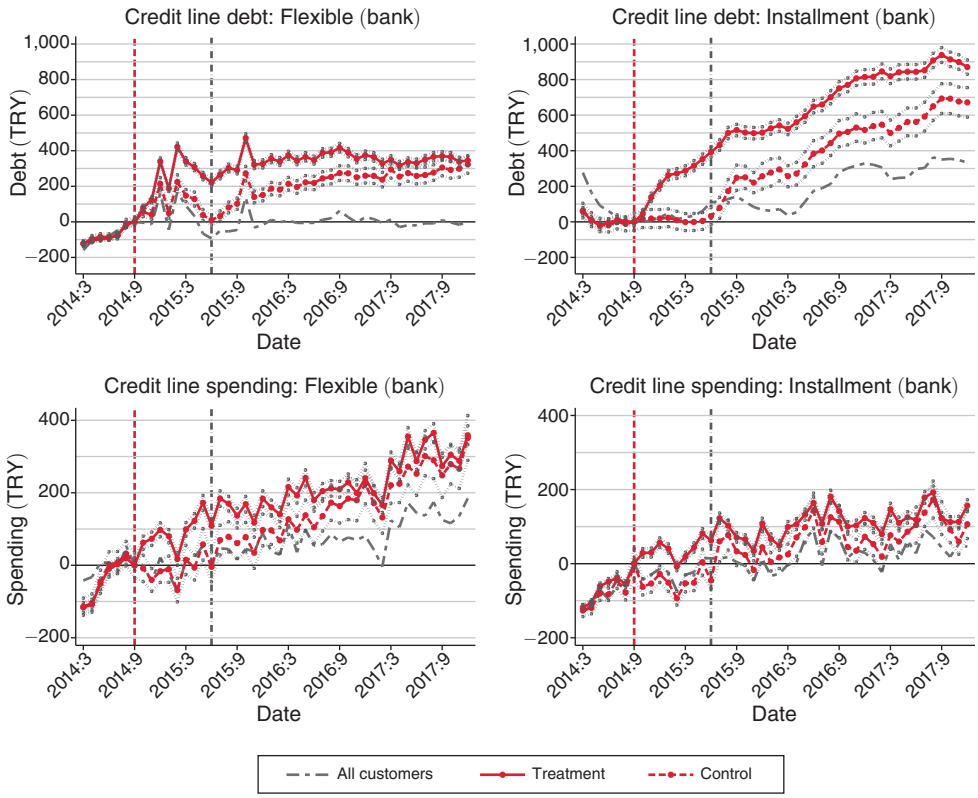

FIGURE 9. EVENT STUDY: CONTRACT CHOICE

Notes: Figures plot the levels of covariates for treatment ($\mathbb{Z}_i = 1$) and control ($\mathbb{Z}_i = 0$) groups by calendar month. The y-axis is normalized to have levels equal to zero at the onset of the experiment.

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 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

lines at other banks ($p = 0.84$); see online Appendix Table A.6. See online Appendix Figure A.3 for these event studies in real terms.

Table 7—Borrowing on Credit Lines: Contract Choice

	Baseline level (TRY)	Intent-to-treat (OLS)				Marginal propensity (2SLS) cumulative		
		$1m$	$1q$	$3q$	$13q$	$1m$	$3m$	$9m$
Δ Debt flexible (TRY)	358	23	126	211	22	0.017	0.049	0.052
(bank)		(11)	(18)	(17)	(28)	(0.008)	(0.012)	(0.012)
Δ Debt installment (TRY)	907	28	185	360	198	0.019	0.072	0.105
(bank)		(14)	(23)	(31)	(48)	(0.010)	(0.012)	(0.012)
Flexible share		0.45	0.41	0.37	0.10	0.47	0.40	0.33

Notes: Intent-to-treat estimates from equation (2) use data on $N = 45{,}307$ participants. Marginal propensity estimates from equation (3) use data on $N \times T = 45{,}307 \times 9$ participant-month observations, where the right-hand-side variable is the change in credit limits at the bank, and standard errors are clustered at the individual level. Cumulative effects calculated as $\Phi_\tau = \sum_{j=1}^{\tau} \phi_j$. Flexible share is defined as the ratio of flexible borrowing to the sum of flexible and installment borrowing.

are increased is relatively modest, and over the course of short-run experimental time frame 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 (SE 11) TRY and 28 (SE 14) TRY, corresponding to 0.9 percent and 1.1 percent of average monthly posttax income of the participants. Over the three-quarter experimental time frame, the intent-to-treat differences in flexible and installment borrowing at the bank is 211 (SE 17) TRY and 360 (SE 31) TRY with flexible debt accounting for 37 percent of the additional borrowing.[27]

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, are 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 (SE 0.8) and 1.9 (SE 1) cents in the month limits are received for flexible and installment borrowing, and an additional 3.2 (SE 1.1) and 5.2 (SE 1) cents in the second and third months. After the third month, flexible borrowing comes to a pause ($p = 0.87$). The second and the third quarters subsequent to the limit increase bring an additional 3.4 (SE 1.7) cents of installment debt. The nine month marginal propensity to borrow is 5.2 (SE 1.2) and 10.5 (SE 1.2) cents, respectively, with flexible debt accounting for 33 percent 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 time frame as well as the subsequent follow-up period, stacking concurrent loans, creating

[27] At the onset of the experiment, 8 percent of participants, 12 percent of those with an active asset account, cohold revolving debt at 24 percent APR and liquid assets of at least 250 TRY (about $100, or 10 percent of average monthly post-tax labor income) at the bank, implying they could have repaid some of the existing expensive revolving debt using liquid assets. The estimated intent-to-treat effect on revolving debt on bank credit lines for the coholding group is 253 (94) TRY (p=0.007). Hence, coholding participants do borrow in revolving form out of the new limits.

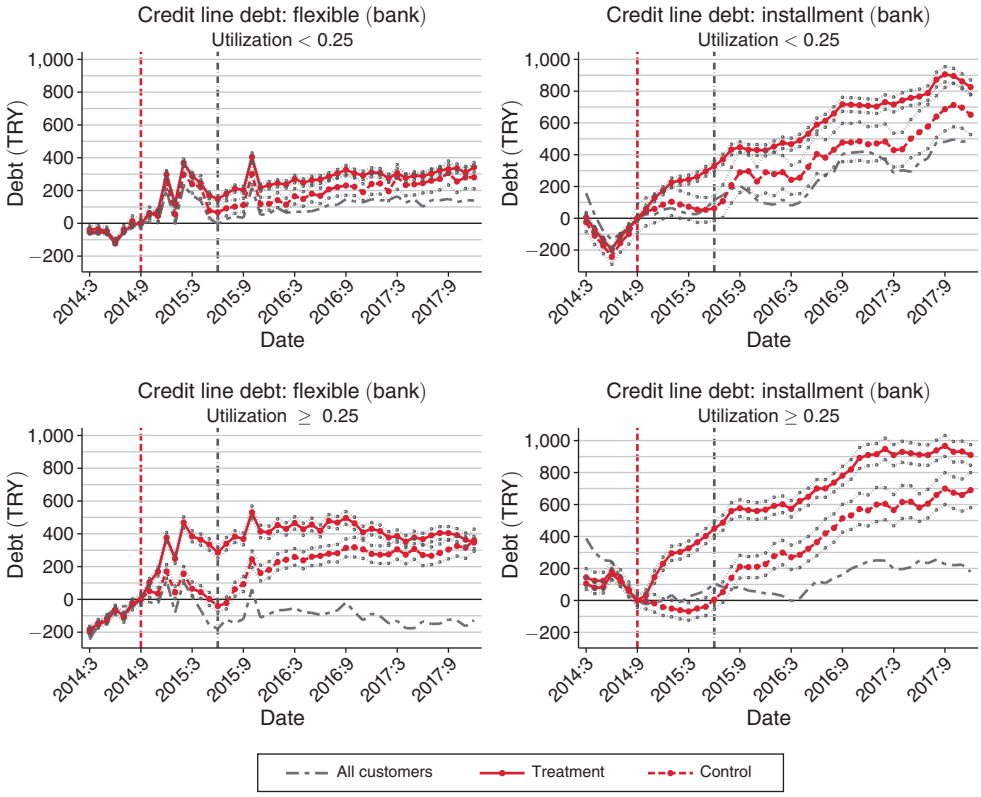

FIGURE 10. EVENT STUDY: CONTRACT CHOICE BY DISTANCE-TO-LIMIT

Notes: Panels plot the nominal levels of covariates for treatment ($\mathbb{Z}_i = 1$) and control ($\mathbb{Z}_i = 0$) groups by calendar month. The y-axis is normalized to have levels equal to zero at the onset of the experiment. Top (bottom) panels focus on participants with a baseline utilization rate of less than (greater than or equal to) 0.25.

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 time frame, 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 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 percent of the borrowing response at the bank credit lines, and the difference

[28] This pattern is reminiscent of the *saving down* behavior discussed in Rutherford (2000) and Bauer, Chytilová, and Morduch (2012).

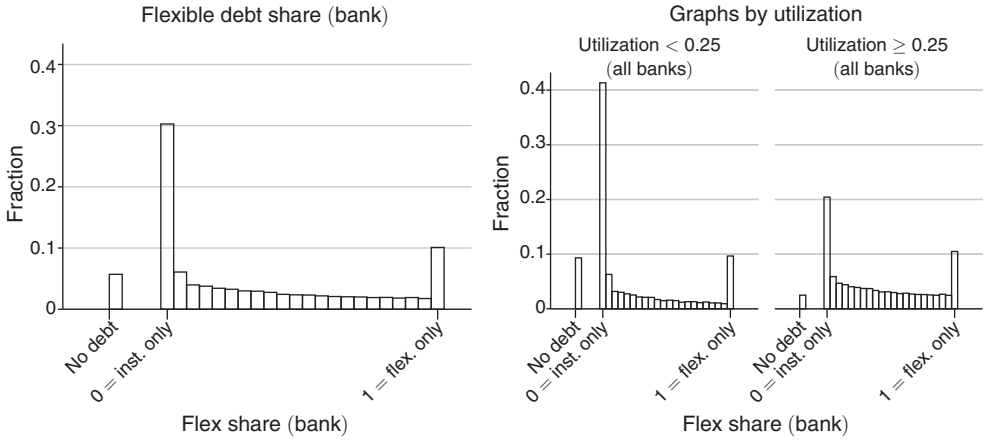

Figure 11. Baseline Contract Share

Notes: 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.

of 22 (SE 28) TRY in flexible debt between the treatment and control groups is not statistically significant ($p = 0.43$).

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 percent of the response. This ratio levels between 41 percent to 44 percent 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 percent) holding only 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 percent, and the flexible debt response is not statistically significant. For participants who hold flexible debt, the flexible debt share is about 40 percent, 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, since 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* commitments, installment contracts preclude the possibility of dynamic revisions of the repayment plans made initially.[29] Hence, borrowers who have a time-inconsistent taste for immediate gratification in *repayment* à 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, as a naïf 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 do 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.[30]

Spending Patterns.—Figure 12 displays the spending patterns by category. These estimates are obtained from equation (3) and report the cumulative three-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.[31] The corresponding sums are displayed using red bars. The upper and lower shadows indicate 99.8 percent 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 require careful interpretation. Credit lines are used as a means of payment and for transactional liquidity, with a float benefit of 1 to 2 cents per dollar spent.[32] For borrowers

[29] Unlike 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. Because of 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 percent 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 the debt on time. See Ashraf, Karlan, and Yin (2006) and Kaur, Kremer, and Mullainathan (2015) for examples of pure commitment contracts and Bryan, Karlan, and Nelson (2010) for a discussion of commitment contracts.

[30] A 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, remains largely unexplored. The latter is partly due to the difficulty in characterizing the 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 Bernheim, Ray, and Yeltekin (2015). See DeAngelo, DeAngelo, and Whited (2011) for a model of *corporate* capital structure dynamics under a pecking order that predicts the issuance of transitory debt.

[31] See online Appendix Table A.11 for more details on these categories.

[32] For example, participants, on average, spend 628 TRY per month in flexible form, and revolve 358 TRY using flexible debt one quarter before the experiment. The interest cost comes to about 358 TRY $\times$ 2% = 7.2 TRY per month at fixed 24 percent APR on the flexible debt. However, flexible spending also comes with a float benefit

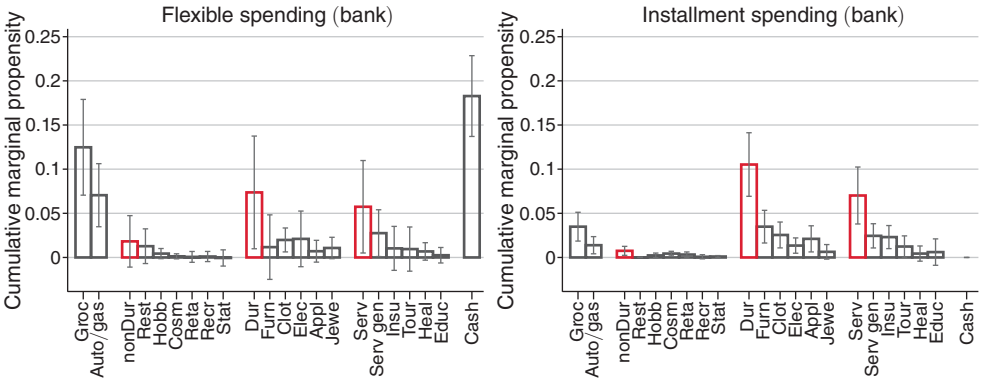

FIGURE 12. SPENDING PATTERNS

Notes: Figure reports the categorical three-quarter cumulative marginal propensity to spend on bank cards. Estimates from equation (3) use data on $N \times T = 45{,}307 \times 3$ participant-quarter observations, using as the left-hand-side variable the categorical spending on bank credit lines, and as the right-hand-side variable the change in bank credit line limits. 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 percent confidence intervals for the estimate of the mean, to account for Bonferroni correction to handle many outcomes, and clustering at the individual level.

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.

The panel on the left displays the gross flexible spending by category. Over the three-quarter experimental time frame, participants in the treatment group, relative to the control and on average, increase their flexible debt by 5.2 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 percent of the total increase in flexible transactions is directed toward groceries, a category in which expenditures and consumption are relatively more tightly linked. Additional spending on groceries represents a 25 percent increase, and the additional cash advances represent a 55 percent increase compared with the control group over the experimental time frame.

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 mainly driven by a statistically significant response in general services, which primarily consists of utility bills.

of about 628 TRY$\times$ 2% $\times$ 0.5 = 6.3 TRY per month, where the 0.5 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 nine month experimental time frame, and obtain an additional float benefit of 11 TRY. This calculation excludes points earned or other perks.

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 three-quarter experimental time frame, participants in the treatment group, relative to the control and on average, increase their installment debt by 10.5 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 46 percent of the total increase in installment transactions, is primarily driven by consumer durables and semidurables associated with extensive margin adjustments, most notably furniture, clothing, and appliances. The services response, which accounts for 30 percent of the total increase in installment transactions, is directed to general services, insurance, and tourism. 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.[33]

The estimated treatment effect on revolving debt is highest for participants closer to the limit, particularly participants who use more than 50 percent of their existing credit limits. To better understand the heterogeneity in spending patterns, and to gain further insight into what marginal spending gets financed by revolving debt, online Appendix Figure A.5 further decomposes the 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 similar for those close to 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.

D. *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

[33] Only a negligible share of the installment purchases are made in lumpy increments, see online Appendix Table A.7. For 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. The ratio of cumulative installment spent (23 cents) to borrowed (10.5) over nine months is compatible with a back-of-the-envelope calculation where borrowers, on average, take four-month installment loans.

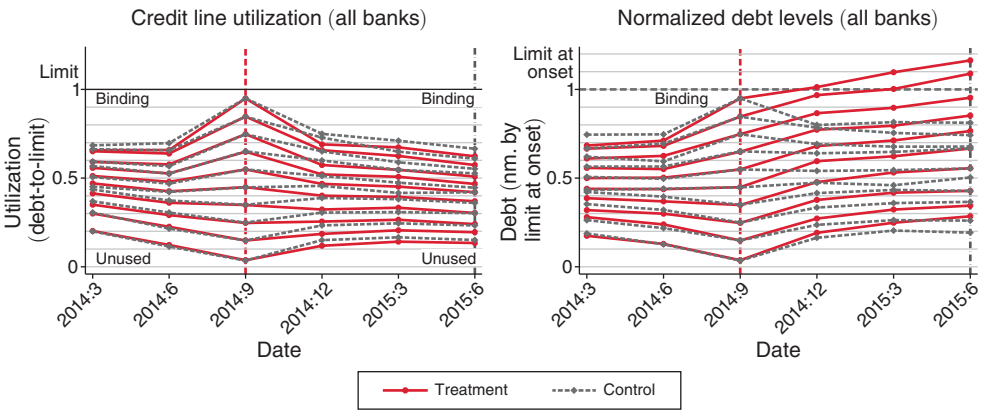

FIGURE 13. DYNAMICS OF BINDING CONSTRAINTS AND PRECAUTIONARY BEHAVIOR

Notes: In these figures the x-axis indicates the calendar date. The dashed and dotted-and-dashed 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 limit, L_{it}. The panel on the right plots a normalized measures of debt levels—the ratio of credit line debt to the limit at the onset, $L_{it=0}$.

at the onset: 0 to 0.1; 0.1 to 0.2, 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 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 them in the future—constitute a sliver of the population.

Analysis of the utilization transition matrix in online Appendix Table A.8 corroborates these findings. For example, of the one in ten (4,786) participants who utilize more than 75 percent of their existing credit limits in the quarter before the experiment, only 28 percent (1,322) were in a similar position one year prior and only 13 percent (644) three years prior. Hence, over a three-year horizon, no more than 1.4 percent of the subjects remain persistently stuck near constraints, as a hand-to-mouth or rule-of-thumb individual who consumes 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, which appear to be persistent.[34]

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

In 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 8 shows that participants with high income risk have larger credit limits and hold higher available credit. Naturally, the level of the buffer 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 percent (33 percent) of monthly income. Similarly, going from the tenth to ninetieth percentile of future (past) income risk is associated with an increase in available credit of 108 percent (62 percent) of monthly income.

IV. Concluding Remarks

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.

[34] 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. See Fulford and Schuh (2020) on the persistence of average utilization rates, and Aguiar, Bils, and Boar (2020) on the persistence of positive-but-low-liquid-asset hand-to-mouth status.

Table 8—What Explains the Buffers?

	Panel A. 100 × (1–*utilization*)		*Panel B.* log *limit*		*Panel C.* log *available credit*		*Panel D. Available credit to monthly income*	
$Buffer_i$	$100 \times \left(1 - \frac{D}{L}\right)$		$\log L$		$\log(L - D)$		$(L - D)/Y$	
	Past	*Future*	*Past*	*Future*	*Past*	*Future*	*Past*	*Future*
β	9.1	11.4	1.2	0.8	1.6	1.1	5.0	7.9
	(6.4)	(5.9)	(0.30)	(0.28)	(0.37)	(0.34)	(0.86)	(0.77)
α	76.6	76.4	9.3	9.4	9.1	9.2	2.8	2.6
	(0.53)	(0.52)	(0.02)	(0.02)	(0.03)	(0.03)	(0.07)	(0.07)
$\beta \times \sigma$	0.60	0.81	0.08	0.06	0.10	0.08	0.33	0.57
$\beta \times p_{10}^{90}$	1.12	1.55	0.15	0.11	0.19	0.15	0.62	1.08

Notes: Estimated coefficients from regression $Buffer_i = \alpha + \beta Risk_i + \epsilon_i$. $Risk_i$ is the individual-level variance of *unpredictable shocks* to quarterly log real income in either *Past*, 12 quarters prior to the onset, or *Future*, 12 quarters after to the onset. $Buffer_i$ is averaged over the four quarters prior to the experiment. Specifications use data on $N = 1{,}981$ participants with a strongly balanced panel of income running from quarters -12 to 12 relative to the onset. $\beta \times \sigma$ and $\beta \times p_{10}^{90}$ stand for the increase in the size of the buffer, corresponding to a one standard deviation increase in Risk_i, or going from the tenth to ninetieth percentile of $Risk_i$. See online Appendix B.4 for details.

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.

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.[35] 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 United States—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

[35] See Kimball (2012) for a proposal along these lines.

contracts in which borrowers carry large balances for extended periods, potentially due to present bias leading to dynamic inconsistencies in debt repayment.[36]

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 create cue-based complementarities. However, borrowers with a systematic bias toward an overoptimistic reading could also perceive the cue as informational about future income prospects.[37] Understanding the importance of these behavioral effects is a promising avenue for future research.

REFERENCES

Adams, William, Liran Einav, and Jonathan Levin. 2009. "Liquidity Constraints and Imperfect Information in Subprime Lending." *American Economic Review* 99 (1): 49–84.

Agarwal, Sumit, Souphala Chomsisengphet, Neale Mahoney, and Johannes Stroebel. 2018. "Do Banks Pass through Credit Expansions to Consumers Who Want to Borrow?" *Quarterly Journal of Economics* 133 (1): 129–90.

Agarwal, Sumit, Chunlin Liu, and Nicholas S. Souleles. 2007. "The Reaction of Consumer Spending and Debt to Tax Rebates—Evidence from Consumer Credit Data." *Journal of Political Economy* 115 (6): 986–1019.

Aguiar, Mark A., Mark Bils, and Corina Boar. 2020. "Who are the Hand-to-Mouth." Unpublished.

Anonymous Firm. 2021. Data for Consumption Response to Credit Expansions.

Ashraf, Nava, Dean Karlan, and Wesley Yin. 2006. "Tying Odysseus to the Mast: Evidence from a Commitment Savings Product in the Philippines." *Quarterly Journal of Economics* 121 (2): 635–72.

Aydin, Deniz. 2015. "The Marginal Propensity to Consume out of Liquidity." Unpublished.

Aydin, Deniz. 2016. "Three Essays in Experimental Economics." PhD Thesis, Stanford University.

Aydin, Deniz. "Consumption Response to Credit Expansions: Evidence from Experimental Assignment of 45,307 Credit Lines." AEA RCT Registry.

Aydin, Deniz. 2022. "Replication Data for: Consumption Response to Credit Expansions: Evidence from Experimental Assignment of 45,307 Credit Lines." American Economic Association [publisher], Inter-university Consortium for Political and Social Research [distributor]. https://doi.org/10.3886/E147523V1.

Baker, Scott R. 2018. "Debt and the Response to Household Income Shocks: Validation and Application of Linked Financial Account Data." *Journal of Political Economy* 126 (4): 1504–57.

Banerjee, Abhijit, Esther Duflo, Rachel Glennerster, and Cynthia Kinnan. 2015. "The Miracle of Microfinance? Evidence from a Randomized Evaluation." *American Economic Journal: Applied Economics* 7 (1): 22–53.

Barro, Robert J. 1974. "Are Government Bonds Net Wealth?" *Journal of Political Economy* 82 (6): 1095–1117.

Bauer, Michal, Julie Chytilová, and Jonathan Morduch. 2012. "Behavioral Foundations of Microcredit: Experimental and Survey Evidence from Rural India." *American Economic Review* 102 (2): 1118–39.

[36] See Skiba and Tobacman (2008) on the relationship between naïve quasi-hyperbolic discounting and patterns of borrowing, repayment, and default; and Kuchler and Pagel (2021) on the role of present bias for credit card paydown.

[37] See 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 (2021) 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*.

Baugh, Brian, Itzhak Ben-David, Hoonsuk Park, and Jonathan A. Parker. 2021. "Asymmetric Consumption Smoothing." *American Economic Review* 111 (1): 192–230.

Berger, David, Veronica Guerrieri, Guido Lorenzoni, and Joseph Vavra. 2018. "House Prices and Consumer Spending." *Review of Economic Studies* 85 (3): 1502–42.

Bernheim, B. Douglas, and Antonio Rangel. 2004. "Addiction and Cue-Triggered Decision Processes." *American Economic Review* 94 (5): 1558–90.

Bernheim, B. Douglas, Debraj Ray, and Şevin Yeltekin. 2015. "Poverty and Self-Control." *Econometrica* 83 (5): 1877–1911.

Blinder, Alan S. 1981. "Temporary Income Taxes and Consumer Spending." *Journal of Political Economy* 89 (1): 26–53.

Blundell, Richard, Luigi Pistaferri, and Ian Preston. 2008. "Consumption Inequality and Partial Insurance." *American Economic Review* 98 (5): 1887–1921.

Bordalo, Pedro, Nicola Gennaioli, and Andrei Shleifer. 2018. "Diagnostic Expectations and Credit Cycles." *Journal of Finance* 73 (1): 199–227.

Brunnermeier, Markus K. and Jonathan A. Parker. 2005. "Optimal Expectations." *American Economic Review* 95 (4): 1092–1118.

Bryan, Gharad, Dean Karlan, and Scott Nelson. 2010. "Commitment Devices." *Annual Review of Economics* 2 (1): 671–98.

Campbell, John Y., and N. Gregory Mankiw. 1989. "Consumption, Income and Interest Rates: Reinterpreting the Time Series Evidence." In *NBER Macroeconomics Annual 1989,* Vol 4, edited by Olivier J. Blanchard and Stanley Fischer, 185–246. Cambridge, MA: MIT Press.

Carroll, Christopher D. 1994. "How Does Future Income Affect Current Consumption?" *Quarterly Journal of Economics* 109 (1): 111–47.

Carroll, Christopher D. 1997. "Buffer-Stock Saving and the Life Cycle/Permanent Income Hypothesis." *Quarterly Journal of Economics* 112 (1): 1–55.

Cochrane, John H. 1989. "The Sensitivity of Tests of the Intertemporal Allocation of Consumption to Near-Rational Alternatives." *American Economic Review* 79 (3): 319–37.

DeAngelo, Harry, Linda DeAngelo, and Toni M. Whited. 2011. "Capital Structure Dynamics and Transitory Debt." *Journal of Financial Economics* 99 (2): 235–61.

Deaton, Angus. 1991. "Saving and Liquidity Constraints." *Econometrica* 59 (5): 1221–48.

Demirgüç-Kunt, Asli, Leora Klapper, Dorothe Singer, Saniya Ansar, and Jake Hess. 2020. "The Global Findex Database 2017: Measuring Financial Inclusion and the Fintech Revolution." *World Back Economic Review* 34: S2–8

Eggertsson, Gauti B., and Paul Krugman. 2012. "Debt, Deleveraging, and the Liquidity Trap: A Fisher- Minsky-Koo Approach." *Quarterly Journal of Economics* 127 (3): 1469–1513.

Farhi, Emmanuel, and Ivan Werning. 2016. "A Theory of Macroprudential Policies in the Presence of Nominal Rigidities." *Econometrica* 84 (5): 1645–1704.

Friedman, Milton. 1957. *A Theory of the Consumption Function*. Princeton, NJ: Princeton University Press.

Fulford, Scott L., and Scott Schuh. 2020. "Credit Cards, Credit Utilization, and Consumption." Unpublished.

Ganong, Peter, and Pascal Noel. 2019. "Consumer Spending during Unemployment: Positive and Normative Implications." *American Economic Review* 109 (7): 2383–2424.

Gerard, Francois, and Joana Naritomi. 2021. "Job Displacement Insurance and (the Lack of) Consumption-Smoothing." *American Economic Review* 111 (3): 899–942.

Gourinchas, Pierre-Oliver, and Jonathan A. Parker. 2002. "Consumption over the Life Cycle." *Econometrica* 70 (1): 47–89.

Gross, David B., and Nicholas S. Souleles. 2002. "Do Liquidity Constraints and Interest Rates Matter for Con sumer Behavior? Evidence from Credit Card Data." *Quarterly Journal of Economics* 117 (1): 149–85.

Guerrieri, Veronica, and Guido Lorenzoni. 2017. "Credit Crises, Precautionary Savings, and the Liquidity Trap." *Quarterly Journal of Economics* 132 (3): 1427–67.

Hall, Robert E., and Frederic S. Mishkin. 1982. "The Sensitivity of Consumption to Transitory Income: Estimates from Panel Data on Households." *Econometrica* 50 (2): 461–81.

Hayashi, Fumio. 1985. "The Permanent Income Hypothesis and Consumption Durability: Analysis Based on Japanese Panel Data." *Quarterly Journal of Economics* 100 (4): 1083–1113.

Heidhues, Paul, and Botond Kőszegi. 2010. "Exploiting Naivete about Self-Control in the Credit Market." *American Economic Review* 100 (5): 2279–2303.

Imrohoroğlu, Ayşe. 1989. "Cost of Business Cycles with Indivisibilities and Liquidity Constraints." *Journal of Political Economy* 97 (6): 1364–1383.

Jappelli, Tullio, Mario Padula, and Luigi Pistaferri. 2008. "A Direct Test of the Buffer-Stock Model of Saving." *Journal of the European Economic Association* 6 (6): 1186–1210.

Jappelli, Tullio, and Luigi Pistaferri. 2010. "The Consumption Response to Income Changes." *Annual Review Economics* 2 (1): 479–506.

Jappelli, Tullio, and Luigi Pistaferri. 2014. "Fiscal Policy and MPC Heterogeneity." *American Economic Journal: Macroeconomics* 6 (4): 107–36.

Johnson, David S., Jonathan A. Parker, and Nicholas S. Souleles. 2006. "Household Expenditure and the Income Tax Rebates of 2001." *American Economic Review* 96 (5): 1589–1610.

Kaplan, Greg, and Giovanni L. Violante. 2014. "A Model of the Consumption Response to Fiscal Stimulus Payments." *Econometrica* 82 (4): 1199–1239.

Kaplan, Greg, Giovanni L. Violante, and Justin Weidner. 2014. "The Wealthy Hand-to-Mouth." *Brookings Papers on Economic Activity* Spring (1): 77–138.

Kara, Hakan. 2016. "A Brief Assessment of Turkey's Macroprudential Policy Approach: 2011–2015." *Central Bank Review* 16 (3): 85–92.

Karlan, Dean, and Jonathan Zinman. 2010. "Expanding Credit Access: Using Randomized Supply Decisions to Estimate the Impacts." *Review of Financial Studies* 23 (1): 433–64.

Kaur, Supreet, Michael Kremer, and Sendhil Mullainathan. 2015. "Self-Control at Work." *Journal of Political Economy* 123 (6): 1227–77.

Kimball, Miles S. 2012. "Getting the Biggest Bang for the Buck in Fiscal Policy." NBER Working Paper 18142.

Korinek, Anton, and Alp Simsek. 2016. "Liquidity Trap and Excessive Leverage." *American Economic Review* 106 (3): 699–738.

Kredi Kayit Burosu. 2021. Consumer credit reports.

Kuchler, Theresa, and Michaela Pagel. 2021. "Sticking to your Plan: The Role of Present Bias for Credit Card Paydown." *Journal of Financial Economics* 139 (2): 359–88.

Kueng, Lorenz. 2018. "Excess Sensitivity of High-Income Consumers." *Quarterly Journal of Economics* 133 (4): 1693–1751.

Laibson, David. 2001. "A Cue-Theory of Consumption." *Quarterly Journal of Economics* 116 (1): 81–119.

Lane, Gregory. 2018. "Credit Lines as Insurance: Evidence from Bangladesh." Unpublished.

Lusardi, Annamaria. 1998. "On the Importance of the Precautionary Saving Motive." *American Economic Review* 88 (2): 449–53.

Mian, Atif, Amir Sufi, and Emil Verner. 2017. "Household Debt and Business Cycles Worldwide." *Quarterly Journal of Economics* 132 (4): 1755–1817.

Nakamura, Emi, and Jon Steinsson. 2018. "Identification in Macroeconomics." *Journal of Economic Perspectives* 32 (3): 59–86.

Olafsson, Arna, and Michaela Pagel. 2018. "The Liquid Hand-to-Mouth: Evidence from Personal Finance Management Software." *Review of Financial Studies* 31 (11): 4398–4446.

Olney, Martha L. 1999. "Avoiding Default: The Role of Credit in the Consumption Collapse of 1930." *Quarterly Journal of Economics* 114 (1): 319–35.

Parker, Jonathan A. 1999. "The Reaction of Household Consumption to Predictable Changes in Social Security Taxes." *American Economic Review* 89 (4): 959–73.

Parker, Jonathan A, Nicholas S. Souleles, David S. Johnson, and Robert McClelland. 2013. "Consumer Spending and the Economic Stimulus Payments of 2008." *American Economic Review* 103 (6): 2530–53.

Persons, C. E. 1930. "Credit Expansion, 1920 to 1929, and its Lessons." *Quarterly Journal of Economics* 45 (1): 94–130.

Poterba, James M. 1988. "Are Consumers Forward Looking? Evidence from Fiscal Experiments." *American Economic Review* 78 (2): 413–18.

Robinson, Louis N., and Maude E. Stearns. 1930. *Ten Thousand Small Loans*. New York: Russell Sage Foundation.

Rutherford, Stuart. 2000. *The Poor and Their Money*. Oxford: Oxford University Press.

Schularick, Moritz, and Alan M. Taylor. 2012. "Credit Booms Gone Bust: Monetary Policy, Leverage Cycles, and Financial Crises, 1870–2008." *American Economic Review* 102 (2): 1029–61.

Shefrin, Hersh M., and Richard H. Thaler. 1988. "The Behavioral Life-Cycle Hypothesis." *Economic Inquiry* 26 (4): 609–43.

Skiba, Paige M., and Jeremy Tobacman. 2008. "Payday Loans, Uncertainty and Discounting: Explaining Patterns of Borrowing, Repayment, and Default." Vanderbilt Law and Economics Research Paper 08-33.

Souleles, Nicholas S. 1999. "The Response of Household Consumption to Income Tax Refunds." *American Economic Review* 89 (4): 947–58.